Does Microfinance have an impact? Three quantitative approaches in rural areas of Bangladesh and Andhra Pradesh, India

Francisco Jose González Carreras

Submitted for the degree of Doctor of Philosophy

Department of Economics

University of Sussex

November 2012

Declaration

I hereby declare that this thesis has not been and will not be submitted in whole or in part to another university for the award of any other degree.

Signature

Francisco Jose Gonzalez Carreras

UNIVERSITY OF SUSSEX

FRANCISCO JOSÉ GONZÁLEZ CARRERAS, DOCTOR OF PHILOSOPHY

DOES MICROFINANCE HAVE AN IMPACT? THREE QUANTITATIVE APPROACHES IN RURAL AREAS OF BANGLADESH AND ANDHRA PRADESH, INDIA.

SUMMARY

Microfinance has attracted, since its inception at the end of the seventies, the attention of many people and institutions, both at academic and donor levels. However, evidence is mixed so far and no definitive conclusion has yet emerged with respect to the positive effects of microfinance, in part because of the great differences among the different microfinance schemes but also because of methodological issues. This work aims to add some further evidence to the impact debate, with three studies in two different rural areas from Bangladesh and India.

The first study is based on the second round of a survey in Bangladesh undertaken by the World Bank. A Propensity Score Matching approach was chosen to study the impact of borrowing on household income and expenditures per capita. In this case positive impact can only be seen in extraordinary expenditures, in particular in house extensions and investments in houses and land, but not in current expenditures or food expenditures

The second and third studies analyse a dataset collected in five districts of Andhra Pradesh, India. The former tries to answer the question of whether borrowing from Self-Help groups (SHGs) has any effect on income and income per capita at household level. Pooled ordinary least squares and difference in differences approaches are used to that end. A significant impact is found in this study on income and income per capita.

In the last empirical work the main interest is focused on the distributional impact, on the understanding that anti-poverty measures should be focused on households at the bottom tail of income and income per capita distributions. Its analysis is based on quantile regression, with cross sectional and panel data approaches. Distributional impact shows, however, that the poorest might not be benefitting from these interventions as much as better-off or not-so-poor households.

Acknowledgements

I am deeply indebted to both my supervisors Andy McKay and Mike Barrow for guiding me through this PhD. Their constant support, encouragement and constructive feedback have been invaluable. I also would like to thank Edoardo Masset for providing the Andhra Pradesh dataset. He also was quite supportive and was always willing to help. Barry Reilly also gave me good suggestions about the quantitative parts. I was especially happy to see him back at the department again. I am quite grateful to all of them.

I would like to thank my wife, Carolina, for all her patience and support while I was writing my thesis. She also took care of the kids when I had to come to the university. And this was too often. Probably I could not have finished this without her. Needless to say, our sons Diego and Nicolas are the cutest children in Europe (UK included).

I would also like to thank my parents for their love and support. Especially my mum, who has suffered a lot this year. I would also have liked to have my father now to celebrate with both.

My family in Spain helped and gave a lot of support to my parents. I guess I will never thank this enough. Estrellas, Javi, Raquel, Canijo, Olalla, Arancha, Jose Carlos, Ana, Alvaro, Sefi, Arturo, Alvaro, Frutos, Carmen, Juan, David. "Mi Manolito" (my brother) and Vicente are not family strictly speaking but I love them as if they were. I also remember some of them that are not with us but they were very important in my life; my grandma, Curra; my great aunt, Vicenta and their cousin Curra.

My family in law was also quite supportive. Especially my mother in law who stayed with us a few months and took care of everything at home. Without her this would have lasted considerably longer. Also Chise, Carmen and Adolfo, who was a great person.

I would also like to remember two friends who died in last few years. Nacho Garcia and Nacho Santamartina. I miss them much. I also deeply admire their mothers, Asuncion and Mari Paz for their strength.

And in Spain many people helped in these difficult times. Antonio y Angela, Tolo, Antonio Aparicio y Antonio Aranda, Marquez, Paquita. Thanks to them all.

To have a lot of friends is quite good because they cheer you up when you need it. The bunch from Guadalupe: Adri, Churum, Juanmi, Jaimito, Bea, Rafa, Andres, Sandra, Josito, Nica, Fatima, Gabriela, Manu, Chelis, Cabaleiro, Antonio Lopez, Richard, Gonzalo, Gabriel Luis. From BBVA-Sirio friends: Rosana, Loli, Tono, Juan Antonio, Perico, Alvaro. From my uni years, Patricia and from Cordoba, Angel, Pilar, Jarca, Rafa. I wish I had had time to meet them more often.

Here at Sussex others had to suffer me with immense patience: Julian, Karla, Genner, Cyntia, Susy, Ashish, Juan, Ana, Hector y Mariana. My neighbours were also very kind with us, Marco and Maido. Thanks.

TABLE OF CONTENTS

List of tables

Table of figures

Table of abbreviations

2SLS	Two Steps Least Squares
AIMS	Assessing the Impacts of Microenterprise Services
ANCOVA	Analysis of Covariance
APRLP	Andhra Pradesh Rural Livelihood Project
ATE	Average Treatment Effect
ATT	Average Treatment on the Treated
BIDS	Bangladesh Institute of Development Studies
CIC	Changes In Changes
CRE	Correlated Random Effects
DFID	British Department For International Development
DPAP	Drought Prone Area Program
FE	Fixed Effects
ITT	Intention To Treat
IV	Instrumental Variable
LAD	Least Absolute Deviation
LATE	Local Average Treatment Effect
MFI	Microfinance Institution
NGO	Non-Governmental Organization
OLS	Ordinary Least Squares
PSM	Propensity Score Matching
QDID	Quantile Differences In Differences
QR	Quantile Regression
QTE	Quantile Treatment Effect
RCT	Randomised Control Trial
RDD	Regression Discontinuity Design
RLP	Rural Livelihood Project
ROSCAS	Rotating Savings and Credit Associations
SGH	Self Help Group
USAID	United States Aid Agency
WB	World Bank

Introduction

Microfinance evolution and main topics

It has been traditionally argued that a lack of credit really frustrates the aspirations of the poor who are unable to finance their projects. As stated in Todaro and Smith (2009), the main problem with the poor is the lack of collateral. Lenders refuse to lend to the poor also because they cannot assess their risk quality. This is a problem of asymmetry of information which is more difficult to surmount in the informal sector. Finally, the costs of lending small amounts of money are greater.

A solution to this led some countries to cap interest rates or create development banks that lent to strategic sectors at capped interest rates. This limitation in interest rates has been called credit rationing in the literature and has been a matter of great concern for a long time. Although these measures were intended to make credit available to those who otherwise would be considered too great a risk or not profitable to lend to, credit ended up reaching mostly well established companies and wealthy people and financing low yielding projects.

This phenomenon is explained in Fry (1978). Riskier projects could not be financed because banks were not allowed to charge the risk premium due to these interest rates ceilings. Thus, they were advised to abolish these interest rate caps. However, Stiglitz and Weiss(1981) proved how even in the presence of financial liberalization in markets with imperfect information these credit restrictions still persist and the poor still have difficulty in obtaining credit.

Muhammed Yunus was a professor at Chittagong University who worried about the poverty he could see all around him. He started lending money to poor people and his initiative was quite successful. He later founded the Grameen Bank working with group lending schemes. Loans were available only to groups that had to self-select to be eligible for borrowing. Members of the groups were jointly liable in the case of any member failing to pay. Thus, borrowers tried to look for those peer borrowers whom they really trusted. This solved the lender's problem regarding asymmetry of information. They also had an incentive to repay loans as this gave them access to further loans. Thus, all members in the group applied pressure when one member did not repay the loan. Overall, problems with collateral as well as screening costs and moral hazard issues were overcome to a great extent thanks to this methodology. This was praised by many very well-known economists (Stiglitz, 1990, Ghatak, 1999, Ghatak, 2000).

The success of microfinance spread around the world, and some other initiatives flourished that used different techniques: to mention a few of the most successful, Foundation for International Community Assistance (FINCA henceforth) through village banking or Bancosol and Caja los Andes, in Bolivia, that used individual rather than group lending (Navajas et al., 2003). The latter institutions belong to a trend within microfinance that started to claim that microfinance institutions (MFIs) should not be subsidized. They contended that self-sustainability was essential to provide the poor with reliable sources of credit that had to be independent of a donor's generosity.

This stress on self-sufficiency and making MFIs commercial banks operating in the market like any other financial institution is a point of great controversy that was very well summarized in Morduch (2000). Commercialization had its main theoretical support in the Ohio School, composed of academics at the Ohio State University. They also argued that these commercial microfinance institutions proved to be quite resilient to crisis (Patten et al., 2001, Rhyne, 2001, Robinson, 2002, Calderón, 2006). All these sources contain examples of commercial microfinance institutions that were successful in this crisis scenario. They also argue that that well assessed commercial microfinance has to be separated from charitable institutions and also from greedy institutions that just pursue profits and maximize their loan portfolio no matter at what cost.

The crisis of Bolivian microfinance is very well described in Rhyne (2001). In a good economic situation the number of microfinance lenders grew and some agents entered the market providing easy loans, even consumption loans for the poor. At some point the numerous agents started to compete fiercely for new customers and this led to a relaxation of the credit scorings. After some time, many households were too indebted and had to borrow from different microfinance institutions to pay for their loans. This phenomenon has been replicated in Andhra Pradesh, where an excessive expansion of microfinance resulted in overindebtedness and social unrest with microfinance institutions (Rhyne, 2010).

On the other hand, commercialization is considered for others as a clear sign of neoliberals taking over. In fact, the microfinance crisis that occurred in Bolivia and its replica at the beginning of this decade in Andhra Pradesh were due to many microfinance agents lending money to borrowers who could not repay their loans. And they should have been aware of that. Also, they were carrying out consumer lending rather than lending to microentrepreneurs, bringing to poor countries consumer credit models that might work in middle or high income countries but that in poor countries would just drive households to household overindebtedness.

Commercial microfinance showed its worst image in April, 2007 with Compartamos Initial Public Offering (IPO). Compartamos was a commercial MFI that grew at a very fast pace thanks to its capacity to charge interest rates that were close to 100%. The IPO was quite successful and 13 times oversubscribed, raising the stock price 22% on the first trading day. The founders made profits of several millions out of a bank that in the past had also received generous subsidies from public institutions (Harper, 2011). Muhammed Yunnus himself, the father of microfinance, also thought that this was setting a bad precedent as microfinance was created to liberate the poor from loan sharks, not to take their place.

However, the main point of this story is whether microfinance is making a difference and, therefore, whether a donor’s or private investor’s contributions are justified. The argument for making profit out of the poor could only be supported if it can be proved that there now exists a customer base that is economically active and also, that this is so because microfinance has ended the capital constraints that have traditionally kept the poor from accessing credit. If credit does not prove to have any effect on the poor, donors will step out of the market, and they will divert their funds to other interventions in which the effect can be proved.

This thesis initially intended to test whether microfinance actually had an effect in times of crisis, allowing households to smooth consumption after shocks had taken place, but ended up trying to cast some light on microfinance impact evaluation. When searching for impact literature in order to have an idea about the evidence of microfinance at a household level, the only thing that we could conclude is that there was no definite conclusion or that there were too many different ones.

A first source of concern was the different types of microfinance reality, Grameen schemes, village banks, individual lending in commercial institutions such as Bancosol or BRI. Also, impact can be measured regarding different dependent variables and, therefore, other classifications can be made. In addition, the approaches for measuring the effects of microfinance were also quite diverse and there were a great number of papers, reports and more informal sources that needed to be systematically narrowed down. These methods ranged from OLS to DID but also qualitative studies and techniques that are not statistically robust like monitoring along time or comparisons before-after.

Fortunately, when this thesis started to take shape in 2008 there were already some sources that had undertaken this task, such as Goldberg (2005) and Armendariz de Aghion and Morduch (2005), although the former is a thorough revision of previous literature while the second is not. The criterion adopted when approaching the questions that we wanted to answer was to use as the theoretical background those impact evaluation works that have a robust statistical/econometric background. They will be enumerated and discussed in depth in the next chapter.

The main questions of this thesis are described in more detail below where the contents of each chapter are discussed. In brief, we wanted to test whether microfinance is having a statistically significant impact on several dependent variables. We chose two different datasets from Bangladesh and India and studied the impact of microfinance at household level. In Bangladesh we found significant impact only in extraordinary expenses. In India the outcomes show also significant impact on income and income per capita using different methodologies. However, in the case of the poorest households the effects were not found significant.

Impact evaluation

Apart from microfinance literature, the other great input of the theoretical pillars of the studies is impact evaluation techniques. White (2007) contends that impact analysis has in practice taken many different meanings and Baker (2000) enumerates the different parts that compose impact evaluation. But currently the main challenge is to find a valid counterfactual. The counterfactual is what would have happened had the intervention not taken place.

In social studies traditionally there was no random allocation of treatment. This was quite problematic and Baker (2000) mentions up to five reasons for why this was very difficult to implement. However, lately Randomized Control Trials (RCTs), in which treatment is dispatched randomly, have rocketed in two ways: in reputation, first, as they are considered the “gold standard” to which any impact evaluation should aspire; and second, the number of studies using the technique, as it has boosted and funds are quite likely to go to this kind of studies.

Provided that allocation into treatment or control group is random, impact evaluation is quite simple as both groups are considered equal and therefore they only differ in their participation status. Impact is calculated by just finding the average differences in outcome variable between the treated and the controls.

However, for some reasons that will be discussed in the theoretical chapter, this technique might be currently over-rated and it is being applied even in cases where doing a proper random allocation of the treatment is quite problematic. An important issue when dealing with microfinance is that the individuals have to take a step forward to be granted a loan and therefore to belong to the treatment group, and this desire to borrow is hardly randomized.

On the other hand, there are studies that do not depart from a randomized dataset and try to solve the problem of the counterfactual using econometric techniques. These control covariates or observed variables and make assumptions about unobserved variables. These might sometimes be quite complex or, as contended in Cameron and Trivedi (2005) with respect to Instrumental Variables (IVs), “heroic” but on many occasions there are also tests that allow rejection or underpinning of the outcomes and inferences. These techniques have evolved as well in recent years and are now applied more soundly and studies are currently more refined than they used to be.

Another concern with respect to these quasi-experimental studies is the quality of the datasets. Sometimes the sampling techniques are not the most adequate and also there are problems in the design of the questionnaires, with questions in the first round of the survey that are formulated in a different way in the second, for example.

In the particular field of microfinance, RCTs are relatively recent and the literature is not as rich as the case of quasi experimental studies. There are also some concerns with randomization as it is quite difficult to assume in many occasions that the attribution of the treatment-control status is purely random. The main drawback for quasi experimental is the quality of the data with poor design of surveys that lack proper control groups or baseline surveys run before the intervention takes place (Roodman and Morduch, 2009, Copestake et al., 2011).

This kind of impact studies focuses on very narrow questions. Qualitative data have proved to be of invaluable help when understanding the processes taking place at a household level as is seen in Collins et al. (2009). They provide inside information, helping to understand outcomes and raising additional questions.

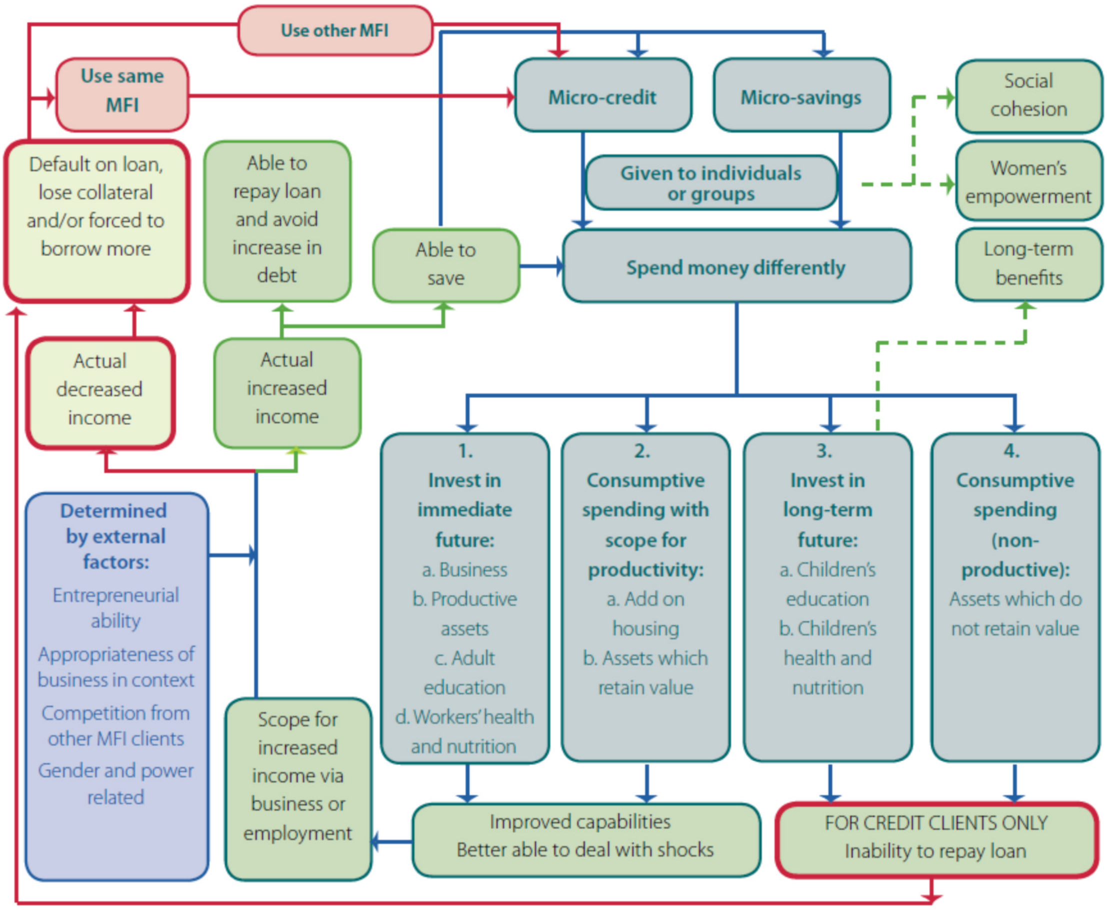
Impact Evaluation in Microfinance

Microfinance essays are crammed with individual examples to illustrate microfinance. The best known is the case of Sufiya, mentioned in Yunus (2007). She was a poor woman who needed to buy bamboo in order to produce stools that she would later sell in the local market. She needed to borrow from a moneylender who fixed the interest rate himself and Sufiya had to survive on just a few pennies a day. Yunus felt that he needed to do something to end slave labour and decided to start his microcredit experiment in the town of Jobra. Armendariz and Morduch (2010) mention the case of Braulia Parra, a woman living in a poor dwelling, who after ten loans was able to install a toilet and a shower at home. Rhyne (2001) also speaks about the thriving knitting sector in Bolivia, and describes how she visited a house that hid German knitting machinery worth more than half a million dollars. They were customers of Bancosol, a microfinance institution.

Despite the power of these images they could be showing a rosy picture of microfinance and impact should be measured with a more scientific approach. The interest is not whether a few individuals are benefitting from microfinance but whether, on average, microfinance is lifting people out of poverty. For that reason, it is important firstly to establish the means through which access to microfinance can improve customers' lives. In addition, it is important to acknowledge the possible presence of selection bias and endogeneity in the analysis which might lead to wrong conclusions.

Access to microfinance allows the poor to have a reliable source of funds that in general is more affordable than moneylenders. Also, they are able to buy their supplies in bulk, getting better prices, increasing their profits, and so on. However the channels through which microfinance contributes to increasing the welfare of the poor are complex. An outline of causality relations (Figure 1) is contained in Stewart et al. (2010). They try to consider all the possible impacts of microfinance. On the positive side, microfinance can become a virtuous circle where access to loans leads to increasing income, which allows expansion in business, education, health and so on (blue arrows and frameworks in Figure 1). Group lending may also increase social cohesion or women's empowerment when group lending is present (green arrows at the top right corner, discontinuous signifies that they found no evidence of that). Microfinance can also end up in decreased income and defaulted loans, which in the graph is represented with red arrows.

Figure 1 Causal chain for microfinance impact on poor people. Source: Stewart et al. 2010



In their theoretical framework they contend that money can be saved or spent in business, training, or worker's nutrition; also, in productive assets or assets that retain value. In these cases, the reward of higher income can be obtained in the short term or immediate future. Rewards for spending on children's health or education might take longer and in the meantime foregone income might lead the household to default. This might happen as well when too much extra income is diverted to consumptive expenditure. The diagram is schematic and might oversimplify some causal relations. For instance, expenditure in nutrition or worker training might not necessarily lead to higher income. Nonetheless it is an illustrative way of showing mainly the income effect of microfinance.

Armendariz de Aghion and Morduch (2010) also warn about the substitution effects that might arise. The increased opportunity cost of females staying at home taking care of the children might lead to decreasing fertility rates. But consequences of access to microfinance are not limited to this, which might be interpreted as a positive outcome. The rise in cost of foregone income can also bring about a reduction of schooling hours of children as they might need to take on household tasks. Some evidence of this is mentioned in Stewart et al. (2010). This substitution effect might counterbalance the income effect and jeopardize the future of the younger members of the household.

Establishing a causality relationship between microfinance and an increase in expenditures, income, female labour supply or bargaining power has been the aim of numerous studies that seek to better understand the way microfinance can improve living conditions for the poor. This task is, however, difficult mainly because a plain comparison between borrowers and non-borrowers can be misleading. The act of borrowing involves the individual deciding to take a step forward with the aim of improving his/her living conditions. Thus, borrowers are self-selected and they might have different characteristics to non-borrowers and therefore the estimates obtained from this simple comparison might be biased. This type of bias is called selection bias and is explained because individuals with more entrepreneurial spirit, business skills, tenacity, perseverance or risk-willing are more prone to take loans. Even in the absence of microfinance programmes, these individuals might on average be better off than non-borrowers.

This take us to the problem of counterfactual, that will be discussed further in the next chapter: what would have happened had this individual not borrowed from microfinance. Given the fact that the same individual cannot be compared at both status (borrower and non-borrower) we need to find a credible counterfactual in order to compare them and to establish the causality between microfinance and the dependent variable.

Another issue when evaluating the impact of microfinance is endogeneity. This can also delude the researcher because the impact observed might not be caused by microfinance increasing individual income, for example, but rather by the fact that the higher an individual's income, the more prone they are to borrow from microfinance institutions. This reverse causality, thus, could be leading the evaluation results and bring the researcher to overoptimistic conclusions.

Randomized Control Trials (RCTs henceforth) are considered to be the best way to overcome these bias and endogeneity issues. In them, the intervention is assigned randomly to different individuals, households or areas and then treated and controls are compared. There are some issues to do with ethical and other concerns regarding this technique that will be discussed below. In the particular case of microfinance, the main problem is that borrowing involves the decision of an individual and therefore it is more difficult to randomize. Recent literature on impact evaluation of microfinance has tried to sort this problem through measuring, for example, the effect of the Intention To Treat (ITT), which tests whether the availability of credit can improve the life conditions of the poor (Banerjee et al., 2009). Other approaches try to assign randomly the loans or grants to individuals or microenterprises (Karlan and Zinman, 2007, De Mel et al., 2008, Karlan and Zinman, 2009).

However, most of the literature is still based on quasi-experimental designs where the samples of treated and controls are not randomly selected. Our work is a set of studies based on quasi-experimental techniques. Issues of self-selection or endogeneity are approached through the use of econometric techniques such as Propensity Score Matching or Instrumental Variables that try to overcome these problems.

The present study

The core of the present study is the development of three different impact studies using different techniques over two different datasets. The first dataset is a very well known survey taken in Bangladesh in 1991-92 and 1998 which was the base initially for two important sources of microfinance impact: Khandker (1998) and Pitt and Khandker (1998). In both it is claimed that microfinance impact is positive and significant. These outcomes were amply contested in the microfinance impact evaluation literature. The second round in 1998-99 tried to track the first round's households and also extended the survey area to additional *thanas* increasing the number of households.

The second dataset comes from a survey in Adhra Pradesh, India. The survey was not purposely developed to study microfinance impact but rather the effect that watershed projects can exert on surrounding villages. These projects in some cases also involved the implementation of a microfinance scheme as a complementary tool against poverty. In areas where the project did not include this microfinance component, the possibility of borrowing was provided by the World Bank (WB) initiative called *Velugu*. In all areas the microfinance schemes followed the village bank model.

The source is panel data that occurred in five inland districts of Andhra Pradesh in 2005 and 2007. They are overall poorer than coastal districts. The main flaw of this survey is that the microfinance projects were already present in the field when the survey took place. Therefore, it was not possible to control for differences between borrowers and non-borrowers before microfinance was available. This dataset is the base for the second and third empirical chapters.

The structure of the thesis follows with the explanation of the reasons and the questions answered in the three papers.

The present **Introduction**

Chapter one, which is split into two main parts. The first part is a necessarily brief literature review of impact evaluation in the field of microfinance interventions, starting with RCTs and following with quasi experimental studies. The second part of the theoretical background describes the main techniques used in the core applied chapters, although within these chapters more succinct discussion about some details is included as well as an explanation of some additional techniques used to underpin the outcomes.

Chapter two contains the first empirical approach. The intention was to try to replicate the outcomes of Khandker (2005) using both 1991/92 and 1998/99 rounds of the Bangladeshi dataset but using a difference-in-difference matching estimator, which had proved to perform very well in Smith and Todd (2005).

However, the dataset for the first round was very poorly documented and the Propensity Score Matching approach had to be restrained to the second round. In fact, since we started working on this paper in 2007 two sources have appeared that attempt a Propensity Score Matching on the first round: Chemin (2008) and Duvendack and

Palmer-Jones (2011). In the latter, the authors mention the problems obtaining the data and their poor documentation.

The main question of this chapter is to find whether borrowing from microfinance has an effect on per capita income or expenditures of different types at household level and test whether our outcomes agree with the previous literature. We split expenditures in different categories, differencing between current expenditures and expenditures in household repairs or investment in home/land. Although this split in expenditures could not be found in the previous literature, we thought that this distinction was relevant given that expenditures (investment) in land are hardly comparable to others such as haircuts. There are other points in which the study is innovative. First, it uses the whole sample of households included in the second round of the Bangladeshi survey. In the literature only the 1,638 households that could be retraced from the first round have been studied whereas the total sample is of 2,599 households. Regarding the methodology, we study the impact at household level and we use a more thorough application of the Propensity Score Matching technique than Chemin (2008) and Duvendack and Palmer-Jones (2011). In particular, we test for matching quality for all different algorithms, and find that only the kernel algorithm kept consistently its good quality figures across the different models and dependent variables. Finally, a sensitivity analysis suitable to this kernel algorithm is carried out.

A consistent and significant impact was found only in those extraordinary or non-current expenditures per capita, in particular in home extensions and investments. The absence of impact in current or food expenditures per capita fails to confirm the outcomes found in Khandker (2005) with panel data techniques.

Chapter three contains the second empirical chapter. The dataset used is the Andhra Pradesh survey.

The main question in this chapter is to test the impact of microfinance at household level on income per capita and income. For this purpose the econometric techniques used are panel fixed effect, panel instrumental variable and MM estimator. The instrumental variable attempts to control for potential endogeneity. In addition, the implementation of the MM estimator is explained because of the presence of many outliers that could be biasing the estimates of the former two techniques.

In summary, the impact of borrowing from microfinance is found to be positive and significant and results are robust to the presence of outliers and to the withdrawal of observations with negative income per capita The main contribution of the study is to apply these techniques to an original dataset that had not yet been the subject of published studies.

Chapter four is the third and final empirical chapter. The methods used in the first and second approaches assume a homogeneous impact for the whole outcome distribution. However, this assumption might be too strong as some other sources had pointed before (Hulme and Mosley, 1996, Copestake et al., 2005, Coleman, 2006, Kondo et al., 2008). They tried to find if microfinance shows different effects for poorer or less influential individuals. The question that this chapter tries to answer is whether the effect of microfinance varies at different quantiles of the income per capita and income distribution – in other words, whether microfinance is complying with its target of bringing welfare to the poorest.

The main contribution to the previous literature is the methodological approach. With quantile regression we do not limit the impact evaluation to different groups of individuals, as was the case of Coleman (2006) or Kondo et al. (2008). Also, it avoids the potential truncation problems that could arise in Copestake et al. (2005) and Hulme and Mosley (1996). In addition, quantile regression allows not only for improvement on these former attempts but it is also less sensitive to outliers than least squared methods and thus quite convenient given the characteristics of our dataset. We also combine quantile regression with panel data using the Correlated Random Effects model, borrowing from the theoretical framework contained in Chamberlain (1982) and Chamberlain (1984) and following the empirical application to birthweights in Abrevaya and Dahl (2008). As far as we are aware there are no studies that use this approach in the field of microfinance impact evaluation, although we also consulted a source that applied it to the impact evaluation of road construction in developing countries (Khandker et al., 2009).

The analysis confirms a positive and significant impact at the middle of the outcome variables' distribution. Thus, the positive impact in the first paper is clarified in this second approach where it can be limited to households that are neither at the top nor at the bottom of the distribution. This is of great importance from a policy point of view as it makes it possible to establish in what part of the population the interventions are effective. Confirming the previous literature, the poorest do not seem to be getting any advantage of the microfinance blessings.

Conclusion: This summarizes the findings and discusses what can be learnt from the present study. First, our outcomes only show consistent impact effect on extraordinary expenditures per capita and never on current or food expenditures per capita as in Khandker (2005). In the second paper we also find a significant and quite robust positive impact. This is slightly moderated when the method less sensitive to outliers is applied. We find also that even when the impact is significant, it is not noticed by the poor as had been suggested in some of the previous sources.

Thus, evidence of impact is found on some extraordinary expenditures, suggesting that loan funds might be diverted to alternative ends. Also, evidence of positive impact is found on income and income per capita. However, households at the bottom of income and income per capita distributions did not seem to notice this impact. Therefore, the utility of microfinance to pull the poorest of the poor out of poverty might be put into question.

Chapter 1: Impact Evaluation and Microfinance: Review of literature and techniques.

The present chapter intends to describe and discuss the theoretical background and review the literature on which the thesis is based. The chapter is divided into two parts. The first part is in turn divided into two main bodies. The former explains the main issue of impact evaluation which is the problem of the counterfactual and how selection bias can be avoided. The latter discusses two different approaches to overcome the selection bias problem: Randomized Control Trials (RCTs) and quasi experimental methods. In particular, the literature on RCTs and quasi experimental methods applied to microfinance impact evaluation is reviewed.

The second part of this first chapter tries to describe in more depth a few quantitative techniques used in the empirical chapters so that the datasets can be described and the main outcomes discussed in later chapters rather than discussing the methods in detail in the empirical chapter.

Part I: Impact Evaluation and Microfinance.

Impact evaluation intends to find the effects of a particular intervention. The main difficulty in this task is to isolate its effect from other factors that can also affect the outcomes but cannot be attributable to the intervention. High quality impact evaluations, in the words of the International Initiative for Impact Evaluation (3ie) [1], "measure the net change in outcomes amongst a particular group, or groups, of people that can be attributed to a specific program using the best methodology available, feasible and appropriate to the evaluation question(s) being investigated and to the specific context".

Impact evaluation is one of the components of a comprehensive evaluation, which also includes other tasks: monitoring, process evaluation and cost-benefit analysis (Baker, 2000). Although they are quite important parts of the whole evaluation process, the following studies are strictly concerned with impact evaluations and other components will be only mentioned when necessary.

[1] http://www.3ieimpact.org/doc/principlesforimpactevaluation.pdf accessed 13/10/2011.

With the spread of microfinance, the increase in the number of sponsors or donors and the allocation of increasing sums of funds to support these activities, impact studies started to be necessary. The aim was to study the effectiveness of microfinance and see whether it is preferable to alternative interventions, such as cash transfers, that otherwise could have been undertaken with the same resources. As said above, evaluation is composed of other analysis and the cost of the interventions also had to be taken into account. Cost-benefit analysis has also been developed in microfinance to put into comparison with alternative uses of resources (Chemin, 2008).

Microfinance impact studies can be very diverse and attempts to classify them have been developed in literature. A popular classification of microfinance impact literature is given by Copestake et al. (2001), with regard to the sophistication of the approaches. This has been quoted in (Goldberg, 2005) and (Tedeschi, 2008), among others. It splits impact studies into three groups. The first is composed of “statistically rigorous studies” with a heavy load of quantitative work. The second, by contrast, relies more on qualitative monitoring and is closer to market research. Finally, the third takes from both approaches but it is still statistically consistent to be considered as a “scientific” research.

The three empirical chapters in this study will basically deal with the first group that intend to be statistically robust. The second type is widely used by practitioners. An example on the latter could be reflexive evaluations in which households are compared before and after the program and for a period of time. Important agents in the microfinance world such as Grameen Bank or Imp-Act use them. They provide insights on the mechanisms through which interventions might work or fail and sources such as Collins et al. (2009) have proved how important qualitative information is. However, they neither solve the counterfactual problem (what would have happened had the program not taken place?) nor are statistically robust (Goldberg, 2005). Hulme (2000) does a similar classification and defends the superiority of the third approach that allows room for rigour in the techniques but is also enriched with the know-how of the staff. In fact, a complementary qualitative component would have been desirable in our studies but there was not any information of this kind available.

Others classifications have arisen, such as Sharma and Buchenrieder (2002) which distinguish between “investment-led” and “insurance-led” studies. The former tries to find out the effect of microfinance on income, wealth or consumption. In the latter the outcome variables are consumption smoothing or others that measure some sort of attenuation of domestic shocks. It is a measure of the "palliative" effects of microfinance. Nonetheless, there might be as many classifications as criteria. For example, classifications regarding whether the approach is randomized or quasi-experimental or classification by the technique used (PSM studies, DID studies...) or regarding the outcome variable. Discussions about these different approaches will be tackled later in this chapter.

Apart from these classifications, there is a clear need for new studies casting light on the effects of microfinance. The task is vast, given the difficulties in having appropriate datasets, the different types of microfinance schemes, the hugely different contexts where microfinance is developed and the different outcomes that can be measured. Our approach restricts the study to two different and well known microfinance schemes, the Grameen approach and village banks. The datasets come from Bangladesh and India (Andhra Pradesh) and the dependent variables are narrowed down to a few: income, income per capita and expenditures of different types, all at household level.

In order to find out the impact of microfinance on these outcome variables, we studied the Andhra Pradesh dataset through two different methodologies. In a first instance, given the controversy with the impact evaluations in Bangladesh developed in Khandker (1998) and Khandker (2005) we question whether some of the impact evaluation outcomes in the latter would be the same if we use Propensity Score Matching and extend the sample to all the surveyed households.

After that, following Khandker (2005) and Tedeschi (2008) we applied a panel fixed effects approach in our aim of isolating the impact estimates from selection bias. In the final empirical paper, a more focused picture of the impact at different points of the outcome distributions was sought through a quantile regression inspired by Abrevaya and Dahl (2008).

The present chapter intends to be a literature review of quantitative microfinance impact studies and also a theoretical background of the techniques that will appear in the empirical chapters. It is organized as follows. The second section contains a discussion about causality, internal validity and the counterfactual problem in impact evaluation. The third section is split into two main parts. The first reviews some RCTs studies. The second focus on quasi-experimental ones. In the fourth part the techniques used in the quantitative chapters are explained and discussed.

Impact evaluation and the counterfactual problem.

Impact evaluation is about establishing the effects of interventions in the targeted population. The main issue is to establish whether the differences in the outcome variables between the treated and controls can be attributed to the intervention or to some kind of circumstance or variable that can potentially be controlled for. This involves the causality problem or whether the variable A is the cause of variable B. Correlation does not necessarily imply causality. Guo and Fraser (2010) explain that A and B might be correlated because:

- C determines both A and B: this is called spurious correlation. When C disappears, the correlation ceases to exist.
- A causes B: Even when we control for a set of covariates, the correlation between A and B remains.
- B causes A and in this case correlation doesn't say anything about the direction of causality.

Literature has established three conditions that have to be complied for when causality takes place. They are enumerated in Guo and Fraser (2010), quoting Lazarsfeld (1959):

- For A to be the cause of B, A has to happen before B.
- Both variables have to be correlated.
- This correlation cannot be spurious correlation.

These main conditions are the pillars for the concept of internal validity. It can be said that internal validity holds when the impact measured in an evaluation can be attributed to the intervention itself and not to changes in alternative covariates that can also affect the outcome. If these covariates exist and affect the outcome, the estimation is not internally valid any more and it is said that the estimation is biased. Bias can be due to many factors, enumerated in Shadish et al. (2002) but they are normally broadly referred to as "bias" as a general term. Guo and Fraser (2010) use the term "selection bias" in broad sense, including in it different types of bias such as self-selection, bureaucratic selection, geographic selection, attrition selection, instrumental selection or measurement selection.

In the search for the value of the impact of a policy, the main issue is the search for the counterfactual. This is a potential outcome, what would have happened had the treatment-assigning event not happened. Obviously, it is impossible to compare one observation with itself under two different circumstances: participation and non-participation. Therefore the comparison has to be done over other individuals or households.

The Neymar's-Rubin counterfactual framework of causality represents the two possible statuses of each individual as follows:

$$Y_i = D_i Y_{i,1} + (1 - D_i)\ Y_{i,0} \tag{1.1}$$

Where Y_i is the value of the outcome variable for the ith observation, $Y_{i,1}$ is the outcome of the ith observation under participation and $Y_{i,0}$ under non-participation (the first subscript i stands for the ith observation, the second 1 or 0 stands for its participation status). The treatment and non-treatment is given by D_i which takes the value of one for participants and zero for nonparticipants. Thus, the outcome variable Y_i will take the value of $Y_{i,1}$ or $Y_{i,0}$ depending on how the terms D_i and $(1 - D_i)$ "switch" the formula (1.1), giving the "on" or "off" to $Y_{i,1}$ or $Y_{i,0}$.

This theoretical framework contends that program impact is found by subtracting the mean outcome of the control group from the mean outcome of the treatment. Thus, the impact of the intervention would be given by the formula:

$$Treatment\ effect = E\left(Y_{i,1} \middle| D = 1\right) - E\left(Y_{i,0} \middle| D = 0\right) \tag{1.2}$$

According to this, the impact can be found out from the observed outcomes of treated and nontreated observations. However, the interest of the researcher would be exactly not on $E\left(Y_{i,0} \middle| D = 0\right)$ but on $E\left(Y_{i,0} \middle| D = 1\right)$ or the non-participant outcome but in the participation status, which is nothing else than the counterfactual of $E\left(Y_{i,1} \middle| D = 1\right)$ but this is not observable in the real world.

This plain comparison between participants and non participants is very likely to provide with a biased impact estimation due to some characteristics or covariates that can affect systematically the outcome variable. Participants and non participants are not two groups which differ only in their participation status. Rather than that, there are other characteristics that make them different and affect both their decision to participate and the outcomes. They would also be different in the absence of intervention. Thus, before attributing differences to intervention it has to be discarded that they are not explained by confounding characteristics.

Another important point when starting an evaluation study is to decide the object of the study or what is the type of impact that we want to get. The two most important measures of interest are the Average Treatment Effect (ATE) and the Average Treatment effect on the Treated (ATT). The former is concerned with the average effect of the treatment on the whole population. The latter measures the effect of the treatment on target individuals for the program. In the discussion about the most convenient measure, Heckman, in some of his works has contended that finding out the ATE would not be correct if the aim is to study the impact of a particular policy (Heckman et al., 1996; Heckman et al., 1997). This policy will normally be focused on some targeted population and therefore it would be the effect on this target that should be studied. ATE would provide the effect of the policy for the whole population including even those whom the policy is not aimed at.

We are more concerned about those for which microfinance is designed and therefore in ATT. Following the definition in Caliendo and Kopeinig (2008, page 34), the expected value of ATT is "the difference between expected outcome values with and without treatment for those who actually participated in treatment".

In general the ATE is written as in (1.2). The expected value of the ATT, however, would be written as follows:

$$ATT = E\left(Y_{i,1} - Y_{i,0} \middle| D = 1\right) = E\left(Y_{i,1} \middle| D = 1\right) - E\left(Y_{i,0} \middle| D = 1\right) \qquad (1.3)$$

As already stated, $E\left(Y_{i,0} \middle| D = 1\right)$ is not observable in the real world. In real world we only see:

$$E\left(Y_{i,1} \middle| D = 1\right) - E\left(Y_{i,0} \middle| D = 0\right) \qquad (1.4)$$

Observed difference in the outcome

or the average difference of the participation outcome for those who participate and the non-participation outcome for those who don't participate.

We can manipulate the equation adding and subtracting the same term, $(Y_{i,0}|D = 1)$ to (1.4), resulting in:

$$\underbrace{E(Y_{i,1}|D = 1) - E(Y_{i,0}|D = 1)}_{\text{Average Treatment effect on the Treated}} + \underbrace{E(Y_{i,0}|D = 1) - E(Y_{i,0}|D = 0)}_{\text{Selection bias}} \qquad (1.5)$$

Following the explanation in Angrist and Pischke (2009), if we apply this theoretical framework to a microfinance intervention, thus, the ATT would be the average causal effect of microfinance on expenditures (assuming that expenditures is the dependent variable) for those who actually borrowed, while the ATE would be the average causal effect of microfinance on the whole population.

The observed difference between borrowers and non borrowers also includes the last term, the selection bias, which might be great. Even in the presence of a positive ATT, a negative average difference might be observed if the selection bias is greater than ATT and of opposite sign. It can be also the case that a positive selection bias might make the observed difference positive when the actual ATT is null. Thus, assessing properly the selection bias is a vital issue in order to trust the impact outcomes.

In this aim of controlling for selection bias, the sampling method is essential. If observations are randomly assigned to treated and control groups we would face a Randomized Control Trial (RCT). However, in most occasions the treated are selected in advance and a control group is picked afterwards to match the treated, and thus assignment is not random. In the former case, RCTs, a simple comparison between controls and treated will assess the impact effect. For RCTs $E(Y_{i,0}|D = 1) = E(Y_{i,0}|D = 0)$ and therefore the selection bias in (1.5) is equal to zero. This is because the distribution of covariates in treated and controls are equal thanks to the randomization.

Whether these randomized experiments are applicable to observational studies is something that is still under discussion. Supporters of Randomized Control Trials (RCTs henceforth) called also “randomistas” argue that these are the best way of evaluating social behaviours. Some academics think on the opposite and contend that in these evaluations $E(Y_{i,0}|D=0)$ cannot be adopted as the counterfactual of $E(Y_{i,1}|D=1)$ (Heckman and Smith, 1995).

In the case of quasi-experimental studies, the control groups are not found out randomly and actually on many occasions the treated are selected first and the sample of controls is found afterwards. As no random allocation precedes the group allocation, a simple comparison between treated and controls would result in a biased estimation. This brings about normally an oversampling of treated observations that has to be corrected afterwards.

In order to counteract this bias, several techniques are normally used in impact evaluation literature. Among them are Ordinary Least Squares (OLS), Difference in Difference (DID), Fixed Effects (FE) panel data, Regression Discontinuity Design (RDD), Quantile Regression (QR), Instrumental Variable (IV), Propensity Score Matching (PSM) and others. The feasibility of each technique needs of some conditions to be complied with and their assumptions are also different. The core techniques in the present studies will be OLS, DID, QR, IV and PSM and they will be discussed in depth in this chapter.

Microfinance impact studies:Randomized and Quasi Experimental approaches

Until 2007 microfinance impact studies were qualitative or quasi experimental but RCTs had not been published yet. In 2005 there were two good reviews of the microfinance impact literature up to that date. One was Armendariz de Aghion and Morduch (2005) which is focused only in research with a robust statistical background. The second, Goldberg (2005) is a compendium of more than 100 microfinance impact studies, in which both qualitative and quantitative approaches are contained. These two are invaluable for researchers as they provide with the main sources to be consulted when tackling a microfinance impact investigation.

To that date, the type of microfinance impact literature more relevant for the coming papers could be classified with the criterion of bundles around some sources. The pioneer robust statistical paper (Pitt and Khandker, 1998) brought about some other studies that challenged its methodology and outcomes. Pitt and Khandker (1998) is based on a cross sectional survey that took place in Bangladesh carried out by the World Bank (WB) and the Bangladesh Institute of Development Studies (BIDS) in 1991-92.

The second round of the survey took place between 1998-99 and it also was the source of some ulterior papers. The second group is constituted by Coleman's [2] studies (Coleman, 1999, Coleman, 2006) based on a pipeline survey in Thailand. Finally, the third is a set of studies funded by USAID under the Core Impact Effect initiative. In these, the same technique, analysis of covariance (ANCOVA), is applied to three different surveys from India, Zimbabwe and Peru.

However, in 2010 Armendariz de Aghion and Morduch (2005) and Goldberg (2005) were getting out of date. Some works had been published after them that had to do with the groups of articles above, Chemin (2008) with the Bangladesh group and Tedeschi (2008) with Coleman's and AIMS studies. Some others were not related to these groups at all, as they did not challenge their outcomes nor used their datasets to apply a different technique. But most importantly, RCTs studies mushroomed and introduced a new and powerful tool that needed an explanation and a review of the outcomes obtained to that date.

Armendariz de Aghion and Morduch (2005) and Goldberg (2005) were revised in 2010. In the case of Armendariz & Morduch's it is contained in the second edition of the book (Armendariz de Aghion and Morduch, 2010). Goldberg (2005) has its continuation in Odell (2010) who was assigned the updating task by the Grameen Foundation. Armendariz de Aghion and Morduch (2010) includes a theoretical framework about the problem of the counterfactual and adds the outcomes of some RCTs in microfinance, taking a pro-RCT stand. Odell (2010) is more comprehensive and together with the RCTs studies it includes additional quasi-experimental examples that were not included in Goldberg's version plus other works that are more focused at the macro level (Burgess and Pande, 2005; Kotikula et al., 2010) and out of the scope of this research which is micro-oriented.

Both updated reviews split the studies into two main groups, RCTs and quasi-experimental approaches. This division is also followed in the present chapter. In it,

---

[2] Although the reference used in this paper refers to the article published in World development (Coleman, B. E. 2006. Microfinance in Northeast Thailand: Who Benefits and How Much? *World Development,* 34**,** 1612.), the outcome had already been published as a working paper from the Asian Development Bank in 2002: Coleman, B. E. 2002. *Microfinance in Northeast Thailand: Who Benefits and How Much?* [Online]. Asian Development Bank. Available: http://www.adb.org/Documents/ERD/Working_Papers/wp009.pdf [Accessed 20/10 2011]. Obviously Armendariz & Morduch's and Goldberg's literature reviews refer to this working paper rather than to the article published in 2006 in World Development, although the outcomes are the same.

RCTs are introduced first by a discussion about their strengths and weaknesses. This is followed by a more detailed explanation of Banerjee et al. (2009) in order to illustrate how an RCT is implemented. This part is completed with the explanation and discussion of some other examples of RCTs so far. For the quasi-experimental approaches, we divide them into groups according to the techniques used in the papers.

The latest reviews of microfinance impact evaluation at the moment of writing were Orso (2011) and Copestake et al. (2011). They were only recently available and a bit late for us to take full account of but a brief overview of them can be outlined. The former is mainly a review of some of the evaluation techniques used in microfinance impact studies and raises the difficulties of assessing the impact with quasi experimental approaches. They are, first, that microfinance programs are normally developed in poor areas (allocation bias). Also, participants have to self-select themselves by applying for loans. Finally, that the causation linkages might be more complicated than just establishing a plain relationship of cause-effect between borrowing and the dependent variable.

Copestake et al. (2011) is a systematic review in the form used in other scientific fields, mainly in health literature. They conclude that microfinance impact literature so far has not provided any serious and robust analysis from which we can infer that microfinance has a positive and significant impact on welfare. Studies, they contend, are based on poorly designed surveys and this lack cannot be compensated for with complicated econometric techniques.

Our classification of quasi-experimental studies follows a methodological criterion in order to organize studies around their technical approaches. There are six groups which include regression discontinuity design, difference in difference and panel data fixed effects, OLS, Analysis of covariance, two-stage least squares and propensity score matching.

Randomized approaches.

RCTs have had, for a long time, the reputation of "the gold standard" establishing causality in other fields such as medicine, agriculture, etc. In economics its application is fairly recent but it still holds this reputation. When these RCTs are implemented the effect of the program can easily be found without having to make complicated or implausible assumptions. Basically it can be assumed that the treated and the control groups differ only in the treatment. This prestige may have been fuelled by the classical medical test for drugs in which a part of the sample takes the medicine and the controls take the placebo, although none of them know what they are having (double blind).

RCTs are put into practice in two stages. First the sample of potential participants is drawn at random from the population of interest. In the second the intervention is assigned randomly to a number of subjects in the sample, the treated, leaving out the rest as controls. In the first stage we are guaranteeing the external validity of the experiment, that is, that the outcomes of the experiment can be applied to that population of interest. In the second, it is the internal validity that is guaranteed. As mentioned above, this internal validity is guaranteed as long as it can achieve that the distribution of characteristics in treated and non-treated is the same. This, however, is not often strictly observed in RCTs and issues with internal validity arise that are discussed below. The same happens with external validity as the outcomes of the analysis cannot be extrapolated to other samples as the same intervention could have very different effects for different groups, even in no remote geographical locations.

Duflo et al. (2007) describe how randomization can be incorporated into a research design resembling clinical trials. They also describe four different strategies to tackle the randomization when the pure clinical trial model cannot be implemented: oversubscription, randomized order of phase-in, within-group randomization and encouragement designs. In the first case limited resources allow to allocate them randomly to only a part of the targeted population. In the randomized phase-in type the program is deployed gradually in different areas, allowing us to use as controls those in which the intervention is delayed. In the within-group randomization, randomized assignment is made in the targeted areas, avoiding in some cases the ethical concerns in the former approach that might bring about a great delay in the intervention. In this case the intervention affects only some groups of the targeted population. Finally, in the encouragement design the researchers inform or encourage, previously and at random, some of the possible subjects of the intervention.

Within-group randomization and encouragement design are clearly the weakest methods. In the former the contamination risk is enormous as control and treated groups are very close. In the encouragement design the invitation to participate could be used as a proper IV as long as invitations are random, a mere comparison between participants and non participants would not be adequate.

Despite their prestige RCTs have scarcely been practiced in development until a few years ago, when the number of studies has rocketed. Randomized interventions have been frowned upon for a long time due to many ethical and economic concerns. But other criticisms of RCTs have aroused with the spread of these studies: external validity, compliance, selective attrition and spillovers (Khandker et al., 2010).

The ethical issues arise when randomization come to a point in which for the sake of the experiments services have to be denied to some people that would be entitled to them otherwise (Baker, 2000). The counterargument is that in these projects the budget is in any case limited so that there will always be some excluded subjects, no matter whether the approach is randomized or not. Randomization uses these "unavoidably" excluded subjects as controls taking advantage of this limitation in order to make a better assessment of the intervention and hence to improve policies. White (2007) and Karlan and Goldberg (2007) agree that randomly allocated interventions do not necessarily have to be ethically reprehensible. Political problems are also argued as it might be very difficult to convince some public institutions of the convenience of a randomization approach when they have vested interests in a particular distribution of funds that may eventually optimize their electoral results.

As stated in Rodrik (2008), the internal validity of the papers published in peer-reviewed journals are strictly scrutinized and academics have to devote whole sections in them to justify it. On the contrary these requisites for the external validity are not so tight and the arguments about it or the possibility of generalizing the results are just mentioned or even omitted. According to Deaton (2010) this is of particular concern and he quotes Worral (2007) to mention the case of Opren, a medicine that treated arthritis and musculo-skeletal pain. The drug passed the RCTs tests to be commercialized but then had to be withdrawn from the market because of the high death rate. The reason was that the trial sample was composed of adults between 18 and 65 years old while the medicine was prescribed mainly to elderly. In the same vein, he argues, a randomized educational program in one country might show successful but in the trial there are some characteristics assumed to be constant which are probably not when the program is taken to another country in another continent or even to another region within the same country. Thus, with respect to external compliance, the outcomes cannot be extrapolated to other similar interventions in other countries or even to other regions and therefore this cannot be contended as an advantage over Quasi-experimental methods.

Compliance problems arise when a part of the subjects selected as treated does not take the treatment or when the controls end up being treated. To surmount this problem, Imbens (2010) contends that if the random assignment to treated/control is used as an instrumental variable and the IV model is used, we can obtain the ATT. This method has been called Local Average Treatment Effect (LATE).

Spillover effects happen when the controls benefit from the intervention as well. In both cases the estimates of a RCT will be biased if they are not taken into account. Finally, selective attrition might cause also what is known as the “survivor bias” and this leads to an under or overestimation of the program impact, depending on the dropout patterns. In practice, however, dropout problems have also been handled by RCTs (Banerjee et al., 2009, Copestake et al., 2009).

Summing up, on one hand RCTs supporters argue that RCTs is a better approach to impact evaluation because it relies on fewer assumptions to establish causality. In addition it is not as “data hungry” a technique as other methods and therefore it can be set up with a considerable saving of funds. Contrary to the critics, they claim that there are currently many situations in which the research can be developed as a RCT and this is not being done. The “random” criterion when assigning the resources of the project can be as valid as the “first come first served” in a scenario where resources are scarce and cannot reach the whole population.

On the other hand, for instance, it is argued that “randomistas” seem to try to set up the research agenda around the questions that are suitable to RCTs diverting the focus from other important issues that still need further research. Even worse, they compete for resources to fund their studies that in some occasions do not deal with other pending essential questions in the research agenda (Copestake et al., 2009).

With regard to technical problems, it can be seen that many issues arise at the time of setting up the RCTs and noncompliance, spillover effects and external validity are questions that have to be discussed in many studies. Development surveys are not as straightforward as drug tests and it is very difficult to replicate a pure random experiment because in almost all occasions some conditions are not met and therefore further assumptions and corrections have to be made to apply RCTs methods.

Regarding the cons of RCTs see Deaton (2010) and Rodrik (2008). For the pros, Imbens (2009) and Copestake et al. (2009) contain a good discussion of the convenience of RCTs within microfinance impact analysis framework.

Example of randomized studies

Banerjee et al. (2009) did an experiment with a microfinance institution located in Adhra Pradesh. This institution is Spandana Bank, which started at the end of the nineties and it had by the time of the study, about 1.2 million clients. The authors claim that so far there had been not an experimental approach in which the impact measured was the new access to microfinance due to this activity coming into the market. Although it can be argued that Coleman's approach could be considered as such and Roodman and Morduch (2009) do so, they do not consider this possibility.

The Spandana bank was in the process of expansion and they selected 120 slums or neighbourhoods (*bastis*) in Hyderabad, the capital of Andhra Pradesh. Spandana offers the traditional group credit in which the members of the group are jointly responsible for repayment and doesn't involve further with business training or any other activities. The eligibility requirements are that the member has to be female, between 18 and 59, resident in the area for at least one year (proof of residence or valid identification required) and at least 80% of the women must own their home.

Within the neighbourhoods, the requirements were: no presence of previous microfinance institutions, neighbourhoods with a share of poor people but predominantly not the poorest of the poor and finally they avoided areas where the concentration of migrant construction workers was not high as residency was considered an incentive for repaying, given the fact that they may need a stable source of funding and their growth might be subject to the availability of future, bigger loans. When dealing with delinquency, it is also easier to find people who have been settled for years.

A first baseline survey in 2005 was done over 2800 households in a first round. After this baseline survey, 16 out of the 120 surveyed areas in the baseline were dropped. Thus the experiment is based on 104 areas only. The bank expanded randomly to 52 of these areas between 2006 and 2007. It has to be noted, however, that at the same time there were also microfinance institutions that expanded both to treatment and control areas and therefore pure control areas were not available. The measured impact is done here through the Intention To Treat (ITT), which is somehow shadowed because of the presence of microfinance in control areas as well.

It has to be stated that ITT does not measure the impact of treatment itself but rather it studies what is the impact of having the possibility of taking advantage of an intervention or the impact of the presence of a project on a community. Thus, what it is compared in this study is the exposure to microfinance rather than strictly borrowing from microfinance.

Early in 2007 a comprehensive census was taken. This census was conceived to enhance the baseline survey in 2005 as this was not random and it was focused in the central areas of slums. It did not try to be a follow up of the 2005 survey either because some households were included but others were not.

In addition, a follow-up survey started in August 2007 in order to question about what were the outcomes after the program had been implemented. Eventually, households were followed up between the 12^{th} and the 18^{th} month after funds were available to be borrowed.

One of the issues of this study is that the control areas were contaminated by the presence of new Microfinance Institutions (MFIs). The authors claim that the important point in order to compare treated and control areas is that borrowing from MFIs (not just from Spandana) has to be significantly higher in the case of the treated areas, which it is, 18.6% vs. 5.3%. Then the ITT, rather than strictly the impact of microfinance, is measured averaging differences in both areas over customers and non-customers of MFIs. The model would be (following the paper notation):

$$y_i = \alpha + \beta Treat_i + \varepsilon_i \quad (1.7)$$

where y_i is the outcome variable (these were diverse: revenues, business profits, etc.), $Treat_i$ is a binary taking the value of one when the household is in a treatment area and the ITT is given by the β coefficient. The standard errors are cluster-robust at area level and corrections were made to acknowledge for the oversampling of Spandana customers. The β coefficient gives, in percentage points, the differences in y between the treatment and the control area.

In terms of outcomes, the study finds that Spandana areas have 32% more new businesses than controls. The impact on revenues, profits and employees is not statistically significant. The treated spend more on durable goods and less on "temptation" goods, such as gambling or eating and drinking out. Regarding welfare indicators, the study finds differences neither in women empowerment nor in school enrolment or expenditures in education. The outcomes regarding the effect of microfinance is far from being impressive but it still seems that microfinance fuels entrepreneurship.

Dupas and Robinson (2009) deal with microsavings in Kenya. They randomly provide a saving account to the subjects of the treated group. They exploit the information of the log books that participants had to keep, in a similar fashion to Collins, Morduch et al., (2009). The main findings of this study is that women who had had a saving account for more than 6 months increased their daily private expenditures by 37-44% and the food expenditures by 14-29%. Also, treated women were less likely to sell their assets to face shocks. They did not find any crowding out effect with respect to ROSCAS[3].

In their studies for Philippines Karlan and Zinman (2009) and for South Africa Karlan and Zinman (2007) loan applications that were marginally creditworthy or marginally rejected. In the South African case, there was a final discretional capacity of the loan officer to accept or reject the application. The main outcomes in the Philippines were a positive and significant increase in profits for male borrowers and also a decrease in labourers in the microenterprises. Also, investment in business decreased in favour of education in the case of participants. Finally, the increase in profits was higher in

[3] ROSCAS stands for Rotating Savings and Credit Associations. In them different individuals contribute regularly to a common fund which is given to one of the members that wins the lottery. This is repeated until all the members have received the pot.

households above the median income. In South Africa a positive impact was found on self-sufficiency, food consumption and some well-being measures such as an index for optimism. On the contrary the index of depression was higher among participants and this is a common feature with the Philippines study.

In De Mel et al. (2008) the authors randomly assigned grants to microenterprises in three districts in Southern Sri Lanka and studied their evolution in 11 subsequent waves of surveys. They made up a lottery in which some of the microenterprises were given a grant in kind or cash ranging from 10,000 LKR and 20,000 LKR, equivalent to 3 and 6 times the median profits respectively. The impact was measured on capital stock, profits and hours worked by the owner. The main findings are that these randomized grants increase profits by at least 60% per year, with significant higher impact when the owners are more able, the enterprise has fewer workers and males are in charge of the business. Spillovers effects seem to be negative in the surrounding enterprises, although they could not be assessed for other businesses not included in the sample. The grants also increase the capital stock and the time worked in the business by the owner.

In De Mel et al. (2009) they used the same dataset to test the difference in returns between male owned and female owned businesses. Male owned businesses increased their returns by more than 11% per month. They claim, thus, that they are more prone to be able to face the repayments of microfinance loans.

So far randomized studies are relatively scarce although they will most likely flourish in the coming years. However, some concerns can be raised about them. Compliance problems force Banerjee et al. (2009) to measure ITT or impact of the exposure to microfinance rather than the impact of borrowing. De Mel et al. (2008, 2009) study the effects of randomly assigned grants, but avoid the issue of self-selection or unobservables that lead some entrepreneurs to borrow. A smart study is Dupas and Robinson (2009) although it just deals with the saving side of microfinance. Internal validity, thus, is a great issue because the pure randomization becomes very difficult to achieve when becoming a member of the treated group involves some kind of self-selection.

In the case of Karlan and Zinman (2007; 2009) the impact is only measured on marginally creditworthy applicants, and therefore conclusions about the clearly creditunworthy or undoubtedly creditworthy customers cannot be made. Also, randomization was not strict.

Overall the arrival of RCTs to the impact analysis of microfinance has to be welcomed as it can contribute greatly with new insights to contrast older studies. The simplicity of the technique, not based on sometimes too complicated and unlikely assumptions is a plus that cannot be overlooked. But not everything is suitable for randomization and, as seen above, problems arise with regard to the questions that could be answered or not through this method. To overstate this limitation, Armendariz de Aghion and Morduch (2005) argue that it is better an unbiased estimation of the impact of exposure to microfinance or ITT than a biased estimation of the impact of microfinance. Likewise, Imbens (2010) argues in favour of using the random criterion of assignment as an IV as this can at least provide with LATE ("Better LATE than nothing") . He also contends that the interaction with other impact measurement technique will enrich the knowledge of the microfinance effectiveness.

It is in this context of complementarity where we think that RCTs can be a valuable resource but given the mentioned limitations with regard to internal and external validity, we can hardly share the enthusiasm when dealing with any kind of social study.

Next, some of the quasi-experimental approaches so far will be described and some of their most relevant outcomes commented.

Quasi-experimental approaches

Randomized studies in microfinance have sprung up since 2007 but were very scarce or inexistent before then. The unavailability of this kind of sources in microfinance programs forced the academics to turn to quasi-experimental methodologies instead. In these, the treatment is not assigned randomly. The first and main step in these methodologies is to identify a treatment and a control group. This can be done prospectively, when both groups are identified in advance, or retrospectively if they are selected after the intervention. In many occasions the treated are picked in advance and a group of controls is found afterwards. It is also common to have an overrepresentation of treated individuals in the sample. So far the majority of the impact evaluations in microfinance belong to the latter group (Karlan and Goldberg, 2007).

The identification of the control group in microfinance is vital but particularly difficult. First, because the non random localization of the program might make the analysis be biased. This can be the case when they are placed in worse off rural areas or, on the contrary, better off urban localizations, as can be seen in Latin-American schemes. Second, the plain comparison between customers versus non customers introduces the problem of selection bias as the borrowers choose to be customers themselves. Thus, individuals would probably be different even in the absence of microfinance. Finally, the risk of a biased estimation does not end when the selection bias is sorted, as it can also arise if the dropout cases or the spillover effects are mishandled.

The search for the adequate control group that could sustain a proper impact analysis showed some weaknesses in the early impact studies. Hossain (1988) compared Grameen Bank members with non members and found a great impact of microfinance on members. There was no control for other characteristics of the individuals and therefore these results might well be overstated.

Hulme and Mosley (1996) developed impact studies in several countries. They found that although microfinance has a positive effect on participants on a range of variables, its effect is not so clear when the customers are poorer. However, their work has been criticised from the methodological point of view, in particular regarding their choice of the control group. They used new customers as the control group. As pointed out by Karlan (2001), just self-selection does not necessarily imply that the groups are comparable. Moreover, dropouts are not taken into account and the characteristics of the "survivor borrowers" deserve a deeper study (Armendariz de Aghion and Morduch, 2005; Tedeschi and Karlan, 2007). In our studies, three different approaches were tackled to give an impact estimation which intends to get rid of this bias.

In the present work, the quasi-experimental literature is grouped by its methodological approach. This division is not perfect as some include more than one econometric technique. In this case the main approach is picked for the classification. The first group is composed of works using a regression discontinuity design approach, in the same fashion as the pioneer work by Pitt and Khandker (1998). The second group includes those works that attempt a difference in difference approach or a fixed effects panel data. The reason for mixing these two techniques is that in all the fixed effects panels the dataset had two time periods. In this case, applying a fixed effect or a difference in difference obtains the same estimates. In the third group the technique used is Analysis of Covariance (ANCOVA), and includes three studies funded by the United States Aid Agency (USAID) that tried to set impact evaluation standards with their approach in the field of microfinance with little success. In the fourth, the methodology is a simple OLS, which includes Coleman's studies and others inspired by them. The fifth group contains a set of studies with different approaches, general equilibrium models, two stage least squares and propensity score matching.

Group 1: Pitt and Khandker(1998) and subsequent regression discontinuity design approaches.

Pitt and Khandker (1998) (PK hereafter) was the first important paper with a rigorous load of quantitative work aimed at overcoming selection bias. This work, based on the cross-section survey in Bangladesh in 1991-1992, takes advantage of the landholding eligibility criterion. According to this, only households with less than half an acre of land could become microfinance customers within program villages. This analysis, then, relies on the fact that there should be no discontinuity in income (or other dependant variables) of those just below and above the eligibility rule (Chemin, 2008).

The approach is a Regression Discontinuity Design which is basically a comparison between observations just above and below the eligibility criterion. In this case, the cut-off threshold has been defined through the instrument of the eligibility rule, which was to have 0.5 acres of land or less. This is a smart approach that is based on the supposed exogeneity of two of the necessary conditions to become a borrower. The first of these conditions was to be eligible: the 0.5 acres or less intends to target poorer people. The second was to form a group with individuals of the same gender. However, Roodman and Morduch (2009) question the exogeneity of these two facts that lead the individuals to borrow: the group formation and the eligibility rule. In the first case PK do not explain how the credit groups are formed by village and gender. In addition, regarding the eligibility rule, they question the argument contained in PK about the condition of the exogenous regressor of landownership in previous literature. Eventually, none of these arguments are formally tested in PK.

The non-compliance of the eligibility rule by a significant share of the borrowers is another main flaw in the study. As Chemin (2008) points out, when the landholding criterion is not strictly complied, the regression discontinuity design cannot longer be considered as "sharp", but has to be treated as a "fuzzy" discontinuity design or otherwise it might well be overestimating the effect of microfinance[4]. Finally, Roodman and Morduch (2009) contend that, in general, regression discontinuity approaches use observations within a particular range around the eligibility rule, while in this particular study all the observations are used.

PK found that 100 additional takas of credit increased household annual expenditure by 18 takas in the case of females and 11 in the case of males. They also found positive impact on non-land assets or schooling of children. These results were also shown in Khandker (1998) and propelled the idea of microfinance as a miraculous tool against poverty. Their estimations, for the reasons above, are considered to be overstated (Chemin, 2008; Roodman and Morduch, 2009).

The grouping of the different individuals regarding this landholding criterion is shown in Figure 4, inspired in a chart contained in Armendariz de Aghion and Morduch (2010). In treated villages there are three different groups:

- A: eligible households (0.5 acres of land or less) that borrow.
- B: eligible households that do not borrow.
- C: non-eligible households.

[4] Theoretical background in Regression Discontinuity Design in Hahn, J., Todd, P. & Klaauw, W. V. d. 2001. Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design. *Econometrica,* 69**,** 201-209. and Angrist, J. D. & Pischke, J.-S. 2009. *Mostly Harmless Econometrics: An Empiricist's Companion,* Princeton, New Jersey, Princeton University Press., Chapter 6.

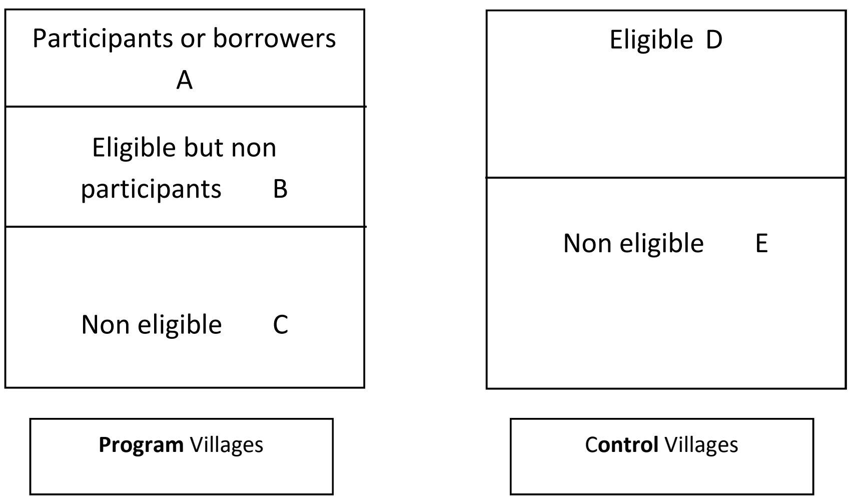
Control villages have two groups:

- D: eligible households.
- E: non eligible households.

These outcomes in any case were striking and have been amply quoted in microfinance literature. The first agnostic commentator on these outcomes was Morduch (1998) who argues that many borrowers did not comply with the eligibility rule. He tests the impact of having access to microfinance, finding the difference between eligible and non eligible individuals [(A&B – D) – (C – E)], controlling for other covariates. His analysis did not find any significant impact of borrowing on expenditures.

Pitt (1999), in turn, challenges Morduch (1998) by criticising that comparing A & B with D is not completely accurate as A still contains non-eligible households that had access to credit. Another main criticism is that Morduch does not account for program allocation bias. The access to credit was given mainly to worse-off villages and not accounting for that would lead to underestimating the effects of microfinance.

Figure 2 Household types by eligibility and villages. Source: Armendariz de Aghion and Morduch, 2010.



Group 2: DID and Panel Fixed Effects

This group of works contains studies that have been designed as difference in difference or panel data. They are bundled together because in all cases except Morduch (1998) they were implemented in two periods of time. In this case, DID and panel FE should produce identical estimates.

Both approaches suppose that there are time invariant unobservables that are correlated with the covariates and that can bias the impact effect. In the case of microfinance, it is usual to assume that these unmeasured characteristics are entrepreneurship, negotiation skills, perseverance and others that would make borrowers systematically different from non-borrowers. Assuming that these characteristics are constant over time, by differencing the variables they are swept out and it is possible to get rid of the bias source.

In the simplest case of two groups (treated-control) and two time periods, the usual approach is to run a baseline survey for the whole sample before the intervention and a second survey after the intervention. The difference in gains from the two groups would be the impact of the program. The time invariant differences between the groups are swept out.

The first study quoted in this group is Morduch (1998), already briefly outlined above. It takes advantage of the particular design of the Bangladeshi database and tackles an approach that is similar to the Difference in Difference (DID) technique but without a time component, which is essential in DID. In his own words "a clean estimate of the average impact of access may be more useful than a biased estimate of the impact of participation" (Armendariz de Aghion and Morduch, 2010 p. 285). This is, actually, the impact of the Intention To Treat (ITT) as in Banerjee et al. (2009). He does not find an increase of household consumption due to access to microfinance but a lower disparity in consumption along seasons. Thus, he concludes that microfinance has a positive effect on household consumption smoothing.

Khandker (2005) uses a second round of the same Bangladesh survey that took place in 1998-99. This second round covered not only the villages of the first round but also included villages from three additional thanas. He estimates fixed effects panel data with the households that could be traced to the second round. In this second survey there were no control villages as the program had extended to all villages. His main findings for 1998-99 are that each additional 100 taka borrowed by females increase annual total expenditures by 20.5 takas, attributing 16.3 to past borrowing (1991-92 survey) and only 4.2 to present borrowing. This might be a sign of decreasing marginal returns to borrowing. Male marginal returns are statistically insignificant.

Given the long period in between, however, the assumption of time invariant unobservables is questionable. In order to avoid measurement errors and reverse causality problems, Khandker (2005) also attempts a panel IV approach. The instrument is the eligibility rule. The Wu-Hausman test rejects the endogeneity of the credit variable, so the DID model is adopted.

Roodman and Morduch (2009) challenges P&K, Morduch (1998) and Khandker (2005). It casts doubts over all of them. They try to replicate these studies and conclude that reverse or omitted-variable causation are leading to wrong estimates. They also contend that the instrumentation strategy is failing and that there is a substantial change in the different subsamples in the credit consumption relationship as well as in borrower's sex and this can explain the differences in impact by gender. Analysis of these three papers leads them to conclude that in social sciences where the endogeneity problem is normally present, RCTs can provide a simpler and neater approach as long as they can get rid of this bias.

Two additional sources can be included in this group, although they are not connected to the Bangladeshi studies. They both use DID. Copestake et al. (2005), mixes qualitative and quantitative approaches. The dataset is gathered in Peru and the microfinance institution is Promuc. The sample is rather smaller than those of other studies mentioned so far. As in Coleman (2006), they first study the impact for the whole sample and then they split it into different groups. In this case it is not rank-and-file members versus committee members but the sample is divided into households below and above the median income level. They find an overall significant impact of microfinance on income. Also, they conclude that the impact of participation for wealthier individuals is around 80% higher compared with poorer individuals. Nonetheless, they clearly state that some selection bias issues might not have been properly addressed. Finally, this splitting and analysing by subsamples might bring about some truncation issues that could have been avoided applying a quantile regression.

The last piece of research in this group is a DID found in Bruhn and Love (2009). The study takes advantage of the simultaneous opening of all the branches of Banco Azteca, using the premises of a well established chain of domestic appliances, Grupo Electra. The opening took place in all those municipalities in which Gupo Electra had a retail shop, as the branches were opened within the shops themselves. They obtained the dataset from the Mexican National Employment Survey (ENE). It contains information collected before and after the appearance of the bank branches and therefore is the only DID approach with baseline information described so far. The other particularity is that the impact is not studied at individual, household or microenterprise level but at municipality level.

It controls for initial differences between municipalities with and without an Azteca branch. The main findings are 7.6% increase in the proportion of informal business, although the increase is only statistically significant for male-owned enterprises. On the contrary, the increase in waged employees is significant only in the case of females. Overall, the increase in total employment, counting new businesses and new waged employees is about 1.4%. The figure is statistically significant but the outcome is not encouraging. It, however, reinforces other studies that find a positive impact and underpins the argument that access to credit in the informal sector can have a positive effect on the welfare of the individuals.

Group 3: ANCOVA approaches from AIMS's Core Impact Assessment studies

Another of the main sources of microfinance impact literature is the "Assessing the Impacts of Microenterprise Services" (AIMS) project which was sponsored by USAID between 1995 and 2002, within the framework of the Core Impact Assessment initiative. They aimed not only at doing studies and publishing reports about the impact of microfinance, but also at settling a "good practice standard" to be followed by future researchers. Among their publications, there are three main reports that were based on longitudinal surveys that took place in Zimbabwe (Barnes et al., 2001), India (Chen and Snodgrass, 2001) and Peru (Dunn and Arbuckle, 2001). All three of them used a common research design and approach.

The studies are based on surveys that were taken in two rounds. In the first round customers from microfinance institutions were randomly selected from a list. Then, samples were picked as controls from the same areas. The impact is measured using Analysis of Covariance (ANCOVA). This method first differentiates between moderating and outcome variables. Moderating variables are those which can influence the levels of the outcome variables. In the first period there is a matching approach in which treated and controls with similar levels in the moderating and the outcome variables are paired together. The assumption is that observations with these similar levels are comparable. In the second period, the effects are measured by finding the differences between the matched observations in the outcome variables.

In terms of regression lines, in the first round ANCOVA estimates two different regression lines, one for the controls and one for the treated. In this period the intercepts of both lines are similar as long as the matching is done between treated and controls with similar levels in the variables of interests. In the second period, however, if the group of participants has on average higher values in the outcome variables, its regression line will have a higher intercept than the regression line of the control group. Therefore, the impact measure will be given by the difference between these two regression lines. The studies measured impact at microenterprise, household and individual levels (Dunn, 2002).

Regarding the attrition bias, AIMS studies distinguish two types of dropouts: panel leavers (households that could not be traced in the second round) and program leavers (borrowers in the first round but gave up in the second). The main aim of the attrition analysis was to test whether the characteristics of panel/program leavers were significantly different from non-leavers at the initial levels with respect to several variables: participation status, gender, sector, location and employment status group. In addition to this, when dropouts were clients in the baseline, some information about these loans was also studied.

In India the study was carried out with Self Employed Women's Association (SEWA) bank customers. Permanency in borrower status was associated with positive effects on food expenditure, household improvements, consumer durables and school enrolment for girls. In Zimbabwe the study was done with the Zambuko trust. Second-time borrowing was associated with an increase in some measures of food and school enrolment for boys, but was found to be insignificant for many other impact variables. Finally in Peru the institution under study was Mibanco. The most remarkable finding in this case was the great positive impact found on income, days of employment and the creation of new jobs.

Criticism can be levelled at these studies. Some of their flaws were already acknowledged in the reports themselves. For example, all the institutions worked with urban but no rural clientele. They did not target the same social strata, and Peruvian Mibanco's customers, for instance, were better off than customers from the other two banks. Finally the baseline survey was not carried out before the intervention and therefore the samples might already be biased, although this is common to most of the DID studies reviewed in this study.

The methodology has also been questioned. Armendariz de Aghion and Morduch (2005) contend that a DID technique would have been more appropriate because ANCOVA cannot control for unobservables and DID, at least, eliminates time invariant unobservables. In fact, none of the relevant works in microfinance impact has used this methodology since then. Probably Tedeschi (2008) contributed definitely to show that this approach was not assessing some sorts of bias properly.

Group 4: OLS approaches

The iconic studies in this group are two papers by Brett E. Coleman, Coleman (1999) and Coleman (2006), which have also been quite influential in later publications. He conducted a survey in 1995-96 in Northeast Thailand in which he gathered information in villages with already established FINCA-methodology village banks and also in other villages where customers and non customers had already been self-selected but the funds were to be disbursed in a year's time, without the knowledge of the borrowers. He found that there were not differences in observables between members and non members in treatment and control villages, thus assuming that selection allocation and self-selection bias was overcome.

He controls for self-selection bias with a member dummy variable. This member variable controls for the unobservables that lead individuals to borrow such as entrepreneurship, willpower and so on. Program allocation bias is accounted for through a village level set of variables. The impact is measured, free of bias, on the covariate of program availability per month.

Coleman (1999) concludes that the impact of microfinance is only significant for two variables: negative on expenditures on men's healthcare and positive in women's indebtedness. He claims that this might be due to the fact that peasants have access to greater loans at lower interest rates at a government bank, BAAC. The size of village bank loans might therefore be too small to be productive and could be diverted to consumption.

The former study is refined in Coleman (2006), using the same dataset. He had observed that most of the relevant positions in the organization of the village banks were undertaken by influential people. Thus, Coleman divided the participant' sample with a status criterion. He changed the specifications and splits the member dummy that controls for selection bias into two different dummies. The first states whether the household had a rank-and-file member and the second whether the household had a committee member. Similarly, access time to microfinance is divided into two "access time" variables depending on whether the household had a rank-and-file member or a committee member.

He concluded that the impact of microfinance on "rank-and-file" members is mostly insignificant. The effects on "committee members" are quite different. They were normally influential people in the village who got involved in questionable practices such as borrowing in the name of individuals who no longer belonged to the bank or in the name of relatives. Coleman reports a positive and significant impact on several of the variables regarding assets, working hours, expenditure on education and even money-lending for these committee members. The outcome of microfinance benefitting wealthier people to a greater extent is consistent with Hulme and Mosley (1996) and Copestake et al. (2005).

In Kondo et al. (2008) we find another pipeline approach. The survey took place in the Philippines, on the islands of Luzon, Visayas and Mindanao, and the intervention was the Rural Microenterprise Finance Project (RMFP). There were two groups of villages (*barangays*), one in the zones where the program had been functioning for some time and expansion zones in which the program was yet to be implemented. In the latter, borrowers had already self-selected but funds had not yet been released. Controls are not new microfinance centres in the same *barangays* in order to minimize the bias due to spillover effects. They also include some old customers such as graduates and defaulted borrowers in the participant' group in order to minimize the attrition bias. The impact of program availability is found positive on income per capita, per capita total expenditures and per capita food expenditures. However, the impact is not significant for the poorer which is in tune again with Hulme and Mosley (1996), Copestake et al. (2005) and Coleman (2006).

Finally, Tedeschi (2008) borrows from Coleman's approach to control for selection bias and applies it to the Peruvian AIMS study dataset (Dunn and Arbuckle, 2001). She contends that some sources of bias are not properly addressed in any of these AIMS studies and this causes an overstatement of microfinance impact.

She first runs an OLS regression in the first round of the survey and she finds no geographical allocation bias. However, the participation dummies are significant, which accounts for a positive selection bias. She later runs a pooled OLS with the two periods and includes borrowing dummies again to control for bias. Her main finding is that there is actually a positive and significant effect of microfinance on weekly and monthly profits of microenterprises, although the impact is lower than in Dunn and Arbuckle (2001). In the latter, she claims, the impact is overstated by selection bias. Finally, her panel approach confirms the main outcomes. A similar approach will be applied to our Andhra Pradesh dataset.

Group 5: General Equilibrium models, Two Stage Least Squares(2SLS) and Propensity Score Matching.

The last classification group is composed of a set of works with miscellaneous approaches from a methodological point of view.

In Thailand, through structural general equilibrium models of growth, Kaboski and Townsend (2005) study the effect of the presence of (pseudo) financial institutions on households that otherwise would have limited access to credit or savings facilities. These economic models are different from all the previous techniques that are also known as reduced-form estimations. They study the direct relationship between an intervention and the outcome variable in the population under examination. The economic models include the interrelationships among different endogenous and exogenous variables and provide a schematic view of the effects of interventions within this created framework. They can show a more holistic view of the possible effects of the policies. They can also become very complex as in the case of macroeconomic frameworks modelling financial regulation or taxes, as the effects are dynamic and heterogeneous in different sectors of the population (Khandker et al., 2010).

The institutions studied are Production Credit Groups (PCGs), which lend mostly cash and also provide saving facilities, women's groups and rice and buffalo banks. PCGs are the most similar to an MFI. The outcome variables under study are growth in assets, the probabilities of consumption smoothing, starting a business, switching the main job or becoming a moneylender customer.

Cash loans (PCGs and women's groups) are associated with stability or expansion of services. These institutions can be associated with growth in assets, in contrast to buffalo or rice banks. Institutions providing extra services such as training, emergency attendance or savings increase the probability of consumption smoothing. Membership of the women's groups increases the probability of switching jobs, having pledged saving accounts and it also increases job mobility and business start-ups. Finally, institutions overall contribute to lessen the reliance on moneylenders.

Kaboski and Townsend (2009) measure the impact of a program called Million Baht Village which consisted of transfer of one million baht to 77,000 Thai villages. The sudden boost in credit availability is a good opportunity to have pre and post-program information using a panel data approach. They use a 2SLS. The impact found is an increase in borrowing, consumption and investments in agriculture. Income from business and market activities is increased. Finally, the increase in wage rates reveals general equilibrium effects that can be extended to non-borrowers as positive spillovers.

Finally, in Chemin (2008) we come back to the first round of the Bangladesh survey (1991-92) to challenge PK outcomes using a Propensity Score Matching approach. The characteristics that a dataset has to comply with in order to be adequate for a PSM study are enumerated in Heckman et al. (1997). Controls and participants should come from the same economic area. All the individuals should respond to the same questionnaire and the set of questions should be rich in order to gather as much information as possible of the relevant variables conditioning participation. The database complies with all of them.

His argument to use this technique is that it does not rely on the eligibility criterion that might bias the calculations in Pitt and Khandker (1998), Morduch (1998) and Madajewicz, (2003). In all of them the underlying assumption is that the eligibility rule is strictly observed when delimiting a group, which is erroneous. It does also neutralize the allocation bias that could have been mishandled in Morduch, (1998), comparing individuals from poorer program villages to the ones from better-off non program villages. In addition, PSM is a non- parametric approach so it does not assume any underlying structure as linear regression would and will only match comparables. His conclusions agree with PK on the positive effect of microfinance on expenditure but to a lesser extent. The impact is estimated as a 3% increase of consumer expenditures, a higher figure than Morduch (1998) but lower than PK whose calculations, Chemin contends, might be upwardly biased.

Finally, Duvendack and Palmer-Jones (2011) use the same dataset and also PSM and point out some limitations of Chemin's analysis, mainly regarding its lack of sensitivity analysis of the estimates to a potential bias from unobservables. They find positive impact on some outcome variables but the estimates were quite sensitive to unobservables. From the methodological point of view, they do not report any test for the matching quality of the different algorithms, which might cast some doubts over some of the estimates.

Table 1 below lists the papers reviewed. The range of dependent variables in these studies is vast and in some papers its number is higher than one hundred. Some kind of grouping was needed in order to tabulate the outcomes. We follow the one used in Copestake et al. (2011) who divided the different dependent variables into three groups, economic, social and empowerment indicators, including some of the following example covariates (in the table the group number is used):

1. **Economic outcome variables**, including business profits and revenues, sales, income/income p.c., consumption/expenditure, assets, employment, savings, debts, poverty indices and others.
2. **Social outcome indicators**: children's school enrolment, school attendance, nutritional status, vulnerability to shocks, social capital, contraceptive use and other.
3. **Political outcome indicator**: women's empowerment.

Table 1 Summary table of reviewed papers

	Study	Dependent variable groups	Technique	Main outcomes
RCTs	(Banerjee et al., 2009) [India]	1, 2, 3	RCT	Measures the ITT. The likelihood of new businesses is higher and statistically significant in treatment areas. No significance is found with respect to business profits, inputs, revenues and employees. Impact on per-capita overall expenditures and non-durable expenditures is insignificant but it is positive and significant on durables, business related durables. No increase in health and education expenditures is found either.
	(Dupas and Robinson, 2009)[Kenya]	1, 2	RCT	Measures ITT, the effect of having assigned to the treatment and of using the account. The main findings are a positive and significant impact on investment in business for women and food and private expenditures. Non-significant impact is found on labour supply, overall expenditures and male investment and expenditures. No crowding out effect with respect to ROSCA is found
	(Karlan and Zinman, 2007) [South Africa]	1, 2	RCT	Profits are increased when owners are males, but not for female-owned. Male owners also increase the school enrolment of children and are more likely to be employed at the family business. Increase of stress is also significant for males. No impact is found on fixed assets, income and expenditures. Formal credit seems to complement rather than substitute informal.
	(Karlan and Zinman, 2009) [Philippines]	1, 2	RCT	They create some indexes. Economic self-sufficiency index, including current employment status and income experience a positive and significant increase for borrowers. The impact on index including decision power and optimism is positive and significant but not so the impact on "investment and durables" index. Borrowers also increase their stress and their consumption. Customers selected randomly did not fall into a debt trap
	(De Mel et al., 2008) [Sri Lanka]	1	RCT	Grants increase profits by 5% per month or 60% per year. Marginal returns highest for more able entrepreneurs and businesses with fewer workers. Impact is higher for male owned businesses and non-significant for female-owned. Grants are also associated with an increase in capital stock and hours worked by the owner. They find also negative spillovers in the economy in the neighbourhood of the granted businesses.
	(De Mel et al., 2009) [Sri Lanka]	1, 2	RCT	They research further the differences between male and female-owned businesses. They do not find significant differences in investment on education, groceries or health. Neither they do on ability, risk aversion or the capacity to increase their hours worked. Male-owners tend to make profitable investment in their enterprises while females did not generate, on average, a sustained source of income from grants.
Quasiexperimental Approaches	(Pitt and Khandker, 1998)[Bangladesh]	1, 2, 3	Regression discontinuity design	Female borrowers increase expenditures by 18 takas per 100 takas borrowed, males by 11 takas only. They also found an increase in school enrolment and health indicators in borrowing households.
	(Pitt, 1999)[Bangladesh]	1, 2	Regression discontinuity design	Questions Morduch 1999 approach. Recalculates impact with additional land specifications and finds PK outcomes quite robust to these changes.
	(Morduch, 1998) [Bangladesh]	1, 2	DID	Criticised PK approach and did not find any significant impact on expenditures as claimed in PK. Microcredit is found to smooth consumption and labour income.
	(Khandker, 2005)[Bangladesh]	1, 2	Panel Fixed Effects	The annual impact of female borrowing on expenditures is 21 takas per 100 extra takas borrowed. Impact of past borrowing is higher than present borrowing. Moderate poverty is reduced by 1.6% per annum and extreme poverty by 2.2% among participants. Attributes 40% of the village-level poverty reduction to microfinance.
	(Roodman and Morduch, 2009)[Bangladesh]	1, 2	Regression discontinuity design, Panel Fixed Effects	Replicate PK, Morduch 1999 and Khandker, 2005. Finds evidence neither of impact on consumption nor of consumption smoothing. Khander's 2005 approach is criticised for its weaknesses in its statistical approach and therefore its outcomes are questioned.
	(Copestake et al., 2005)[Peru]	1	DID	Being a microfinance customer is associated with a higher monthly income. This impact is estimated to be 80% higher for wealthier individuals. Concerns were raised with respect to the methodology.

	(Bruhn and Love, 2009)[Mexico]	1	Panel FE	Impact studied at municipality level. Opening of Azteca branches increased the fraction of informal business owner, male in particular. It also increases the fraction of female wage-earners but not males and impact on income is significant after controlling for time trends. No significant impact is found with respect to the share of people above the minimum wage.
	(Barnes et al., 2001)[Zimbabwe]	1,2,3	ANCOVA	Compares continuing clients with new clients and non-clients. Impact is positive and significant on the number of household durable assets. Departing and continuing clients experience a rise in the education of boys between 6-16 years old, but not for girls. Consumption smoothing effect also observed on departing clients. Limited impact on monthly revenue and assets of enterprises and none on employment. Also training was associated to improvements on management and participation in MFIs increases confidence.
	(Chen and Snodgrass, 2001)[India]	1,2,3	ANCOVA	It compares borrowers vs. only savers vs. non-clients. Overall, borrowers and savers are better off than non-clients. Borrowers show higher income in both periods and savers the highest rate of growth. Borrowers increase their poverty rate in the second period, not the rest of groups. Repeating borrowers have greater income and food expenditures.
	(Dunn and Arbuckle, 2001)[Peru]	1,2,3	ANCOVA	Great impact at enterprise level. Shows a positive and significant impact on net revenues, enterprises fixed assets, employment, sources of input supplies. At household level, income is increased for treated and education expenditures are decreased for new entrant households. At individual level, the feel of being more prepared to face the future is increased among participants.
	(Coleman, 1999)[Thailand]	1, 2, 3	OLS	Finds no significant impact on physical assets, savings, production, sales, productive expenses, labour time, and most measures of expenditure on health care and education. The impact is positive and significant for women's high interest debt, women's lending out with interest and negative and significant on men's health care.
	(Coleman, 2006)[Thailand]	1, 2, 3	OLS	No remarkable impact on rank-and-file members. Committee members experience positive and significant impact on household wealth: women's wealth, nonland assets and consumer durables. It is also positive on savings, women's self-employment sales and expenses and educational expenses for boys at committee member households.
	(Kondo et al., 2008)[Philippines]	1,2	OLS	Impact of the presence of microfinance is positive and mildly significant on income, total expenditures and food expenditures, all in per capita terms. It becomes insignificant and even negative when households are poorer. Also increases also savings accounts and amounts in those accounts. Also increases program client's activities and their number of employees. No significant impact was found on household assets, health or education.
	(Tedeschi, 2008)[Peru]	1	OLS (pooled) & panel FE	Challenges Dunn & Arbuckle, 2001. Increase in enterprises net revenues is still positive and significant but much lower than reported in the former.
	(Kaboski and Townsend, 2005)[Thailand]	1, 2	General Equilibrium Models, 2 Stage Least Squares (2SLS) & Simultaneous Equation, Maximum Likelihood	Production Credit Groups (PCGs similar to credit institutions) and women's groups can be associated with a positive impact on asset growth. Women's group membership increases also consumption smoothing, job mobility and moneylender reliance. PCGs, on the contrary, decreases job mobility, the likelihood of starting a business and is not significant with respect to moneylender reliance.
	(Kaboski and Townsend, 2009)[Thailand]	1,2	2SLS	Million Baht program: boost the availability of credit without crowding out other sources. There is an increase in consumption levels and income growth but the impact on asset growth is negative. No differences between female and male headed households with respect to credit or agricultural income but female-headed show higher business income and lower probability of education expenditures.
	(Chemin, 2008)[Bangladesh]	1, 2	Propensity Score Matching	Estimates impact on expenditures is 3%. Consumption smoothing is not found significant.
	(Duvendack and Palmer-Jones, 2011) [Bangladesh]	1,2	Propensity Score Matching	They replicate Chemin (2008) and add some additional treated and control groups. Their conclusion is that the estimates cannot be trusted as they are extremely sensitive to potential unobservables. They do not test for matching quality and their sensitivity analysis could have been applied to the kernel instead to the nearest neighbour.

Conclusion

This review discusses many of the main references regarding microfinance impact effect. Random studies are a promising alternative or complement to quasi experimental approaches. Some issues have been raised regarding their validity and the randomization processes that have to be considered before taking for granted the randomized experiment assumptions. They are however at a quite early stage with regard to social sciences but their repetition will allow us to test whether the reservations put forward by some top academics (Rodrik, 2008; Deaton, 2010) are confirmed or not. Quasi-experimental studies have a richer background of techniques that try to improve in the quest for unbiased estimates although they need more complex assumptions. However, this is not enough reason to state that they are inferior as impact evaluation techniques.

Outcomes show that overall microfinance makes a difference in some variables and not in others. Although they can be contradictory for the same variable in different studies, the fact that it shows significant in many of them should encourage further study regarding the extent of these effects. The pointed concerns with respect to internal validity of all these studies in Copestake et al. (2011) also have to be taken into account. Better surveying techniques are needed to confirm this optimistic view of microfinance, as in their opinion almost none of the studies so far can support the argument of a positive impact. Another question would be the effect of microfinance at different strata of the targeted population. This has been pointed out by Coleman (2006), Copestake et al. (2005), Hulme and Mosley (1996) and Kondo et al. (2008) and will also be addressed in one of the studies of the present set of papers.

The following sections comprise a brief description of the theoretical background of the techniques used in the present set of studies to find the impact of microfinance. In the first section Propensity Score Matching is described and some differences with respect to OLS are discussed. PSM is applied to the second round of the Bangladeshi dataset in 1998-99 . In the second empirical chapter OLS and DID or panel Fixed Effects are used as in Tedeschi (2008). This will be the first chapter dealing with the Andhra Pradesh dataset. In addition, the second empirical chapter on Andhra Pradesh dataset tries to describe the quantile regression approach. In particular it will be found the distributional impact of microfinance basing on the analysis done in (Abrevaya and Dahl, 2008). The latter, in turn, is based, on Chamberlain (1982) and Chamberlain (1984).

Part II: Techniques of impact evaluation

There are several methods to estimate the impact and each method tackle the problem of the missing counterfactual in a different way. Regarding Blundell and Costa Dias (2000), the appropriate methodology depends on three factors: the information available, the model and the parameter of interest. Khandker et al. (2010) enumerate and explain in detail seven different approaches in their book:

- Randomized evaluations
- matching methods
- double difference methods
- instrumental variable methods
- regression discontinuity methods
- distributional impacts
- structural and other modelling approaches.

In our work some of these are used to some extent and a description of the essential theoretical background of the most relevant techniques used in the empirical chapter is included below.

Matching estimators and Propensity Score[5].

Assumptions

As already seen, in the absence of random experiments, researchers have to turn to quasi-experimental methods to solve the problem of selection bias. Within these, matching is one of the most popular and Propensity Score Matching in particular has been widely used in the last few years. In essence, it tries to resemble a random experiment. Basically the method assumes that, once observables have been controlled for, the differences between the treated and the control group is just participation. Thus, differences in the dependent variable (income, expenditures or any other) between the treated and the control group can be attributed to intervention.

Both OLS and matching methods and PSM in particular rely on this Conditional Independence Assumption (CI) in the sense that they assume that bias is avoided by just

[5] This section relies heavily on Caliendo, M. 2006. *Microeconometric Evaluation Labour Market Policies. Lecture Notes in Economics and Mathematical Systems No. 568,* Berlin, Springer-Verlag., Chapter 1

controlling for observables. But they also have differences, the main being that OLS assume an underlying linear functional form and that PSM is a non-parametric method.

This CI assumption could be described more formally saying that outcomes values are independent of the participation, given a set X of covariates:

$$Y_{i,1}, Y_{i,0} \perp D_i | X_i \qquad (1.8)$$

where $\perp$ means "statistically independent of". Thus:

$$E\left(Y_{i,0} \middle| X_i, D_i = 1\right) = \left(Y_{i,0} \middle| X_i, D_i = 0\right) \qquad (1.9)$$

and therefore the selection bias is not present any more. Apart from *Conditional Independence* this assumption has been named in literature as *Ignorability*, *Unconfoundedness* or *Selection on Observables*.

However, a great size of X_i (number of covariates) might bring about difficulties in the matching process. Rosenbaum and Rubin (1983a) showed that matching can be done more easily conditioning on $P(X_i) = \Pr(D_i|X_i)$. In order to do this, they establish a second condition that has been called Overlapping Assumption or Common Support[6] (CS) condition:

$$0 < P(D_i = 1|X_i) < 1 \qquad (1.10)$$

This second condition entails that all the individuals have a positive probability of belonging to both the treatment and the control group. Rosenbaum and Rubin (1983a) name these two conditions, unconfoundedness and common support, as the "strong ignorability" condition.

When only the ATT is of interest, these conditions can adopt a laxer form. In the case of the unconfoundedness assumption, the following will be enough:

$$Y_{i,0} \perp D_i | X_i \qquad (1.11)$$

also called "unconfoundedness for controls".

---

[6] Support is a statistical concept that includes the values where the density function is found to be different from zero. In this case it would be the values for which the probability is not zero. Ibid.

In the case of CS:

$$P(D_i = 1|X_i) < 1 \qquad (1.12)$$

also called “weak overlap”.

These are enough to calculate ATT, as the $P(D = 1)$ with participation is directly observable.

The overlapping condition establishes that individuals with the same propensity score can be observed in both states. However, Heckman et al. (1997) showed that matching is only justified in the common support range, where the propensity scores of both treated and controls are positive.

Bias

The selection bias is already defined in (1.5) above. Conditioning on covariates and omitting i subscripts, bias is defined as $B(X)$:

$$B(X) = E(Y_0|X, D = 1) - E(Y_0|X, D = 0) \qquad (1.13)$$

The bias is defined over the group of covariates X that are shared by both participants and non participants. Heckman et al. (1996) establish that the support of X for participants is different from that for non-participants. The former is referred to as S_{1X} or $support(X|D = 1)$ and the latter as S_{0X} or $support(X|D = 0)$. The mean of the bias can be defined as follows:

$$\bar{B}_{S_X} = \frac{\int_{S_X} B(X)dF(X|D = 1)}{\int_{S_X} dF(X|D = 1)} \qquad (1.14)$$

where $S_X = S_{X1} \cap S_{X0}$ the common support area or the area where density of X is positive for both participants and non participants. $F(X|D = 1)$ is the conditional density of X given participation or $D = 1$.

The selection bias can be rewritten as:

$$B = \int_{S_{1X}} E(Y_0|D = 1, X)\, dF(X|D = 1) - \int_{S_{0X}} E(Y_0|D = 0, X)\, dF(X|D = 0) \tag{1.15}$$

and they decompose the formula further until they end up contending that selection bias is composed of three different sources of bias:

$$B = B_1 + B_2 + B_3 \tag{1.16}$$

The first source, B_1 can be written as:

$$B_1 = \int_{S_{1X}\backslash S_X} E(Y_0|D = 1, X)\, dF(X|D = 1) - \int_{S_{0X}\backslash S_X} E(Y_0|D = 0, X)\, dF(X|D = 0) \tag{1.17}$$

where $S_{1X}\backslash S_X$ is the support of X given $D = 1$ that is out of the overlap area S_X and $S_{0X}\backslash S_X$ is the same for $D = 0$. This first source of bias comes from the fact that it is sometimes difficult to find good matches or counterparts in the overlapping or common support area S_X.

The second source of bias, B_2 is expressed as follows:

$$B_2 = \int_{S_X} E(Y_0|X, D = 0)\, [dF(X|D = 1) - dF(X|D = 0) \tag{1.18}$$

In this case the problem is of weights within the common support area S_X. This is caused because even within this common support area there might be great differences in the distribution of covariates X for participants and controls.

Finally, the third bias source is given by B_3:

$$B_3 = P_X \bar{B}_{S_X} \tag{1.19}$$

where $P_X = \int_{S_X} dF(X|D = 1)$ is the proportion of density of X given $D = 1$ in the common support area. This third source of bias is given when the unconfoundedness assumption doesn't hold because the observables don't explain participation or the outcomes properly. They are also affected by non-observed characteristics.

How does PSM deal with these sources of bias? The first source, B_1 is avoided by matching observations that are within the common support area only and therefore treated and controls outside Common Support area (belonging to $S_{1X} \backslash S_X$ or $S_{0X} \backslash S_X$, respectively) are discarded. The second problem of reweighting (B_2) is fought through the matching process. In it, the weights of controls would depend on the algorithm used in the matching process. Finally, Heckman et al. (1998) contend that the only source of bias that cannot be avoided in this process is the third (B_3), although they contend that it is the smallest source and therefore outcomes might well be relied upon.

Propensity score vs. OLS in handling selection bias.

Propensity Score Matching and OLS techniques are both based on the Conditional Independence assumption, that is, they rely on the observables to handle the selection bias. The main difference with PSM is that OLS assumes a linear functional form. According to Caliendo (2006), in the case of OLS in the Neymar's Rubin formula (1.1) (omitting the subscripts i) the term Y_1 is substituted by $X\beta_1 + U_1$ and Y_0 by $X\beta_0 + U_0$, where U refers to the error term. In this case the Conditional Independence is given by the formula:

$$U_0, U_1 \perp D|X \tag{1.20}$$

that is, the error term has to be independent of participation given the set of covariates.

The main differences between the PSM and OLS approaches are described below:

- First, PSM is a non parametric estimator and therefore the underlying functional form is not important. In the case of OLS, it is assumed that this functional form is linear. Thus, even when the appropriate covariates are used the estimates could be biased if we fail to include the proper higher order or interaction terms. On the other hand, if the outcome equation happens to be linear and we correctly set up the model, the estimates are more efficient.
- Also, assumptions for the bias are different for PSM and OLS. In the case of the former, it is only necessary that mean of error in the treated, given *X,* has to be equal to the mean of error in the controls. This means that:

$$B_X = E(U_1|X, D = 1) - E(U_0|X, D = 0) = 0 \quad (1.21)$$

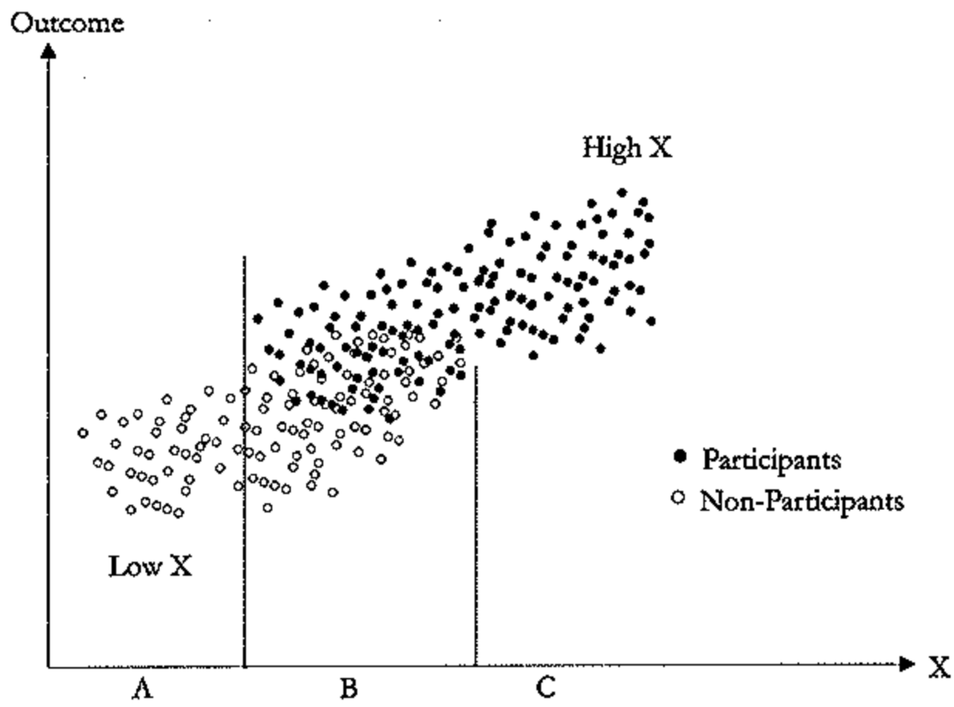
Thus, as long as this holds, it is possible to use covariates that are correlated with the error term. This is not possible in the case of OLS, as it has to hold that:

$$E(U_1|X, D = 1) = E(U_0|X, D = 0) = 0 \quad (1.22)$$

in order to guarantee independence between covariates in the outcome equation and the error term.

- Another important difference is that PSM takes place only over the common support area. PSM discards observations in $S_{1X} \backslash S_X$ (C in Figure 3) and $S_{0X} \backslash S_X$ (A in Figure 3) and calculates impact only in the common support area S_X (B, also in Figure 3). On the contrary, OLS is not limited to observations that are close with respect to the set of covariates X (B in Figure 3) but it uses all of them (A + B + C). When no observations are present, the linear function substitutes them and extrapolates to $S_{1X} \backslash S_X$ or $S_{0X} \backslash S_X$. In the graph, no conditional mean outcome with no treatment is present in C, but OLS will calculate it with the observations in A and B and extrapolate to C.
- Finally, PSM is more adequate to catch heterogeneity in the impact effect and it is less problematic with regard to the scale of the outcome variable.

Figure 3 Common Support Area. Source: Caliendo (2006), page 37



The choice of the PSM approach in the first paper rather than OLS is justified mainly because being a non-parametric technique and given the difficulty of knowing the appropriate functional form, we do not need to know the underlying outcome function. Also, we can use covariates in the model that are related to the error term in the outcome equation. The common support area is not such a concern as, as will be seen later, only a marginal share of observations lie out of its bounds.

Sensitivity analysis

Different types of bias were described above. While the first was handled by matching in the common support and the second reweighting the treated observations, the bias due to unobservables (B_3) cannot be avoided through PSM. Despite the fact that it is assumed by a literature that this is the minor source of bias, it has been attempted to model the possible effect of these unobservables.

The main source for the handling of hidden bias is Rosenbaum (2002), where the theoretical background of other attempts to assess this bias are based (Becker and Caliendo, 2007). He distinguishes between overt bias and hidden bias. The first is observable from the data. The latter is one that “cannot be seen because the required information was not observed or recorded”. In the case of randomization we end up with two samples, treated and controls, with equally distributed observables and unobservables and this hidden bias can be discarded.

The theoretical framework created in Rosenbaum (2002) is briefly described as follows. There are two individuals i and j and the set of observables for each of them are x_i and x_j respectively. They have the same observables and therefore $x_i = x_j$. The chance of receiving treatment is different, though, and therefore $P(x_i) \neq P(x_j)$. These observations can be matched in order to control for bias on observables. The odds of i and j receiving treatment are, respectively $P(x_i)/(1 - P(x_i))$ and $P(x_j)/(1 - P(x_j))$ and the odds ratio:

$$\frac{\frac{P(x_i)}{1 - P(x_i)}}{\frac{P(x_j)}{1 - P(x_j)}} = \frac{P(x_i)(1 - P(x_j))}{P(x_j)(1 - P(x_i))} \tag{1.23}$$

It is also assumed that the odds ratio has at most a value of $\Gamma \geq 1$ for units with the same x. Thus:

$$\frac{1}{\Gamma} \leq \frac{P(x_i)(1 - P(x_j))}{P(x_j)(1 - P(x_i))} \leq \Gamma \tag{1.24}$$

In the case in which $\Gamma = 1$, $P(x_i) = P(x_j)$ and therefore the estimation is not biased due to hidden bias. However, when $\Gamma = 3$, for example, despite the fact that $x_i = x_j$ the odds of receiving treatment change dramatically and one is three times more likely than the other. In these circumstances the sensitivity bias will look at the estimates with different values of Γ. If the odds are quite different and the estimates do not change much, the estimation would be said to be robust to big departures from the standard of no hidden bias or $\Gamma = 1$. On the other hand, for values of Γ not quite high where the estimates change a lot, it would be said they are quite sensitive to the presence of hidden bias.

Rosenbaum (2002) established techniques to find out the bounds of inference quantities, p values and confidence intervals and a few methods of sensitivity analysis. Some other approaches have been developed with regard to sensitivity analysis but all are based on

Rosenbaum's theoretical background. (Nannicini, 2007, Becker and Caliendo, 2007, Gangl, 2007, Ichino et al., 2007) are all based on the former. We adopt the approach contained in Ichino et al. (2007) and Nannicini (2007).

OLS, DID and Panel Data

The main issue with the quasi experiments is that they are not random. In the case of the India dataset, it would have also been desirable that the pipeline dataset, corresponding to 2005, would have been collected before having implemented the microfinance scheme. However the dataset was collected long after the microfinance schemes had been introduced. Therefore, there was no way of studying some households before and after the implementation of the projects. This characteristic is, though, common to many of the studies based on two periods databases reviewed so far: Khandker (2005), Copestake et al. (2005), AIMS projects and Tedeschi (2008).

The base for Tedeschi (2008) is Coleman (1999 and 2006) in terms of theoretical ground and Dunn and Arbuckle (2001) for the dataset. In Coleman (1999), the model is the following:

$$Y_i = \propto + X_{ij}\beta + MEMBER_{ij}\delta + AccesTime_{ij}\phi + V_j\theta + \varepsilon_{ij} \qquad (1.25)$$

where

- Y_i is the dependent variable related with income, consumption, health and others.
- X is a set of HH level variables
- $MEMBER$ is a dummy that takes the value of 1 for participants and self selected households in those areas where the village banks are not serving money yet.
- $AccesTime$ is a time variable which is the total time that the program has been available to members who have self selected. This is positive for participants in program villages only. It is zero for non participants in program villages and for all the individuals in control villages, both those self-selected to receive loans in the future plus those who did not show any interest in being village bank members.
- V_j is a vector of village fixed effects (dummies) variables that corrects programme allocation bias.

Basically Coleman controls for observables through some covariates. Village variables (V) control for allocation bias and finally he controls for unobservables that lead individuals to borrow with a dummy ($MEMBER$) that takes the value of one for borrowers and zero for non-borrowers. Thus, the estimate for the variable containing the access time to credits ($AccesTime$), he contends, gathers the unbiased impact of program availability per month.

In Coleman (2006) he changed the specifications and split $MEMBER$ into 2 different categories, $MEMBERrk$ if the houshehold had a rank and file member and $MEMBERcm$ if the household had a committee member. Similarly, $AccesTime$ is divided into $AccesTimerk$ and $AccesTimecm$ expressing the different length of time of access to the program by rank and file members and committee members respectively. The model is:

$$Y_i = \propto + X_{ij}\beta + MEMBERrk_{ij}\delta + MEMBERcm_{ij}\gamma + AccesTimerk_{ij}\phi + AccesTimecm_{ij}\eta + V_j\theta + \varepsilon_{ij} \tag{1.26}$$

This is interesting because it allows estimation of the impact for subsamples of participants, which will be relevant in one of our works.

Tedeschi (2008) follows Coleman's model but applies it to a panel dataset with two time periods from one of the AIMS studies (Dunn and Arbuckle, 2001). She first discards allocation bias and then runs a pooled OLS model which includes a dummy for the second year round (1999) and that is basically Coleman's equation (1.26):

$$Y_{it} = X_{it}\beta + year99\epsilon + NumDaysCr_{it}\phi + Amount_{it}\psi + Everborr_i\delta + \varepsilon_{it} \tag{1.27}$$

where

- Y: In Tedeschi's case is microenterprises profits, either weekly or monthly and X is a group of several variables including credit variables and household/entrepreneur's variables. The loan variables are dummies categorizing the subjects by their borrowing behaviour.
- X includes a set of variables at individual level
- $year99$ is a dummy for year 1999

- $NumDaysCr$ is a variable with the time length of credit,
- $Amount$ is the quantity borrowed and the variable
- $Everborr$ is a dummy that takes the value of one if the subject has borrowed at least once and zero otherwise.

The latter variable controls for those unobservables that lead the household to borrow. If we compare with Coleman's model above (1.26), the variable $Everborr$ does the same as $MEMBER$. Everborrow takes the value of one when the observation borrowed at least once. Thus, it considers as treated not only those that borrowed twice (survivors), but also those that borrowed only in the first or the second period.

She compares this model with another "naïve" model. They are called naive because they plainly compare borrowers vs. non borrowers not taking into account that borrowers have unobserved characteristics that make them better off than non borrowers even in the absence of microfinance. These are gathered by the member (or everborrow) dummy. In the naive model $Everborr$ is not present and therefore the impact of microfinance is overstated but it is reduced when this dummy is included.

To underpin this conclusion, she finally does a fixed effects panel data approach:

$$Y_{it} = X_{it}\beta + NumDaysCr_{it}\phi + Amount_{it}\psi + \rho_i + \varepsilon_{it} \qquad (1.28)$$

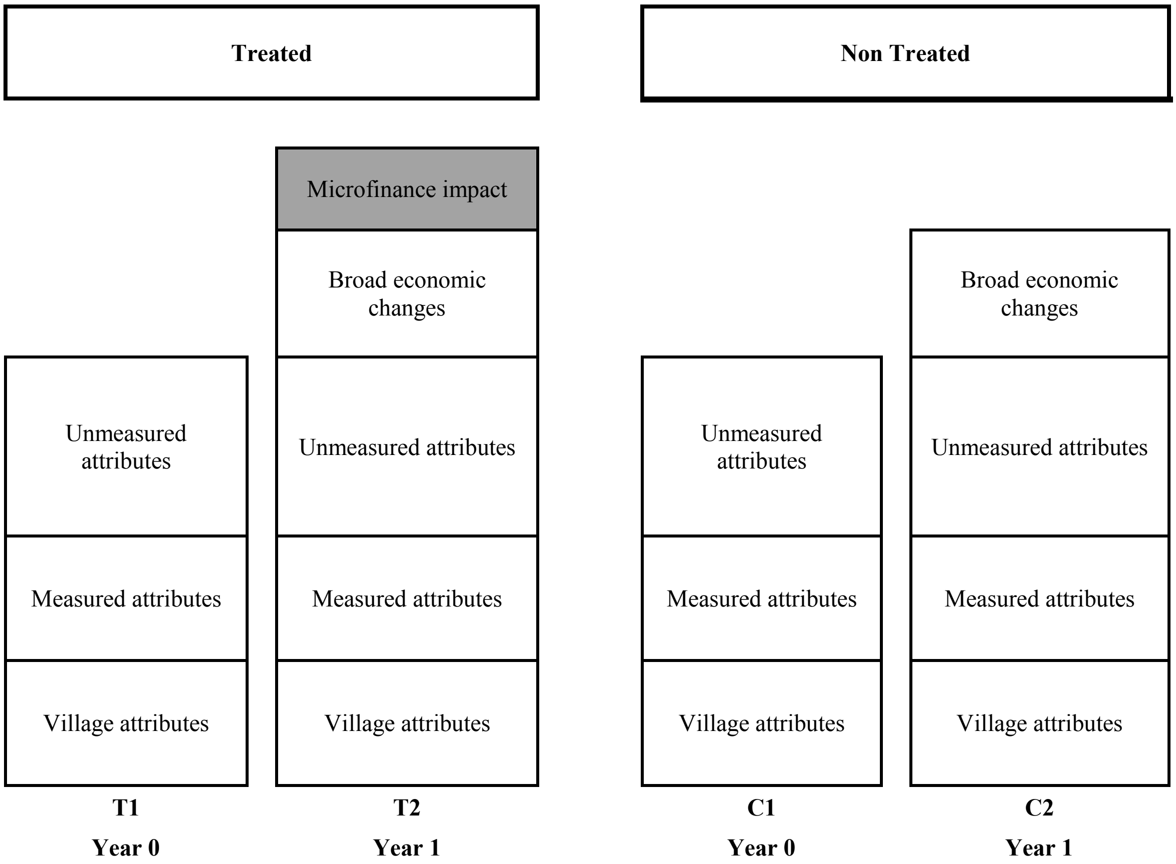
where time invariant variables are withdrawn from X and ρ_i is a household fixed effect. She finds a positive and significant effect of microfinance on weekly and monthly profits of microenterprises, although the impact is lower than in Dunn and Arbuckle (2001) as in the latter, she claims, it is overstated due to the selection bias.

Given the assumption that the unobservables that can influence both the outcome variable and participation are controlled with the participation dummy, differences in evaluations between panel and pooled OLS approaches are not expected to be great. This is so in Tedeschi (2008) and also in our study. Both the DID approach and panel data fixed effects approach provide with exactly the same estimates in this context of a panel with two time periods.

DID

The DID approach is clearly described in Figure 4 taken from Armendariz de Aghion and Morduch (2005). In particular, in the field of microfinance impact studies, this technique has been used by Morduch (1998), Copestake et al. (2005) and Kondo et al. (2008). Its main point is to assume that unobservables influencing both participation in microfinance and the outcome variables (called a above) are time invariant so they can be swept out by differencing and therefore end up with a clean, unbiased estimation of the impact of microfinance.

Figure 4 DID approach graphically. Souce: Armendariz de Aghion and Morduch, 2010



The idea is to isolate the effect of the shadowed area (Microfinance impact) on income. In a first stage, two subtractions are done: (T2 – T1) in the case of the treated and (C2 – C1) in the case of the control group. In the case of the treated, we are left with the effects on income caused by the broad economic changes plus the microfinance impact. In the control group, just with the effects caused by the economic changes. This is the first difference. The second difference that isolates the effect of microfinance is given by (T2 – T1) – (C2 – C1), resulting in the effect of the shadowed cell on income. Again, as is warned by these authors, it is assumed that the effects of personal characteristics such as entrepreneurial vocation, negotiation skills, ability or education are constant along time. In the present dataset, with information collected within a two years period, this is not a strong assumption.

The difference in difference model is explained more technically following Wooldridge (2002, chs. 13 & 14). Suppose we have the following equation in a pooled dataset:

$$Y_{i,t} = \beta_0 + \beta_1 Year2 + \beta_2 Program_{i,t} + \beta_3 X_{i,t} + a_i + u_{i,t} \qquad (1.29)$$

for t = 1, 2 and where i refers to observations, the units studied: individuals, households, states…, t stands for time and $Y_{i,t}$ is the outcome variable. On the right hand side:

- β_0 the intercept,
- $Year2$ is a dummy variable taking the value of 0 for observations in the first period and 1 for those in the second period.
- $Program_{i,t}$ is also a dummy variable taking the value of 1 if the observation i belongs to the treated group and 0 otherwise, in time period t.
- $X_{i,t}$ is the vector for observed variables such as age, sex, religion, household size and others.
- a_i does not have the t subscript because it gathers those unobservables, time invariant factors that influence $Y_{i,t}$. It is known as "unobserved effect" or "fixed effect" to remind that it is constant over time and "unobserved heterogeneity". Therefore, model (1.29) is also known as "unobserved effects models" or "fixed effects models".
- $u_{i,t}$is called the idiosyncratic error, unobservables that affect $Y_{i,t}$ and that change over time.

In order to estimate the impact of the program or the intervention, a "naïve" approach, as named in Tedeschi, (2008) would be to run a standard OLS regression:

$$Y_{i,t} = \beta_0 + \beta_1 Year2 + \beta_2 Program_{i,t} + \beta_3 X_{i,t} + v_{i,t} \qquad (1.25)$$

for t = 1, 2 and where $v_{i,t} = a_i + u_{i,t}$ and is often called the composite error.

This approach would not provide with consistent estimators as $v_{i,t}$ is not uncorrelated with $X_{i,t}$ due to the correlation between $X_{i,t}$ with a_i. This is true for single or pooled OLS regression for both years. Although this kind of error has been named "heterogeneity" bias, it is not more than a bias due to omitted (time constant) variables (Wooldridge, 2002). In Tedeschi (2008) the inclusion in the pooled OLS of the "everborrow" variable is the equivalent to controlling for a_i and it avoids this "heterogeneity bias".

Having the equations for years 1 and 2, a simple operation can be done to sweep out the unobserved, time invariant factors:

$$Y_{i,2} = \beta_0 + \beta_1 + \beta_2 Program_{i,2} + \beta_3 X_{i,2} + a_i + u_{i,2} \quad (1.26)$$

$$Y_{i,1} = \beta_0 + 0 \; + \beta_2 Program_{i,1} + \beta_3 X_{i,1} + a_i + u_{i,1} \quad (1.27)$$

Subtracting (1.27) from (1.26), we end up with:

$$\begin{aligned} Y_{i,2} - Y_{i,1} = \beta_1 + \beta_2 (Program_{i,2} - Program_{i,1}) \\ + \beta_3 (X_{i,2} - X_{i,1}) + (u_{i,2} - u_{i,1}) \end{aligned} \quad (1.28)$$

also written as

$$\Delta Y_i = \beta_1 + \beta_2 \Delta Program_i + \beta_3 \Delta X_i + \Delta u_i \quad (1.29)$$

This is called the first differenced equation. Thanks to this simple operation the time invariant unobservables are wiped out. A standard OLS regression would provide with consistent estimators of the parameter of interest, β_2 which will be the impact of the program. This is called the first-difference estimator or difference in difference estimator.

The former is true provided that the error term $u_{i,t}$ is uncorrelated with the observable characteristics $X_{i,t}$ in both periods, and therefore, Δu_i is uncorrelated with ΔX_i. This is called the strict exogeneity assumption, and can be expressed as

$$E(X_{is} u_{it}) = 0 \quad (1.30)$$

for *s,t = 1, 2, ...T* and *s≠t* and where explanatory variables in each time period have to be uncorrelated with the idiosyncratic error at any time period.

This method has an advantage for the present study due to its simplicity and easiness when omitting the selection bias. However, some comments have still to be made:

- The unobserved characteristics such as entrepreneurial abilities, negotiation skills and others are assumed to be constant, when they might improve with experience, for example. Therefore, this might be a strong assumption when working with a long period of time. In a 2 years period this assumption seems quite sensible.

- When subtracting one equation from the other, it is not only time invariant unobservables that are wiped out, but also observables that do not change over time or rarely do: gender, religion or mainly a district location. Therefore, the influence of these time invariant factors cannot be identified and this might be a problem from a policy point of view.

Panel data

In order to avoid the fixed effect, a_i, an alternative technique is used in econometrics, the fixed effects transformation within the framework of panel data. As stated in Wooldridge (2002)[7] when there are two periods of time and the same observations in both times, fixed effects estimation and DID produce identical estimates and inference. For illustrative purpose we briefly introduce the technique.

The transformation in this case is slightly different. We depart from the following equation:

$$Y_{i,t} = \beta_0 + \beta_1 X_{i,t} + a_i + u_{i,t} \tag{1.31}$$

We can find the average per observation i over time and write:

$$\bar{Y}_i = \beta_0 + \beta_1 \bar{X}_i + a_i + \bar{u}_i \tag{1.32}$$

where

$$\bar{Y}_i = T^{-1} \sum_{t=1}^{T} Y_{i,t}\,, \qquad \bar{X}_i = T^{-1} \sum_{t=1}^{T} X_{i,t} \; and \; \bar{u}_i = T^{-1} \sum_{t=1}^{T} u_{i,t}$$

Subtracting (1.32) from (1.31)

$$Y_{i,t} - \bar{Y}_i = \beta_1 (X_{i,t} - \bar{X}_i) + (u_{i,t} - \bar{u}_i) \qquad \mathrm{t} = 1, 2, \ldots \mathrm{T} \tag{1.33}$$

also expressed as

$$\ddot{Y}_{it} = \beta_1 \ddot{X}_{it} + \ddot{u}_{it} \qquad \mathrm{t} = 1, 2 \; \ldots \mathrm{T} \tag{1.34}$$

[7] Chapter 10, exercise 10.3 proving that DID estimates are equal to panel FE when there are two time periods.

where $\ddot{Y}_{it} = Y_{i,t} - \bar{Y}_i$, $\ddot{X}_{it} = X_{i,t} - \bar{X}_i$ *and* $\ddot{u}_{it} = u_{i,t} - \bar{u}_i$.

This transformation is also called the within transformation and it also allows us to remove a_i. Equation (1.34) can be estimated through a pooled OLS estimator also called fixed effects estimator or within estimator. This permits correlation between a_i and the explanatory variables. Under strict exogeneity, (idiosyncratic error $u_{i,t}$ uncorrelated with each explanatory variable across all periods 1, 2… T) the fixed effects estimator is unbiased. The same drawbacks with respect to time invariant variables apply.

Instrumental Variable[8]

The design of the model in the second study is quite sensitive to the possibility of reverse causality. This is explained because of the relationship between per capita income and microfinance borrowing with higher income households borrowing more than lower income households.

In formal terms, one of the axial assumptions of the OLS model, that the variables are not stochastic (E(u|**X**) = 0), does not hold. When the dependent variable is correlated with the error term, the OLS estimator $\hat{\beta} = {\sum_i x_i y_i}/{\sum_i x_i^2}$ is not consistent, that is, it does not converge to the population parameter when the number of observations in the sample grows (Cameron and Trivedi, 2005). In this case, the instrumental variable approach provides consistent and unbiased estimates for the variables of interest (Reilly, 2009a).

This instrumental approach uses a variable, called z, whose main characteristic is that its variations are linked to changes in x, the endogenous regressor, but it is independent of the dependent variable (y) variations.

Formally, if the error term is defined as $u_i = y_i - \beta x_i$, then the variable z_i should be such that :

$$\frac{1}{n}\sum_{i=1}^{n} z_i(y_i - \beta x_i) = 0 \text{ or } cov(z_i u_i) = 0. \qquad \text{(IV.1)}$$

---

[8] The description of the IV theoretical background relies heavily on Reilly (2009a).

From the above estimator, the instrumental variable estimator $\breve{\beta}$ can be expressed as:

$$\breve{\beta} = \sum_{i=1}^{n} z_i y_i \Big/ \sum_{i=1}^{n} z_i x_i \qquad \text{(IV.2)}$$

This is the Two-Stage Least Squares procedure (2SLS). If both numerator and denominator are divided by $\sum_{i=1}^{n} z_i^2$, then it can be seen that the 2SLS estimator is the ratio of a bivariate regression of y_i on z_i (numerator) and the denominator of x_i on z_i.

In the 2SLS approach, there are two equations, the structural equation and the reduced form equation. The structural equation can be defined as follows:

$$y_i = \beta_0 + \beta_1 x_i + \beta_2 w_{1,i} + \cdots + \beta_k w_{k,i} + u_i \qquad \text{(IV.3)}$$

where y_i is the dependent variable and x_i is the endogenous variable which has to be substituted by the instrumental variable, correlated with x_i but independent of the error term.

The reduced form equation is:

$$x_i = \pi_0 + \pi_1 z_{1,i} + \cdots + \pi_m z_{m,i} + \pi_{m+1} w_{1,i} + \cdots + \beta_{m+k} w_{k,i} + v_i \qquad \text{(IV.4)}$$

where x_i is a function of all exogenous variables in the structural equation plus a set of instrumental variables $z_1 - z_m$. When the number of instrumental variables is equal to the number of endogenous regressors x_i, it is said that the model is exactly identified. When the number of instruments is higher than the endogenous regressors, the model is said to be overidentified.

The OLS regression is applied to the reduced form equation to obtain the estimates of the endogenous variables, $\widehat{x_1} \dots \widehat{x_n}$. These estimates are then inserted into the structural equation in substitution of the problematic variable(s), x_i:

$$y_i = \alpha_0 + \alpha_1 \widehat{x_i} + \alpha_2 w_{1,i} + \cdots + \alpha_k w_{k,i} + u_i \qquad \text{(IV.5)}$$

Again, an OLS is run. If the instrument (z_i) is independent of the error term and correlated to the endogenous or problematic variable, then $\widehat{x_i}$ is independent of the error term and therefore the estimates are consistent.

When implementing the instrumental variable approach, the main difficulty is to find a proper instrument. In theory, if the instruments satisfy the conditions they should be suitable for this approach. The main difficulty is, however, that testing independence from the error term has proved to be non-conclusive. In addition, the independence tests can only be run in the presence of overidentification, when the number of instruments is higher than the number of endogenous variables. There have been a number of tests for independence, the best known of which is the Sargan test. In it, even when the null of orthogonality to the error term cannot be rejected, we cannot conclude that the instruments are valid.

For this reason, the validity of the instrument has to be supported by strong arguments based on economic theory or prior empirical evidence (Cameron and Trivedi, 2005). Also, instruments that are created through some sort of random process, such as sending invitations randomly to potential participants, have become popular.

However, validity also depends on the degree of correlation between the instrument and the endogenous variable. When the association between them is not relevant the instruments are said to be weak. The main drawback in the presence of weak instruments is that the estimation is less precise, standard errors grow larger and t statistics remain much smaller. When the association between the instrument and the endogenous variable is high, the instrument is said to be relevant. The relevance of the instrument is tested with an F-test in the reduced form equation:

$H_0: \pi_1 = \pi_2 = \cdots = \pi_m = 0$

$H_a: H_0$ is not true

As a rule of thumb, if the F value is less than 10, the instrument set is said to be weak and if above 10, the instrument can be considered relevant.

With respect to orthogonality to the error term, the Sargan test is implemented as follows:

1. We derive the residuals, $\widehat{u_i}$, obtained from equation (IV.5)

2. Then these residuals are regressed over the instruments and the exogenous regressors:

$$\widehat{u_i} = \phi_0 + \phi_1 z_{1,i} + \cdots + \phi_m z_{m,i} + \phi_{m+1} w_{1,i} + \cdots + \phi_{m+k} w_{m+k,i} + e_i \tag{IV.6}$$

The Sargan test (ST) is defined as:

$$ST = (n-g)R^2 \sim \chi^2_{m-r}$$

where

- n is sample size
- g is the number of parameters in the structural equation
- r is the number of endogenous or problematic variables
- m is the number of instruments.

The null hypothesis show potential independence of the instrument(s) from the error term, although failing to reject the null does not necessarily imply that the instrument is exogenous.

Finally, when we are before a relevant and independent instrument, an exogeneity test can be done over the potentially endogenous variable. One of the best known is the Wu-Hausman test. In it, the residuals from (IV.4), are named $\widehat{v_i}$. They are included afterwards as a dependent variable in the following OLS regression:

$$y_i = \beta_0 + \beta_1 x_i + \beta_2 w_{1,i} + \cdots + \beta_k w_{k,i} + \gamma \widehat{v_i} + u_i \tag{IV.7}$$

The test is implemented as a t-test, with the following hypothesis:

$H_0: \gamma \quad = 0$

$H_a: \gamma \quad \neq 0$

Under the null hypothesis we cannot reject exogeneity and therefore the original OLS model is more adequate than the IV approach.

An Instrumental Variable approach will be used in the second empirical chapter, the first on the Andhra Pradesh dataset. This intends to test for potential endogeneity but the Wu-Hausman test eventually rejects endogeneity.

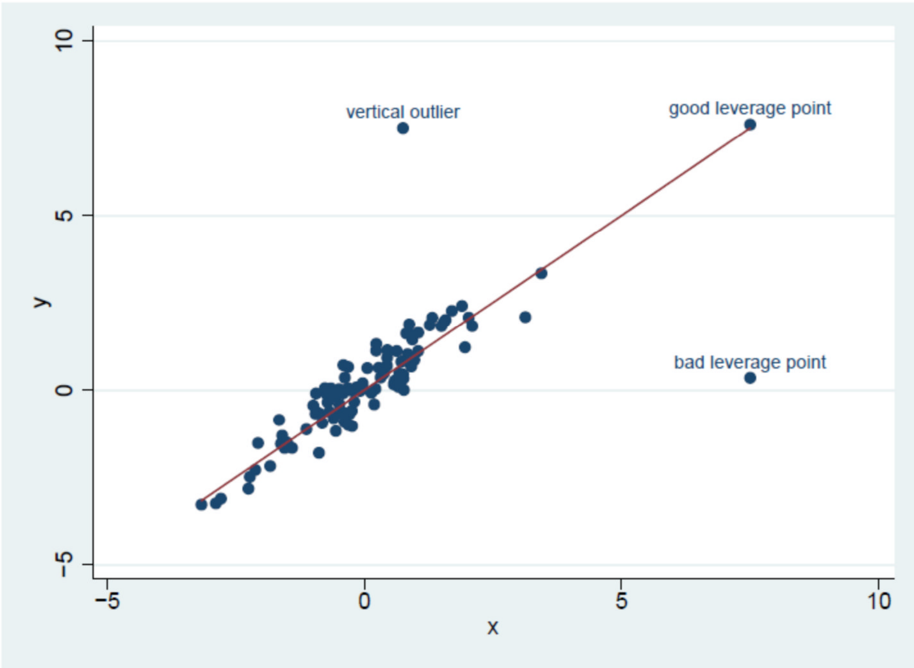
MM estimators and Outliers

The presence of outliers in a dataset challenges the estimates obtained through Least Squares methods, as they are sensitive to these abnormal values. The presence of a good number of outliers in the Andhra Pradesh dataset lead to the implementation of a MM estimator regression. The main reason is that when outliers are present the least squares estimation gives much more importance to observations with large residuals and this can bias the estimation of the parameters (Verardi and Croux, 2008).

Outliers can be sorted according to their abnormal values. There can be abnormal values of Y, which has been already shown in the graphs as income/income per capita outliers. Also, there might be observations with combinations of explanatory variables that strongly influence the estimates. The former have been given the name of vertical outliers. They mainly influence the estimated intercept and their effect on the least squares estimation is termed "discrepancy". The latter are called bad leverage points (Rousseeuw and Leroy 2003). They affect the least squares estimation by changing not only the intercept but also the slope of the regression line (Verardi and Croux, 2008). This effect is called "leverage". Therefore, the effect of outliers can be summarized as follows (Kohler and Kreuter, 2005):

Influence = discrepancy x leverage

Figure 5. Outliers classification. Source: Verardi and Croux (2008)



There have been several alternative methods to surmount the drawbacks of Least Squares methods (LS) in the presence of outliers. In LS the process of squaring gives more weight to those observations with greater deviation from the regression line. Median regression, also known as Least Absolute Deviation (LAD), minimizes the sum of the absolute values of the residuals and can be adopted as a solution to this problem. However, although this method is efficient against vertical outliers, it is not so against bad leverage points. Therefore, it solves the problem of discrepancy but not the leverage issues.

Another option is the M-estimators. They basically identify the outliers and use a Weighted Least Squares (WLS) approach to find the estimates. The implementation of the method is an Iteratively Reweighted Least Squares (IRLS) algorithm which reduces the weights of observations identified as outliers. However, the outlier identification process has some inconsistencies that make this option not robust in some instances, especially in the presence of clusters of outliers. Therefore it does not solve the problem of bad leverage points in all cases. Finally, S-estimators try to solve the problem of LS by replacing the square function in the former by a loss function that gives less importance to large residuals. Its drawback is that it has a Gaussian efficiency of only 28.7%.

MM-estimators, proposed by Yohai (1987), is an option that combines S-estimators with M-estimators and surmounts the problems of robustness and efficiency. In a first stage, the estimates and residuals are found through an S-estimator, which has a high resistance to outliers. The M method is used later to estimate the scale of the former residuals and then to iterate weighted least squares to find M-estimates of the regression coefficients (Andersen, 2008). Its name of MM estimator refers to the fact that the M method is used twice, first to estimate the scale of the residuals from the first step and then for the iteration process. The MM method used in our study was implemented by Verardi and Croux (2008). Its aim is to test for the robustness of the panel and IV estimates in the second empirical chapter to the presence of these outliers.

Distributional Impact

The above approaches (PSM, OLS, DID, Fixed Effects panel) and impact analysis in general has been generally developed within a framework of homogeneity across the subjects under study: individuals, households, etc. Khandker et al. (2010) quoting Heckman et al. (1997) state that the mean impact can only justify a study if the total output increases total welfare. When negative effects are present on part of the population, this has to be compensated with a transfer, either from a social program or from family or acquaintances.

The impact is often not uniform for all the households and the interest is to find out how microfinance impacts at different levels of the dependent variable distribution. This concern has been raised in impact literature of other programs which try to catch if the programs suffer from “elite capture”. This phenomenon takes place when the better off or the more educated households take advantage of the programs while the poorer rarely or do not benefit at all. In studies focused on populations characterized by great inequalities this is of special relevance as the programs need to ensure that they correct these inequalities and do not increase them.

In microfinance impact literature this issue has been raised by Hulme and Mosley (1996) who find that microcredit mostly benefits the less poor. Coleman (2006) revisits his Thai dataset and finds that committee members of the village banks, normally more influential and better off village dwellers, are the only ones who reap benefits from the programs. The issue is also mentioned in Copestake et al. (2005) which concludes that the impact for the upper income median is massively greater than the impact for the lower. Finally, Kondo et al. (2008) also discard impact on the poorest.

Quantiles and Quantile regression

In order to study the possibility of an heterogeneous impact across the distribution of the dependent variable the option was to use quantile regression. It started with Koenker and Basset (1978) and has showed some other convenient properties apart from a more accurate characterization of the data. Quantile regression has been mostly developed in labour economics in which the interest is to find the impact of education or training at the different points of the distribution of the dependent variable, normally the log wage (Koenker and Hallock, 2001; Koenker, 2005). It has also been used, for example, to find out the effects of pre-natal circumstances or conducts in the birthweight of the babies (Abrevaya, 2001).

The idea of a uniform impact for all households has proved to be inaccurate in some of this literature. OLS and DID/Panel provide a single estimate for the whole distribution as PSM does. However, our interest is to check whether this impact is really homogenous for the whole sample or there are differences at different points of the distribution. As a graphical image of the quantile analysis developed in our study it could be said that the conditional distributions of these variables are first sliced up into several pieces. Then, the impact of microfinance is found for each of these pieces. This cannot be done by just finding an OLS for the observations within each quintile as will be explained below. All observations are needed though, when tackling quantile regression.

The explanation of the quantiles and quantile regression will follow the notation and pace from Cameron and Trivedi (2005) . Having one continuous random variable y, the population q^{th} quantile is the value μ_{q} so that y is smaller or equal to μ_{q} with probability q. In mathematical notation:

$$q = \Pr[y \leq \mu_q] = F_y(\mu_q) \qquad (1.35)$$

In the formula, therefore, F_y is the Cumulative Distribution Function (CDF) of y, which has been also noted in other occasions as Φ_y. Therefore if $q = F_y(\mu_q)$, inversely:

$$\mu_q = F_y^{-1}(q) \tag{1.36}$$

As an illustrative example, if the median of y is equal to 30, it would be said that $q = 0.5$ because we are looking for the median and $\mu_{0.5} = 30$. The CDF of 30 would be $F_y(30) = 0.5 = q$. Finding out the inverse of the CDF of the quantile results in the value of the median of y: $F_y^{-1}(0.5) = 30$. If the variable of interest is income and an individual is said to be at the, say, q = 0.25 quantile (also known as the first quartile) of the distribution, the 25% of the individuals will have a lower wage while (1-0.25 = 0.75) 75% will have higher wage.

The importance of Koenker and Basset's work is that they adapted the quantile concepts to the regression framework and allowed us to estimate conditional quantile functions (Reilly, 2009b). Within this framework the quantiles of y are expressed conditional on x. Following the same notation, $\mu_q(x)$ is the function of the quantile of y conditional on x so that y is equal or less than $\mu_q(x)$ with probability q, given that the probability is calculated with the conditional distribution of y given x (Cameron and Trivedi, 2005). Again, the inverse of the conditional CDF of y given x can be used to find out $\mu_q(x)$ given the quantile q:

$$\mu_q(x) = F_{y|x}^{-1}(q) \tag{1.37}$$

where $F_{y|x}^{-1}$ is the inverse of the conditional CDF of y given x.

One example of quantile regression is the median regression where q = 0.5 which is also named as Least Absolute Deviation regression. The main difference between OLS and quantile regression is that in the former the value to minimize is the sum of the squared errors while in the latter minimizes the weighted sum of absolute errors.

In the case of OLS, the β vector is chosen to minimize:

$$\text{SSE} = \sum_{i=1}^{n}[y_i - x_i'\beta\ ]^2 \tag{1.38}$$

where y is the dependent variable, i refers to individuals and x is the vector of covariates. In the case of quantile regression the minimization is done over:

$$Q_n(\beta_q) = \sum_{i:y_i \geq x_i'\beta}^{n} q\left|y_i - x_i'\beta_q\right| + \sum_{i:y_i < x_i'\beta}^{n} (1-q)\left|y_i - x_i'\beta_q\right| \qquad (1.39)$$

where the β_q is used as different quantiles will result into different estimators. The formula assigns a weight q to observations that are above their predicted values or (1-q) otherwise. Therefore, positive residuals are weighted by q and negative residuals are weighted by (1-q).

In the case of median regression or LAD q = (1-q) = 0.5 and all the observations receive the same weight and therefore the formula simplifies to:

$$Q_n(\beta_{0.5}) = \sum_{i=1}^{n} |y_i - x_i'\beta_{0.5}| \qquad (1.40)$$

It has to be noted that quantile regression uses all the observations of the dependent variable. It is not accurate to split the sample into different groups with regard to the unconditional distribution and then try to fit these groups with OLS. This procedure brings about a problem of truncation on the dependent variable that will provide confusing results (Koenker and Hallock, 2001).

The objective formula (1.39) cannot be differentiated and therefore the minimization process to find $\widehat{\beta_q}$ has to be done through a linear programming (LP) approach. This is an iterative process which departs from set or parameters that are refined in each iteration until it has found the $\widehat{\beta_q}$ that minimizes (1.39). The latter is achieved, in the case of median regression, when 50% of the computed residuals are negative. In general for the *qth* quantile, the iteration process find its optimum when (q x 100)% of the computed residuals are negative. Thus, in the case of q = 0.4, the optimum is achieved when (0.4 x 100 = 40)% of the residuals are negative (Reilly, 2009b).

Buchinsky (1998) enumerates some of the convenient features that make quantile regression more desirable in some cases. Apart from being able to find different estimates at different points of the distribution of the outcome variable, the technique deals better with the presence of outliers. In the particular case of LAD, it has been used as a complementary or alternative approach in their presence. When dealing with heteroscedasticity problems, quantile regression might also be a more efficient method than OLS. However, this is not always true. As stated in Reilly (2009b), the variance-covariance matrix contained in Koenker and Bassett (1978) has been proved to be erroneous in the presence of heteroscedasticity.

This problem is solved by using the bootstrapping method. It consists of a series of random withdrawals with replacement from the original sample in order to approximate the true sampling distribution. In the quantile regression method the re-sampling is done by picking the dependant variable with the corresponding row of the X matrix in a way that is known as "paired bootstrap". Once the sampling distribution is found out, it can be used for the conventional test statistics.

The coefficients have been proved to be consistent and asymptotically normal and therefore the conventional test statistics are valid. It has been also shown that the accuracy of the estimations depends on the points density at each quantile. This means that those quantiles with less observations, normally at the top or at the bottom of the conditional distribution, might not be accurately calculated. This leads to a weaker power of test statistics and therefore more cases of not rejecting the null when false. Another interesting property of quantile regression is its equivariance to monotone transformation.

Not all the properties are convenient in the quantiles. The conditional expectation is a linear operator and this is very useful in the case of the DID approach with two time periods. This property implies that

$$E(y_{i,2}|x_{i2}a_i) - E(y_{i,1}|x_{i1}a_i) = E(y_{i,2} - y_{i,1}|x_{i1}, x_{i2}, a_i),$$

where, in the present study, i stands for the household, 1 and 2 for periods, x is the vector of covariates and a_i is the time invariant unobservables specific for each household. This property allows to use the DID approach. However, conditional quantiles are not linear operators so that $Q_q(y_{i,2}|x_{i2}a_i) - Q_q(y_{i,1}|x_{i1}a_i) \neq Q_q(y_{i,2} - y_{i,1}|x_{i1}, x_{i2}, a)$ and therefore the time invariant unobservables cannot be differenced out. This rules out the use of a DID or fixed effect approach in the case of quantile regression.

Abrevaya and Dahl (2008) try to solve the problem with a different approach. The fixed effects model allows for the unobserved, time invariant characteristics to be correlated with the covariates. Once differenced out, the former are wiped out. Given that this is impossible to do with conditional quantiles, they turn to another approach within the panel data framework: random effects. The random effects approach assumes these unobservables to be uncorrelated with the covariates. This might be considered implausible in this particular study as the households are quite likely to have some characteristics that make them more prone to participate or to succeed. Nonetheless, they base their study in a type of random-effects model that allows them to explain the time invariant unobservables in terms of the covariates. They use the "Correlated Random Effects model" developed in Chamberlain (1982;1984) which explains the unobserved fixed effects as a linear function of the covariates in both time periods. Its particularities will be discussed further in its corresponding chapter.

Conclusion

Three different techniques will be used in the empirical chapters. All are quasi-experimental methods and therefore rely on assumptions that vary with the model. In the first empirical approach, PSM is used to revisit one of the best known microfinance surveys. PSM makes use of the assumption that matching on observables is enough to get rid of unobservables. It is a useful technique that follows the same principles as random experiments and it is non parametric, and therefore there is not any underlying functional assumption and this is less restricting to researchers.

In the second, a DID/Panel approach will be attempted, replicating the way that Tedeschi (2008) studied the AIMS dataset in Peru formerly studied by Dunn and Arbuckle (2001). In the panel, unobservables are wiped out through differentiation. Outliers in the sample and possible endogeneity issues were also dealt with through MM and IV estimations, respectively.

These two methods assume an uniform effect over the whole sample, which might possibly be inaccurate. A more focused picture of the impact of the program is possible through quantile regression, in its Correlated Random Effects in order to apply to panel data. In this model the unobservables are controlled for assuming that they are linearly related to the observed covariates. This will allow us to test for a uniform or heterogeneous impact along the dependent variables distributions.

Thus, in all three techniques some assumptions are required and at some points they are possibly more complicated than the relatively simple assumptions in RCTs. However, given the difficulties found in doing proper randomization in social studies, quasi-experimental techniques do have an essential role in impact evaluation. Two of them have already been used in the impact evaluation of microfinance. The Correlation Random Effects model has not yet been used to study the distributional impact of microfinance as far as we are concerned. All three aim to cast light on the impact evaluation literature. Limitations are acknowledged regarding the datasets, as they are surveys that are probably not properly designed for our purpose. Adequately designed surveys, as stated in Copestake et al. (2011), are scarce but this should not deter research evaluating the impact of microfinance even at the risk of poorer estimations.

Chapter 3: Revisiting Bangladesh Microfinance Impact Evaluation Studies: a PSM approach.

Introduction, description of the survey and descriptive statistics.

This study is based on the original Bangladeshi dataset exploited first in PK. Its conclusions were quite controversial and they were soon challenged by other studies, the first of which was Morduch (1998). Further published and unpublished studies, followed, some underpinning the positive impact findings in PK, (Khandker, 1998; Pitt, 1999; Khandker, 2005; Chemin, 2008) and some finding no significance in the impact of microfinance (Roodman and Morduch, 2009; Duvendack and Palmer-Jones, 2011). Khandker (2005) from the second round gathered the households that could be traced back to the first round of the survey and constructed a fixed effects panel data approach, with findings that confirm the positive effect of microfinance on expenditures or the reduction of extreme poverty. Roodman and Morduch revisit PK, Morduch (1998) and Khandker (2005) and find no evidence of the positive impact of microfinance. Works by Chemin (2008) and Duvendack and Palmer-Jones (2011) are of special interest for the present study from a methodological point of view as they implemented a PSM with the first round of the sample, in order to challenge the impact evaluation contained in PK.

The present chapter contributes to the microfinance literature in several ways. First, previous iconic studies have mostly sought to study the impact of microfinance at individual level, whereas the present approach is at household level. In addition, the reference dataset has always been either the first round of the survey (PK; Morduch, 1998; Chemin, 2008; Duvendack and Palmer-Jones, 2011) or the sample of households that could be surveyed in both years (Khandker, 2005; Roodman 2009). In the present chapter the research is based on the whole second round of the survey, with 2,599 households. The study also uses a different approach regarding the dependent variables. With respect to the previous PSM studies, the present chapter includes a sensitivity test that is not included in Chemin (2008). It is also a more adequate test when using the kernel algorithm than the one attempted in Duvendack and Palmer-Jones (2011).

The impact is calculated at household level and therefore the results are not strictly comparable, given the fact that all the previous works on this Bangladeshi dataset have studied the impact at individual level, distinguishing between males and females. Impact so far had been found to be higher for females. At household level, we intended to control for the gender of the household head, but the variable was not significant in any of the different specifications and it was withdrawn.

So far the second round of the survey has been underexploited. An additional number of households was taken in addition to those surveyed in the first round. They were sampled within the same villages where the first round took place and also in three additional thanas not included in the first survey (Khandker, 2005). In total, our sample contains 2,599 households, 36.3% more than the 1,798 in the first survey used in PK, Morduch (1998), Chemin (2005) and Duvendack and Palmer-Jones (2011). The only studies taking into account the second round are limited to the 1,638 households that could be retraced in the second round (Khandker, 2005 and Roodman, 2009). Still, around 37% of households from the second survey remained unstudied.

In the definition of dependent variables we focused on income and expenditures. The former had not yet been used in any of the studies related to this Bangladeshi dataset. Khandker (2005) uses headcount poverty at village level as one of the dependent variables. This is a variable that uses income at the background and the different scales of poverty are defined by setting different thresholds of income. However, it had not yet been used as a dependent variable by itself in the set of Bangladeshi studies. Also, we divided expenditures into three categories as some of the non-food expenditures defined in the survey were not actually current expenditures. Thus, the survey included expenditures on home extensions or home/land purchases, which can be interpreted as investments rather than expenditures. In our approach the impact is measured on food expenditures, current expenditures and two different definitions of non-current expenditures.

We also test the impact of microfinance using different treated and control groups, as done in Duvendack and Palmer-Jones (2011). We agree on the argument that having households with access to different sources of funds is quite relevant when defining treated and control groups and thus comparing different specifications of borrowers and non borrowers will help cast light over the impact of borrowing from microfinance and other funding sources.

Finally, a sensitivity test is implemented in order to find the likelihood of the existence of unobservables that can challenge the estimates obtained. This test is not found in Chemin (2008) but Duvendack and Palmer-Jones (2011) attempts it in their study. However, they did not test for the matching quality of the algorithms. This theoretically would not let them choose the best match possible. Their sensitivity test also should have been chosen according to the best possible matching algorithm but they just apply a sensitivity test on the Nearest Neighbour estimate. Our sensitivity test is also based on the theoretical background developed in Rosenbaum (2002) but we adopt a more adequate approach when using a kernel algorithm, whereas the one in Duvendack and Palmer-Jones (2011) is only suitable for the nearest neighbour algorithm.

As mentioned above, our first intention was to mix PSM and Difference in Difference (DID) and apply them to the Bangladeshi dataset, similar to what Smith and Todd (2005) did with Lalonde's (1986) dataset. They found that a difference-in-difference matching estimator provides with the best performance when trying to replicate Lalonde's experimental outcomes. Reconstruction of the first round of the Bangladeshi dataset from files contained in the World Bank (WB) website was quite difficult and will be discussed later. Eventually, a PSM was attempted on the second round of the survey, exploiting the households that had not yet been subject of research.

Smith and Todd (2005) contend that the main contribution of the PSM-DID approach is to get rid of the geographical mismatches and problems of measurement in the dependent variable. Unlike Lalonde's data, in the Bangladesh survey treated and controls are taken from the same geographic area and all individuals were interviewed with the same questionnaire. Thus, these sources of bias would not be as important and therefore PSM should perform well avoiding it. This will be tested through sensitivity analysis following Ichino et al. (2007) and Nannicini (2007), a slightly different approach from that used in for example Caliendo (2006) or Duvendack and Palmer-Jones (2011).

The aim of this study was to find the impact of microfinance on income per capita and expenditures per capita at household level, including current expenditures and also some extraordinary expenses. Our evidence suggests that there seems not to be a significant impact of microfinance on income per capita or current expenditures per capita. In the case of the former, the effect, if any, would be negative rather than positive. The estimates, however, are only marginally significant and they are not consistent across the different treatment and control group definitions. However, the effect of borrowing from microfinance and other sources consistently shows a great increase in expenditures on home repairs or home/land investments.

The chapter follows with a description of the survey. Then the variables income and expenditures per capita are described. Then the PSM process is explained and applied to the dataset. Finally conclusions are stated.

Description of the survey

The two rounds survey was implemented by the Bangladesh Institute of Development Studies (BIDS) and the World Bank. Descriptions of the first round can be found in Pitt and Khandker (1998), and for both rounds in Khandker (2005). An additional description is contained in the accompanying documentation to the database available in the World Bank (WB) website.

One of the main issues when undertaking the research work was the bad quality of the dataset and documentation available. In the case of the first round, the dataset was incomplete and documentation was almost illegible. The documentation for the second round was better although still a lot of work was needed as the documentation was poor. For example, information about household weights was not contained in either datasets or in the documentation. Duvendack and Palmer-Jones (2011) managed to reconstruct this first round but they refer to the same issues in their work. Finally, we decided to restrict the analysis to the second round of the survey.

The baseline survey took place in 1991/1992. One of the administrative levels of division in Bangladesh is the thana. A thana contains several villages and a district contains several thanas. Twenty nine of these thanas were selected for the study. Out of them, 8 were targeted by Grameen Bank, 8 by Bangladesh Rural Advanced Committee (BRAC) and another 8 by the "Rural Development-12" program of the Bangladesh Rural Development Board (BRDB). This adds up to 24 and the remaining 5 were non microfinance-targeted thanas and villages within these thanas operated as control villages.

Within these 24 microfinance thanas, 3 villages were randomly selected from those where the program had been running at least for three years. Among the 5 non-program thanas 3 were also picked, adding a total of (24 x 3) + (5 x 3) = 87 villages. Villages with less than 51 and more than 600 households were discarded. Altogether, 1,798 households were selected in those 87 villages. Data was collected during the three rice seasons: *Aman*, *Boro* and *Aus* and in the last season 29 households dropped leaving a total of 1,769.

In order to guarantee that they observe their aim of targeting the poor, Grameen, BRAC and BRDB shared an eligibility criterion: all those owning half an acre of land or less were the target household within a village. Obviously in the 15 non program villages those fulfilling this condition were the "would be eligible" had the program been running in the village. This was the criterion used by Pitt and Khandker (1998) in order to implement their approach. The fact that not all participants complied with this half an acre requisite is one of the reasons of controversy regarding the techniques applied.

This study was based in the second round, which took place in 1998/99. In it, the same 87 villages were visited again but only 1,638 households out of the original 1,769 could be re-interviewed resulting into an attrition rate of 7.4%. 237 of these 1,638 had split into 546 new households but they were treated as single households, adding up to 1,947 households[9].

In addition to the previous households new ones were selected among the original 87 villages. On top of that 3 new thanas were selected, adding up to a total of 29 + 3 = 32 thanas. In each new thana 3 villages were picked and 20 households, both eligible and non eligible were drawn from each of these 9 new villages. In total the second round included 2,599 households, 2,226 from old villages and 373 from new villages.

The main point in this second round of the survey was that by this time there were not any control villages since in all the villages there was at least one microfinance scheme going on. Apart from the three mentioned microfinance institutions (MFIs), others such as *Proshika* and *Asa* were already present in the field. Thus with no control villages available the study cannot be based on the Intention To Treat as all villages are exposed

[9] 1,638 – 237 (hh. that split) + 546 (amount into which those 237 split) = 1,947 households.

to microfinance. However, the study controls for village level characteristics in order to avoid bias due to program allocation.

The criterion for eligibility is still to own half an acre of land or less. The fact that some of the participant's households show greater landholdings is due to two main reasons. First the criterion is accounted for individually so that the overall amount of land within the same household might add up to more than half an acre. Second, the criterion is not strictly observed and some of the non-eligible households were borrowing.

Descriptive statistics

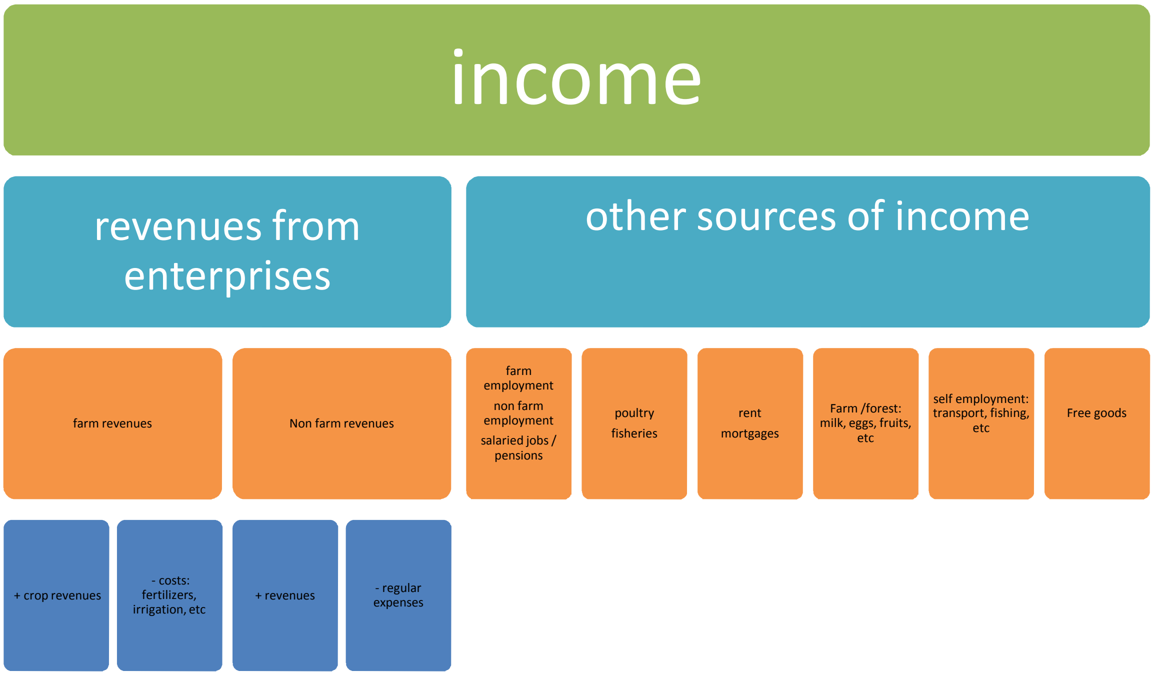
The study is mainly focused on the impact of borrowing from microfinance or credit from other sources on income per capita and expenditures per capita. First it is explained how income and current expenditures are created and then the descriptives of income and expenditures per capita are discussed. Per capita variables are just created dividing income/expenditures by the household size.

Income

Income is composed mainly of two groups. The first is composed of revenues obtained by farm and non-farm enterprises. In the former case, the revenues come from collected crops and are net of normal costs such as fertilizer, irrigation, seeds and labour costs. Crops are varied. The main is rice but also other products such as wheat, spices, sugarcane or oilseeds are cultivated. In the case of non-farm enterprises the revenues are net of regular expenses. These are defined in the survey as expenses that are paid at least once a week when the enterprise is operating. Any non-regular expenses were not withdrawn from revenues as they were not properly reported in the dataset[10].

[10] Expenses on non regular inputs were reported but the frequency was not documented and therefore frequency codes 1, 2, 3, 4, 5 could not be matched with a temporal frequency: daily, monthly, quarterly... assumptions in this case might have lead to very wrong conclusions. The absence of these expenses makes revenues to be less accurate and therefore income and income per capita.

Figure 6 Income components



The second part of income is split into several headings. The first is wages from farm employment in Aush, Aman and Boro rice seasons, non-farm jobs, other salaried jobs and pensions. The second includes income from poultry or fisheries activities. This could be understood also as farm revenues but the criterion of the survey was to split them and this was done also in this study. The third group is income from rent and mortgages. With respect to the fourth group, income from farm and forests resources, it could be understood as overlapping with the above farm revenues, but no crops are included here, just milk, eggs, fruits and trees. The remaining groups are self-employment revenues from activities such as rickshaws, boats, fishing, doctors or lawyers and goods received for free including vegetables, fish, left-over crops. Income from rents, farm/forests and self employment activities are accounted net of costs. The income variable is constructed such that it doesn't include the amount of money borrowed from the different credit sources.

The inputs in income variable were mostly given in monthly terms and their value was multiplied by 12 in order to transform it into annual terms. There are some issues that makes the variable income quite volatile. The first is that the calculation of net revenues is sometimes subject to accounting rules that can in some occasions be questionable. For example animals that are consumed at home are accounted as an animal sale at market price. Also animals that are born at home are accounted as a purchase of an animal at market price. In addition to these rules, the presence of many inputs aggravates the measurement error that is pervasive in this kind of household surveys in poor areas as no records are normally kept. Finally, profit reversals in a particular year might place better off households in lower quantiles than that which would correspond to them in their normal economic situation.

The sample showed a small number of negative income values, just 34 observations, around 1.3% of the total sample. In most cases the negative values resulted from the fact that revenues in agriculture were sufficient to cover the costs. There were also a few occasions in which the negatives were given by a great negative figure in the losses of fisheries/poultry accounts. These negative values are problematic and will be dealt with in the analysis separately as done in Shaefer and Edin (2012) and Hunter et al. (2002), dropping the negative income values and rerunning the models afterwards. Not surprisingly, outcomes are very similar in terms of the estimates and their significance and we conclude that these negative values do not unduly influence our results. This should be remembered when studying the descriptive statistics and the later analytical results.

These drawbacks have to be taken into account when studying the descriptives and in the analysis approach.

Figure 7 Income per capita by mfi borrowing status

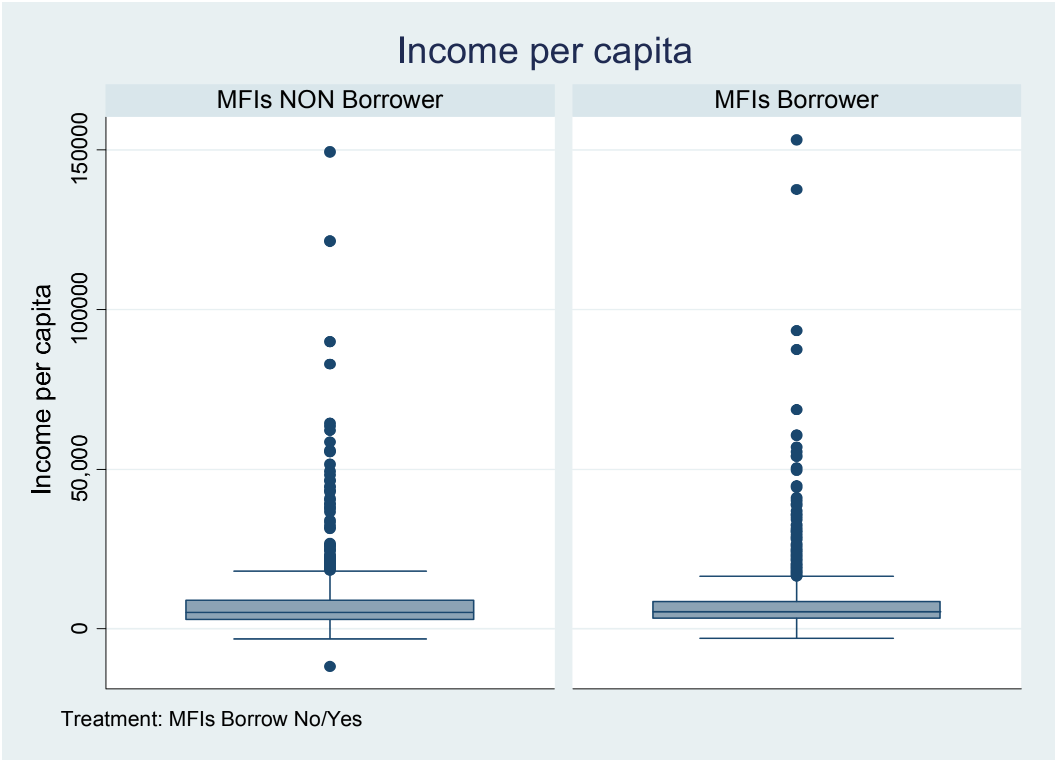


Figure 7 shows the graph by MFI borrowing status. Income per capita shows some negative values for both groups. There are numerous outliers at the top of the distributions. MFI non borrower distribution also shows a single negative outlier that can clearly be distinguished in the graph. The maximum values of both groups are quite close though. Table 2 below shows some of the descriptives in income per capita. Means are well above the medians and MFI borrowers have lower mean income. The skewness and kurtosis have the expected values given Figure 7. Inter-Quartile Range (IQR) and standard deviation inform of a higher dispersion in the distribution of MFI non borrowers. An histogram can be seen at the appendix (Figure 19).

Table 2 Income per capita descriptives, by MFIs borrowing status

	Income per capita	
	MFIs non borrowers	MFIs borrowers
mean	8,160	7,297
median	5,231	5,326
sd	11,668	8,704
min	-11,798	-2,862
max	149,247	153,104
range	161,045	155,966
iqr	6,092	5,283
skew	6	8
kurtosis	51	97

Regarding the distribution by quantiles, Table 3 shows that the figures are higher for the quantiles corresponding to the participants in the first three quintiles although this turns the other way round from that point towards the top of the distributions. Differences are especially remarkable at the 95th and 99th percentiles.

Table 3 Income per capita quantiles by MFIs borrowing status

	Income per capita	
	MFIs non borrowers	MFIs borrowers
p1	- 35	308
p5	800	1,337
p10	1,435	1,952
p20	2,503	2,912
p25	2,908	3,340
p30	3,297	3,775
p40	4,254	4,464
p50	5,231	5,326
p60	6,240	6,366
p70	8,030	7,625
p75	9,000	8,623
p80	10,548	9,954
p90	15,816	12,958
p95	22,676	16,713
p99	55,865	38,986

With respect to borrowing from different sources Table 4 shows that the proportion of households borrowing from microfinance programs is clearly higher than the rest of sources. This is not abnormal given the spread of microfinance in this area by the time the second round of the survey was done. With respect to this source not great differences are seen among quintiles two to four. It is only remarkable the lower proportion in the first quintile. For the rest of the sources, the pattern is on the contrary to microfinance borrowers. Borrowers are more concentrated at the first quintile of income per capita. Households non borrowing from any of the sources (no borrow column) are more frequent in the first quintile. Percentages rows add up to more than 100% due to the fact that there are some households borrowing from more than one source.

Table 4 Percentage of borrowers by income per capita quintiles

	Percentage borrowers by income per capita quintile				
	MFIs	**Bank**	**Informal**	**Relatives**	**No borrow**
q1	38.1%	10.1%	10.1%	19.7%	38.9%
q2	51.3%	5.8%	6.2%	18.6%	31.5%
q3	52.8%	4.6%	6.8%	16.1%	32.7%
q4	57.4%	4.4%	6.4%	15.1%	29.8%
q5	50.0%	9.3%	5.4%	18.0%	33.5%

In Table 5 the mean loan size is shown taking into account only borrowers. In the case of microfinance the mean loan size roughly increases progressively with the quintile. In the rest of sources the first quintiles show a high mean loan size and the highest figures are shown in the fifth quintile. In the case of informal loans the mean at the fifth quintile is four times the value at the fourth quintile. The mean loan size for the whole sample, including non MFI borrowers, is found multiplying the values in Table 4 by values in Table 5.

It doesn't seem plausible that the poorest households can borrow on average even more than households at the fourth quintile. Thus, the reason for these high mean loan sizes at the first quintile might well be the above mentioned issue of households that are located at the first quintile due to profit reversals. They have a better credit scoring and they have access to greater loans than households that are in the first quintile because they are poor. It is just that they are more heavily indebted and a reversal makes them look poorer than what they actually are.

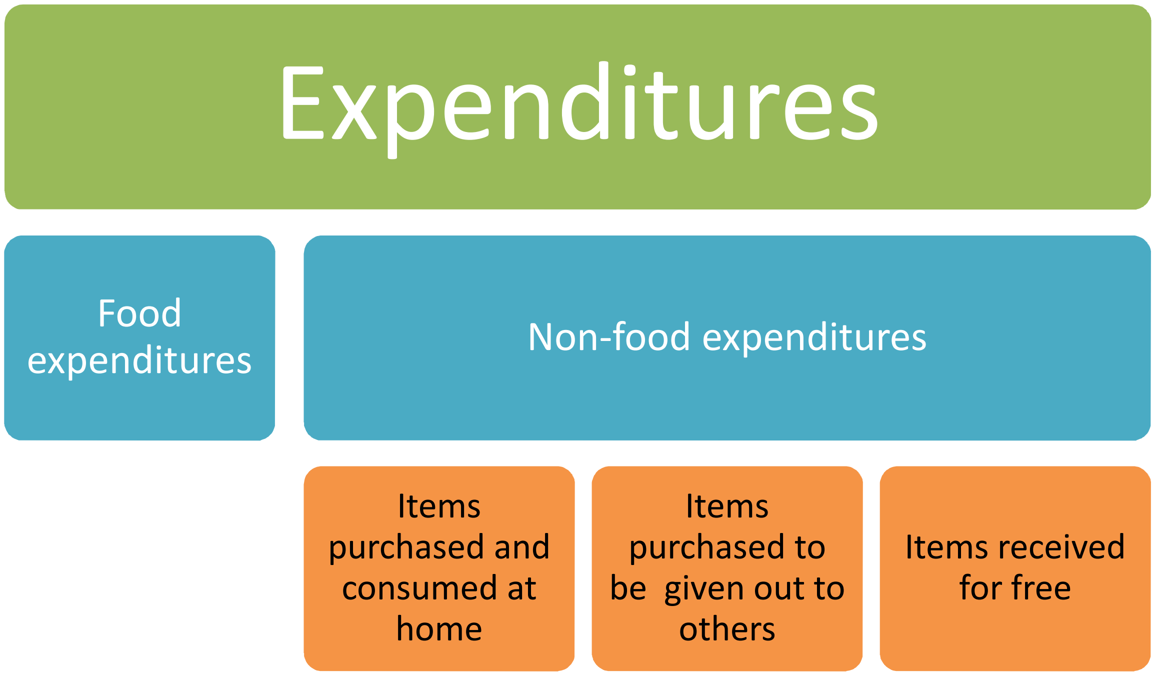
Table 5 Mean loan size by income per capita quintile

	Mean loan by income per capita quintile			
	MFIs	Bank	Informal	Relatives
q1	20,032	10,385	11,377	10,910
q2	19,607	6,463	6,375	9,484
q3	23,586	8,042	5,980	6,627
q4	29,545	21,304	10,545	19,314
q5	36,272	37,521	45,482	25,423

Expenditures per capita

First we need to define the current expenditures variable. Its original composition is found in Figure 8. Expenditures in the survey are split into two main categories, food expenditures and non food expenditures. Food expenditures are given in the survey in weekly terms so they had to be multiplied by 52 to get the annual figures. Non-food expenditures were in monthly terms so they were multiplied by 12. Non-food expenditures are split into three different groups. The first is just the amount spent on different domestic items. The second is the amount spent on gifts, dowries or inheritance. Finally the third category is composed of these same items received for free. These are different from the "Free goods" accounted for in the income variable (see Figure 6). Free goods in income were referred to seeds, fertilizers and other goods that could be used for farm or business activities.

Figure 8 Expenditures components



Not all the elements within the expenditures section were included in the calculation of our current expenditures variable. As long as the interest in expenditures is to have a proxy of normal or current consumption, amounts spent on items to be given out were not considered. Thus the second leg in the graph above is excluded from the calculation. Interest paid on loans is not included in the expenditures variable. Also some of the items accounted as expenditures in the survey should not be considered because they can be assumed as investments or extraordinary disbursements that do not reflect consumption in normal circumstances. These are expenditures in

1. Extension / repair of homes
2. Investment in new houses or plots[11]
3. Dowries
4. Social or religious ceremonies such as marriages, births or deaths.

Henceforth unless said otherwise, the (current) expenditures variable will be composed by items bought and consumed at home and items received for free and will not include the above four extraordinary expenditures (or investment). Some analysis will be developed for the latter to test whether borrowing from MFIs or other credit sources has any effect on these expenses.

[11] How much was invested in houses and how much in plots cannot be separated in the dataset

Figure 9 Box & Whisker graph expenditures per capita by MFI borrowing status

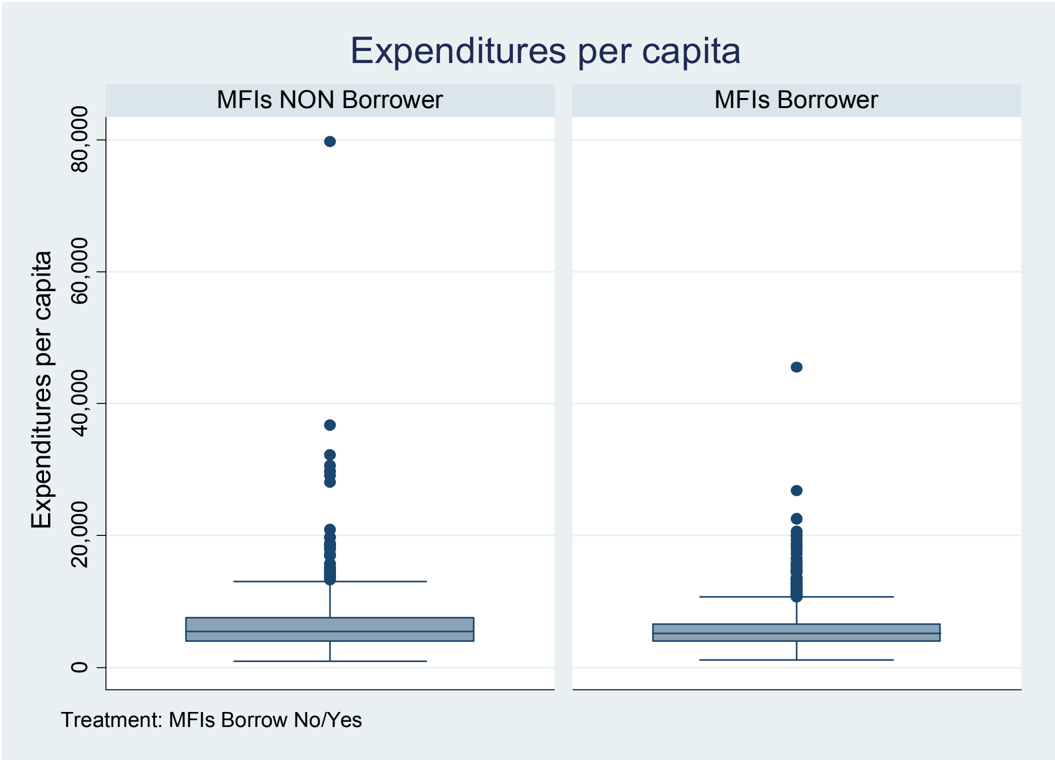


Figure 9 shows the box & whisker graph for expenditures per capita, split by MFI borrowing status. In general non participants show greater outliers and interquartile range. Table 6 shows that medians are lower than the means for both MFI non borrowers and borrowers but in the latter differences are not so noticeable. As happened with income per capita, the mean is lower for the MFI non borrowing group. The expenditures per capita distribution is also right-skewed. A histogram can be seen at the appendix (Figure 20). MFI non borrowers show higher values for dispersion statistics, IQR, standard deviation and range. There is an outlier that might clearly be leading some of the statistics for MFI non borrowers. If the outlier is withdrawn, the skewness drops to three and the kurtosis to 16. Not any zero values are found, which is consistent with the fact that a minimum of consumption level has to be kept in order to survive.

Table 6 Expenditures per capita descriptives by MFIs participation

	expenditures per capita	
	MFIs non borrowers	MFIs borrowers
mean	6,344	5,692
median	5,416	5,161
sd	4,434	2,863
min	977	1,122
max	79,701	45,477
range	78,724	44,355
iqr	3,666	2,720
skew	6	3
kurtosis	88	31

In Table 7 quantiles of expenditures per capita show that the MFI non borrowers have consistently higher expenditures per capita figures from the 25^{th} and then at all the upper percentiles calculated. In the case of income per capita this started to be so at the 70^{th} quantile. The difference in expenditures per capita between MFI borrowers and non borrowers increases at the top quantile but not to the great extent that was seen in income per capita.

Table 7 Expenditures per capita quantiles by MFIs borrowing status

	Current expenditures per capita	
	MFIs non borrowers	MFIs borrowers
p1	1,518	1,802
p5	2,504	2,622
p10	2,930	3,073
p20	3,678	3,704
p25	3,939	3,925
p30	4,206	4,141
p40	4,725	4,666
p50	5,416	5,161
p60	6,162	5,620
p70	7,131	6,279
p75	7,606	6,645
p80	8,106	7,200
p90	10,530	8,862
p95	12,943	10,649
p99	19,658	15,607

Current expenditures quintiles could also be tabulated with borrowing variables. Table 8 shows the percentage of borrowers per quintile for the different credit sources. With respect to microfinance, quantiles at the middle of the distribution tend to show higher proportion of borrowers. Unlike income per capita the proportion of MFI borrowers is not clearly lower in the first quintile. More importantly, the rough evolution in the rest of the sources is that the proportion of borrowers rises as the quintile rises. This progression is smoother than in the case of income per capita and the first quintiles do not show higher proportion than others. This can be a clear sign of a lower volatility in expenditures per capita. The proportion of non borrowers is clearly higher in the first quintile, which is coherent with the income table.

Table 8 % borrowers by expenditures per capita quintiles

	% borrowers by expends. per capita quintile				
	MFIs	Bank	Informal	Relatives	No borrow
q1	45.6%	3.1%	5.4%	12.0%	42.2%
q2	50.3%	6.4%	7.0%	16.1%	33.3%
q3	58.2%	4.8%	7.7%	15.9%	26.9%
q4	51.8%	9.9%	7.9%	19.7%	28.0%
q5	43.6%	10.1%	6.8%	23.8%	36.0%

Table 9 contains the mean loan size by expenditures per capita quintile, not taking into account non-borrowing households. The greatest mean sizes at all quintiles but the fifth are found in the case of microfinance and overall mean size increases as quintiles rise in this case. For the rest of the sources, the progression is not as smooth as in Table 8 but it is clear that loan sizes are higher at the top quintiles and the lowest values are found at the first quintile. The top quintile is showing as well a great difference with respect to the rest.

Table 9 Mean loan size by expenditures per capita quintiles

	Mean loan by expends. per capita quintile			
	MFIs	Bank	Informal	Relatives
q1	18,147	6,750	6,679	9,210
q2	23,650	15,364	8,306	8,152
q3	24,504	10,820	8,753	12,078
q4	28,884	9,939	10,610	11,378
q5	36,292	35,106	38,723	24,760

Income and expenditures as proxies for welfare

The use of income per capita and current expenditures per capita tries to mimic welfare at household level. However, they are different variables and there might be some discrepancies between them when placing households at different quintiles. In the case of income, for instance, there will be some households with negative income that are at the first quintile but they might not be at the first quintile of expenditures and therefore would not be considered so poor in expenditures terms.

Table 10 is a two-way matrix with households and quintiles for income and expenditures. The rows correspond to quintiles by income and the columns to quintiles by expenditures. In each cell, the first row is the number of households and the second is the percentage that this number represents with respect to the total number of households within that income quintile. The diagonal, therefore, gives information about the households that are in the same quintile for both income and expenditures variables. In other words, those households for which quintiles overlap. For example at quintile one for income per capita and expenditures per capita, 42.4% of the observations overlap and around a 54% in the case of the last quintile.

Table 10 Two-way table quintiles income-expenditures per capita

Quintiles income p. capita	**Quintiles current expenditures p. capita**					
	1	**2**	**3**	**4**	**5**	**Total**
1	219 42.4	116 22.4	70 13.5	71 13.7	41 7.9	**517** **100**
2	166 32.1	139 26.9	107 20.7	63 12.2	42 8.1	**517** **100**
3	74 14.3	131 25.3	152 29.4	116 22.4	44 8.5	**517** **100**
4	47 9.1	92 17.8	119 23.0	148 28.6	111 21.5	**517** **100**
5	11 2.1	39 7.6	69 13.4	119 23.1	278 53.9	**516** **100**
Total	**517** **100**	**517** **100**	**517** **100**	**517** **100**	**516** **100**	**2,584** **100**

Table 10 might not be so clear for households that move by one quintile only. Maybe a better way of seeing this is showing Table 11. It shows the difference between income p/c quintile and expenditures p/c quantile following the equation $[Q_{income\ p/c} - Q_{expenditures\ p/c}]$. It can be seen that 936 households coincide in income and expenditures quintiles, which represent the 36% of all households. This can also be obtained adding up the figures in the diagonal of Table 10. About a 38% of the households differ just by ∓ 1 quintile between income and expenditures distribution, so around a 74% of the households are within a range of plus/minus one quintile which can be considered a good overlapping.

Table 11 Q_income - Q_expenditures

Q_income p/c- Q_expenditures p/c		
Qinc-Qexp	**Freq**	**Percent**
-4	41	1.6
-3	113	4.4
-2	177	6.9
-1	450	17.4
0	936	36.2
1	535	20.7
2	235	9.1
3	86	3.3
4	11	0.4

With regard to the big differences between distributions almost a 10% of households differ by three or four quintiles. Households with a value of four in Table 11 are households that despite being at quintile five in income distribution, they are placed at the first quintile of expenditures. For values of minus four this would be on the contrary. Regarding these greater leaps between quintiles, it seems more likely to have households that score at high quintiles in expenditures per capita but low at income per capita than the other way round. These might correspond with households that are not poor but score at the lowest quintiles of income because they spent on the inputs and a bad year of crop or business didn't allow them to get their returns.

Finally, the correlation coefficient between income per capita and expenditures per capita was calculated in 0.3681 and it was significant at 1% level. Also the non parametric Spearman's rank correlation coefficient was significant at 1% level with a value of 0.5128.

Overall there is a good overlapping and we can think of these variables as not distant proxies. There are a number of reasons that make us to trust more expenditures per capita figures than income per capita ones, though. There are a high number of inputs in the calculation of income per capita and also the mentioned accounting rules might value some assets/stocks in ways that bring volatility to the variable. Also, expenses in non regular inputs could not be included in the calculation of revenues because they were not correctly shown in the dataset.

In addition to this, profit reversals in one year might make a household to move down to an income per capita quintile that would not correspond to its wealth. Given the quintiles tables for income per capita (Table 3 and Table 4), the relatively high proportion of borrowers and mean loan sizes at the first quintile might be a sign of this happening.

Propensity Score Matching process

Propensity Score Matching (PSM) tries to resemble a random experiment. In it the random choice of treated and controls, if done properly, leads to the existence of two samples that are equally distributed and they just differ into the treatment. Therefore descriptives of treated and controls from random experiments should show similar values and no statistically significant differences.

Matching methods are used when randomization is not possible and the statistics of treated and controls are different. It is assumed that once the observables (covariates vector X) have been controlled for, the treatment and control groups differ just in their participation status. Thus comparing treated and controls will result into the treatment effect. This assumption is called Conditional Independence.

The process of PSM follows some steps. The first would be to find the covariates that will be used in the logit model to find the propensity scores. Once propensity scores are found and before going on with the matching, the Common Support area has to be defined choosing one of the alternatives. Then the matching can be undertaken adopting one or more of the matching algorithms. Depending on which of these algorithms is chosen, the quality of the matching will need to be assessed following different alternatives. The reliability of the impact estimate will depend on this assessment. Finally, when the estimates for impact effects show statistical significance, it makes sense to test for sensitivity to unobservables that can affect the outcome variable or the participation status.

Sensitivity analysis is based on (Rosenbaum, 2002) work and try to test whether the unobservables are leading the impact estimates. However, this analysis does further assumptions about unobservables and therefore brings more assumptions to the model.

The explanation of the PSM process, for the sake of clarity, is split in the paper as follows. In all parts first there is a theoretical discussion followed by the application to the particular case of our dataset[12]:

1. Covariates choice, logit model and choice of the Common Support area
2. Matching algorithms, matching process and matching quality assessment
3. ATT calculated with the different algorithms
4. Sensitivity analysis

Covariates choice, logit model and choice of the Common Support Area

Covariates choice

When facing matching techniques, increasing the number of covariates X can easily become an insurmountable difficulty. This has been referred to in the literature as "the curse of dimensionality". In their seminal paper, Rosenbaum and Rubin, (1983b) show that the matching can be done over a balancing score b(X), which is a function of X. In the case of PSM, b(X) is the probability of participating in the microfinance scheme, P(X). This probability is calculated with a logit model, although a probit is also feasible. Other transformations of P(X) can be used and the same authors (Rosenbaum and Rubin, 1985) suggest to use $q(x) = \log[(1-P(X))/P(X)]$, because it shows more convenient properties. One of them is that even when the correct weight of the

[12] This section relies heavily on the steps and theoretical background provided in Caliendo, M. & Kopeinig, S. 2008. SOME PRACTICAL GUIDANCE FOR THE IMPLEMENTATION OF PROPENSITY SCORE MATCHING. *Journal of Economic Surveys,* 22**,** 31. and Caliendo, M. 2006. *Microeconometric Evaluation Labour Market Policies. Lecture Notes in Economics and Mathematical Systems No. 568,* Berlin, Springer-Verlag. as a guide to discuss the relevant issues and implement the technique.

observations are unknown, matching on q(x) = log[(1-P(X))/P(X)] provides with a correct probability of participation. In practice P(X) is normally used and this is our choice as well, with the exception of the kernel algorithm where the log odds ratio is used.

These covariates have to be such that the Conditional Independence condition is complied with, that is, participation has to be independent of the outcome variable given P(X). Caliendo (2006) establish the characteristics of the covariates. First, they have to be picked among those which influence both participation and the outcome variable at the same time. Second, no variables that affect just participation are to be part of the outcome model. Besides, perfect predictors should not be included as some variation is needed. Thus, those variables in which P(X) is always either 0 or 1 for some values should not be included into the model. This has to do with the support condition, $0 < P(X) < 1$, that would not be observed in this case.

Sianesi (2004) establishes the arguments that should guide the choice of variables when building the model. Economic theory, a priori considerations, institutional set-up and previous literature should give the foundation when tackling with the inclusion of variables.

When there is not a clear-cut criterion, some authors recommend keeping the model as parsimonious as possible. This is argued by Bryson et al. (2002) who contend that too many parameters increase the risk of including irrelevant variables that might aggravate the support problem. The variance might also be increased. On the contrary, Heckman et al. (1997) argue that leaving aside relevant variables can increase the bias in the resulting estimates. Following this argument, Rubin and Thomas (1996) contend that variables are only to be discarded if they are clearly irrelevant or they have no influence on the outcome. Otherwise, their opinion is that variables should be taken into account when attempting the model.

The dependent variable has three different versions, following the different specifications that were also included in Duvendack and Palmer-Jones (2011). In the first specification (spec 1) the treated group is composed of households that borrowed from MFIs and the controls are those that did not borrow from them. Included in the controls are households that borrowed from other sources together with those that did not borrow at all from any source. In the second specification (spec 2), the treated are borrowers that got their loans from any source (MFIs but also banks, moneylenders and family or relatives) and controls are households that did not borrow at all. Finally, in the third specification (spec 3) the comparison is between borrowing households that got their loans strictly from MFIs vs. households that did not borrow at all. In this case the sample is reduced because those borrowing from alternative sources are dropped from the study, and therefore the sample is reduced to 1,878 observations.

Following the advice in Sianesi (2004), the covariates included in the model that finds the scores were chosen according to the previous literature, such as PK, Coleman (1999), Tedeschi (2008), Chemin (2008) and Duvendack and Palmer Jones (2011). The latter could not exactly replicate the outcomes in Chemin (2008) due to the different reconstructions that they made from the original PK dataset, although they are very similar. The configuration of the models is similar in all these sources, adding covariates that control for individual characteristics of the individual, such as age, gender, religion and so on. Also, other variables are added that control for the composition of the household, education, labour and assets that proxy for individual' or household' wealth. Finally, covariates controlling for village characteristics or village dummies are also normally present in these set ups.

The description of the model is approached splitting it into groups of variables. First, the variables that have to do with the characteristics of an individual: in this case we take the characteristics of the household head: age, gender, religion. The gender of the household head was initially included in the logit regressions. Only in 262 households out of the 2,599 in the sample was there a female household head. This represents around 10%. Using the three different treatment specifications (specs 1-3) as the dependent variable in the logit, five versions of the model were run in the same way as in Table 13. In none of the five versions of the model used for each of the specs of the dependent variable was the gender covariate significant. The coefficients obtained for the less parsimonious version of the model (Table 13, model 5) in the three different specs of the treated-control groups are shown below in Table 12. The rest of the covariates were quite similar. Thus, the variable was withdrawn from the model while keeping household characteristics that showed significance in most of the models, such as the presence of children.

Table 12 Coefficients for gender in the different treated - control specs

	Treated - control specs		
	Spec 1	Spec 2	Spec 3
Gender of houshehold head (dy/dx)	0.019	-0.020	-0.063
z	-0.71	-0.41	-0.99

The models also include variables regarding education. In our case, we used the highest education level of an individual within the household as well as a set of dummies with the different levels: primary, secondary and higher secondary or university education. The category corresponding to non-educated was omitted.

Former models also include variables that have to do with employment and entrepreneurial activities[13]. Our model includes dummy variables that state whether the household head is self employed in agriculture, self employed in other type of activities, wage employed in agriculture or wage employed in non agricultural jobs. The category of unemployed household head was omitted in this case.

Other variables are proxies for household' wealth. We include a proxy for home quality which is "no toilet" (homes without toilet facilities) given the fact that only wealthier households tend to have a toilet.

The presence of children is included in order to capture whether the household is more likely to borrow when they face childcare or education expenses. In addition, given the fact that microfinance is strongly related to entrepreneurial employment, we found that it would also be relevant to control for the distance from a market, bazaar or any kind of business centre where commercial activity could be developed. Village level characteristics are also controlled through the inclusion of average wages for females, prices of staple foods and average interest rates charged by moneylenders. Finally, capturing borrowing from alternative sources is included in the case of the first

[13] In the case of labour information, the criterion is to classify a household under the activity to which the household head devotes more time. In the small number of cases in which the time was equal for agricultural and non agricultural activities, the prevalence was given to the agricultural one. When this conflict happened between self employment and wage employment, the prevalence was given to self employment, understanding that people are more prone to be identified with their own businesses.

specification of treated and controls. For the second and third specs, these variables were perfect predictors of participation or non-participation and thus they were dropped.

The sample size and the numerous questions contained in the questionnaire satisfy the appetite of this "data-hungry technique" (Heckman et al., 1997). In addition, the dataset complies with the requisites that make the CIA plausible. First, the data was gathered with the same questionnaire for all the individuals, treated and controls. Second, there was not a difference between the areas from which the controls and the treated came from and third the survey took place within a short period of time for all the interviewed.

All this makes less likely to have the biases due to geographical mismatch and measurement differences in the outcome variables, for which Smith and Todd (2005) claim that PSM is not designed. Hence, conditions under which the PSM is undertaken are favourable and the CIA is perfectly plausible.

Logit regression

The choice to find out the propensity scores was the logit model. A probit model could also have been used in this case. The dependant binary variable is participate, which takes the value of 1 when the household has a member that has borrowed from microfinance at least once in the past and 0 otherwise. The logit will result into the probability of participation of the household. This will be the propensity score of the household to participate and it is on this score where the matching is done.

For spec. 1, Table 13 contains the coefficients for the different models iterated, from the simpler one (model 1) to the final (model 5). The fifth version resulted into the highest pseudo-R^2, which gives information about the explanatory power of the model regarding the participation probability. Caliendo (2006) and Chemin (2008) adopt the same criterion when selecting the final specification. These low values for pseudo-R^2 are not unusual in these kind of surveys.

Table 13 Logit regression for propensity scores

	Dep variable: Participate in microfinance					
	Model1	Model 2	Model 3	Model 4	Model 5	Model 5 (dy/dx)
Age of the h. head	0.098***	0.115***	0.123***	0.121***	0.121***	0.025***
	(4.80)	(5.45)	(5.79)	(5.67)	(5.67)	(5.67)
Age squared of the h. head	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	0.000***
	(-4.93)	(-5.50)	(-5.47)	(-5.37)	(-5.35)	(-5.35)
Nr. Children < 6	-0.084*	-0.110**	-0.130**	-0.118**	-0.105*	-0.021*
	(-1.65)	(-2.12)	(-2.47)	(-2.21)	(-1.95)	(-1.95)
Nr. Children 6-15	0.136***	0.149***	0.148***	0.153***	0.162***	0.033***
	(3.44)	(3.69)	(3.60)	(3.71)	(3.88)	(3.88)
H. head married	0.187	0.268**	0.116	0.077	0.084	0.017
	(1.41)	(1.97)	(0.79)	(0.52)	(0.57)	(0.57)
H. head islamic	-0.354**	-0.391***	-0.372**	-0.444***	-0.430***	-0.088***
	(-2.41)	(-2.61)	(-2.45)	(-2.87)	(-2.77)	(-2.77)
Village avg. wage male for no agric job	-0.006***	-0.006***	-0.007***	-0.007***	-0.007***	-0.001***
	(-2.82)	(-2.79)	(-3.14)	(-3.34)	(-3.00)	(-3.00)
Village avg. price rice	0.069***	0.072***	0.072***	0.074***	0.074***	0.015***
	(3.44)	(3.56)	(3.53)	(3.56)	(3.55)	(3.55)
Village avg. price flour	-0.108***	-0.104**	-0.099**	-0.132***	-0.149***	-0.030***
	(-2.60)	(-2.46)	(-2.29)	(-2.95)	(-3.31)	(-3.31)
Village int. rate 3mths	-0.003	-0.003	-0.003	-0.002	-0.002	0.000
	(-0.71)	(-0.76)	(-0.70)	(-0.45)	(-0.54)	(-0.54)
Int. Rate 3-6mths	0.002	0.002	0.001	-0.000	-0.000	0.000
	(0.35)	(0.37)	(0.18)	(-0.10)	(-0.00)	(0.00)
Hat/Bazar village	-0.421***	-0.424***	-0.377***	-0.338***	-0.332***	-0.068***
	(-4.73)	(-4.67)	(-4.06)	(-3.60)	(-3.52)	(-3.52)
FoodXeducation	-0.063	-0.090	-0.065	-0.039	-0.006	-0.001
	(-0.52)	(-0.73)	(-0.52)	(-0.31)	(-0.05)	(-0.05)
HH highest education		-0.231***	-0.231***	-0.264***	-0.244***	-0.050***
		(-3.81)	(-3.72)	(-4.19)	(-3.83)	(-3.83)
HH head primary		-0.097	-0.147	-0.166	-0.159	-0.032
		(-0.90)	(-1.32)	(-1.48)	(-1.41)	(-1.41)
HHhd secondary		-0.380**	-0.388**	-0.411***	-0.399**	-0.082**
		(-2.54)	(-2.53)	(-2.66)	(-2.57)	(-2.57)
HHd secondary+		-1.003***	-0.895***	-0.922***	-0.906***	-0.185***
		(-3.30)	(-2.87)	(-2.95)	(-2.86)	(-2.86)
Main s-emp agric			0.076	0.106	0.117	0.024
			(0.53)	(0.74)	(0.81)	(0.81)
Main s-emp no-agric			0.881***	0.889***	0.896***	0.183***
			(6.25)	(6.27)	(6.28)	(6.28)
Main wage agric			0.109	0.204	0.188	0.038
			(0.64)	(1.19)	(1.09)	(1.09)
Main wage no-agric			0.843***	0.868***	0.846***	0.173***
			(4.64)	(4.75)	(4.61)	(4.61)
No toilet				-0.353***	-0.384***	-0.078***
				(-3.17)	(-3.43)	(-3.43)
Dist Pucca rd				-0.038	-0.045	-0.009

	Dep variable: Participate in microfinance					
	Model1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 5 (dy/dx)**
				(-1.12)	(-1.31)	(-1.31)
Dist busns centr				-0.043**	-0.046**	-0.009**
				(-2.08)	(-2.18)	(-2.18)
Source bank					-0.000*	0.000
					(-1.95)	(-1.95)
Source mlender					-0.000	0.000
					(-0.10)	(-0.10)
Source relative					-0.000***	0.000**
					(-3.51)	(-3.51)
Constant	-0.004	-0.087	-0.843	-0.049	0.033	0.033
	(-0.01)	(-0.10)	(-0.99)	(-0.06)	(0.04)	(0.04)
Observations	2,508	2,508	2,508	2,508	2,508	2,508
Pseudo R-squared	0.038	0.061	0.084	0.09	0.096	0.096
chi2	125.519	201.875	278.212	296.063	317.936	317.936
p	0.000	0.000	0.000	0.000	0.000	0.000

*** p<0.10 ; ** p<0.05 ; *** p<0.01**

Table 13 also shows the logit coefficients and marginal effects of the fifth specification in the last column. Out of the total of 2,599 households, 2,584 could be used for the descriptives and only 2,508 for the analysis. The remaining 91 households had at least one missing value for the variables used. This represents a 3.5% of the sample, but it was not considered important for the outcomes. Also, in the third specification of treated-controls, there are some households that had to be withdrawn because they were borrowing exclusively from non-MFI sources.

Further analysis will be carried out dropping households with negative per capita income values in order to test whether the results are much affected by these abnormal income values. The logit models used to find out the scores when these negatives are withdrawn are shown in Table 57 for the three different specifications of models. No remarkable differences were found with respect to the previous estimates.

With respect to the logit model in Table 13, household maximized its probability of participation when the age of the household head is around sixty years old. The signs of variables were outlined before when commenting the mean test. The null of all the coefficients being jointly equal to zero can be rejected in all the specifications (p = 0). After matching, however, this null should not be rejected if the matching is properly done.

The presence of children below 6 years decreases the probability of participation by 2.1 percentage points on average and ceteris paribus. Also, the increase of one taka in the average wage male for non-agricultural jobs decreases the probability of borrowing from microfinance by 0.7 percentage points. This is very low but it makes sense as with high wages people might want to work instead of risking their money. Interest rates of moneylenders' loans seem to have no significant effect.

With respect to education, the more educated the household head, the less likely the household is to participate by borrowing from an NGO. Both secondary and secondary+ (which includes higher secondary and tertiary) education has negative signs but the coefficient is larger in the latter. This is consistent with previous literature (Pitt and Khandker, 1998; Khandker, 2005) and it is a clear indicator of how microfinance targets households with less educated members. Also, household heads not employed in agriculture increase their probability to participate in microfinance to a similar extent than waged and self-employees. Alternative credit sources show negative signs as expected, although estimates are very close to zero.

There are two additional specifications of treated and controls, however. The second would be borrowers from any source vs. non borrowers at all (spec. 2). Basically it is borrowers vs. non borrowers. Finally, a third specification (spec. 3) is MFI borrowers vs. non-borrowers at all. In this third spec, households that are borrowing only from non MFIs sources are dropped from the analysis and therefore the sample is reduced to 1,878 observations[14].

Once we have selected the model that will be used to find the propensity scores, it is common to do a t-test of the differences between the treated and the control groups. This can be seen in Table 14, regarding spec. 1.

[14] All outcomes for spec 2 and spec 3 are shown in Table 56

Table 14 Mean differences before matching

	Variable	Mean		t-test	
		Treated	Control	t	p>|t|
home	HH. head age	45.3	45.1	0.29	0.768
	HH. head age^2	2,206	2,260	-0.97	0.334
	Children 1-5	0.73	0.79	-1.74	0.083
	Children 6-15	1.44	1.19	5.21	0.000
	HH. head married	0.89	0.87	1.72	0.086
	HH. head islam	0.88	0.91	-2.44	0.015
	No toilet facility	0.25	0.27	-0.71	0.477
	Distance to pucca road	1.20	1.38	-2.98	0.003
	Distance to business centre	3.55	3.81	-2.57	0.010
Village	Average wage male no-agricultu	72.49	75.50	-3.45	0.001
	Average rice price	15.29	15.01	2.92	0.004
	Average flour price	12.74	12.80	-1.58	0.115
	Int. rate mlender 3 months	26.23	27.34	-0.64	0.525
	Int. rate mlender 3-6 months	23.35	24.12	-0.53	0.597
	Hat/Bazar in village	0.43	0.33	5.04	0.000
	FoodxEducation in village	0.16	0.14	0.95	0.343
education	HH head highest educ	1.25	1.49	-6.25	0.000
	HH head primary educ	0.25	0.26	-0.51	0.613
	HH head lower secondary educ	0.12	0.18	-4.58	0.000
	HH head above secondary educ	0.01	0.05	-5.14	0.000
job	HH head self employed agric	0.19	0.25	-4.11	0.000
	HH head slf-employed no-agric	0.40	0.27	6.93	0.000
	HH head wge-employed agric	0.14	0.16	-1.33	0.183
	HH head wge-employed non-agric	0.13	0.08	3.41	0.001
credit	Bank/coop loan size	694	2,217	-2.89	0.004
	Informal loan size	716	1,552	-1.85	0.065
	Relative loan size	1,670	3,952	-4.60	0.000

In the table it is tested whether the mean (in the case of dummy variables, the proportion) of the treated (MFI borrowers) is equal to that of the control group (MFI non-borrowers) or not. The *p* values corresponding to the variables for which the null of equal means can be rejected at 5% significance level is shadowed in grey. Thus, the sample is unbalanced between treated and it cannot clearly be assumed that treated and controls have the same distribution as it would be the case if there had been a random allocation of the treatment.

The aim of propensity score matching is, using the observables, to try to find a sample of controls as similar as possible to the treated and then to match treated with controls.

For the former step it is necessary to match on the Common Support area. Doing so we are avoiding the B_1 component of bias as explained in the theoretical chapter (Caliendo, 2006). For the latter step, different algorithms choices will give different weights to the controls used and thus B_2 component of selection bias is avoided.

Common Support (CS)

0 < P(X) < 1 implies that "perfect predictors" (P(X) = 0 or P(X)) = 1) should be avoided and matching has to be done in the region of common support, where a counterpart of the treated can be found among the non treated.

The relevance of the CS condition is also related to the matching algorithm choice. Kernel matching uses all the control observations when matching and therefore it needs to be defined which ones have to be discarded. In the case of Nearest Neighbour or Radius matching the matching is against the closest p-score in the control group and this might not be such an issue but for participants at the tails of the distribution.

(Caliendo, 2006) mentions two different methods to establish the CS area. Although a simple histogram of the distribution of the propensity scores of both groups could give a clear idea of the overlapping, two different approaches have been used in the literature.

Maxima and minima

This method establishes the bounds of the CS with the lower and upper bounds of the distributions of the propensity score of participants and controls. The limits of the CS area will be those within which both control's and participant's distributions have positive density values. If the propensity scores of the control group ranges within the interval [0.1, 0.85] and the participant's [0.05, 0.99], the CS will be given by [0.1, 0.85] as below and above those only the participant's distribution of propensity score.

There are two main drawbacks with this method. The first is that there might be voids in the overlap interval (bin voids in the histogram). The case might be, for example, when there are not controls within the interval [0.20, 0.30] and, nonetheless, the participants in that range are matched. The second is not taking into account observations with propensity scores that might be very close to the limits but outside the range.

Trimming method.

This method is used by (Smith and Todd, 2005). They first study both distributions and discard all the observations which density is equal to zero. They continue their "trimming" by eliminating also the observations that in spite of having a positive density, it does not surpass a minimum threshold. Therefore, in addition to those with null density, a *q* percent of observations with low density are discarded. This method would solve the above mentioned problem with "bin voids" by discarding those observations.

Application of Common Support

As discussed above, the matching has to be done between observations within the CS area, as doing otherwise increases the bias of the estimation. In this area of common support the propensity scores of controls and treated are overlapped. Figure 21 at the appendix shows this clear overlapping and it suggests that most of the observations lie within this common support area. Another graphical representation is found in Figure 22, in this case in histogram form. In the latter it is seen that there are not any "empty bins" in the common support area. Only a small number of observations are left out of common support, at the tails. Thus the main drawback of the maxima and minima criterion to establish the common support area is not present and this method will be used instead of the trimming method. (Nannicini, 2007) suggests that when the option adopted is minima and maxima method there should be a test of the sensitivity of the results to matching with observations that are very close to the bounds of the common support area. However, given the scarce number of observations left outside the bounds, their effect are likely to be negligible.

Output from the *pscore* command (Becker and Ichino, 2002) command for the implementation of the propensity score is found at the appendix, under the heading ***"Outcomes from the propensity score using pscore command"*** which is just a transcription of the results after executing that command in Stata. It can be seen that the range of the common support is eventually established between the values [.06616695, .935185]. This area leaves 919 controls to match with 1,584 treated households. The rest will not be considered in the matching process. After that, it is established whether participants and non participants are balanced. This has to do with the matching quality and will be discussed later when dealing with this issue.

Summing up, in this first stage we have first followed previous literature to find the covariates. These were found through a logit model, choosing the one that maximized pseudo-R^2. Finally, the CS area was established through the maxima-minima criterion as graphs did not show any issues that would suggest using the trimming method instead.

Matching algorithms, matching process and matching quality assessment

Matching algorithms

The models used to obtain the propensities scores are normally probit or logit model, although other options are also used. Once the propensity scores have been found, each treated individual has to be matched with others in the range of values within the common support.

There are several algorithms that have been developed when matching propensity scores. Once the CS area has been defined, the core point is to decide what weights are to be assigned to the matched controls (Blundell and Costa Dias, 2000). A general expression for the matching estimator is given by (following notation in the former paper):

$$\widehat{\alpha_M} \sum_{i\epsilon T} \left\{ Y_i - \sum_{j\epsilon C} W_{ij} Y_j \right\}$$

where T and C are the set of Treated and Controls, i and j denote observations in T and C respectively, W_{ij} is the weight given to the *jth* observation in the control group matching the *ith* observation in the treated group and w_i is the weight to shape the distribution of the outcome for the control group. Different combinations of this general form lead to different matching estimators:

Nearest Neighbour (NN henceforth) estimator

The *ith* observation is matched with one or, less commonly, a set of observations in the control group whose estimated propensity score p_j is the closest to the one of the observation in the treated p_i:

$$C(i) = \min_j \| p_i - p_j \|$$

The matching can be done with or without replacement. The first is considered to be more adequate and operationally useful as better matches can be done for each treatment and the bias will be lower (Caliendo and Kopeinig, 2008). Multiple Nearest Neighbour matching can be seen as a way of reducing variance because more controls are available. But it also allows for worse matches, increasing the bias.

Radius Matching and Caliper Matching

They have the common feature that the matching is done with controls that lie within a neighbourhood (or caliper or radius) of the propensity score of the treated. This value is arbitrarily low. The difference is that caliper matching is a one to one matching and radius matching is done with all the controls that comply with the radius condition.

$$C_i = \{p_j \mid \ \| p_i - p_j \| < r\}$$

where r is the value of the tolerance level.The caliper estimator keeps away the risk of bad matches if the NN is not close enough, but also might impede some matches that could have be done had this tolerance level not been established (although increasing the variance). The radius reduces the risk of bad matches but also might reduce the variance[15] when several good matches are available (Caliendo and Kopeinig, 2008).

Kernel matching

In this case all (or nearly all) the observations in the control group are used and the matching is done over a weighted average where the closest are given the highest weights and the most distant the lowest ones.

Algebraically:

$$\hat{\alpha}_K = \sum_{i \in T} \left(Y_i - \frac{\sum_{j \in C} Y_j G\left(\frac{p_j - p_i}{h_n}\right)}{\sum_{k \in C} G\left(\frac{p_k - p_i}{h_n}\right)} \right) N_T^{-1}$$

Where N_T is the number of elements in the treated sample, G(.) is a kernel function and h_n is the bandwidth parameter. As stated in (Caliendo and Kopeinig, 2008), the choice of the kernel function is not of great importance but the case of the bandwidth parameter

[15] The higher the number of matches the lower the variance of the estimate but also is more biased.

is indeed. Higher bandwidth may lead to a better fit and lower variance but can increase the bias and therefore the trade-off has to be balanced when doing the bandwidth choice. In this study bandwidths of 0.02, 0.04 and 0.06 will be used.

Mahalanobis Metric Matching

The Mahalanobis technique was invented before PSM. The technique is based in the distances between treated and controls. The first step is to sort randomly observations from both groups. Then, all the distances $d(i,j)$ between the each treated and all controls are found. This distance is defined as:

$$d(i,j) = (u - v)'C^{-1}(u - v)$$

Where u is the matrix of the matching variables for participants i and v are the corresponding for non participants j. C is the covariance matrix of all variables for the control group. After finding all distances, the treated i is matched with the control j that minimizes this distance and both observations are dropped from the sample. This goes one until all treated have been matched. The problems mentioned above with respect to no replacement are applicable as well. This algorithm becomes less convenient as the number of covariates rise as the distance, as well as the calculation burden, rises with it. Other versions of this algorithm are found for different versions of C and when the propensity score variable is included as an additional covariate (Guo and Fraser, 2010).

The above methods are to be used in the analysis of the microfinance impact and they should produce similar results or otherwise the impact analysis would not be reliable.

Matching process

Once the samples have been delimited and before applying the algorithms, there is still another issue to handle. When randomization is not the sampling strategy, the share of treated in the survey are normally well above the proportion in the population. Therefore, treated tend to be overrepresented. This is the case for the present survey. (Smith and Todd, 2005) contend that not knowing the correct weights would result in wrong estimation of the probability of participation. Nonetheless, (Heckman and Todd, 2009) state that only a monotonic transformation of the propensity scores is needed to surmount this problem, even when these propensity scores have been found without the proper weights. According to them the odds ratios found with mispecified weights can be used as scores. This is applicable just to match algorithms in which the absolute distance between observations is considered (Kernel and Local Linear Regression matching). In order to avoid this problem, in the case of the kernel algorithm the matching is done on the log odds ratio of the propensity score, $\log(P(X)/_{1-P(X)})$

This is not applicable to NN or caliper, though, as long as in them the rank of proximity is what determines the matching.

The first of the matching algorithms[16] used was one-to-one NN with replacement in order to increase the quality of the matches and reduce bias. This, however, reduces the number of controls matched and increase the variance of the estimator. (Smith and Todd, 2005; Caliendo and Kopeinig, 2008).

Also the caliper method was attempted within 0.01, 0.001 and 0.0001 neighbourhoods. The restriction of matches to a particular caliper is nothing else than establishing a more restrictive common support. The idea is to keep the matches as close as possible. Caliper matching, on the contrary to radius matching, is a 1-1 technique. Radius matching is a 1 to N match, where N is the total number of controls within the "propensity range". The reduction in the size of the neighbourhood also limits the number of matches and increases their quality.

[16] See section 4 for an ample description and discussion about the implementation of the different matching algorithms.

Finally, the kernel algorithm matches on the log odds ratio of the propensity score, $\log(P(X)/_{1-P(X)})$. The approach uses a Gaussian kernel, although other options are available such as Epanechnikov or tricube. Literature states that this choice has not any effect on the final outcome of the impact analysis. The other main choice when dealing with kernel algorithm is the bandwidth. Bandwidth is the proportion of observations of the sample that are taken to do the local averaging in the kernel. This choice has a consequence in the shape of the fitted curve and therefore on the estimates. It is advisable to use more than one bandwidth specification. In our approach it is used 0.02, 0.04 and 0.06 bandwidths. The higher the bandwidth the smoother is the curve and the lower the variance. In kernel algorithm all controls are used.

Testing the significance of the ATTs is another cause of concern when approaching PSM. The main issue is that the variance of the ATT does not take into account the variance of the calculation of the propensity scores or the choice of the common support area (Caliendo and Kopeinig, 2008). The most common choice in the literature and the ones that are easily available in the software are bootstrapping. This is used in (Heckman et al., 1997; Sianesi, 2004), for instance. However, Abadie and Imbens (2008) find that bootstrapping is not adequate to establish statistical significance of the estimates in the case of NN, radius and caliper matching. In these cases, only when the number of controls is higher than the treated in the sample the bootstrapped standard errors are a conservative estimation of the real standard errors.

In the case of NN and caliper matching the option taken is to use the matching variance estimator suggested in Abadie and Imbens (2006)[17]. Although this solution relaxes the homoskedasticity of the approximated standard errors, it still doesn't consider properly the estimation of the propensity score through the logit or probit models. Another solution is suggested in their working paper, Abadie and Imbens (2011), but no software implementation is available yet. In the case of the kernel algorithm, correct inference is obtained from bootstrapped standard errors (Abadie and Imbens, 2008).

[17] PSM was implemented with psmatch2 command: Leuven, E. & Sianesi, B. 2003. PSMATCH2: Stata module to perform full Mahalanobis and propensity score matching, common support graphing, and covariate imbalance testing". *http://ideas.repec.org/c/boc/bocode/s432001.html* Version 4.0.4. The option for Abadie and Imben's solution is "ai" and it will be used in both NN and caliper matching.

Matching quality

The process of matching should be able to balance the covariates in both groups. The different approaches to test the matching quality have been summarized by (Caliendo, 2006). The main point of these tests is to confirm that once we have conditioned on P(X), there should be not any dependence on X. Therefore, an additional condition on X should not provide with any additional information.

$X \perp D_i \mid P(D=1 \mid X)$.

Caliendo (2006) mentions four different indicators that have been used so far by different academics:

- T-test: Based on doing a two sample t-test that show if the difference between the means of each covariate is significant. This is the way shown in Table 14 and Table 55, at the appendix.
- Stratification tests: observations are divided into strata attending to their propensity scores. At each strata the means of propensity should be equal for controls and participants and later t-test are implemented to check that the distributions of variables in both groups are the same, checking first and second moments. Stata command pscore (Becker and Ichino, 2002) not only finds the propensity score of each variable but also split the sample into strata and tests for the means to be equal between the different groups. If they are, it is said that the balance property holds and the CI assumption is more plausible.
- Standarised bias, used in (Sianesi, 2004). For each variable X, it is found out the difference of the sample means over the square root of the variances of both groups, in percentage terms. The same is calculated after the matching, and it is considered enough if the reduction is around or below the 5%. $SB_{before} = 100\left(\dfrac{\left(\overline{X}_T - \overline{X}_C\right)}{\sqrt{\dfrac{V_T(X)+V_C(X)}{2}}} \right)$ and after matching the standarised bias is

given by $SB_{after} = 100\left(\frac{(\overline{X}_{TM} - \overline{X}_{CM})}{\sqrt{\frac{V_T(X)+V_C(X)}{2}}}\right)$ where X stands for mean, V for variance, T for treated C for control and M for matched.

- Joint significance and Pseudo-R^2: The Pseudo-R^2 measures the degree to which the covariates explain the probability of participation. Therefore, the covariates should explain participation before matching but not after the match, as the differences in distributions of variables of participants and controls should not be different any more. Hence, Pseudo-R^2 will be low after matching. By the same token an F-test implemented to test the null of joint insignificance of all regressors should be rejected before matching but not after matching.

If the quality tests fails the explanation could be either a bad specification of the model or that the CIA doesn't hold. In the first case, new specification should be made, including even higher order covariates. In the second, the approach should be to explore additional techniques to do the impact analysis. (Caliendo, 2006)

Matching quality assessment

Quality is normally assessed after the matching because different matching algorithms result into different outcomes for the different quality indicators above mentioned. However, quality is discussed first and then the ATT shown in the paper will come from the algorithm/s with best performance while the rest will be shown in the appendix.

We can start where we left when the outcomes of the *pscore* command (Becker and Ichino, 2002) were used to set the Common Support area. In the appendix, under the heading ***"Outcomes from the propensity score using pscore command"*** close to the bottom it is reported that the sample has been split into seven blocks and that the balancing property is satisfied. This command follows (Dehejia and Wahba, 1999, Dehejia and Wahba, 2002) in their approach to test the quality of the matching. Is is a stratification test. But they tested also the second moments and the *pscore* command only tests for means.

In Table 15 the remaining techniques are shown (pseudo-R^2, the p-value when testing the joint insignificance of the covariates and the median absolute bias) all before and after the matching. It has to be stated that these tests depend on the propensity scores and the common support area, which has been fixed already in the interval [.06616695, .935185] and are independent of the outcome variable (income, expenditures, etc) because they only care about the matching and not about the mean differences in the dependent variable.

The first column of the table contains the pseudo-R^2 before and the second after the matching, the p-value after the matching is in column three and the last two are devoted to the median absolute bias before and after the matching. The first pseudo-R^2 and the first median bias are common to all algorithms because they are calculated in the same way. The after-matching measures show that calipers 0.01 and 0.0001 together with Mahalanobis have a high pseudo-R^2 of more than 0.01 after the matching. This agrees with their p-values. The null of joint insignificance of coefficients can be rejected for these algorithms at 5% level even after the matching. This means that the covariates still have some explanatory power and therefore matching is not as good as we would have expected. Finally, there is not a threshold value with respect to the median of absolute bias to assess if the matching is good or bad. In the table it is clear that the values are lower after the matching. (Caliendo and Kopeinig, 2008) state that a value between 3% and 5% could be adequate. It is also clear that the highest values of absolute bias correspond to the algorithms that had higher p-values.

These matching quality indicators provide with some guidance about how reliable are the outcomes shown in the tables with respect to algorithms. Judging by them, the ATTs calculated with NN, caliper 0.01 and kernels would be the most reliable.

Table 15 Matching quality MFI borrowers vs. MFI non-borrowers

	Matching quality model MFI borrowers vs. MFI noborrwers				
	Pseudo-R square before	Pseudo-R square after	P-value after	Median absolute bias before	Median absolute bias after
NN	0.097	0.007	0.219	10.44	2.05
Radius .01	0.097	0.007	0.225	10.44	1.97
Radius .001	0.097	0.010	0.046	10.44	2.79
Radius .0001	0.097	0.028	0.003	10.44	4.02
Kernel .02	0.097	0.004	0.888	10.44	2.10
Kernel .04	0.097	0.004	0.896	10.44	2.38
Kernel .06	0.097	0.004	0.899	10.44	2.34
Mahalanobis	0.097	0.014	0.000	10.44	2.49

Finally, Table 55 (appendix) shows what was seen in Table 14 but after the matching. It can be realised that after the matching there is only two variables for which the null of equal means can be rejected at 10% significance level, but none at 5% level. These variables are distance to Pucca Road and average rice price in the village. The matching in Table 55 is done with a kernel with 0.02 of bandwidth[18].

A final comment about matching quality is that some papers just report how good an estimate is regarding these matching quality parameters. They do not go on with sensitivity analysis. They just trust the quality parameters and do not add further assumptions in the model. This is the case in (Blundell et al., 2005, Sianesi, 2004). Other sources resort to sensitivity analysis to test whether the unobservables might be leading the estimation (Caliendo, 2006, Duvendack and Palmer-Jones, 2011, Ichino et al., 2007).

ATT calculated with the different algorithms

After all these steps and knowing that the kernel algorithms show the best matching quality, outcomes of these algorithm are shown in Table 55. Also, they had not the inference problems as happened with NN, caliper or radius. The tables contain the ATT estimates, the t value and the lower and upper bound of the confidence interval in order to inform about the statistic significance of these estimates.

[18] Additional tables with 0.04 and 0.06 bandwidth and the outcomes were quite similar.

Outcomes obtained from other algorithms are reported at the appendix but they do not differ much from what it is shown in other algorithms. The outcomes shown refer to several variables and to the different treated and control groups created. All the steps so far have been illustrated with MFI borrowers versus MFI non borrowers. In this case, differences that might come from borrowing from another credit source were accounted for by including the loan size variables. These are the treated and control groups for which the descriptives and all the steps involving covariates choice, logit, matching, etc has been explained so far.

However, it is of interest also to create different groups of treated and controls. In order to study the impact of borrowing from any credit source, we just compared borrowers from any source (banks/cooperatives, MFIs, moneylenders and relatives) vs. non borrowers at all. In a third stage the comparison was done between MFI borrowers vs. non borrowers at all. In this last one, households that borrowed from MFIs and any additional source were not taken into consideration.

Each time the treated and control groups change, the logit model has to be rerun in order to find the propensity scores. Also, the matching quality has to be tested again. The logit regression outcomes for these other two definitions are found in Table 56 (appendix). The matching quality for the case of Borrowers vs. Non borrowers at all is in Table 58 (appendix) and when the comparison is between strictly MFI borrowers vs. Non borrowers at all the table is Table 59 (appendix). Non remarkable differences are found in the logit with the new definition of treated and controls and again the kernel algorithms show overall better matching quality parameters.

Contrary to the two previous PSM approaches, (Chemin, 2008; Duvendack and Palmer-Jones, 2011) the outcomes do not show any significance in the case of expenditures per capita. This difference is most likely given because of the different samples used. They use the first round of the survey and our outcomes are based on the second round, where the households were questioned many years after and there is a large number of households that were not present earlier.

There are three variables showing statistical significance. It is shown a negative impact effect on income per capita in the first two specifications of the treated and control groups. In the case of MFI borrowers vs. MFI non borrowers it is negative and marginally significant at 5% using kernel 0.06. With a mean income per capita or around 7,233 thakas, the impact represents a 13.04% decrease in annual income. It shows also negative and significant when testing borrowers vs. non borrowers and the estimates become non significant when the comparison is between MFI borrowers and non borrowers at all.

Significance at 5% level is marginal and disappears when we exclude borrowers from alternative sources from the comparison and purely MFI borrowers are matched with non borrowers. This might be a sign of credit from other sources leading the decrease in income. In any case, the given that it doesn’t show significance in all estimates and the problems in the construction of the income variable no strong conclusions can be made from these outcomes.

Table 16 Impact effects for three groups specifications

		Algorithm	ATT	t-statistic	Lower bound C.I.	Upper bound C.I.
MFI borrowers vs. MFI non borrowers	Income per capita	kernel_02	-987	-1.86	-2127	8
		kernel_06	-943	-1.98	-2065	-101
	Expenditures per capita	kernel_02	-106	-0.62	-545	188
		kernel_06	-128	-0.89	-382	121
	food expenditures per capita	kernel_02	52	0.55	-153	222
		kernel_06	42	0.44	-118	375
	expenditures 5up	kernel_02	222	1.44	-112	510
		kernel_06	270	2.05	18	481
	home repairs/investment	kernel_02	466	2.81	197	816
		kernel_06	485	2.59	156	838
Borrowers any src. vs. non borrowers	Income per capita	kernel_02	- 1019	-1.96	-2170	-41
		kernel_06	- 983	-1.85	-2066	-8
	Expenditures per capita	kernel_02	-82	-0.40	-514	302
		kernel_06	-86	-0.44	-484	249
	food expenditures per capita	kernel_02	74	0.73	-115	254
		kernel_06	65	0.63	-125	294
	expenditures 5up	kernel_02	527	3.71	247	792
		kernel_06	516	3.59	211	795
	home repairs/investment	kernel_02	646	3.47	297	1010
		kernel_06	612	3.25	222	930
MFI borrowers vs. Non borrowers at all	Income per capita	kernel_02	- 853	-1.47	-1774	395
		kernel_06	-756	-1.41	-1836	236
	Expenditures per capita	kernel_02	-198	-1.06	-570	128
		kernel_06	-226	-1.26	-572	113
	food expenditures per capita	kernel_02	27	0.26	-202	215
		kernel_06	0[19]	0.00	-209	220
	expenditures 5up	kernel_02	345	2.75	103	599
		kernel_06	340	2.74	93	589
	home repairs/investment	kernel_02	487	2.42	134	876
		kernel_06	482	2.48	76	835

Also, borrowing from MFIs or other sources seem to have a positive and significant impact on extraordinary expenses and expenses on home extensions/investments. In the former case, the significance is found at all group specifications, although in the comparison between MFI borrowers and MFI non-borrowers, the ATT calculated with the kernel 0.02 algorithm is not significant. The maximum impact is found in the

[19] Value 0.2 ,but all figures in the table are rounded to integer values.

comparison between borrowers vs. non borrowers at all. In this the impact is estimated at a rise of 44.5%-47% calculated over the mean in extraordinary expenses, 1,375 takas.

Among these extraordinary expenses, those referred to home repairs, extensions and investments are the ones which show the most consistent positive and significant impact in all models. The extent of the impact is also quite remarkable and again the maximum impact is found when comparing borrowers from any source versus non borrowers at all, with a 47%-48% increase in these expenditures with respect to the mean of 1,094 takas. Impact on these extraordinary expenditures, thus, seems to be quite robust and increases when the treatment group includes borrowers from all sources.

Finally, in none of the models borrowing had any effect on (current) expenditures per capita or food expenditures per capita. This is clearly against the conclusion contained in Khandker (2005) where this impact was clearly positive and significant. Therefore, we reach to the same conclusion as Roodman and Morduch (2009) who cannot find a significant impact either, although they replicate the panel approach in Khandker (2005).

Robustness of estimates to the presence of negative per capita income values.

A further calculation was carried out after dropping the negative values. In the second specification of the treated and control groups the number of observations is decreased further as those borrowing just from alternative sources are not taken into account.

Once tested for the quality of the match, again the kernel algorithms performed best, while radius and NN were the worst. The ATT calculated is almost the same for all the specifications of the treated and controls (see Table 17) . For all the specifications the ATT is quite similar whereas the only differences are shown in the significance. In the first specification the kernel with 0.06 bandwidth is not significant, unlike in Table 16. For the second spec, the 0.06 kernel also loses significance. It should be noticed that this significance was marginal. There are no changes in the third specification. This confirms the inconsistency of the impact of borrowing on income per capita in the present study, in contrast with the repeated significance in the case of home repairs and investment expenditures. There might be a negative impact on per capita income but the outcomes are not conclusive. Following Caliendo (2006) no sensitivity analysis is carried out as the evidence of significance is weak.

Table 17 ATT with no negative observations for income per capita

	Algorithm	ATT	t-statistic	Lower bound C.I.	Upper bound C.I.
spec 1	kernel_02	-916	-1.80	-1831	127
	kernel_06	-891	-2.01	-1744	47
spec 2	kernel_02	-1033	-2.09	-2109	-77
	kernel_06	-974	-1.74	-2133	153
spec 3	kernel_02	-827	-1.59	-1838	216
	kernel_06	-756	-1.40	-1881	212

Sensitivity analysis

The PSM analysis is based on the CIA assumption that states that once matched on the P(X) outcomes are independent of the participation status. Thus, a comparison of these outcomes between these treated and controls would result into the impact effect. As stated above, the assumption was like this:

$$Y_{0\,,}Y_1 \perp D|P(X) \tag{1.41}$$

This assumption is not testable but there have been some attempts to try to model the effect that one unobservable might have on participation and outcome variables. Most of these approaches are based on the works by Rosenbaum and Rubin (1983a), Rosenbaum (1987) and Rosenbaum (2002). They are not so widespread and some references used in this study do not use them (Sianesi, 2004, Blundell et al., 2005). Eventually, this kind of sensitivity analysis also relies on assumptions about the unobservables. Thus, more assumptions are called into the estimation. Applications of this analysis can be found in Caliendo (2006); Ichino et al., (2007) and Guo and Fraser (2010).

The sensitivity approach assumes the presence of a variable, U, which is unobservable. The assumption in (1.46) is not valid any more as long as U can affect participation and outcome variables. Thus, the new CIA should take into account not only X but also U and thus:

$$Y_0 \perp D|X,U \tag{1.42}$$

In the new framework the outcomes of the controls are independent of participation given the covariates and the unobservable U. Hence,

$$E[Y_0 \perp D = 1|X] \neq E[Y_0 \perp D = 0|X, U] \quad (1.43)$$

and it has to be changed by:

$$E[Y_0 \perp D = 1|X, U] = E[Y_0 \perp D = 0|X, U] \quad (1.44)$$

The unobservable U is also assumed to have some characteristics. It is a binary variable and it is conditionally independent of treatment. In the theoretical framework used in Ichino et al. (2007) the outcome variable is also assumed to be binary ($Y_0, Y_1 \in \{0,1\}$). This is not incompatible with continuous variables as they are transformed using a threshold (mean, median) above which the outcome variable switches from zero to one.

The confounder U is characterized by the different combinations of treatment and outcome variables taking the value of one or zero. Its distribution is fully characterized by these parameters:

$$p_{i,j} \equiv Pr(U = 1|D = i, Y = j) = Pr(U = 1|D = i, Y = j, X)$$

where D refers to the treatment and Y to the outcome and $p_{i,j}$ is the probability of U being equal to 1 for D = i and Y = j where $i, j \in \{0,1\}$.

They result into different probabilities of U = 1 that are attributed to each observation (household). These values are included in the model as a new variable and then the propensity score together with the ATT are calculated applying matching algorithms as usual.

The approach also calculates the odds ratio of U for outcome and selection effects, Γ and Λ respectively, as follows:

$$\Gamma \equiv \frac{\frac{Pr(Y = 1|T = 0, U = 1, X)}{Pr(Y = 0|T = 0, U = 1, X)}}{\frac{Pr(Y = 1|T = 0, U = 0, X)}{Pr(Y = 0|T = 0, U = 0, X)}}$$

and

$$\Lambda \equiv \frac{\frac{Pr(T = 1|U = 1, X)}{Pr(T = 0|U = 1, X)}}{\frac{Pr(T = 1|U = 0, X)}{Pr(T = 0|U = 0, X)}}$$

In the case of the outcome effect, Γ is interpreted how much U increases the relative probability of Y taking the value of one (or being above the threshold used to transform it into a binary if Y is continuous). Λ is interpreted as how much U increases the relative probability of D taking the value of one, or, in other words, the relative probability of participation.

The interpretation of the sensitivity analysis has to take into account then not only the recalculated ATT estimates but also the outcome and selection effects. Estimates will be much more robust when they do not deviate much from the original when the outcome (Γ) and selection effects (Λ) are high. Confounders that don't increase the mentioned relative probabilities are less informative as they are not expected to cause much change in the point estimates. The approach attempted in the present study is based in Nannicini (2007) and Ichino et al. (2007) through the stata command sensatt[20].

Sensitivity analysis applied

Sensatt command allows creating binary confounders (or binary transformations of continuous variables). These confounders are nothing else than new variables added to the model that are thought to cause changes in the outcome and selection variables. One advantage of this approach is that it allows keeping all the variables in the original model on the contrary to other sensitivity analysis in which some different specifications of the model are compared.

The confounders were found among variables that were considered to have an effect over both outcome and selection variables. At household level they are:

---

[20] Other commands are available in stata for this sensitivity analysis. At the time of writing the command called mhbounds, by Marco Caliendo and Sascha O. Becker is used only when the outcome variable is binary and ours is continous. In the case of ***rbounds*** (by Markus Gangl) the dependent variable can be continuous but it only calculates the sensitivity of the NN one-to-one algorithm. The matching quality of the kernel algorithms was far better and therefore the choice was to test sensitivity for these estimates. The only command that allowed testing estimates found out with a kernel algorithm was ***senssatt.***

- Old household head: taking the value of one when the age of the household head is above the 75th percentile, 55 years old.
- Electric: the home has electricity or not.
- Agricultural job: if the household head has an agricultural job.
- High value for domestic assets: the value of domestic assets is above the 75th percentile.
- High extension of land: the extension of land of the household is above the 75th percentile

And at village level:

- Average price rice: whether it is above the 75th percentile or not.
- Development activities: whether in the village or around there are some development activities such as construction of a road.

Old household heads and homes with electricity would be expected to be less likely to participate. Also, old household heads would as well be expected to get less out of the loans and households with electricity would be expected to have higher returns from the money borrowed. On the other hand, households with high extensions of land and high value of domestic assets should also be less likely to participate as they are not target clientele for MFIs. At village level, villages with high rice price might increase the likelihood of participation as dwellers have to keep consumption levels even when staple foods are expensive. On the contrary, in villages with food for education programs households should ease their needs of buying food as some are covered by these programs.

Outcomes can be seen in Table 18 for the case of MFI borrowers vs. MFI non borrowers. The table contains all the confounders and the variables that showed statistical significance. Outcomes are limited to the kernel algorithm (for 0.06 bandwidth). The column ATT(target) refers to the values originally calculated (contained in Table 16). The ATT (conf) are the estimates after adding the confounder U. The column % is the percentage calculated with the following formula: $\frac{\text{ATT(target)} - \text{ATT(conf)}}{\text{ATT(target)}}$. And the confounder effects columns refer to how much the confounder increases (or decreases) the relative probability of having the outcome variable above the mean (in this case as continuous outcome variables are dealt with). In the case of the treatment variable it refers to the relative probability of participating (D = 1).

Hence, for instance, let's see the case of income per capita the confounder electric (electricity at home or not). We assume that the unobservables follow a distribution like this confounder. This confounder breaks the CIA because it affects the relative probability of having income per capita above the mean by a factor of 3.64. Also decreases the relative probability of being borrower by more than 30%. Under these circumstances, that deviate from CIA, the ATT is calculated as -594. This is higher than the originally calculated at 943 with kernel .06. This is a remarkable swing that led us to question the significance of the ATT estimates with respect to income per capita.

Overall, in this case, what it is seen is that the income per capita estimates are more prone to swing around the ATT calculated without confounders. This means that the outcomes are not so robust. These confounders, apart from "electric" and "asset high" do not pose an extreme challenge on the original estimates as they do not make relative probabilities to increase very much. Thus, a more stable behaviour would be expected if the estimates were robust.

The case of extraordinary and housing expenditures is slightly different as no such swings are found, meaning that the estimates are more robust in this case. This pattern is repeated in the sensitivity tables for Borrowers vs. Non borrowers at all and MFI borrowers vs Non borrowers at all (Table 63 and Table 64 at the appendix, respectively) which underpins the argument that expenditures estimates are less sensitive to unobservables than income per capita outcomes. In the last table income per capita is not included because the estimates are not statistically significant.

Table 18 Sensitivity analysis MFI borrowers vs. MFI non-borrowers

		MFI borrowers vs. MFI non borrowers				
		estimates & sensitivity			Confounder effect	
		ATT(target)	ATT(conf)	% U	Outcome	Selection
Income p/c	Hhead Old	-943	-863	8.5%	0.53	0.76
	Electric	-943	-594	37.0%	3.64	0.63
	Agriculture job	-943	-934	1.0%	0.32	0.73
	Asset high	-943	-1,045	-10.8%	5.71	1.47
	Land high	-943	-1,088	-15.4%	1.02	0.22
	Av. Rice price high	-943	-792	16.0%	1.33	1.23
	Development activities	-943	-787	16.5%	1.31	1.66
Extraordinary expenditures p/c	Hhead Old	270	281	-4.1%	0.80	0.77
	Electric	270	269	0.3%	1.72	0.65
	Agriculture job	270	257	4.8%	1.29	0.67
	Asset high	270	303	-12.0%	1.90	1.52
	Land high	270	265	2.1%	1.59	0.21
	Av. Rice price high	270	309	-14.2%	1.13	1.10
	Development activities	270	289	-7.1%	1.98	1.67
Housing expenditures p/c	Hhead Old	485	488	-0.6%	1.28	0.76
	Electric	485	471	3.0%	1.60	0.62
	Agriculture job	485	492	-1.3%	0.88	0.71
	Asset high	485	491	-1.2%	1.34	1.60
	Land high	485	484	0.3%	1.48	0.22
	Av. Rice price high	485	480	1.0%	1.26	1.16
	Development activities	485	478	1.5%	1.24	1.99

Some literature doesn't include this kind of analysis as it eventually relies on assumptions that can be as correct or as wrong as the assumptions used in the PSM technique. Examples of these are Sianesi (2004) and Blundell et al. (2005) in which they just turn to the tables of matching quality indicators to support their estimates. On the contrary, Caliendo (2006) and Ichino et al. (2007) turn to this sensitivity analysis.

Conclusion

The present chapter tries to apply PSM to find out the impact effect of microfinance borrowing or borrowing from any source on income per capita and on current and extraordinary household expenditures per capita at household level. First MFI borrowers are compared with MFI non-borrowers but given the presence of other sources of credit, it was also tried to estimate the impact of borrowing from any source and the impact of MFI borrowers vs. non borrowers at all.

The impact on income per capita is negative and marginally significant at 5% when MFI borrowers are compared with MFI non-borrowers and when borrowers are compared with non borrowers at all. Thus, this can be interpreted that the negative impact happens when borrowing is done strictly from other sources than MFIs. However, no strong statements could be done on either of these outcomes. First because we found problems in the construction of the variable and it remains quite volatile. Also, estimates are marginally significant and the sensitivity analysis pose some doubts on them as they tend to swing when confounders are included in the model, even when the confounders are not quite challenging. This means that unobservables might be leading these outcomes.

In the comparison with the past literature, the most remarkable outcome in the study is that no impact of borrowing from microfinance or any other source is found on household current expenditures per capita in general or on household food expenditures per capita. This is in contradiction with the conclusions of Chemin (2008), Duvendack and Palmer-Jones (2011) and Khandker (2005). It might be due to the fact that we use different datasets. With respect to the former two the sources are completely different and in the case of Khandker (2005), its sample from the second round of the survey is only two thirds of the households that we use. Additional problems of reconstruction of the dataset might be possible, as happened with Duvendack and Palmer-Jones (2011), due to the complexity of the configuration of the datasets and their poor documentation. The dataset reconstructed by Roodman and Morduch (2009) is now available and an interesting approach would be to use both rounds to implement a DID-matching approach in the same fashion as Smith and Todd (2005).

The main outcome of the study is that when splitting the expenditures into current and non-current, there are some extraordinary expenses that increase when households borrow from MFIs but also from the rest of the sources available. In fact the rise is higher when borrowers from any source are compared with non borrowers at all. When extraordinary expenses refer to house extensions and investments in home/land the estimates show significance in all the specifications of the treated and control groups. The sensitivity analysis underpins this significance as the estimates are quite stable in the presence of confounders and not great swings are found around the original estimates.

Thus, the study finds a positive and significant impact, although not in the variables in which previous literature had found it. It also contributes to microfinance impact evaluation showing that extraordinary expenses and specially expenses per capita in housing repairs, extension and investments are strongly increased when households borrow from microfinance and other sources. This is a sign of how households might be diverting funds to non entrepreneurial activities.

Chapter 3: Microfinance impact in India: A case study in Andhra Pradesh.

Watershed programs and village banking

Watershed programs

The main aim of the present chapter is to find out the impact of microfinance on income and income per capita at household level. For that purpose, a panel dataset with two time periods, 2005 and 2007, is used. The database at hand comes from surveys that tried to evaluate watershed programs in five districts of inland Andhra Pradesh, India, under two different approaches: the Drought Prone Area Programme (DPAP henceforth), developed by the Indian Ministry of Rural Development and the Andhra Pradesh Rural Livelihood Project (APRLP or RLP henceforth), developed by the Department For International Development (DFID).

In the definition by Kerr (2002), a watershed is “an area from which all the water drains to a common point”. Watershed programs basically create infrastructures that allow to store this water so that rain fed crops do not need to rely so heavily on the weather conditions, mainly in drought-prone regions. In India, these watershed programs date back to the sixties and seventies when they were basically top-down designed with no participation of the communities. This approach changed progressively to a bottom up configuration, in which participation is a key element of the projects.

The relevance of microfinance within this watershed framework comes from the fact that these projects don’t foster just to ease the effects of variations in rain patterns, but also involve other measures such as road construction, regeneration of common lands, encouragement of participation for all social groups and the issue of rights and bans.

In both projects, DPAP and RLP, microfinance is one of the key measures adopted. In the former as a complementary project originally designed by the World Bank called *Velugu*. In the latter, the creation of Self Help Groups (SHGs henceforth) is encouraged and assessed by project personnel and members also have access to capacity building and microfinance. These SHGs also cluster themselves in Village Organizations (VO) which, in turn, participate in the decision organs of the project and suggest measures at village level. In these organizations there is a minimum share of target groups such as women, or the poorest in order to guarantee their participation in decisions about the projects.

Other measures to grant a correct distribution of benefits from the project are training programs that increase the employability of the less favoured individuals, exclusive fishing rights in water bodies for the landless or rights to tank/lift irrigation to landless and exclusive grazing rights to shepherds and others. All these measures, together with the minimum quotas in the project decision organs are thought to distribute equally the benefits of these watershed interventions.

In this dataset, general information at household level and credit data in particular allows to do a proper assessment of the impact of credit sources on the outcome variables, with a particular interest in microfinance.

Microfinance in watershed programs: Village Banking.

The microfinance scheme followed both in DPAD and RLP is “village banking”, that has some differences with the Grameen approach, for example, and also differs from others as some examples seen in Latin America that focus on individual loans.

These SHG are mainly created for the credit scheme but other reasons such as capacity building for women and program participation are encouraged. The groups are formed by around 10-20 women from a similar socioeconomic background in order to avoid discrimination within the SHG. Once the group is formed, they start to meet regularly to assimilate the rules and objectives of the SHG and then they start saving and keeping records of the meetings and amounts saved.

The amount can be very small but contributes to inculcate financial discipline and reduces the dependence on moneylenders. Also special savings can be accepted in the group and the decision about the interest paid to them should be decided by the group. The savings are deposited into one account in the bank and the first source of loans to the members will be these savings. In addition to this, with the passage of time and provided that the SHG runs smoothly its lending activities, there will be additional sources for the loans. These are interests accrued from the lending activities, grants or even bank loans (made to the SHG, not to individuals).

These credit activities within SHGs have several advantages Masset and White (2006):

- The interests paid for the loans do not end up in the moneylender's pockets (or banks, landlords, traders, etc) but go back to the group fund.
- Money is quickly available, the process relies on trust and peer monitoring so there is not so much paperwork.
- There is a prioritisation to the neediest in terms of group training and quicker access to funds.

SHGs are also a forum to exchange opinions and knowledge. In the particular case of RLP watershed projects, SHGs are encouraged to submit proposals to Watershed Committees in order to involve them in the implementation of measures. The main interest of this study is the credit schemes but they are incorporated within this watershed framework and, therefore, some discussion of them will be necessary in the following sections.

This chapter intends to measure the impact of microfinance on income and income per capita at household level. For this, Coleman's model is used in the same way as done in Tedeschi (2008). She runs a pooled OLS and a Fixed Effects (FE) panel data approach. In order to control for the possible effects on the estimation of outliers or endogeneity, we also attempt alternative methods. Overall, it is found that the impact of microfinance on income per capita is positive and significant, even using methods less sensitive to extreme values.

The chapter is structured as follows. In the next section the sampling strategy is discussed, together with some descriptives of the outcome variables and credit sources. The third part includes the main analysis of microfinance impact using a a panel fixed effects, MM estimator and IV approaches. Then, the negative values for income per capita withdrawn and the same models are run to test the robustness of the outcomes when these abnormal values are not present. The final section revises the results and concludes.

Survey and descriptive statistics

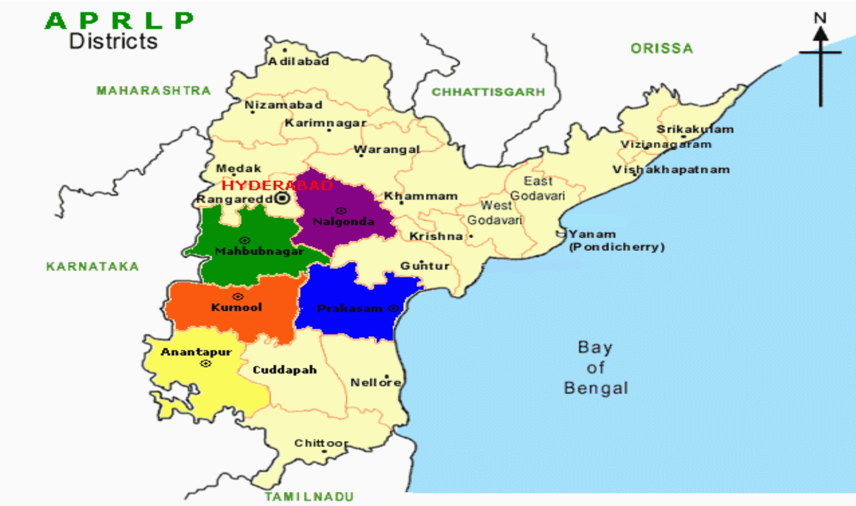
Survey

The survey was thought up with the watershed programs in mind and therefore a description of this general framework is needed. The interest of the study, however, is not on watershed areas but on microfinance. The sampling strategy includes a control area where watershed projects are not present. However, this does not apply to microfinance services. Microfinance was present in all villages and thus no control group was available. This happened also in the case of the second round of the Bangladesh survey because microfinance organizations were present in all villages. It follows a description of the sampling strategy that is mainly focused on the watershed issues. This does not affect the main aim of the study, though, which is the assessment of microfinance impact.

The survey was implemented in order to compare the approach of the Rural Livelihood Project (RLP), designed by the British Department for International Development (DFID) with the DPAP approach by the Indian Ministry of Rural Development that started in the mid-1980s. Although the latter methodology greatly increased the autonomy of villages in the decision-making with the new 1994 guidelines, the RLP insists on some measures that encourage the participation of the poorer and ensure more egalitarian distribution of benefits. An additional control group which had not had any watershed interventions at all in the last 5 years or more was also included.

The survey took place in the state of Andhra Pradesh. Indian states are divided into districts which, in turn, are sub-divided into mandals. Each mandal is made up of a number of villages. The RLP was operating in five districts of Andhra Pradesh (Prakasam, Kurnool, Anantapur, Mahabub Nagar and Nalgonda), seen in Figure 10, mainly in the south of the state, the most drought-prone area. In each district 2 RLP villages, 2 DPAP villages and 2 non-project villages were selected, producing a total of 30 villages.

Figure 10 Districts



The RLP is implemented in 500 watersheds within these five districts. In the selection of watersheds, the RLP chose mandals in a first instance and then the villages within these mandals in a second stage.

Firstly, the criteria to select mandals were based on a score which includes two different parts:

- One part is based on resources degradation, which includes rainfall and land degradation data, based on graphical information obtained from satellite pictures.
- The other is based on a social deprivation index, including inputs of income poverty, services access, etc.

The aim was to have a range of mandals with uniform characteristics within and across districts.

The RLP targeted villages within these selected mandals. An additional score was used to pick the project villages. It was based on nine criteria: among them, for instance, the percentage of small and marginal farmers, the percentage of women organized in SHG, status of groundwater, livestock population, etc. Each of these nine characteristics was given a different weight and their combination provided a score. Villages with a higher number of watersheds or villages where leaders belonged to the ruling party were prioritized in the case of equal scores. Information from the census of 1991 was used for this score selection.

Some exclusions were applied:

- Villages already covered by watershed programs
- Villages of less than 300 hectares
- Urban villages and Mandals headquarters

Finally, there was a further validation of the selection based on qualitative techniques and consultation with the authorities at village and district levels. This was aimed at confirming that all the chosen villages were actually deprived in geographical and social terms.

Thus, the RLP first selected mandals and, in a second stage, villages within these mandals. In the survey used for this study, the sampled RLP villages were picked in the following way. First there was a random selection of two RLP villages in each district. For each of the RLP villages a DPAP and a “non-watershed” village had to be pulled out. The intention was to have control DPAP and “non-watershed” villages that had similar characteristics to the RLP ones.

For this purpose, those control villages located within the same mandal of the RLP village were first prioritized, assuming that they would be more similar in terms of micro-climatic characteristics and ground water levels. There was more than one eligible village in each mandal. Therefore, within each mandal the matches were selected among those DPAP and “non-watershed” villages that had the same nine-criteria score previously used to appoint RLP villages within the selected mandals. Even after matching on this score, there was normally more than one eligible DPAP or “non-watershed” control.

Whenever more than one potential match for the RLP village was available for selection, a propensity score was used, estimated from data based on the 2001 census. In this way, the matching was not based only on level characteristics but also on trend similarities. This second score included ten variables, among them population size, literacy rate, percentage of farmers or distance to the nearest town (Masset and White 2006).

The population of the districts where the project is present differs. Districts also differ in terms of geographical and social characteristics but for practical reasons the number of villages selected by districts is the same, although it could have been done in proportion to district population size. Thus the sampling of project villages is stratified by district and the analysis requires sampling weights that are defined below.

The weight (w) of the *ith* household is the inverse of its probability of selection (p_i) :

$$w_i = \frac{1}{p_i}$$

where the probability of selection is the product of the probability of district selection (p_{id}) times the probability of village selection for household i (p_{iv}) :

$$p_i = p_{id} x\, p_{iv}$$

In turn, the probability of selection at the district level is the fraction of the district population (N_d) over the total project population (N). The probability of selection at village level is the ratio of village population (N_v) to the district population:

$$p_{id} = \frac{N_d}{N}$$

$$p_{iv} = \frac{N_v}{N_d}$$

The survey took place twice during the years 2005 and 2007. Due to the seasonal characteristics of agriculture, the time in the year when the surveys take place is crucial. Hence, surveys were done at the same time of the year in order to minimize the effect of seasonality. The first round was conducted in June 2005, covering the 2004 kharif and 2005 rabi season. The second had to be done in June 2007, gathering information from the same seasons in 2006 and 2007 (Masset and White 2006).The number of total households interviewed was normally 50 per village, with some exceptions: in one village the number of households interviewed was 43 (Penchikala Pahad), in another there were 46 (Nela Marri), there were 2 villages with 48 (Amangal, Kambhan Padu) and 3 with 49 households (Chiramandoddi, Muddinayapalle, S. Kothapalli). This adds to a number of 1,482 households in 2005, 21 of which had migrated when the second round took place. These migrant households could not be traced back and therefore are not present in the second round of the survey.

The breakdown of these villages is given as follows. The survey used 30 different villages, 10 RLP projects, 10 DPAP projects and 10 where there had not been a watershed project in the previous 5 years at least. The number of household interviewed in each project is 495 for RLP, 500 in DPAP and 487 in non-watershed areas.

Migrants

As mentioned above, 21 out of the 1482 households present in the first period could not be traced back in 2007. In Table 19 means of the subsample of households that did not migrate are compared with means of the migrants in 2005. The migrant households tend to be smaller in size and this drives the fact that also the rest of the household composition variables such as number of males, females, etc. are different and statistically significant.

Migrants are also less prone to be self employed, and the total time they work in the household is lower. However, this difference is accounted for by the differences in sizes, as there are not significant differences in the time worked per working member of the household. They are also poorer in terms of assets. They had suffered fewer shocks in the last 12 months and their mean loan sizes were in general smaller than those of non-migrants. The household income is clearly similar, but the income per capita is far higher in the case of migrants, which has to do with the fact that their mean household size is scarcely greater than half that of the non-migrants.

The share of migrant households is small and excluding them is unlikely to cause any significant differences in the estimations. In Table 65 (appendix) the mean test is done for 2005 between the mean calculated with all households and the mean calculated after dropping the migrants. It can be seen that the mean differences are small and in all cases quite far from being statistically significant even at 10% level. Thus, they are dropped and will not be included in the statistics henceforth.

Table 19 Mean test repeating vs. migrant households

	ttest Panel sample = 1461 Migrants = 21			
	Panel	migrants	t_value	p_value
HH head age	45.8	50.2	1.13	0.26
HH head age^2	2,232	2,703	1.23	0.22
HH head sex (1 = male)	0.90	0.75	1.18	0.24
HH head married	0.90	0.60	2.01	0.05
HH head muslim	0.96	1.00	6.69	0.00
HH size	4.86	2.77	4.85	0.00
HH nr of males	2.53	1.36	4.19	0.00
HH nr of females	2.33	1.41	3.06	0.00
HH nr child <=6	0.23	0.12	1.88	0.06
HH head education	1.40	1.11	4.58	0.00
Female max education	1.62	1.27	1.49	0.14
Male max education	2.12	1.25	3.41	0.00
HH max education	2.24	1.73	2.50	0.01
Scheduled caste	0.22	0.30	0.52	0.60
Scheduled tribe	0.12	0.00	12.36	0.00
Backward caste	0.48	0.50	0.26	0.79
Upward caste	0.18	0.21	0.20	0.84
HH head unemployed	0.10	0.18	0.72	0.47
HH head main job agric	0.77	0.61	1.11	0.27
HH head main job non-agric	0.13	0.21	0.63	0.53
HH head no job	0.10	0.18	0.72	0.47
HH head self employed	0.55	0.22	3.32	0.00
HH head wage employed	0.35	0.60	1.90	0.06
HH head job time[21]	1,193	1,202	0.03	0.98
HH job time[22]	3,439	2,302	2.70	0.01
Job time per capita[23]	1,278	1,335	0.17	0.86
Walls of composite material	0.13	0.03	4.05	0.00
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Value of Jewelry	5,174	6,291	0.33	0.74
Asset Index	0.72	0.64	0.45	0.65
Land area (acres)	3.37	1.89	1.90	0.06
Land value (rupees)	32,080	16,343	2.28	0.02
Livestock value (rupees)	3,367	1,034	4.60	0.00
Suffered shock	0.61	0.23	3.72	0.00
Borrowed from bank y/n	0.36	0.18	1.78	0.08
Borrowed from SHG y/n	0.29	0.16	1.36	0.18
Borrowed from NGO y/n	0.01	0.02	0.44	0.66
Borrowed from moneylender y/n	0.49	0.53	0.25	0.81
Borrowed from family-friends y/n	0.15	0.03	3.64	0.00
Borrowed from others y/n	0.09	0.02	3.36	0.00
Loan size bank	16,685	17,971	0.25	0.80
Loan size SHG	6,144	2,131	4.19	0.00

[21] Time worked by the household head
[22] Time worked by all members of the household
[23] Total number of hours worked by all members of the household divided by the number of household members

	ttest Panel sample = 1461 Migrants = 21			
	Panel	migrants	t_value	p_value
Loan size NGO	18,970	3,000	1.16	0.27
Loan size moneylender	19,689	10,673	4.01	0.00
Loan size family-friends	23,399	5,000	4.78	0.00
Loan size others	15,566	6,000	4.81	0.00
Total amount borrowed all sources	26,465	15,773	2.81	0.01
Income (rupees)	26,552	26,484	0.02	0.99
Income per capita (rupees)	5,768	12,216	2.11	0.04

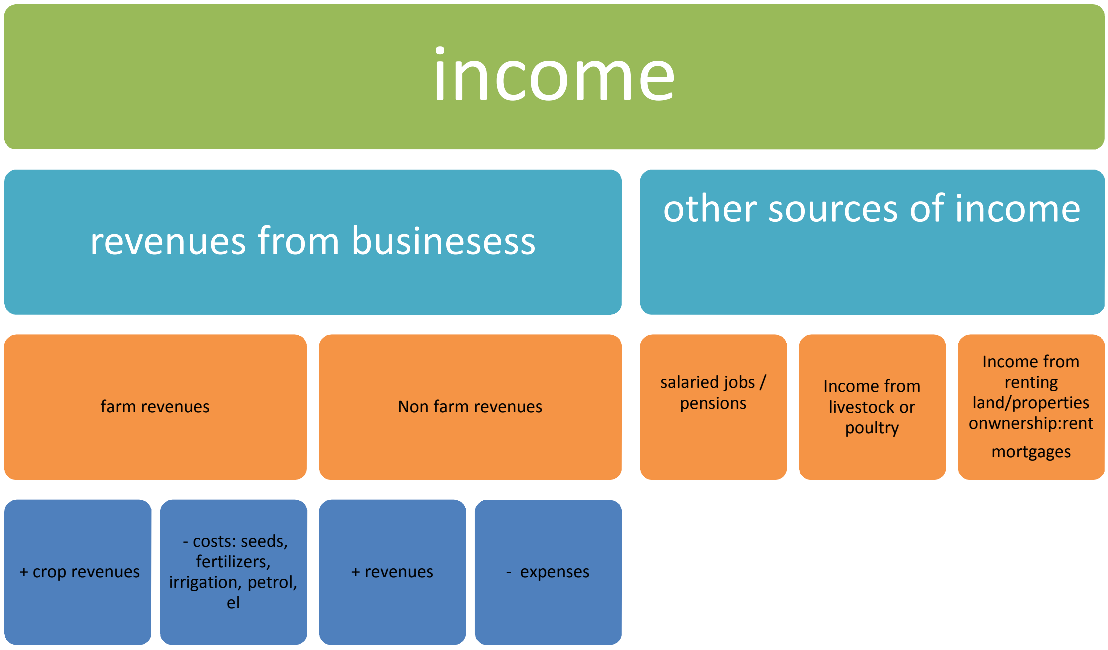
Descriptives of the outcome variables.

The present study intends to find out the impact that borrowing from SHG schemes might have on income per capita and income. These variables are normally skewed to the right due to the high values of the wealthiest observations, households in this case. Henceforth, descriptives and analysis outcomes of income per capita will be normally be shown and discussed in the text while, in many occasions the ones referred to income will be contained in the appendix.

As happened with the calculation of income in the Bangladeshi study, there are several households that show negative values for income and income per capita. Income per capita is calculated by dividing the total income by the number of household members and thus we will first describe the composition of the income variable.

The survey includes personal information about the members of the household as well as the activities in which they are involved. Income is made up as follows:

Figure 11 Income variable composition



The variable is composed of two main blocks: The first is composed by the revenues obtained from businesses and the second by revenues from other activities. In the former group the most important activity is farming, although there is information about other activities that are under the "non farm revenues" heading. In both cases we found the net revenues, subtracting the cost of inputs from the sales of the harvests or any other products.

The second block of the income variable is divided into three different parts. The first is composed of salaries from waged jobs and pensions. The second is the income obtained from raising livestock and finally the third includes the resources obtained from rentals of land or other kind of properties. Livestock could have been considered as a part of farm revenues but it was separated in the survey.

As happened with the Bangladesh dataset, there exists a number of observations that show negative income (and therefore income per capita) values. This was also discussed earlier and it is mainly due to the fact that households invest in inputs such as fertilizers, seeds or others and then the revenues obtained from the sales of the crop or any other product is not enough to cover the costs. In this case there is an actual inflow and outflow of cash and this is measured to find the net income.

In the case of livestock, accounting rules were followed that might be questionable. Animals consumed at home are accounted as a sale at market price and animals born at home as purchases at market price. In these cases there were not outflows or inflows of cash and therefore it brings to the variable an element that might add to measurement error issues.

In the survey there was no section on expenditures which would have been a more adequate dependent variable for our impact evaluation purposes. The presence of negative values might pose some questions about the validity of the regressions and this will be dealt with in the analysis section. In total, there were 284 households that reported negative income/income per capita in one of the years under consideration, 173 in 2005 and 111 in 2007. Apart from that 31 households had negative income in both years. Thus, in total in 2005 we had 173 + 31 = 204 (almost 14%) households and in 2007 111 + 31 = 142 (9.7%) households with negative values.

Mean and median of both income and income per capita can be seen in Table 66 (appendix). In the case of income per capita, the mean was 5,768 rupees while the median was 3,866, which means that the median is 49% lower than the mean. This confirms the skewness of the distribution in 2005 and this pattern continues in 2007. In this year the mean slightly decreases with respect to 2005 and the median does on the contrary. A similar evolution is seen in income.

In order to observe the possible presence of outliers or extreme values that might be skewing the distribution box and whisker graphs are used. The outliers and extreme values[24] lie outside the whiskers. The graph (Figure 12) includes all the observations and this aim of including the whole range squeezes extremely the Interquartile Range (IQR henceforth) which becomes small in comparison with the whole range although it contains 50% of the observations. Observations taking the most extreme values are the wealthiest households, although there are also extreme values for negative income per capita. A similar result is shown when the graph is about income, in Figure 23 (appendix).

---

[24] Strictly speaking, outliers are those observations between 1.5 and 3 times the interquartile range (in the forthcoming IQR) above (below) the 3rd (1st) quartile. Extreme values are those observations 3 times the IQR or more above (below) the 3rd (1st) quartile. However, we will use both terms henceforth without distinction.

Figure 12 Box & Whisker Income per capita by year

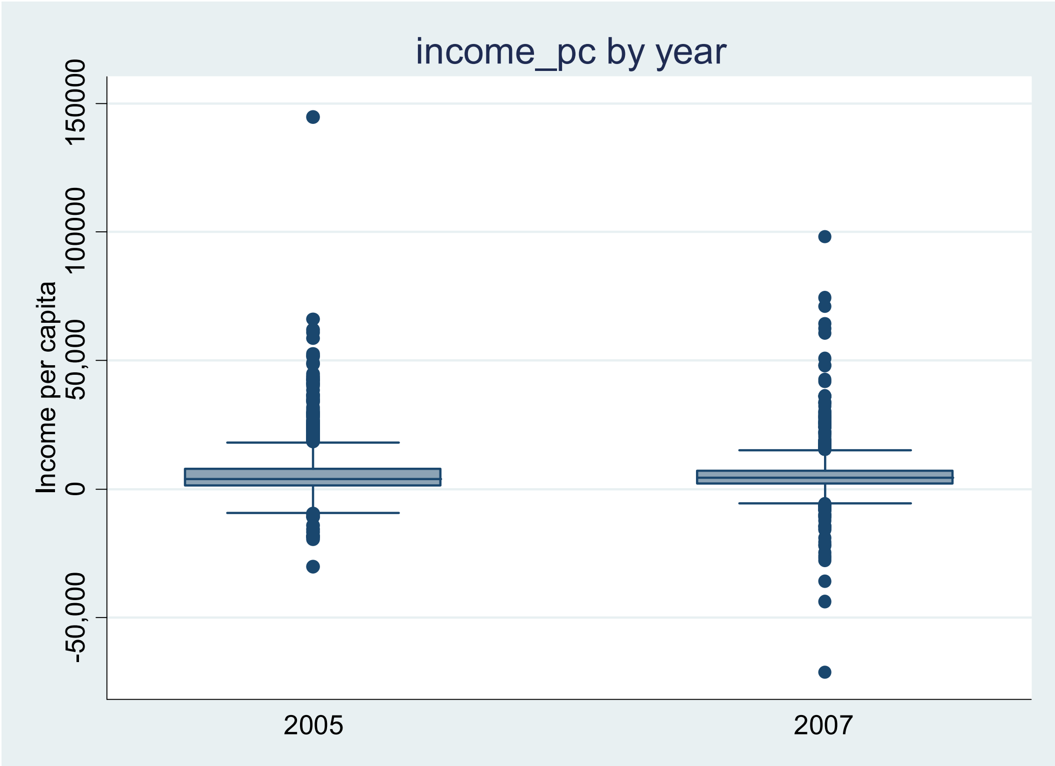


Table 20 and Figure 13 try to show more clearly the positions of some quintiles of the income per capita distribution. It is clear that at the tails of the distribution the differences between the 1st and 10th percentiles or between the 90th and the 99th are great. This is really clear at the top of the distribution where the difference between the 90th and the 99th percentiles is more than four times the IQR. There are also great differences at the bottom of the distribution, where income per capita takes quite negative values. This is less extreme than at the top of the distribution though. With respect to years, 2005 shows the highest extreme value at the top of the distribution while 2007 does at the bottom. It is remarkable that at the middle of the distribution the values of 2007 are bundled more closely, with a lower IQR but also lower difference between p90 – p10.

Table 67 and Figure 24 in the appendix show the same information regarding income.

Outliers and extreme values are problematic as they might make ordinary least squares (OLS) estimates biased and some other techniques have been implemented to surmount these problems. Among them, Least Absolute Deviation (LAD), M-estimator[25] and

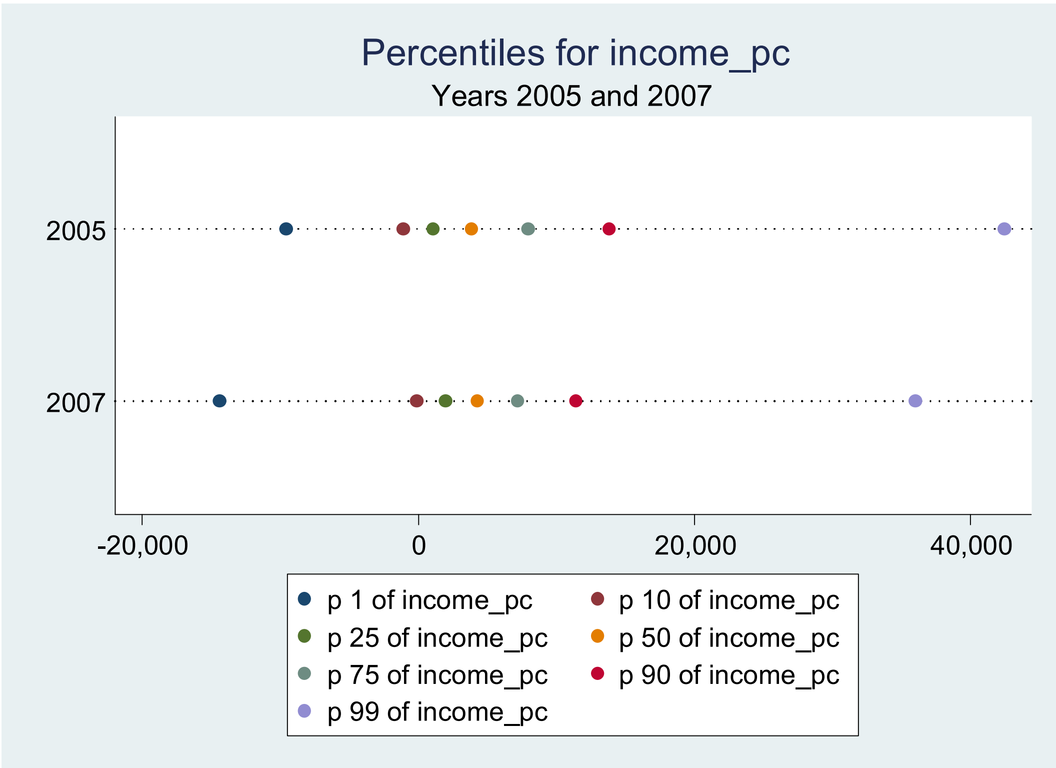
[25] M is not an abbreviation itself, it just stands for "Maximum likelihood type"

MM-estimator methods or quantile regression, for example. In the present study an MM estimator will be used to contrast the panel fixed effects estimates, which are more sensitive to outliers. In the next chapter the technique will be quantile regression, which is less sensitive and also aims at answering a different question.

Table 20 Quantiles income per capita distribution

		p1	p10	p25	p50	p75	p90	p99
year	2005	-9,556	-1,096	1,069	3,866	7,934	13,839	42,448
	2007	-14,400	-115	1,983	4,271	7,200	11,448	35,980

Figure 13 Quantiles income per capita distribution



The outcome variables can also be studied by district and year. Table 21 shows the mean and the median (the latter shadowed in grey, below the mean figure) for income per capita by district and year. The most striking feature is the low figures in Anantapur district in 2005. Although this is the driest and the poorest district of all, the difference is still too high. The explanation for this difference seems to be the drought suffered in the area in 2005, as Anantapur is the most vulnerable district of all. But other reasons are not found in the survey information. As can be seen in the rainfall[26] table (Table 22) 2006 was a better year, with all districts above normal figures. In 2007 rainfall figures are again closer to those in 2005, and this would not be coherent with the rise in Anantapur figures for outcome variables.

Among the rest of the districts, the best figures are showed by Prakasam, Kurnool and Mahabub Nagar for year 2005. These three districts experience an overall decrease in the statistics in 2007. The case of Anantapur and Nalgonda is on the contrary. They started from below in 2005 and experience a rise in their figures in 2007. This makes Nalgonda overtake Mahabub Nagar in the ranking of all figures in 2007. The median values do not change so widely between years, with the exception of Anantapur. Median values, thus, might be more reliable than means in this case.

This can be seen more clearly in a bar chart (see Figure 14) where the levels of bars is overall more alike between years in the case of medians. Given the fact that the median is less sensitive to changes at the extremes of the distribution, changes in means could be predominantly driven by the changes of observations at the top and bottom of income per capita distribution, as suggested by Figure 13 above. For income, see Table 68 and Figure 25 at the appendix, with similar overall patterns, although the changes in median are more noticeable.

[26] http://www.apdes.ap.gov.in/Dist@glance/rainfall.htm 22/09/11 @ 11.00 am and http://www.apdes.ap.gov.in/publications/Outline%202006-2007.pdf 22/09/11 @ 11.00 am.

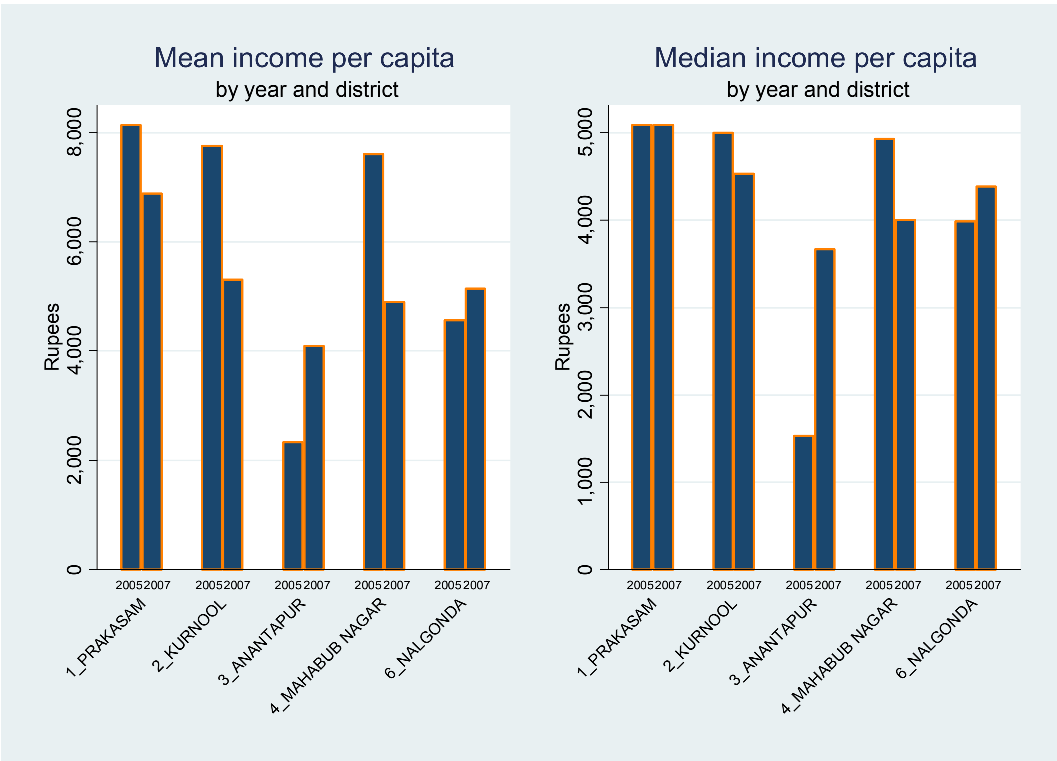
Table 21 Mean & Median income p.c. by district and year

mean income p.c. / median income p.c.	year 2005	2007	Δ2005-2007
1_PRAKASAM	8,136	6,882	-15.42%
	5,086	5,090	0.07%
2_KURNOOL	7,752	5,306	-31.56%
	5,005	4,532	-9.46%
3_ANANTAPUR	2,336	4,099	75.47%
	1,533	3,667	139.21%
4_MAHABUB NAGAR	7,607	4,897	-35.63%
	4,929	4,000	-18.85%
6_NALGONDA	4,558	5,145	12.89%
	3,988	4,383	9.90%

Table 22 Rainfall (in mm) by district

	Rainfall			
District	2004-05	2005-06	2006-07	Average
PRAKASAM	550	1010	709	871
KURNOOL	543	840	543	670
ANANTAPUR	437	791	407	553
MAHABUB NAGAR	413	973	484	604
NALGONDA	519	1000	547	751

Figure 14 Mean & Median Income per capita by year and district



The dependent variables can be tabulated with respect to their watershed location, given the original purpose of the survey. Areas with watershed projects are better off in terms of both outcome variables in 2005, although this difference becomes very small in 2007 (income per capita in Table 23, income in the appendix, Table 69).

Table 23 Income per capita by watershed

mean income_pc median income_pc	2005	2007	Δ 2005 - 2007
non-watershed	4,531	4,907	8.3%
	3,086	4,156	34.7%
watershed	6,307	5,289	-16.1%
	4,190	4,383	4.6%

Credit sources

As stated in Masset and White (2006), the normal sources of credit in the rural areas had been so far moneylenders, landlords, family or relatives. Formal banks did have a scarce presence in these areas, in which they were operating mostly as deposit entities. However, the emergence of SHGs initiatives and the "bank linkages" has brought up the appearance of formal financial entities in the rural areas in India. Thanks to these "bank linkages", village banks can open an account in a commercial bank once they are created. This opens the possibilities to have access to bank services to other individuals or other groups. Therefore, the presence of village banks broadens the potential clientele of commercial banks that are willing to open new branches in areas that were not considered before because they were not profitable.

In Table 24 the evolution of the number of borrowing households for the five districts can be observed. The common pattern is an overall decrease of the borrowing activity. The number of households borrowing from the different sources clearly decreased in 2007 by around one million households, a 21.3%. The only exception of this decrease is the number of households borrowing from family and friends, which increase by around 120,000. In the case of SHG borrowers, the figure remains stable while the decreases in the case of banks, NGOs, moneylenders-landlords and others are higher than 30%. In terms of source share, the main source is moneylenders-landlords in both years. SHG is the third in 2005 and second in 2007.

Table 24 Borrowing households

Nr. of Borrowing Households			Share per source		Δ in nr. households
	2005	2007	2005	2007	
Bank	1,281,391	891,044	25.7%	22.7%	-30.5%
NGO	34,736	19,931	0.7%	0.5%	-42.6%
SHG	1,026,335	1,020,629	20.6%	26%	-0.6%
Moneylender-Landlord	1,772,616	1,120,788	35.5%	28.54%	-36.8%
Family-Friend	548,353	667,085	11%	17%	21.6%
Other	325,318	208,024	6.5%	5.3%	-36.1%
Total	4,988,749	3,927,500			-21.3%

When proportions are observed in terms of amount borrowed from each source, the figures change due to the differences in loan sizes. Table 25 shows how, in quantitative terms, the main source of credit is still moneylenders and landlords. They represent around the 40% of the total loan volume in the area. The banks are the second source, followed by family and friends. In terms of loan volume, the share of SHG ranged between 7.8 and 10.6 per cent. This is in contrast with the share of households that borrowed from SHG, which ranged between 20.6 and 26 per cent. SHG is the only source that increases the amount of money lent between the surveys. However, Table 24 clearly explains that although more than 20% or 25% of the households turn to SHG, the total amount borrowed from this source is more than 3 times lower than that borrowed from moneylenders or banks, due to the smaller size of SHG loans, as can be seen in Table 25.

Table 25 Sources in terms of amount borrowed (millions of rupees)

	Total Amount borrowed		Percentages		Δ in amount borrowed
	2005	2007	2005	2007	
Bank	21,400	17,700	26.5%	27.9%	-17.3%
NGO	649	90	0.8%	0.1%	-86.1%
SHG	6,280	6,730	7.8%	10.6%	7.2%
Moneylender-Landlord	34,700	24,900	42.9%	39.3%	-28.2%
Family-Friend	12,800	11,500	15.8%	18.1%	-10.2%
Other	5,060	2,490	6.3%	3.9%	-50.8%
Total	80,889	63,410		-21.6%	-21.6%

In order to find out more about the households borrowing from different sources, mean income and income per capita are calculated by credit source at both years. The overall decrease in means in 2007 is consistent with the descriptives seen above, but again the medians seem to be slightly higher at 2007. The most remarkable feature is that means are slightly higher for SHG borrowers than for other sources. This would have been more expected for bank borrowers, but these do not score second but normally a bit below. Thus, this kind of microfinance programs might not be serving the most deprived households, although the areas where the sample was taken were poor rural areas. The table with the medians can be seen in the appendix (Table 70) and comparatively again SHG show the higher medians for income and income per capita.

Table 26 Mean outcome variables by source and year

	Income per capita		Income	
	2005	2007	2005	2007
Bank	5,154	4,255	25,836	22,089
NGO	5,737	3,399	22,837	19,824
SHG	6,060	5,142	30,598	24,876
Moneylender-Landlord	5,082	4,792	22,491	22,354
Family-Friend	5,092	5,304	23,134	24,694
Other	4,431	3,558	18,698	20,886

It could be also of interest to have a look at the means or medians for these outcome variables by the number of sources. There are households that do not borrow at all from any of the sources and others that borrow from more than one source. In the following table (Table 27), mean income for borrowers is shown by year. The highest values are seen in households that did not borrow from any source, followed by those that borrowed from one only. Households using two or more show overall lower income and income per capita means. Thus, households that use credit sources more intensively tend to be poorer. This would be coherent with the fact that they need to keep consumption levels and therefore they turn to any source of credit. However, they might have also a lower repayment capacity and therefore heavy indebtedness could make them fall into a debt trap.

Table 27 Outcome variable by nr. of credit sources used

	Income per capita		Income	
	2005	**2007**	**2005**	**2007**
No source	7,150	5,856	31,200	25,496
One source	6,085	5,425	27,207	25,009
Two sources	5,038	3,920	25,082	20,192
Three or +	4,761	5,638	21,731	27,572

Borrowing from SHGs at different time periods

The main issue in studies where there is not randomization is the handling of bias, in our case the selection bias is of particular interest. This is because in order to borrow, individuals have to somehow self-select themselves. Borrowers, thus, might be composed by individuals that are systematically more entrepreneurial, for example, than non-borrowers and they would have higher income per capita even in the absence of microfinance.

Another important issue is the treatment of households that give up borrowing. Among the first impact evaluations of microfinance, there were studies that compared new borrowers vs. old borrowers (Hulme and Mosley 1996). The latter might suffer from the survivor bias. This kind of bias has been studied in Karlan (2001) and he suggests that all the borrowers, dropped or not, should count as members of the treated group.

The panel structure of the present study is like the one used in Tedeschi (2008) with a range of households interviewed in 2 periods. Despite the fact that in the analysis outcomes for all credit sources are also shown, the main interest of the study is about borrowers from Self Help Groups (SHGs).

As seen in Table 28, some households kept their SHG-borrowing status throughout the surveys, either by SHG-borrowing in both years or by borrowing in none of them. There are also some households that borrowed from SHGs only in one of the years. Following Tedeschi's nomenclature, households that never borrowed from SHG are under the category "never" and those that borrowed both times under "always". Those which did only in 2005 will be "drop" and the rest borrowing from SHG only in 2007 are named "new". Also following Tedeschi (2008) a very important variable is created with the name "everborrow". This will be a dummy with the value of one in case the household is under "always", "drop" or "new" and zero otherwise.

Table 28 is expressed in percentages. In it, the share of "always" households is 13.03%, 15.38% in the cases of "drop" and "new", with a 55.20% of households that never borrowed and belong to the "never" group. The category "everborrow" for SHG borrowers will add up to 13 + 15.4 + 15.4 ≃ 43.8% of the sample. Migrants are included in this case only for illustrative purposes, adding up to around 1% of the total sample.

"Everborrow" is of great importance in the theoretical framework in Coleman (1999); Coleman (2006) and Tedeschi (2008). It is assumed to gather the unobservables that characterize borrowers and therefore it contributes to avoid selection bias. It also includes in the treated group "drop", "new" and "always" borrowers. This contributes also to avoid "survivor bias" as recommended in Karlan (2001). Finally, as long as there are several alternative sources of loans, there will be one "everborrow" variable for each of these credit sources together with "always", "drop", "new" and "never": banks, moneylenders and landlords, NGOs, family and friends and others.

Table 28 Evolution of proportion of borrowers

		2007			
		Borrowers	**Non Borrowers**	**Migrants**	**Total**
2005	**Borrowers**	13%	15.4%	0.2%	28.6%
	Non Borrowers	15.4%	55.2%	0.8%	71.4%
	Total	28.4%	70.6%	1%	100%

Focusing on our variable of interest, SHG borrowing, descriptives of interest would be the borrowing patterns by social groups. There are four of them in the dataset. The group "scheduled caste" is composed by the untouchables, the lowest caste. The second group, "scheduled tribe", normally live out of the villages, have their own cultural traditions and tend to be the poorest households together with the "untouchables". A third group would be composed by households in backward castes. In the context of this survey, taken in a rural area, these backward castes are normally better off than the former two groups. Finally the last group corresponds to the upward castes.

Their borrowing patterns are shown in Table 29. Its first column informs about the composition of the sample by social extraction. It can be seen that the most numerous group is composed by backward castes, while the most deprived households, "scheduled" groups represent only around a 35% of the sample. The second and third columns show the proportion of SHG borrowers by social group. As expected, the highest proportion of borrowing households comes from the most numerous social group.

It is also interesting to have a look at columns four and five. In them is shown the percentage of households that are borrowing within each social group. It is seen that the social group with by far the highest share of borrowing households is the scheduled tribe. Around 43% of these households borrow from SHGs. On the contrary, the lowest percentage is shown by upward castes.

Table 29 Proportion of SHG borrowers by social extraction

	Proportion over total sample	% SHG borrowers over total borrowers		% SHG borrowers within each group	
		2005	2007	2005	2007
Scheduled caste	22.1%	20.1%	22.0%	29.4%	29.7%
Scheduled tribe	12.4%	16.3%	17.8%	42.5%	43.1%
Backward castes	47.9%	48.6%	44.9%	32.9%	28.0%
Upward castes	17.5%	15.0%	15.3%	27.7%	26.2%

Income and income per capita can be tabulated in conjunction with SHG "everborrow" variable. Table 30 shows the mean and median of income per capita corresponding to SHG "ever-borrowers" and SHG non-borrowers. Median income per capita is consistently lower for SHG borrowers. However, the mean was higher in 2007. This might suggest that SHG borrowers might be poorer than SHG non-borrowers which points to selection bias. In the case of income (Table 71, appendix) SHG borrowers show the highest values for mean and median at both years. This suggests the importance of household size when comparing treated and controls.

Table 30 Income per capita Mean &Median, by SHG-borrowing status

		Household income per capita			
		mean		median	
		SHG non-borrow	SHG everborrow	SHG non-borrow	SHG everborrow
year	**2005**	6,041	5,424	3,961	3,699
	2007	4,951	5,453	4,365	4,222

Summing up, the median figures show that the apparent decrease in income and income per capita in 2007 might be driven by the values at the tails of the distributions in this latter year. Also, although all districts had bad rainfall figures in 2005 and 2007, the poorest and the driest one, Anantapur, suffered them more intensively and are expected to show a negative coefficient. For the contrary reasons Prakasam should show a positive one. Changes between 2005 and 2007 are more remarkable at district means than at the medians.

The number of borrowers from SHG microfinance remains almost equal to that of 2005 and it is the only source that increases its amount of money lent in 2007. However, it is still neither in terms of borrowers nor in loans volume the first source of credit. From the means of the outcome variables, it seems that SHG borrowing households might be slightly better off than the rest of borrowing households. The majority of them come from a relatively better off social background. SHG borrowers show higher figures in income than SHG non-borrowers but lower at income per capita, indicating that household size might be an important covariate in the analysis

Analysis

The methodologies used in the analysis will be three. First, a panel fixed effect. The possibility of reverse causality leads to the implementation of an IV approach and a check as to whether the amount borrowed from SHGs is endogenous. Finally, a MM estimator is attempted in order to test whether the outcomes are robust to the presence of outliers;

The panel data approach could also be a DID given that we have a panel with two time periods. We used the proper fixed effects as in Khandker (2005), Tedeschi (2008) and Bruhn and Love (2009).

The model is set up as follows:

$$Y_{it} = \beta_0 + \beta_1 y\_07_i + \beta_2 X_{it} + \beta_3 SHG_{it} + \beta_4 L_{it} + \mu_i + \varepsilon_{it}$$

Y_{it} is the outcome variable, mainly income per capita although outcomes for income are also reported in the appendix. y_07 is a dummy for year 2007. X_{it} is a vector of household characteristics. In particular, the choice of variables is taken among those used in previous panel approaches (Khandker, 2005; Tedeschi, 2008) including also an asset index described below. In a similar way, Chemin (2008) includes variables such as livestock value or savings. There was no information about savings and livestock value is included in the calculation of income and therefore should not act as an independent variable.

It has to be stressed that the fixed effects approach strictly limits the variables that can be used in the equation. Time invariant variables such as gender, religion, geographical location (districts, for example) or social extraction cannot be taken into account. In addition, variables that do not vary much over time will suffer from large standard errors.

The household characteristics variables included in X are:

- age of the household head,
- size of household
- a dummy expressing whether or not the household has suffered any shock in the last 12 months.

- an asset index is a variable created with 6 different assets: radio, television, fridge, bicycle, motorcycle and car. The variable takes the value of 0 if none of these assets are present in the household. It can take up to the value of 6 when the household owns all the mentioned assets. The most expensive asset of this index is the car. The number of cars is small in the sample, only four households in 2005 and nine in 2007. The correlation between the asset index (with no car) and the binary for car ownership is positive and significant[27] and thus it was included in the index.

With respect to the remaining variables, SHG_{it} is the amount of money borrowed by the household and the impact of microfinance on income per capita is measured by β_3. L_{it} is a vector of variables that include the amount of money borrowed from other sources: banks, moneylenders/landlords, NGOs, family and friends and others. The last includes cooperatives, suppliers, etc. Finally, μ_i gathers the unobservables specific to each household (entrepreneurship, tenacity, etc) that might be biasing the impact estimation. In this fixed effects approach they are swept away, leaving the impact estimation free of this type of bias.

Despite the fact that unobservables are eliminated in the fixed effects approach, one of the drawbacks of the dataset is that microfinance schemes were already present in the first round of the survey. However this is an issue in most of the microfinance impact literature using this fixed effects approach (Kandkher, 2005; Tedeschi, 2008; Copestake et al., 2005).

Table 31 shows the outcomes for income per capita. Under the FE (panel fixed effects) column can be seen the impact of SHG borrowing on income per capita at household level. The mean amount borrowed from SHG is then multiplied by the estimate and this product gives the average increase in income per capita. Thus, this is estimated as [6,368[28] x 0.086] = 548 rupees per year. This is around 10% of the mean income per capita. With respect to the rest of the sources, the NGOs are the only one showing statistical significance.

[27] Pearson correlation coefficient: 0.1011. P-value: 0.000.
Spearman's rank correlation coefficient: 0.0833. P-value: 0.000.
[28] Mean SHG loan size.

An important issue in this regression would be that reverse causality between amount borrowed from SHG and income might arise because those with higher income borrow more money. At the same time, the fact of borrowing more might cause higher income. Therefore, SHG borrowing, inter alia, determines income and, inter alia, income determines SHG borrowing. This leads to a biased estimation of the impact. This comes from the fact that the variable amount borrowed from SHG (SHG_sum henceforth) is not independent of the error term. In more formal terms, the condition of explanatory variables being stochastic or independent of the error term (or $E(u,X) = 0$) does not hold.

The main aim of this study is to find out the impact of SHG microfinance. The rest of the sources are secondary regarding our research interests. Thus, given the impossibility of finding good instruments for every source of credit, the approach is only attempted for the SHG borrowing and not for the rest of sources.

Table 31 FE, IV and MM approaches. Income per capita including negative values.

	Income per capita		
	FE with negs	IV with negs.	MM with negs
Year 2007 intercept	-839.269**	-828.257**	419.025**
	(-2.26)	(-2.20)	(2.38)
Age of the HH head	-3.869	-1.694	28.808***
	(-0.09)	(-0.04)	(4.04)
Household size	-501.461**	-475.227*	-140.934**
	(-2.03)	(-1.85)	(-2.57)
Shock in last 12 mths	-1491.329**	-1445.542**	-1176.350***
	(-2.13)	(-2.03)	(-6.88)
Asset index	563.463	549.217	127.040
	(1.07)	(1.02)	(1.21)
SHG	0.086**	0.110*	0.073***
	(2.78)	(1.82)	(3.59)
BANK	0.006	-0.003	-0.031***
	(0.12)	(-0.06)	(-5.41)
NGO	0.200***	0.200***	0.289***
	(5.36)	(5.15)	(32.51)
Moneylenders/others	-0.005	-0.005	0.001
	(-0.16)	(-0.16)	(0.15)
Family/Friends	-0.017	-0.018	0.010
	(-0.79)	(-0.81)	(0.82)
Others	-0.067	-0.062	-0.024
	(-1.49)	(-1.32)	(-0.74)
Constant	8876.049***		3623.643***
	(3.62)		(8.70)
Obs.	2906	2906	2906
F	3.375		9.09
P	0.002		0.000
t statistics in parenthesis	**** p<0.10 ** p<0.05 *** p<0.01***		

There are two different instruments. Both are in relation to participation in SHGs. The first is the maximum SHG membership duration within the household. Thus, in the case that a household had two different members, the value of this variable would be the membership duration of the household member that joined first. The second instrument is the maximum percentage of attended meetings within the household. Thus, if one person attends all the meetings, this variable takes the value of 100 and if only half of them the value would be 50.

These variables are significantly correlated with the variable containing the amount borrowed from SHGs[29]. On the other hand, SHGs have a tradition in Indian society and they are not constrained to a way of having access to loans. They can be incardinated as a traditional Indian community organization and aim at giving response to not only development issues but also other type of problems. In Andhra Pradesh, for example, these SHGs were created around education or anti-alcohol campaigns. The creation of bank-linkages has contributed to their proliferation but, for instance, they have also been promoted by politicians, such as Chandrababu Naidu, as community organizations in the Janmabhoomi program.

In the particular framework of the present study, SHGs participate in watershed programs as their members are represented in the watershed committees. These are in charge of the day-to-day management of the activities in the projects, and thus this is another incentive to belong to a SHG. In the RLP, this task is assigned to the Village Organizations, in which SHGs are also represented. They can take decisions about whether more funds should be assigned to soil conservation rather than to water harvesting which can benefit small rather than wealthier farmers, for example.

Although pure independence between instruments and outcome variables is impossible to prove unless we could use an instrument obtained through some sort of randomization, an attempt to show the plausibility or our argument is to find out correlations among variables. Table 72 at the appendix shows Pearson' and Spearman' correlation coefficients between instruments and outcome variables. In all cases the correlation coefficients are low. We test the null of them being equal to zero and it cannot be rejected in any case.

In addition, the variables used as instruments were regressed over a set of variables[30] plus dummies of income per capita quintiles (Table 74 for outcomes using income per

---

[29] We can strongly reject the null of correlation = 0. Correlations were calculated:

- Pearson's correlation coefficients:
  - SHGs borrowed sum – duration: Coefficient = 0.4214; p-value = 0.000
  - SHGs borrowed sum – % attendance: coefficient = 0.4593; p-value = 0.000
- Spearman's correlation coefficients:
  - SHGs borrowed sum – duration: Coefficient = 0.5362; p-value = 0.000
  - SHGs borrowed sum – % attendance: coefficient = 0.5038; p-value = 0.000

[30] Independent variables were: intercept for year 2007, age, age squared and gender of the household head, household size, dummies with the social groups (omitted category: highest caste), district dummies (omitted: Nalgonda), index of asset ownership and household population of the village. Finally, quantiles of income per capita (and income), where the omitted category was the fifth quintile.

capita and income quintiles). None of these quintiles show any significance and this comes to underpin the argument of independence. Thus, although the IV approach in any study might be slightly vague because of the difficulty of proving independence, we find no correlation and, when controlling for other variables, income per capita quintiles dummies do not show any significance. These outcomes suggest that they can be used as instruments.

The specification of the model remains as it was for the fixed effects approach. The approach used in this case is two stage least squares (2SLS) for the IV applied to panel data. This was run with the command *xtivreg2*.

The first point when tackling the IV approach is to test for the relevance of the instruments. This is done through the F value. Quite a common approach is to follow the Stock and Staiger "rule of thumb" which establishes that for an instrument to be considered relevant the F value has to be higher than 10. If the instrument is not relevant this means that the correlation with the endogenous variable is too weak and the deviation of the standard errors with respect to the original fixed effect model would be too great to do a proper inference.

Apart from being relevant, the instruments have to be orthogonal to the error term. This is tested through the Hansen J statistic, based on the same grounds as the Sargan-Hansen test. The null in this case is that the instrument is orthogonal to the error term and therefore if we can reject the null then the instrument would not be considered adequate because there might be some kind of relationship between the instrument and the error term. The main problem with this test is that not rejecting the null does not guarantee that the instrument is not related in any way with the outcome variable. This is why IV approaches should be grounded on very solid theoretical arguments or, preferably, on some kind of randomized instruments.

Once we are satisfied with the relevance and orthogonality of the instrument, it is possible to test whether the variable is endogenous or not. The endogeneity test is based on the Wu-Hausmann test and the null hypothesis is that the variable is exogenous. If we cannot reject the null then there is no point in using the IV approach as the bias of the standard errors is greater.

Outcomes for these tests are found in Table 32. The relevance, orthogonality and exogeneity tests are discussed first. The F value to apply the Stock and Staiger "rule of thumb" is far higher than 10 so we are confident that the instruments are relevant. In the case of the orthogonality test we cannot reject the null of the instrument being independent of the error term. Thus, we have relevant and orthogonal instruments and therefore we can go on testing for their exogeneity. In the case of the Wu-Hausman test, again we cannot reject the null of exogeneity and therefore it would be more adequate to use the original panel fixed effects approach rather than the IV. IV estimates are in any case shown and briefly commented on for illustrative purposes.

The estimates of the panel IV approach can also be seen in Table 31 above. They are quite close to the estimates from the original panel FE. Again, the impact of microfinance is positive and significant, slightly higher than in the case of the FE. The rise in income per capita would in this case be 618 rupees per annum, on average and ceteris paribus.

In the descriptive statistics the presence of outliers was clear. This is not unusual in dependent variables such as income/income per capita. It is also quite common in variables such as profits or revenues so a discussion on this issue would have been expected in Tedeschi (2008). The chosen approach to deal with the outliers issue was an MM estimator, running a user-written command in Stata, *mmregress* (Verardi and Croux, 2008).

The main drawback of the presence of outliers is that their residuals are abnormally high and in the process of squaring them, they become even greater. Thus, the minimization of squared residuals tends to fit these extreme values at the expense of the remaining observations. The consequence is that the coefficients from these regressions might be biased. There are methods that attempt to deal with these issues, such as Least Absolute Deviation, in which the absolute value of the residuals is minimized instead of their squared values. Other methods just try to look for "influential" observations using Cook's D[31] value to test for this influential quality. Those values with a $D > 1$ are

[31] Cook's D tries to measure whether an observation is influential or not. Basically it is calculated doing two regressions, one with all the sample and the second with all observations but the controversial one. Then the sum of squared differences of both predictions is found. Finally, this is divided by two times the Mean Squared Error. If the value is higher than one the observation is considered influential.

withdrawn from the sample. However, this approach might not be robust to outlier clustering.

Other methods already discussed are S and M estimators, but both have drawbacks, the former in terms of Gaussian efficiency and the latter regarding robustness. MM combine the former two in order to surmount these drawbacks.

This MM regression cannot be run as a panel and therefore the method had to be applied to the pooled dataset. In Table 31, under the column MM, the estimates are shown. The coefficient of SHG borrowing remains statistically significant at 5% level although in this case the impact seems to be slightly smaller. The average rise in income per capita at household level would in this case be around 465 rupees per annum, around a 8.5% of the mean income per capita. As with the fixed effects approach, SHG borrowing shows positive and significant impact and in this case bank borrowing also shows significance but with a negative value.

In general the fixed effects approach and the IV produce quite similar coefficients. This is not so in the case of the MM approach where most of the covariates change. However, the variables household size, shock and SHG and NGO borrowing remain significant and with the same sign, which lead to the conclusion that these estimates are quite robust to the presence of outliers. In the particular case of SHG borrowing, it comes to support the argument of the positive effect of borrowing from SHGs.

Table 32 Tests of the IV approach

	Relevant	Orthogonal		Exogenous	
		Hansen J		Wu-Hausman	
	F	Stat	p-value	Stat	p-value
Income p/c	37.5	0.353	0.5526	0.000	0.9969

Negative values.

The presence of households reporting negative income/income per capita might cast doubts over the reported estimates. The reasons for the presence of these negative values have been discussed above, although there is also the possibility that some households just misreported their income.

The presence of negative income values is not unusual and in development economics is particularly problematic when calculating poverty indexes. Sandoval and Urzua (2009) suggest setting the negative income values to zero. In other sources such as and Hunter et al., (2002) and Shaefer and Edin (2012) the approach is to test for sensitivity of the conclusions in the original models after they drop the negative income observations and rerun the same models.

The same steps are followed in the present study. The observations with negative income per capita values are withdrawn and therefore they are not considered in the model. The idea is to check if without these abnormal observations the estimates remain similar and statistically significant or if the change is great or the significance is lost. The outcomes are shown in the following Table 33.

There is actually a small difference with respect to the SHG estimates. In the case of the FE, the estimate is slightly higher but remains statistically significant at the 1% level. The average rise in income per capita would in this case be 643 instead of the former 548 from the model including negative values. The estimate for the MM approach is slightly lower and the IV approach provides a slightly higher estimate.

Table 33 FE, MM and IV with no negative income values

	Income per capita (no negative values)		
	FE	MM	IV
Year 2007 intercept	-645.727* (-1.87)	76.897 (0.48)	-637.442* (-1.80)
Age of the HH head	-22.683 (-0.72)	18.490*** (2.81)	-21.870 (-0.67)
Household size	-793.078*** (-3.47)	-186.322*** (-3.68)	-786.635*** (-3.34)
Shock in last 12 mths	-1281.124* (-1.79)	-579.940*** (-3.69)	-1323.187* (-1.83)
Asset index	714.269 (1.15)	258.171*** (2.76)	702.487 (1.10)
SHG	0.101*** (2.95)	0.067** (2.49)	0.131* (1.72)
BANK	0.084* (1.89)	-0.017** (-2.25)	0.087 (1.55)
NGO	0.202*** (6.35)	0.286*** (55.88)	0.203*** (6.25)
Moneylenders/others	0.013 (0.28)	0.004 (0.85)	0.014 (0.30)
Family/Friends	-0.008 (-0.39)	0.011** (2.08)	-0.009 (-0.42)
Others	0.026 (0.92)	-0.003 (-0.22)	0.027 (0.93)
Constant		4434.248*** (11.56)	
t statistics in parenthesis	**** p<0.10 ** p<0.05 *** p<0.01***		

Regarding the relevance of the instruments, the F-value is noticeably lower than that found when the dataset includes negative income values. The value is now 10.89 which is at the limit of the Stock and Staiger rule. Thus, the option in this case was to run a Limited Information Maximum Likelihood (LIML) which is more adequate in the presence of weak instruments. The outcomes regarding the relevance, orthogonality test and exogeneity test are shown in the appendix Table 75.

Again, the outcomes show relevant instruments that are orthogonal to the error term. In addition, we cannot reject the null of exogeneity and thus the original FE model should be used. With respect to the alternative sources of funds, again NGOs show consistently a positive and significant impact and the rest are either non significant or they do not show consistent patterns.

The outcomes in Table 33 underpin the outcomes originally obtained, given the fact that they do not seem to be quite sensitive to the presence of the negative income/income per capita values. This is a sign of the robustness of the positive and significant impact of microfinance on income per capita at household level.

With respect to income, the outcomes are quite similar and outcomes can be found in Table 76 when the dataset includes all the observations and in Table 77 when the negative income observations are dropped from the dataset. The impact from SHG borrowing remains significant and positive across the different models and datasets.

Conclusion

The aim of the present research was to study the impact of SHG microfinance in Andhra Pradesh on household income per capita and income. The study follows mainly the theoretical and technical framework in (Coleman, 1999 and 2006) and its adaptation to a two-period panel dataset in Tedeschi (2008) where she runs a panel data fixed effects approach. This technique has also been used in the past (Copestake et al, 2005; Khandker, 2005 and Bruhn and Love, 2009).

The dataset contained a good number of outliers, some of them negative income and income per capita observations that had to be considered in the analysis. In order to test the robustness of the fixed effect approach to the presence of outliers, an MM method was applied. In addition to that, an IV approach was also run to test for the possible endogeneity of the SHG borrowing variable.

In a second stage the three models were run again on the dataset where the negative income observations had been dropped. The aim of these second regressions was to test whether the original results could be driven by the presence of these negative values.

The final conclusion is that the effect of borrowing from SHG has a positive and significant impact at household level. This outcome is quite robust across the different models and remains after dropping the households reporting negative income.

The positive and significant outcomes come to confirm previous outcomes with respect to microfinance and provides a quite robust argument in favour of these village banks or microfinance in general, in tune with other studies such as, , Pitt and Khandker (1998), Khandker (2005), Coleman (2006), Chemin (2008), Kondo et al. (2008) and Tedeschi, (2008). However, the moderation in impact raised by the MM method might suggest that outcomes mainly in the latter, but also in other studies could be revised.

Finally, although this study answers the question of whether there is any impact of microfinance on income and income per capita, the outcomes refer to the whole sample and it is not possible to see whether it is affecting the poorest households. Descriptives suggest that borrowers come mainly from slightly better off backgrounds and therefore our next interest would be to test whether this impact is homogeneous at all points of the outcome variables distribution or whether it is significant only at some of them.

Chapter 4: Distributional Impact of Microfinance in Village Banks in Andhra Pradesh, India

Introduction

The previous chapter showed a positive and significant impact of borrowing from microfinance at household level for both income per capita and income. The more conservative estimations from MM method reported increases of annual income per capita and income of 4.1% and 5.6% respectively. This means that if a household withdrawn from the population is a SHG borrower, it is expected to have a higher income/income per capita than a non SHG borrower, ceteris paribus. In these models, it does not matter whether the individuals are poorer or richer as the impact is considered constant for the whole sample.

The interest of the effective impact on the less favoured extends to the case of pro-poor interventions in general, not just microfinance literature. When evaluating the effects of a particular policy, it might happen that those most in need of it are not benefitting at all. As Heckman argued when confronting Average Treatment Effects (ATE) and Average Treatment Effects on the Treated (ATT), in some occasions the core aim of a research study is not whether the impact is significant for the whole population. The main issue is if it is making any difference on those agents at which the policies are aimed. OLS or DID significant estimates might be driven by the significant impact at some points of the conditional distribution of the dependent variable. But this policy might not have been designed for them. Thus it is pertinent to complement this analysis with additional techniques that provide with a more focused picture of impact effects.

In microfinance literature in particular there have been attempts to answer the question of significant impact for different groups, mainly defined by poverty measures. For example, Hulme and Mosley (1996) conclude that the impact of microfinance takes place at those households that are relatively better off. Coleman (2006) distinguishes between "rank and file" and "committee" members in village banks in Thailand. The impact is not significant in the former group but it certainly is in the latter, which is normally constituted of wealthier individuals that used to forge the rules and manage to borrow more money than they would be entitled to. But this issue is also raised in other studies such as Copestake et al.(2005) or Kondo et al. (2008), who find that impact could be even negative for the poorest

They use different means to find the estimates for the poorest. For example, in Coleman (2006) the impact is differentiated between committee and rank-and-file members, the latter being the poorest. In Copestake et al. (2005) they just split the sample into two parts and find the DID of observations above and below the median. Quantile regression is a more efficient way of finding the estimates at any point of the outcome variable distribution. By using all observations quantile regression also avoids truncation problems. Quantile regression can slice up the conditional distribution into pieces and can find the impact estimate at each of these slices. Thus, if the impact is found at the lower income per capita quantiles it could be said that this is the impact for the poor. If this is done at the top of the distribution we could state that this is the impact for the richest.

Quantile regression has its origin in the 18th century (Hao and Naiman, 2007). It was conceived as a median regression and is not based on least squares but on least absolute distance. This is more useful when the distribution is very skewed, for example. The median is just the second quartile or the 50th percentile, but other distributional locations can be used. Thus, quantile regression allows to measure not just a single impact effect for the whole sample but the different effects that intervention exerts on households allocated at different points of the conditional distribution (Hao and Naiman 2007).

The quantile regression as such was first introduced by Koenker and Bassett (1978) and has been applied for more than 30 years, mainly in labor economics. An example is Chamberlain (1994) where it is found that the premium for union workers is higher for those at the bottom of the wage distribution. Koenker (2005) quotes other examples referred to alcohol consumption, (Manning et al., 1995) and birthweights (Abrevaya, 2001) and in both cases there are differences between OLS estimates and estimates at different levels of the outcome variable distribution.

Literature extended from labour to other fields within economics such as development economics. Examples of these are Mwabu and Schultz (1996) studying the effect of education on wage returns. Khandker, et al. (2009) focus on the impact of road construction in rural Bangladesh at households with different levels of income and

Emran et al. (2009) find that the impact of an anti-poverty program in Bangladesh is more effective at the top deciles of income distribution of eligible households than at the bottom, where it doesn't show any significant effect. This latter outcome links with the above mentioned works by Hulme and Mosley (1996) and Coleman (2006) as an instance of policies that affect the relatively better off among the poor.

As already discussed in the past chapter, an additional reason in favour of quantile regression is the presence of outliers, as it is a method that is insensitive to vertical outliers. Other methods such as MM methods are more appropriate in the presence of "bad leverage points" (Verardi and Croux, 2008). Quantile regression is also convenient in the present case as the presence of a relatively high share of negative values in the dependent variables does not recommend to drop these observations and log transform the positive ones.

Thus, so far it has been found a positive and significant impact of microfinance over income per capita and income through pooled OLS and DID techniques. The interest now is whether or not this impact is different along the conditional distribution of the outcome variables. Quantile regression has been scarcely used, if at all, in microfinance impact evaluation literature. Significant differences at different points would suggest that other policy interventions complementing or substituting microfinance might be needed. The main aim of this study is, therefore, finding whether these differences in impact effect are taking place and where the impact is not noticed by households.

It follows some descriptive statistics of the dataset in terms of the different quantiles. Then the study includes the outcomes of the cross sectional quantile regression in years 2005 and 2007. After this, the quantile regression is done over differenced dependent and independent variables and also including 2005 level variables in order to control for initial values. Then some review about quantile regression models and panel datasets follows before introducing the panel analysis. The panel quantile regression technique is applied in the same fashion as in Abrevaya and Dahl (2008) and Khandker et al. (2009), using the Correlated Random Effects (CRE) model. Finally, conclusions are obtained.

Overall, the outcomes of these three approaches confirm a positive and significant impact at the middle of the income per capita and income distribution. This underpins what had been suggested in the previous literature regarding the different impacts in different distribution locations and refines the former studies by placing impact at a location within the dependent variables distribution. This might be especially problematic for the poorest. Thus, alternative or complementary policies might be needed to fill this void.

Descriptive statistics

Descriptives of the dependent variables. Percentiles and other statistics

The dataset has been amply described in the third section of chapter three. In this section we will focus mainly in the quantiles of the dependent variable distribution. Non normality in the distribution has been clearly described in the previous chapter. There, in Table 66 it was seen that in both years the means are above the medians. In 2007, the value of the means decreases and the medians do the contrary, which is a sign of decreasing skewness.

Table 34 shows some of the percentiles of the distributions of the dependent variables for both years (quintiles are shadowed in grey). In both variables the figures in 2007 show higher up to the third quintile where this trend turns the other way round. This is clearly seen in the CRE model below, where the year 2007 dummy is positive and significant at the lower end and negative at the top end of the distribution of income and income per capita.

Table 34 Quantiles Income Income PC

	Income		Income PC	
	2005	2007	2005	2007
p1	-35,604	-48,700	-9,556	-14,400
p5	-12,835	-10,950	-2,640	-2,257
p10	-4,308	-490	-1,096	-115
p20	2,289	5,318	494	1,408
p25	4,445	8,150	1,069	1,983
p30	6,450	10,980	1,561	2,465
p40	12,351	15,000	2,736	3,390
p50	17,274	18,807	3,866	4,271
p60	23,987	23,900	5,061	5,168
p70	31,732	29,568	6,867	6,352
p75	36,851	33,140	7,934	7,200
p80	43,003	38,143	9,290	8,196
p90	65,018	54,250	13,872	11,448
p95	92,699	70,986	20,114	16,200
p99	205,958	155,020	42,448	35,980

A more intuitive view was found in Figure 7 (See also Figure 12 for income at appendix of chapter 2). It was clear the presence of vertical outliers at the top and bottom of the distribution. This makes quantile regression an appropriate estimation method, not only because it allows studying the impact at the different points of the conditional distribution but also because it is not sensitive to the presence of these vertical outliers. It was also shown in the descriptive statistics section of the previous chapter the presence of a considerable share of negative observations and it was not possible to discard a small share of them in order to log the dependent variables.

In Table 35 it can be seen that although the range is mainly driven by the presence of outliers, 2007 normally shows lower values in the dispersion measures such as standard deviation or interquartile range. Also, the skewness decreases as it is suggested by the Table 66 . Despite this, skewness and kurtosis still remain very high. This feature is one of the main drawbacks when using income and income per capita as dependent variables in impact studies. It would have been preferable to have measured the impact on consumption using a measure such as consumption expenditures. They tend to be less volatile than income, as it was seen in the first impact study. No information was available in the present survey about the latter, though.

Table 35 Other statistics of distributions

	Income		Income_pc	
	2005	**2007**	**2005**	**2007**
min	-94,555	-198,722	-30,117	-71,476
max	395,330	490,500	144,614	98,100
range	489,885	689,222	174,732	169,576
sd	41,399	37,307	9,939	8,305
skew	3.24	3.21	5.28	1.72
kurtosis	20.79	37.43	61.08	34.16
Coeff of variation	1.56	1.55	1.72	1.61
iqr	32,406	24,990	6,865	5,217

Income per capita is better suited for development studies as takes into account the size of the household. In developing countries, where the household's number of members tend to be large, this is an important point given the fact that a great impact found at household level might need to be shared among too many members of the household ending up into a negligible effect at individual level. Therefore, the analysis will be centred around income per capita and additional comments will be done about income.

The distribution of the dependent variables is split into quintiles in the descriptive section. In the analysis the split is done over 19 quantiles for the sake of precision, but descriptives based on all these quantiles would be too detailed and therefore quintiles were preferred for this section. Other quantiles could have been used, though. The presence of two time periods enriches the study and allows to control for the time effect as will be seen in the CRE model.

Descriptives of differenced dependent variables

Another way of taking advantage of variation between years is to find the impact over the differenced variables. This was seen in the former chapter where a DID approach was attempted. However, quantile regression is not linear as it is the case of OLS and the use of the differenced variable will be used as a preliminary approach.

In Table 35 it could be seen that income and income per capita don't follow a normal distribution. Differencing them clearly reduces the skewness but still the kurtosis remains far from the normal (Table 36). The differenced versions of both dependent variables are slightly skewed to the left and thus median and mean are different. This is again due to the presence of outliers and therefore quantile regression is again a plausible approach.

Table 36 Descriptives differenced dependent variables

	mean	sd	median	skewness	kurtosis
Δincome	-2,388	54,050	1,420	-0.449	17.2
Δincome p/c	-496	11,908	273	-0.533	20.4

In the quantitative section, two different approaches are attempted with differenced dependent variables. In the first only differenced covariates are included in the model. Literature suggests, however, that level variables should be included as explanatory variables in order to control for differences that were already present in the first period. In the present case it was of interest to check whether wealthier households in 2005 were more prone to have a higher increase in the dependent variables with the past of time, that is, were more prone to become richer.

As a first approximation to this possibility it was found the rate of growth of each dependent variable. Then the sample was split into the quintiles of income per capita or income in 2005. Finally, the growth variable (in percentage) was regressed over the level explanatory variable in order to know how the level of income per capita or income can explain the percentage increase of the variable between years. The formula would be:

$$Growth\, rate(depvar)_q = constant + depvar_{2005,q}$$

where *depvar* can be either income or income per capita and *q* refers to quantiles one to five.

The outcomes are found in Table 37. In the case of the first quintile an increase of one rupee in 2005 income per capita would increase the growth rate in income per capita by 0.2494 of a percentage point. The estimate is not statistically significant however. Significant estimates are consistently negative meaning that the level of dependent variables in 2005 and growth rate have a negative relationship. This means that poorer households in 2005 show greater growth rates than better off households, something that seems to be quite consistent for both variables apart from the first quintile.

Table 37 Correlation coefficients differenced income per capita

	Regress growth over Income PC	
	estimate	**p-value**
q1	0.2494	0.35
q2	-0.1091	0.00
q3	-0.0169	0.00
q4	-0.0054	0.00
q5	-0.0012	0.00

Dynamic descriptives by quintiles and districts.

The first two parts of the analysis are based on the application of quantile regression to a cross sectional dataset with level variables, differenced variables or both. In the third part the quantile regression is applied to the proper panel dataset, with a time variable and two observations per household. Different techniques have been used to apply quantile regression to panel data. Due the particular characteristics of our dataset, only CRE could be used as it will be discussed below.

Quintiles are shadowed in grey in Table 34. It is of interest to study the quantile mobility: how households change among quantiles. For this purpose the sample was tabulated into unconditional quintiles of the dependent variables using two-way tables. Outcomes are shown regarding income per capita variable, although the case of income is similar. The diagonal of Table 38[32] contains those households that remain in the same quantile of income per capita at both years. The maximum value is seen at quintile 5, meaning that approximately a 29.7% of households in quintile 5 in 2005 remained at the same quintile in 2007 (see row 5, col 5). The first quintile (row 1, col 1) is the second in this diagonal with about a 26% of non moving households.

[32] In each cell there are three rows. The first row is the number of household in those coordinates. The second row of the cell is the percentage that the first row number represents with respect to the total households in the quintile in 2005 (totals in the right hand side column). The third row is the percentage that the number in the first row of the cell represents with respect to the total of households in that quintile in 2007 (totals down the column).

Table 38: Quintile mobility. Income per capita. All districts

Quintiles 2005	Income per capita all districts					
	Quintiles 2007					
	1	2	3	4	5	Total
1	77 26.12 26.23	59 20.03 20.11	62 21.3 21.38	37 12.56 12.61	59 19.98 20.11	**293** **100** **20.09**
2	53 18.03 17.98	63 21.51 21.45	59 20.24 20.18	69 23.6 23.52	48 16.61 16.6	**291** **100** **19.95**
3	49 16.81 16.83	78 26.68 26.72	70 24.07 24.09	51 17.56 17.57	44 14.88 14.94	**293** **100** **20.03**
4	45 15.28 15.3	55 18.95 18.98	63 21.59 21.61	75 25.5 25.51	55 18.67 18.74	**292** **100** **20.03**
5	69 23.78 23.66	37 12.81 12.75	37 12.8 12.73	61 20.91 20.79	86 29.7 29.61	**291** **100** **19.9**
Total	**292** **20** **100**	**292** **20.01** **100**	**292** **20.01** **100**	**292** **20.02** **100**	**291** **19.96** **100**	**1460** **100** **100**

The most apparent feature of the table is the great mobility among quintiles. In this there is a remarkable figure which is the share of households that were in 2005 in quintile 5 and moved lower down the distribution to quintile 1 in the second period (in table row 5, column 1). They represent almost 24% of households that were in quintile five in 2005, only below the 29.5% of those that remained. On the other hand, around 20% of households that were in 2005 in quintile one moved to quintile five in 2007. This proportion is also quite high given the big difference in income per capita between quantiles one and five. These swings are even clearer by districts. Prakasam and Anantapur are examples of these changes from quintiles one in 2005 to five in 2007 and Kurnool for the contrary (At the appendix table 78, 79 and 80 for Prakasam, Kurnool and Anantapur, respectively)

In order to see how the proportion of households of the different districts moved of quintiles among years, Table 39 is created with income per capita quintiles. The figures are found by subtracting, at each quintile, proportion2005 – proportion2007. Thus, in the case of Anantapur, the proportion of households in the first quintile in 2005 was

35.43% and in 2007 this is reduced to 25.32% and therefore the Anantapur value for the first quintile is -10.11%.

The main feature of Anantapur is that in 2007 less households were at the poorer quintiles and more at the top two quintiles. Kurnool shows a great increase in the households at the first quintile and a decrease at the top of the distribution. This same pattern although to a smaller extent is found in Mahabub Nagar. The last row of the table finds the mean of the changes in absolute values. These means are higher at the first and fifth quintiles, which confirms that the swings at the bottom and the top of the distributions are greater.

Table 39 Swings 05 - 07 by district[33]

	Income p.c. $share_{2005} - share_{2007}$				
	1	**2**	**3**	**4**	**5**
Prakasam	-0.99	3.6	-0.54	-2.72	0.65
Kurnool	12.48	4.03	-9.31	1.97	-9.17
Anantapur	-10.11	-4.69	0.85	6.05	7.9
Mahabub Nagar	8.48	-3.64	6.61	-3.72	-7.71
Nalgonda	-6.53	1.84	4.1	-4.83	5.44
mean (abs values)	**7.72**	**3.56**	**4.28**	**3.86**	**6.17**

In order to check whether this pattern of big swings is followed by a particular group of households, rather than randomly, a binary variable was created. It takes the value of one for households moving by four quintiles between 2005 and 2007 and zero otherwise (moving three quantiles or less and not moving households). Then, some variables that are considered relevant in rural areas were chosen. These were “household head’s main activity is agriculture” and “household head’s is self-employed”. Also household’s plots area, a binary indicating if the household had suffered a shock or not (**shock**) and the proportion of SHG borrowing households at each group. Then a mean test is done, using a two-sided and a single sided test.

The test is whether household swinging by four quintiles (swing = 1 in the table) have a higher proportion of household heads self employed and working in agriculture or if the average plot size is higher in this group of households. Also, if they are more likely to have a shock or to borrow from SHGs. It seems plausible to think that these households

[33] Quintiles referred to the national income per capita.

that moved four quintiles could come from better off farmers that invest bigger sums of money per year. Thus, an average year should take them to the top of the income or income per capita distribution. However, bad years can also move households further down their normal quintiles as they might lose more money than average. The test is formulated as follows[34]:

H_0 : Mean(treated =1) = Mean(control = 0)

Ha_1: Mean(treated =1) > Mean(control = 0).

Outcomes in Table 40 show that it is quite likely that these households are composed as described. It has to be remarked also that the average plot size for swingers almost doubles the plot size of those that did not swing. In addition, swinger households are more prone to suffer a shock at some point. These differences are not found, however, in the proportion of households that have borrowed at least one year from SHGs[35]. In this case we cannot reject the null of equal proportions at 5% significance level.

Table 40 Mean test swingers/not swingers

t-test quintiles income per capita				
variables	**mean (swing = 1)**	**mean (swing=0)**	**p-val (Ha_1)**	**p-val (Ha_2)**
HH head self-employed	0.78	0.539	0.000	0.000
HH agriculture	0.822	0.771	0.035	0.069
Plot area	6.554	3.337	0.000	0.000
Shock	0.627	0.569	0.040	0.080
SHG borrower	0.424	0.473	0.930	0.140

In general, one of the main issues with income and income per capita in datasets from poor areas of developing countries is its volatility. This feature is more acute in this case as households normally depend on the same sources of income, mainly the harvests and livestock production. The economies are poorly diversified, as not many alternative activities are possible, and highly weather-dependent. These great fluctuations may be due to profit reversals in farming and livestock production. But also the accounting

[34] In table it is also reported the p-value versus the alternative Ha_2: Mean(treated =1) ≠ Mean(control = 0)

[35] From last chapter, the participation dummy for SHG borrowers was called “everborrow” and took the value of one if the household had borrowed at least one of the years, 2005 or 2007.

methods to calculate the variables together with the measurement errors in the inputs used to create them can be a source of this volatility.

In order to check if this quantile mobility matches with wealth mobility, changes in other proxies can also be observed. Two of these proxies are the total cultivable land area owned by the household and also a domestic asset index. The latter is the same index used in DID models in the former chapter.

Table 81 at the appendix shows some quantiles of the plot area variable. The main feature is that most of the observations below the median have no cultivable land. In 2007 it cannot be seen one acre of land until the third decile. Differences increase above the median.

Table 41 is another transition matrix to show how households moved among cultivable area quintiles. It is clearly much less volatile. The diagonal, giving the households that do not move of plots area quintiles, shows the greater figures. Also, mobility happens mostly between quintiles which are closer to each other. There were only two cases of households that moved from the first quintile to the fifth and two observations moved from quintile five to quintile three. Thus this proxy for wealth shows no such mobility as seen in the income/income per capita cases. Mobility in plot area quintiles can also be seen in Table 82 at the appendix.

Table 41 Plot area mobility

Quintiles 2005	Plots area all districts					
	Quintiles 2007					
	1	2	3	4	5	Total
1	376	35	11	8	2	**432**
	87.04	8.1	2.55	1.85	0.46	**100**
	100	17.77	3.65	2.38	0.8	**29.61**
2	0	129	8	2	2	**141**
	0	91.49	5.67	1.42	1.42	**100**
	0	65.48	2.66	0.6	0.8	**9.66**
3	0	33	273	31	4	**341**
	0	9.68	80.06	9.09	1.17	**100**
	0	16.75	90.7	9.23	1.61	**23.37**
4	0	0	7	224	17	**248**
	0	0	2.82	90.32	6.85	**100**
	0	0	2.33	66.67	6.83	**17**
5	0	0	2	71	224	**297**
	0	0	0.67	23.91	75.42	**100**
	0	0	0.66	21.13	89.96	**20.36**
Total	**376**	**197**	**301**	**336**	**249**	**1459**
	25.77	**13.5**	**20.63**	**23.03**	**17.07**	**100**
	100	**100**	**100**	**100**	**100**	**100**

With respect to asset index variable[36], Table 42 shows that 439 household didn't own any assets at both years. Overall, in year 2005 households with none or just one asset represented the 82.4% of the households, while in 2007 this share decreased to 72.2%. The mobility, although a bit higher than in the case of plot area, is not as volatile as the case of income or income per capita. None of the households owned the set of six assets in 2007 and only one did in 2005. As seen in Table 83 at the appendix, the 84.9% of the households remain within the range of ∓ 1 index value.

[36] Although already defined, as a reminder, asset index is a variable created with 6 different assets: radio, television, fridge, bicycle, motorcycle and car. The variable takes the value of 0 if none of these assets are present in the household. It can take up to the value of 6.

Table 42 Asset index mobility

Asset Index in 2005	Asset Index in 2007						
	0	**1**	**2**	**3**	**4**	**5**	**Total**
0	439	184	92	22	2	0	**739**
	59.4	24.9	12.45	2.98	0.27	0	**100**
	71.85	41.53	30.77	25	11.11	0	**50.62**
1	133	187	109	32	3	0	**464**
	28.66	40.3	23.49	6.9	0.65	0	**100**
	21.77	42.21	36.45	36.36	16.67	0	**31.78**
2	28	50	76	18	5	0	**177**
	15.82	28.25	42.94	10.17	2.82	0	**100**
	4.58	11.29	25.42	20.45	27.78	0	**12.12**
3	10	15	20	13	4	0	**62**
	16.13	24.19	32.26	20.97	6.45	0	**100**
	1.64	3.39	6.69	14.77	22.22	0	**4.25**
4	1	4	2	2	4	0	**13**
	7.69	30.77	15.38	15.38	30.77	0	**100**
	0.16	0.9	0.67	2.27	22.22	0	**0.89**
5	0	2	0	1	0	1	**4**
	0	50	0	25	0	25	**100**
	0	0.45	0	1.14	0	100	**0.27**
6	0	1	0	0	0	0	**1**
	0	100	0	0	0	0	**100**
	0	0.23	0	0	0	0	**0.07**
Total	**611**	**443**	**299**	**88**	**18**	**1**	**1460**
	41.85	**30.34**	**20.48**	**6.03**	**1.23**	**0.07**	**100**
	100	**100**	**100**	**100**	**100**	**100**	**100**

Given the scarce variability of these plot area and asset index variables, it would not be feasible to run quantile or other sort of regression to analyse the impact of borrowing from SHGs. However, they contribute to show that the volatility in income and income per capita quintiles might not match exactly social mobility as other factors are intervening as well. Also, they are good indicators of the incidence of poverty in the dataset used.

Shocks

Another vital issue with respect to this volatility is the presence of shocks[37]. The studied survey devoted a whole section to shocks and collects detailed information about the

[37] As explained in the former chapter, shock is defined in several ways: loss of property or livestock, death, severe illness or injury of a member or failure of crops.

type of shock, whether the shock seriously affected the household, how the household responded to the shock and, when it was due to crop failure, the causes of it. The abundance of these shocks would justify to some extent this volatility between quintiles.

The information is summarized by quintile in Table 43 and Table 44. In the tables are shown the households that suffered a shock together with some percentages. The column *%fail crop* represents the proportion of these shocks that were due to a bad crop. In addition to this, it was seen that overall the main reason for failed crops was a lack of water. Column *% lack water* represents the percentage of those failed crops caused by the lack for water.

Table 43 Shocks by quintiles 2005

Shock by quintile 2005				
Quintiles income per capita	***Shock=1***	***% shocks***	***% fail crop***	***%lack water***
1	228	78%	89%	83%
2	195	67%	79%	76%
3	169	58%	84%	82%
4	152	52%	77%	69%
5	152	52%	78%	68%
Averages (Total for column *Shock = 1*)	**896**	**61%**	**82%**	**76%**

Table 44 Shock by quintiles 2007

Shock by quintile 2007				
Quintiles income per capita	***Shock=1***	***% shocks***	***% fail crop***	***%lack water***
1	203	70%	86%	74%
2	171	59%	74%	59%
3	128	44%	60%	55%
4	149	51%	56%	44%
5	155	53%	53%	49%
Averages (Total for column *Shock = 1*)	**806**	**55%**	**66%**	**57%**

It is clear that households in the sample were subject to many shocks and the incidence of crop failures is quite high, especially in 2005. On average, 82% of the shocks were due to this in 2005 and the 66% in 2007. In this kind of weather dependent economies one of the main points is to see whether the rainfall is important. It is also clear that the lack of water is important in 2005 and 2007 although the proportion of shocks for this reason is higher in 2005.

The first quantile suffered in proportion more shocks than the rest, followed by the second quantile, and this happened in both years, although differences are clearer in 2005. By district, Kurnool and Anantapur show a higher percentage of shocks and Prakasam and Nalgonda a bit lower than overall (Table 45). In the former two the share of shocks due to failed crops is very high as well as the proportion of shocks for this reason in 2005. The pattern shows that Prakasam and Nalgonda suffered proportionately less than Kurnool and Anantapur in both years. These figures are consistent with the big switches found in these latter districts.

Table 45 Shock by districts 2005 and 2007

	Year 2005			Year 2007		
District	**% shocks**	**%fail crop**	**%lack water**	**% shocks**	**% fail crop**	**% lack water**
PRAKASAM	53%	57%	44%	54%	49%	46%
KURNOOL	64%	96%	92%	58%	76%	71%
ANANTAPUR	68%	83%	80%	63%	73%	65%
MAHABUB NAGAR	64%	93%	88%	48%	63%	46%
NALGONDA	55%	74%	66%	49%	57%	47%
Total	**61%**	**82%**	**76%**	**55%**	**66%**	**57%**

Given the importance of rainfall in this type of agricultural economy, in Table 46 rainfall is shown by district. Normal rainfall is the average rainfall in the district. The columns are found with the equation [(actual rainfall / normal rainfall) – 1], to have proportionate figures rather than the absolute values. Measures are taken from June of the current year to May of the following year. Regarding normal rainfall, it is clear that Prakasam and Nalgonda are less dry districts and probably for this reason the drop in rainfall doesn’t affect them so greatly. On the other hand Anantapur is the driest and probably the most sensitive district to the scarcity of rainfall so that a lower percentage decrease in rainfall with respect to the average brings about worse consequences. The differences are not so high between 2005 and 2007.

Table 46 Rainfall by district (% over mean rainfall[38])

	Normal	% Δ normal rainfall		
District	**Average**	**2004-05**	**2005-06**	**2006-07**
PRAKASAM	871	-37%	16%	-19%
KURNOOL	670	-19%	25%	-19%
ANANTAPUR	553	-21%	43%	-26%
MAHABUB NAGAR	604	-32%	61%	-20%
NALGONDA	751	-31%	33%	-27%
Overall	**675**	**-27%**	**36%**	**-21%**

Finally, a rough regression could be done to see the effects on the differenced quintiles. The regressand is the difference $quintile_{07}$-$quintile_{05}$ and ranges from -4 to 4. The regressors will be binaries for shocks. The survey allows to distinguish between those shocks that were reported to seriously affect the life of the household members and those which were not. The equation would end as follows:

$$\Delta q(depvar) = constant + \ serious\_shock_t + non - serious\_shock_t + u_i$$

where Δq is equal to $quintile_{07}$-$quintile_{05}$, $depvar$ can be either income per capita or income and *t* refers to 2005 or 2007.

Outcomes are shown in Table 47 and are different for shocks happening in years 2005 and 2007. In 2005 the base category (no shock) shows a negative estimate meaning that not having a shock has a negative impact of the differenced quantiles. On the contrary, an affecting shock means an increase in the dependent variable. Actually, households that suffered a shock in the first period increase Δq for almost half a quintile, on average and ceteris paribus. This is because they have more potential to grow as they depart from level of income / income per capita that is lower than usual. No significant effect is found for any category in 2007, although the sign is negative as expected.

[38] [38] http://www.apdes.ap.gov.in/Dist@glance/rainfall.htm 22/09/11 @ 11.00 am
and http://www.apdes.ap.gov.in/publications/Outline%202006-2007.pdf 22/09/11 @ 11.00 am

Table 47 Δ Quintiles over shocks

	Regression dep. var: $quintile_{07}$-$quintile_{05}$			
	Income quintiles		Income per capita quintiles	
	Year of the shock			
	2005	2007	2005	2007
Shock seriously affected	0.482***	-0.159	0.552***	-0.145
	(4.659)	(-1.584)	(5.440)	(-1.466)
shock not seriously affected	0.081	0.153	-0.050	0.171
	(0.517)	(0.975)	(-0.323)	(1.113)
Constant	-0.268***	0.047	-0.262***	0.062
	(-3.471)	(0.642)	(-3.460)	(0.872)
Observations	1460	1455	1460	1455
Adjusted R-squared	0.014	0.001	0.021	0.001

Borrowing

In the presence of shocks households respond in different ways: they might sell animals or jeweller or they can ask for help from a relative or friend, etc. The survey contains several of these responses under categories and they gather information about the first, the second and the third response or means through which the household tried to get over each shock faced. The most usual response was to borrow money in a proportion of 77.9% in 2005 and 69% in 2007. Previous literature suggests that it might be important to know the amount borrowed by quintile as some sources contend that those groups that had access to bigger loans were the ones reaping the benefits of microfinance (Coleman 2006).

Tables will show whether there is some concentration of borrowers at some quintiles or, more importantly, if there is a pattern in the amount of money borrowed at different quintiles of income per capita. In the first case, shown in Table 48, there seems not to be any pattern and no great changes are seen between years. Overall, the most popular source of credit is moneylenders and landlords, followed by banks and SHGs.

Table 48 Nr of borrowers per source and quintile

	Nr of households everborrow=1 for different sources by quintiles Income_PC							
	Bank		SHG		Moneylender		Family Friends	
	2005	2007	2005	2007	2005	2007	2005	2007
q1	159	175	139	125	192	186	96	81
q2	138	142	136	134	180	160	85	101
q3	119	110	116	134	174	199	85	83
q4	116	117	160	150	188	186	97	96
q5	142	130	133	141	178	181	97	99

In Table 49 it is found the average sum borrowed by income per capita quintile for the different sources. Overall there is a decrease from 2005 to 2007 except in the case of SHG where this is not so evident. In the case of banks high values are shown at the tails of the distribution. In these figures households that swing between the first and fifth quintiles may well have something to do with it. In the case of moneylenders the higher values are found at the bottom of the distribution and theoretically they should serve to the informal sector. It is seen that it is not only the most popular source but also the one that shows greater average loan sizes.

Table 49 Mean loan size by quintiles, source and year (take into account non borrowing households)

	Sums borrowed from different sources by quintiles Income PC							
	Bank		SHG		Moneylender		Family Friends	
	2005	2007	2005	2007	2005	2007	2005	2007
q1	7,049	9,362	1,332	1,856	11,173	9,357	3,302	2,550
q2	5,890	4,675	1,312	1,776	10,696	7,187	1,902	3,345
q3	5,247	2,286	1,294	1,610	7,772	6,638	3,428	4,069
q4	4,266	3,848	2,160	2,253	9,970	5,202	3,766	2,896
q5	7,451	4,668	2,723	1,959	8,777	6,587	5,615	3,301

In the case of SHGs, the amounts are sensibly lower than banks. Certain rules in village banks cap the amount that a member can borrow. These have to do with the amount that the member has in savings, for example, or the total size of the group's fund. These norms can explain the smaller average. The two upper quintiles are the ones that borrow more intensively from SHGs. Coleman (2006) argues that "committee members" get advantage of microfinance because they manage to access greater loan sizes. However, as it will be seen in the analysis, this doesn't seem to hold here. The impact is found at the middle of the distribution instead where mean loan sizes don't reach their maximums.

Thus, descriptives show some differences between 2005 and 2007 in the distribution of the dependent variable. Given the weather dependence of household economies, some of these differences between years and also among districts could well be explained by differences in rainfall and number of shocks suffered.

Quantile mobility is omnipresent when dealing with income and income per capita variables. This volatility is most likely due to the sensitivity of these variables to current profit/losses as other proxies for wealth such as owned land extension or an asset index weren't so volatile. Explanation for the biggest swings in quintiles (first to fith quintile or vice versa) could be profit reversals of relatively wealthy farmers. Shocks can explain some of this volatility but the number of households switching quintile is abnormally high.

The extra volatility could therefore most likely be attributed to the accounting methods when calculating income as in many cases the valuing methods cause great differences. For example when valuing the inputs produced at home as no prices are established for them. Also, as in any household survey of this kind, the measures of variables included in income are greatly affected by measurement errors.

There is not a especially high concentration of borrowers at the middle quintiles. Nor do they borrow more intensively. However, as will be seen in the analysis, the impact of SHG microfinance is mostly seen at the middle of the distribution.

Finally, it has to be made clear that the impact is said to take place with respect to some quantiles. When two time periods are included, impact takes place at the different points of the distribution of the dependent variable at which the difference between participants and non participants is significant. As clearly explained in Dammert (2009) the impact clearly do not refer to the individuals in particular, unless it could be assumed that the same households remain at the same points of the dependent variable distribution. This assumption is called "rank preservation assumption" and is quite important in other quantile approaches for panel data such as Quantile Treatment Effects (QTE) or Quantile Difference In Difference (QDID). It clearly doesn't hold in the present case.

Analysis

PSM, OLS, DID... etc all are techniques that provide a single estimate of the impact. Quantile regression allows the researcher to refine further this coarse estimation and find the impact at a particular point of the conditional distribution of the dependent variable. Quantile regression started to be used as Least Absolute Deviation (LAD) which is a quantile regression at the median. It tries to minimize not the sum of the squared errors (OLS) but the sum of absolute residuals. These residuals are given a weight of q (where q is the quantile) when the residual is positive and (1-q) when it is negative[39]. The formula was already shown above (1.39). As it is not differentiable minimization is done through linear programming. As errors are not squared, it is a less sensitive method to vertical outliers and therefore suitable for the present dataset.

If we are able to measure the impact at many quantiles (q) of the conditional distribution of the dependent variables, the method is extremely interesting in our quest of the effects of microfinance at different points of this distribution, especially at the bottom tail which includes the poorest.

The analysis that follows is based in three main approaches. In the first stage the quantile regression is done over the cross sectional datasets for 2005 and 2007. We still stick in these regressions to the assumption from Coleman's model in which unobservables leading participation are controlled for with the "everborrow" dummy, which takes the value of one if the household borrowed at least once (2005 or 2007 or both). In the second stage, the quantile regression is done over the differenced dependent and independent variables from stage one. Later, the level variables from 2005 are also called into the model in order to control for initial conditions. In the third stage the Correlated Random Effect model is applied, showing the graphs of the evolution of estimates.

All the quantile regressions henceforth are implemented over 18 quantiles starting at the fifth percentile and going up by 5 percentiles each time until the 95th. In quantile regression literature it is usual to report only the 10th, 25th, 50th, 75th and 90th although we tried to report more quantiles in order to show outcomes as clearly as possible at the appendix.

---

[39] In the case of Least Absolute Deviation, the quantile used is the median q = 0.5, (1-q) = 0.5 and therefore all the residuals are equally weighted.

Cross sectional quantile regression for years 2005 and 2007

The first approach is a cross sectional quantile regression done over both years independently. The models try to replicate as closely as possible the models previously seen in the former chapter and "everborrow" still controls for unobservables. Again the impact is measured with the loan size variables. The standard errors are calculated through bootstrapping method, withdrawing random samples with replacement by the method of paired bootstrap.

Cross sectional: Income per capita

In this first stage, the main outcome is that SHG borrowing has a significant impact at the middle of the conditional distribution of income per capita in 2005. In 2007 the impact is reduced and affects lower percentiles, closer to the bottom: 10^{th} to 30^{th}. Banks show positive estimates at the bottom and positive at the top of the distribution but significance changes with years. Moneylenders show also differences in significance.

Outcomes for a limited number of per capita income percentiles in 2005 and 2007 are shown in Table 50. At the appendix, Table 84 shows more percentiles for the quantile regression in 2005. Variables controlling for having ever borrowed from any of the sources show, when significant, a negative bias rather than a positive one. Thus no evidence of positive selection bias is found, according to Coleman's set up.

The impact of borrowing from different sources showed negative and significant estimates in the case of banks at the lowest quantiles and it turned positive and significant at the top of income per capita distribution. In the case of moneylenders, estimates are positive and statistically significant only at the middle of the distribution.

Our main interest is on the impact of borrowing from SHGs. This shows a more homogeneous pattern of positive and significant impact at percentiles ranging from the 20^{th} to the 70^{th} percentiles, almost matching the interquartile range. In 2005, a rise of 100 rupees in a microfinance loan increased income per capita in 2005 by 10.1 rupees at the 20^{th} percentile and only by 7 at the 70^{th}, with slight downward trend. Benefits from microfinance were grabbed neither by the richest nor by the poorest households given this analysis.

In 2007 (Table 50 for limited number of percentiles, Table 85 at the appendix with more percentiles), in the case of borrowing from banks, significant impact disappears from the top quantiles and shifts towards the bottom of the distribution of income per capita. Below the median there is an overall negative and significant impact and the positive estimates above the median are no longer significant. In the case of moneylenders, the impact in 2007 is found negative and significant from the 5^{th} to the 25^{th} percentiles.

The shift towards the bottom of the distribution can be clearly seen also in the case of SHG borrowing. In 2007 the range of quantiles where the estimates are significant is only between the 10^{th} and the 40^{th} percentiles. Also, at quantiles that are significant at both years the estimates are lower in the case of 2007. Interpreted from a policy point of view, in 2007 the benefits seemed to be reaped by poorer households which would be the target for microfinance programs and this would be a sign of the success of microfinance programs.

Table 50 Cross sectional quantile regression income per capita. Years 2005-2007

	Income_pc 2005					Income_pc 2007				
Source \ Percentiles	**0.1**	**0.25**	**0.5**	**0.75**	**0.9**	**0.1**	**0.25**	**0.5**	**0.75**	**0.9**
banks	-0.026	-0.006	0.015	0.125**	0.193***	-0.136***	-0.036*	-0.032**	0.012	0.131
z-scores	(-1.448)	(-0.331)	(0.519)	(2.327)	(4.132)	(-2.686)	(-1.721)	(-2.163)	(0.347)	(1.337)
NGO	0.384	0.326	0.290	0.247	0.173	1.176	0.246	0.307	0.021	0.430
z-scores	(1.159)	(1.037)	(0.867)	(0.519)	(0.267)	(0.850)	(0.331)	(0.685)	(0.047)	(0.694)
Moneylender	0.016	0.010	0.027**	0.018	0.001	-0.047**	-0.038**	-0.008	-0.007	-0.018
z-scores	(0.864)	(1.161)	(2.021)	(1.308)	(0.012)	(-2.267)	(-2.168)	(-1.007)	(-0.738)	(-1.012)
Family & Friends	-0.020	-0.002	0.022	0.013	-0.007	-0.008	0.007	0.004	0.001	0.037
z-scores	(-1.243)	(-0.087)	(1.137)	(0.831)	(-0.116)	(-0.187)	(0.583)	(0.363)	(0.022)	(0.839)
Other	-0.039	0.005	0.058	0.067	0.052	-0.243*	-0.110	-0.080	0.031	0.747*
z-scores	(-0.785)	(0.111)	(1.358)	(0.998)	(0.413)	(-1.907)	(-1.240)	(-1.055)	(0.114)	(1.939)
SHG	-0.002	0.099**	0.087**	0.065	0.041	0.065*	0.049**	0.027	0.063	0.001
z-scores	(-0.034)	(2.475)	(2.542)	(1.348)	(0.586)	(1.820)	(2.202)	(0.966)	(1.513)	(0.006)

t statistics in parenthesis; * p<0.10; ** p < .05; *** p < .01; Year 2005 Nr. of observations: 1,460. Year 2007 Nr of observations 1,466. In yellow, blue and orange estimates that are significant at least at 10% level for loan size from SHG, bank and moneylenders respectively.

Quantile regression over differenced variables

In this approach the significance of bank borrowing is only present when level variables from 2005 are included in the model. Moneylender borrowing, when significant, shows negative signs at different quantiles on income per capita and income. Borrowing from SHG shows a positive and significant effect on income per capita and income, although in the latter case this is constrained to a few quantiles.

One of the advantages of the present dataset is that it contains two years and this allows for time variability. However, quantile regression is not linear and this implies that $Q_q(y_{i,2}|x_{i2}a_i) - Q_q(y_{i,1}|x_{i1}a_i) \neq Q_q(y_{i,2} - y_{i,1}|x_{i1}, x_{i2}, a)$ that is, the difference of quantile regressions is not equal to the quantile regression of the difference. Therefore, one of the basic properties that allowed us to implement the DID technique and remove the selection bias does not hold. However, the implementation of the quantile regression over differenced dependent variables can also be illustrative of the impact of microfinance over income and income per capita.

Variables again refer back to the previous chapter and basically reproduce the model used in the DID approach, just that in this instance the applied technique is quantile regression. The standard errors are again found through paired bootstrapping.

In a first step the differenced dependent variables are regressed over the differenced independent variables. The model is set as follows:

$$\Delta y_i = \beta_0 + \beta_1 \Delta X_i + \beta_2 \Delta loansize_i + \beta_3 \Delta SHG\_sum_i + \varepsilon_i \tag{1.45}$$

where y can be income or income per capita, X is a vector of variables including household size, shocks in the household, the maximum of education level achieved in the household, the asset index and the work per working capita. “Loansize” refers to the amount borrowed by the *ith* household from the different sources and the SHG_sum is the amount borrowed from the village banks.

Some variables from the right hand side of the equation were dropped in comparison with the cross sectional models. This is due to the fact that many of the variables could not be differenced out, such as district dummies. Others such as marital status scarcely change and variation is zero in most of cases.

Outcomes are shown with respect to income per capita in Table 51, for regressions with and without level variables in 2005. (For a wider range of variables and percentiles see Table 86 and Table 87, respectively, at the appendix of the chapter). In the model with no level variables, estimates of the change in the amount borrowed from banks, NGOs, family and friends and others do not show any significance at any of the quantiles. When the source is moneylenders and landlords, the estimates show consistently negative and significant only at the median and some higher quantiles from the 75th percentile upwards.

The impact of borrowing from SHGs is positive and significant consistently from the 20th percentile upwards the differenced income per capita distribution, with an upward trend in the impact as we move up the quantiles. The differences between SHG loan size estimates at different quantiles were tested in order to see if they were statistically significant. A Wald test[40] is used being the null hypothesis that the estimates are equal across quantiles. The null of equal estimates could not be rejected in the following pairwise comparisons: 25-50, 25-75, 25-90, 50-90, 75-90. Thus, the impact is homogeneous across the different quantiles.

Table 51 Quantile regression Δ income per capita without and with 2005 level variables.

	Δincome per capita					Δincome per capita with 2005 levels				
Source \ Percentiles	**0.1**	**0.25**	**0.5**	**0.75**	**0.9**	**0.1**	**0.25**	**0.5**	**0.75**	**0.9**
banks	-0.069	-0.017	-0.004	-0.005	-0.061	-0.040	-0.068**	-0.022	0.012	0.077
z-scores	(-1.19)	(-0.48)	(-0.19)	(-0.26)	(-1.40)	(-0.859)	(-2.233)	(-0.709)	(0.376)	(1.032)
NGO	0.250	0.188	0.240	0.275	-0.033	0.844	0.427	0.241	-0.034	1.064
z-scores	(0.50)	(0.44)	(0.59)	(0.93)	(-0.11)	(0.616)	(0.525)	(0.356)	(-0.036)	(1.029)
Moneylender	0.000	-0.009	-0.020*	-0.019*	-0.037***	-0.119*	-0.019	-0.003	-0.001	-0.002
z-scores	(0.02)	(-0.50)	(-1.73)	(-1.86)	(-3.00)	(-1.871)	(-0.803)	(-0.194)	(-0.057)	(-0.064)
Family & Friends	-0.016	-0.008	0.017	0.005	0.019	-0.044	-0.039	-0.006	0.002	0.035
z-scores	(-0.39)	(-0.43)	(1.17)	(0.28)	(0.84)	(-0.524)	(-1.241)	(-0.256)	(0.058)	(0.697)
Other	-0.068	-0.021	-0.012	0.037	-0.035	-0.257	-0.150	-0.196	0.150	0.670**
z-scores	(-0.92)	(-0.51)	(-0.24)	(0.38)	(-0.28)	(-1.139)	(-1.276)	(-1.251)	(0.681)	(2.093)
SHG	0.021	0.086***	0.080***	0.128***	0.127*	0.057	0.036	0.096	0.124*	0.110
z-scores	(0.28)	(3.97)	(2.69)	(3.02)	(1.86)	(0.358)	(0.731)	(1.537)	(1.703)	(0.619)

t statistics in parenthesis; * p<0.10; ** p < .05; *** p < .01; Nr. of observations: 1,460. In yellow, blue and orange estimates that are significant at least at 10% level for loan size from SHG, bank and moneylenders respectively.

Regressions done over differenced variables can be improved by controlling for differences at the beginning of the survey. This can be done in this case adding 2005 level variables. This is a very common practice also in DID analysis (Masset and White 2006).

The model will therefore end up as follows:

[40] The Wald test is implemented following Hao, L. and D. Q. Naiman (2007). Quantile regression, SAGE Publications, Inc., page49. Wald statistic = $\frac{(\hat{\beta}_j^{(p)}-\hat{\beta}_j^{(q)})^2}{\hat{\sigma}^2_{(\hat{\beta}_j^{(p)}-\hat{\beta}_j^{(q)})}}$ where p and q refer to the different quantiles and $Var\left(\hat{\beta}_j^{(p)}-\hat{\beta}_j^{(q)}\right) = Var\left(\hat{\beta}_j^{(p)}\right) + Var(\hat{\beta}_j^{(q)}) - 2Cov(\hat{\beta}_j^{(p)},\hat{\beta}_j^{(q)})$. The null hypothesis of equal estimates, the Wald statistic follows a χ^2 distribution with one degree of freedom.

$$\Delta y_i = \beta_0 + \beta_1 \Delta X_i + \beta_2 \Delta loansize_i + \beta_3 \Delta SHG\_sum_i + \beta_4 X_{i,5} + \beta_5 loansize_{i,5} + \beta_6 SHG\_sum_{i,5} + \varepsilon_i \tag{1.46}$$

where the subscript 5 stands for year 2005. $X_{i,5}$ includes household size, shocks in the household, maximum education level in the household, asset index and work per working capita in levels of year 2005. $loansize_{i,5}$ is the amount borrowed from the different credit sources by household i in year 2005. And finally, $SHG\_sum_{i,5}$ is the amount borrowed from Village Banks in 2005.

Table 51 shows the outcomes regarding income per capita (Δ income per capita). With respect to the differenced loan size variables, the effect on the differenced income per capita is negative for quantiles below the median with respect to banks and moneylenders.

Our interest is focused on the differenced microfinance loan size. The estimates are significant at 10% level only and in the 30th, 35th, 45th, 60th, 65th, 70th and 75th (not all seen in table). An increase of 100 rupees in the differenced loan size from SHG raises the differenced income per capita by 8.1 rupees at the 30th percentile and by 12.4 at the 75th. There is a clear upward trend moving up the differenced income per capita distribution. A similar pattern is found in the case of income (Table 90 and Table 91, appendix).

Overall, the addition of 2005 level variables constrains the number of quantiles at which the impact of SHG borrowing is significant. This is shown to a smaller extent when quantile regression is done over differenced income per capita but in the case of income significance almost disappears.

Overall, it can be said that the impact of SHG borrowing is significant at the middle of the conditional distribution of income per capita, as it was shown in the 2005 cross sectional outcomes and at the differenced models, with and without 2005 level variables. For income the pattern is similar although when including 2005 level variables the significance is seen only in two quantiles.

The differenced approach in quantile regression is the weakest when controlling for selection bias. The time invariant unobservables are not wiped as happens when doing

DID. Everborrow takes the value of one in both years and therefore it would work as other time invariant variables such as race or sex that cannot be included in the differenced models. The outcomes are illustrative of the possible impact of microfinance, but should be improved. On the other hand, cross sectional approaches control for selection bias through the "everborrow" variable but cannot get advantage of the time dimension of the dataset and only relies on the variation across households.

Thus, the aim is to surmount the weaknesses of the former approaches by finding a method that allows us to exploit the time dimension of the panel but also to avoid selection bias. For this purpose, panel techniques are combined with quantile regression.

Quantile regression applied to panel datasets.

Non linearity of quantile regression has forced to find some means to allow for time variability. Some have been suggested in the literature in order to surmount this problem. The one adopted in the present study is the Correlated Random Effect, used in Abrevaya and Dahl (2008) and Khandker et al. (2009) for datasets with similar characteristics.

Khandker et al. (2010) describe some other ad hoc techniques that have been proposed so far for this purpose. The first of them is Quantile Treatment Effect (QTE) which is found by calculating the horizontal differences between the quantiles of the treated and control distributions. The method assumes that there is a baseline survey and a subsequent random allocation of the program. For this reason its use becomes problematic when the dataset at hand comes from a non-randomised intervention. The technique is developed in Firpo (2007) and practical approaches can be found in works about the impact of cash transfers in Mexico (Djebbari and Smith 2008) and Guatemala (Dammert 2009). Also in labour economics (Bitler, Gelbach et al. 2008).

Athey and Imbens (2006) show other methods such as Changes in Changes (CIC) and Quantile Difference in Difference (QDID). In both it is assumed that the intervention takes place after the baseline survey. Poterba et al. (1995) and Meyer et al. (1995) pioneered in the use of QDID and later studies include Song and Manchester (2007) in the field of labor economics and Bitler et al. (2006) in connection with the study of impact of a program targeting the ultra poor in Bangladesh.

Finally, Koenker (2004) suggests an approach for conditional quantile regression with panel data under the assumption that T is increasing together with N, covariates are fixed and the disturbance terms are serially independent. The fixed effects are assumed to be constant for every value of q but in this case T is limited to two time periods.

The dataset at hand neither comes from a randomised intervention nor includes a baseline survey collected before the intervention had taken place. Also it has just two time periods. These characteristics make the former techniques ill suited to attempt a quantile regression analysis with them. Abrevaya and Dahl (2008), however, use the Correlated Random Effect (Chamberlain 1982; Chamberlain 1984) to find out the effect of different variables on two birthweights from the same mother. Khandker et al. (2009) borrow from this technique in order to find the distributional impact of rural roads in Bangladesh. This method suits the characteristics of the present dataset, where baseline survey before the intervention is not available and only two time periods are available. Thus, it will be applied in our aim of improving the cross sectional and differenced variables approaches.

Correlated random effects model (CRE).

Basics of the model

In this method it is assumed that the unobservables are linearly related to the observables. If this can be argued, unobservables can be controlled for and the impact effect is freed of selection bias providing with an accurate impact effect estimate. With respect to the time dimension, the model assumes that the impact of covariates remains constant at both periods and the time effect is measured on a dummy for the second year. It follows a more technical description of the CRE model followed by how it is applied to quantile regression and the section ends with the discussion of the outcomes when this technique is used.

This model is based in Chamberlain's work (Chamberlain 1982; Chamberlain 1984). We depart from the standard linear panel data model:

$$y_{it} = x'_{it}\beta + a_i + u_{it};\ \ t = 1,2 \tag{1.47}$$

where a_i stands for the time invariant unobservables for individual (household) i and u is the error term. a_i is supposed to be correlated with the observables x. It is also assumed that the error term is uncorrelated with the observables and the unobservables, that is

$$E(u_{i1}|x_i a_i) = E(u_{i2}|x_i a_i) = 0 \quad \forall i \tag{1.48}$$

The unobservables are considered as a linear function of the covariates:

$$a_i = \psi + x'_{i1}\lambda_1 + x'_{i2}\lambda_2 + v_i \tag{1.49}$$

where ψ is a scalar, λ 1 x K vectors (where K is the number of independent variables but not the time dummy) and v_i a disturbance uncorrelated with the covariates. If (1.47) is represented for the 2 time periods and a_i is substituted by its formula in (1.49), it results into:

$$y_{i1} = \psi + x'_{i1}(\beta + \lambda_1) + x'_{i2}\lambda_2 + v_i + u_{i1} \tag{1.50}$$

$$y_{i2} = \psi + x'_{i1}\lambda_1 + x'_{i2}(\beta + \lambda_2) + v_i + u_{i2} \tag{1.51}$$

The estimation of the parameters $\psi, \lambda_1, \lambda_2, \beta$ can be done through a OLS method but also GMM estimators can be used. The effects of the covariates on the contemporaneous dependent variable are split in two parts: a direct effect which is measured through the coefficient β and an indirect effect which is measured through the coefficient λ. This is nothing else than the effect of the unobservables. Covariates of the previous or ulterior time period have only an indirect effect on the dependent variable given by the λ coefficient.

Adding a final assumption:

$$E(v_i|x_i) = 0 \tag{1.52}$$

we can find the value of β:

$\frac{\partial E(y_{i1}|x_i)}{\partial x_{i1}} = \beta + \lambda_1$ and $\frac{\partial E(y_{i2}|x_i)}{\partial x_{i1}} = \lambda_1$, and therefore

$$\beta = \frac{\partial E(y_{i1}|x_i)}{\partial x_{i1}} - \frac{\partial E(y_{i2}|x_i)}{\partial x_{i1}} \tag{1.53}$$

In the same vein, it can be seen that

$$\beta = \frac{\partial E(y_{i2}|x_i)}{\partial x_{i2}} - \frac{\partial E(y_{i1}|x_i)}{\partial x_{i2}} \tag{1.54}$$

Estimation of effects on conditional quantiles with panel data with two time periods

Abrevaya and Dahl (2008) borrow from the correlated random effects framework above described. The utility for the present study is that they find a solution to the problem of the non observables under non linearity in the case of a panel with two time periods. They try to find the effect of the covariates on the conditional quantiles in the same manner as in equations (1.53) and (1.54) for conditional expectation:

$$\frac{\partial Q_q(y_{i1}|x_i)}{\partial x_{i1}} - \frac{\partial Q_q(y_{i2}|x_i)}{\partial x_{i1}} \tag{1.55}$$

$$\frac{\partial Q_q(y_{i2}|x_i)}{\partial x_{i2}} - \frac{\partial Q_q(y_{i1}|x_i)}{\partial x_{i2}} \tag{1.56}$$

If for conditional expectation the necessary assumption was (1.52) in the conditional quantiles we need to establish the assumption of independence of all the disturbance terms, v_i, u_{i1}, u_{i2} with respect to x_i. Hence, the equations for the conditional quantiles would be:

$$Q_q(y_{i1}|x_i) = \psi_q^1 + x'_{i1}\theta_q^1 + x'_{i2}\lambda_q^2 \tag{1.57}$$

$$Q_q(y_{i2}|x_i) = \psi_q^2 + x'_{i1}\lambda_q^1 + x'_{i2}\theta_q^2 \tag{1.58}$$

where ψ_q^1 and ψ_q^2 are location shifts in the q^{th} quantiles in years 1 and 2, respectively which gather the effect of disturbances. λ_q^1 is the effect of unobservables on the conditional quantiles for the first year. Therefore, the searched effect of the observables on the conditional quantiles for year one (1.55) will be equal to $\theta_q^1 - \lambda_q^1$ and $\theta_q^2 - \lambda_q^2$ for the second time period (1.56).

Abrevaya and Dahl (2008) add another assumption which is that the effect of covariates is constant along both periods and therefore $\theta_q^1 - \lambda_q^1 = \theta_q^2 - \lambda_q^2 = \beta_q$. This is the same assumption that is done in equation (1.47) that characterizes the “within” estimator in panel data approaches.

With this restriction, equations (1.57) and (1.58) can be rewritten:

$$Q_q(y_{i1}|x_i) = \psi_q^1 + x'_{i1}\beta_q + x'_{i1}\lambda_q^1 + x'_{i2}\lambda_q^2 \tag{1.59}$$

$$Q_q(y_{i2}|x_i) = \psi_q^2 + x'_{i2}\beta_q + x'_{i1}\lambda_q^1 + x'_{i2}\lambda_q^2 \tag{1.60}$$

The estimation of the parameters is done through a pooled linear quantile regression for each q in which the observations corresponding the same household are stacked together:

$$\begin{bmatrix} y_{11} \\ y_{12} \\ \cdots \\ y_{21} \\ y_{22} \\ \cdots \\ \vdots \\ \cdots \\ y_{M1} \\ y_{M2} \end{bmatrix} \quad \text{and} \quad \begin{bmatrix} 1 & 0 & x'_{11} & x'_{11} & x'_{12} \\ 1 & 1 & x'_{12} & x'_{11} & x'_{12} \\ \cdots & \cdots & \cdots & \cdots & \cdots \\ 1 & 0 & x'_{21} & x'_{21} & x'_{22} \\ 1 & 1 & x'_{22} & x'_{21} & x'_{22} \\ \cdots & \cdots & \cdots & \cdots & \cdots \\ & & \vdots & & \\ \cdots & \cdots & \cdots & \cdots & \cdots \\ 1 & 0 & x'_{M1} & x'_{M1} & x'_{M2} \\ 1 & 1 & x'_{M2} & x'_{M1} & x'_{M2} \end{bmatrix}$$

Source: (Abrevaya and Dahl 2008)

The parameters ψ_q^1, $\psi_q^2 - \psi_q^1, \lambda_q^1$ and λ_q^2 are estimated. $\psi_q^2 - \psi_q^1$ stands for the "year effect" as is the difference between the locational shifts between the first and the second years. This is equivalent to the constant in the DID approach when an OLS is done over differenced variables. Time invariant variables can be added but it would be impossible to distinguish what share of their estimates is due to direct and which to indirect effects (Abrevaya and Dahl 2008).

As mentioned above, the standard asymptotic formula in Koenker and Bassett (1978) is not normally used in quantile regression. Bootstrapping is used instead. However, quantile regression is normally used with cross-sectional data where each observation corresponds to one subject. In the case of panel data, the observations are clustered by subject and therefore they are not independent. The bootstrap method used to find out the standard errors is the paired bootstrap method but each time two observations corresponding to the same household are withdrawn from the sample (Abrevaya and Dahl 2008; Khandker, Bakht et al. 2009).

This model is original in the sense that it takes advantage of the linear approach of the correlated random effects model and applies it to quantile regression. In both cases, the assumption of independence of the covariates with respect to the disturbances is essential. If the assumption doesn't hold the linear form (1.50)/(1.51) is valid only in certain cases of conditional quantile functions.

Abrevaya and Dahl (2008) contend that although the conditional quantiles are linear on the covariates under very particular circumstances, even when the data-generating process is linear, this approximation should not be seen as restrictive. In traditional quantile approaches, working with cross sectional datasets, they fit a linear model to y_i, using the formula

$$Q_n(\beta_q) = \sum_{i:y_i \geq x_i'\beta}^{n} q\left|y_i - x_i'\beta_q\right| + \sum_{i:y_i < x_i'\beta}^{n} (1-q)\left|y_i - x_i'\beta_q\right| \qquad (1.61)$$

and in the seminal work by (Koenker and Bassett 1978) the conditional quantile regression is assumed to be linear (Angrist and Pischke 2009). The approach, thus, should be seen as a reduced form trying to model the conditional quantile and not the data generating process.

Panel Correlated Random Effects

Thus, the model includes the parameters λ_q^1 and λ_q^2 which control for the effects of the unobservables over the dependent variable so that the estimates of the independent variables will be neat of bias. Following Abrevaya and Dahl (2008) in a first stage a quantile regression was implemented over the pooled cross sectional dataset, without the parameters of the CRE[41]. Subsequently, parameters λ_q^1 and λ_q^2 are included to control for the unobservables, constituting thus the CRE model. The main difference in the calculation of the quantile regression in this model is the bootstrapping process. In the case of cross sectional data, the bootstrapping is done over observations. However, in the CRE model the resampling is done at household level. Thus, when a household is dropped the two observations of the sample corresponding to that household are dropped.

Hao and Naiman (2007) establish a number between 500 and 2,000 in order to estimate confidence intervals, in our case 750 repetitions are used. Then, the 5th, 50th and 95th percentiles are found. The median is reported to be the estimate of the quantile. The 5th and the 95th percentiles will constitute the limits of the 90% confidence interval. If zero

[41] Estimates available upon request

is not within this confidence interval, the estimate is said to be significant and otherwise if it is.

Tables include the estimates and the corresponding asymptotic t values are also reported. There is also a graphical representation in the case of estimates for income per capita. The outcomes omits the coefficients of the CREs, λ_q^1 and λ_q^2, reported separately in the appendix. In the graphs, the envelope corresponding to the 90 confidence interval is shadowed in grey and the quantile regression line of the CRE approach has a solid pattern.

As comparative references, the dashed lines correspond to the cross sectional quantile regression pooling both years, when λ_q^1 and λ_q^2 are not included and therefore no unobservables are controlled for. Finally the dashed-dotted straight line provides with the pooled OLS estimate of the variable. This is included to highlight how the quantile regression provides with a richer picture of the impact of covariates and doesn't restrict this to a single figure.

Outcomes for Figure 15 to Figure 18 below refer to income per capita and Figure 26 to Figure 29 at the appendix refer to income. The confidence intervals of both the OLS and the cross sectional regressions are not included for the sake of clarity.

Evolution of variables for income per capita

The effects of borrowing from SHG on income per capita can be seen very intuitively in Figure 15 which corresponds to the year 2007 dummy. When the envelope doesn't cross the zero line we can say that the estimate is significant. In the case of SHG borrowing the impact is mostly noticed at the middle of the dependent variables distribution. The estimates evolve from positive and significant at the bottom of the distribution to negative and significant at the top.

With regard to the household level variables (Figure 16), the negative impact of shocks and household size show the expected negative sign. The great disparity between the dashed line and the continuous line in the case of the worktime variable illustrates the great difference in estimates between the simpler pooled regression and the CRE model.

In the case of the credit source variables (Figure 17), the evolution of estimates for banks starts with negative estimates at the bottom of the distribution and become positive at the upper half. This is consistent with the outcomes observed in the first cross sectional analysis. The estimates are not well determined, though. Thus, no statistically significant impact is obtained. In the case of moneylenders, estimates are negative along the whole distribution but they are only significant at the top.

Figure 15 Year 2007 dummy. Income per capita.

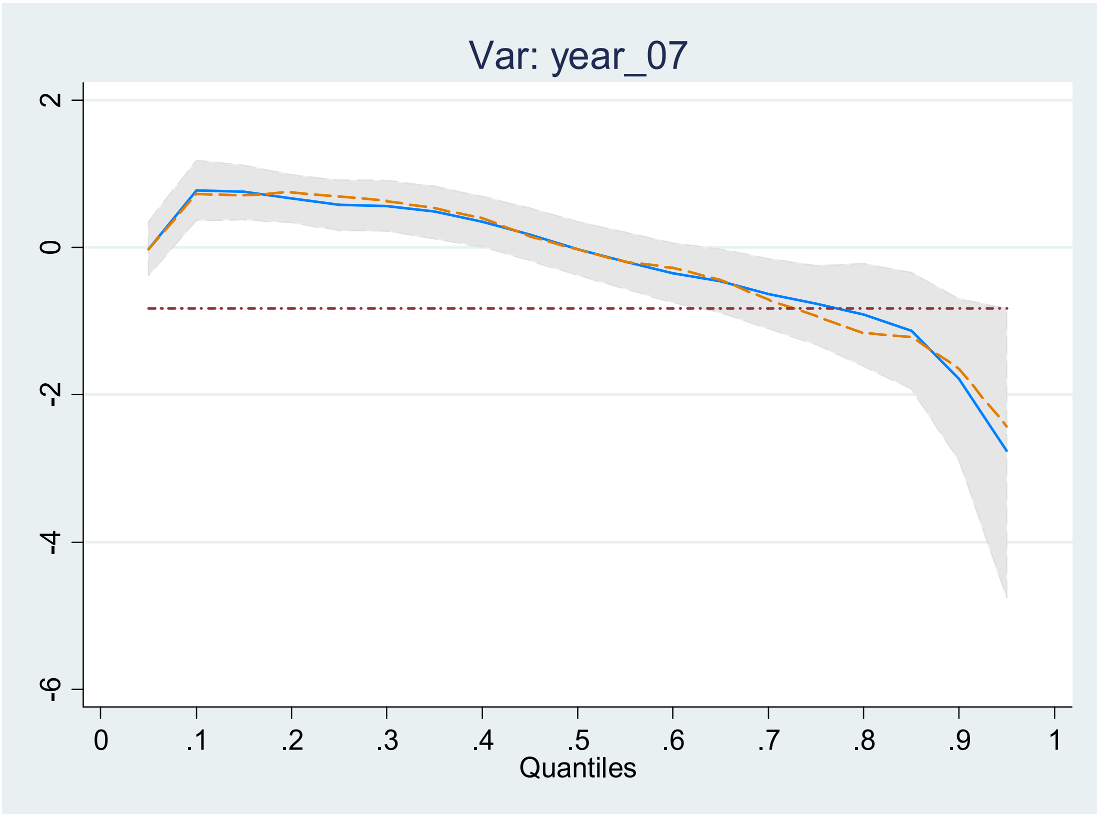


Figure 16 Household variables. Income per capita

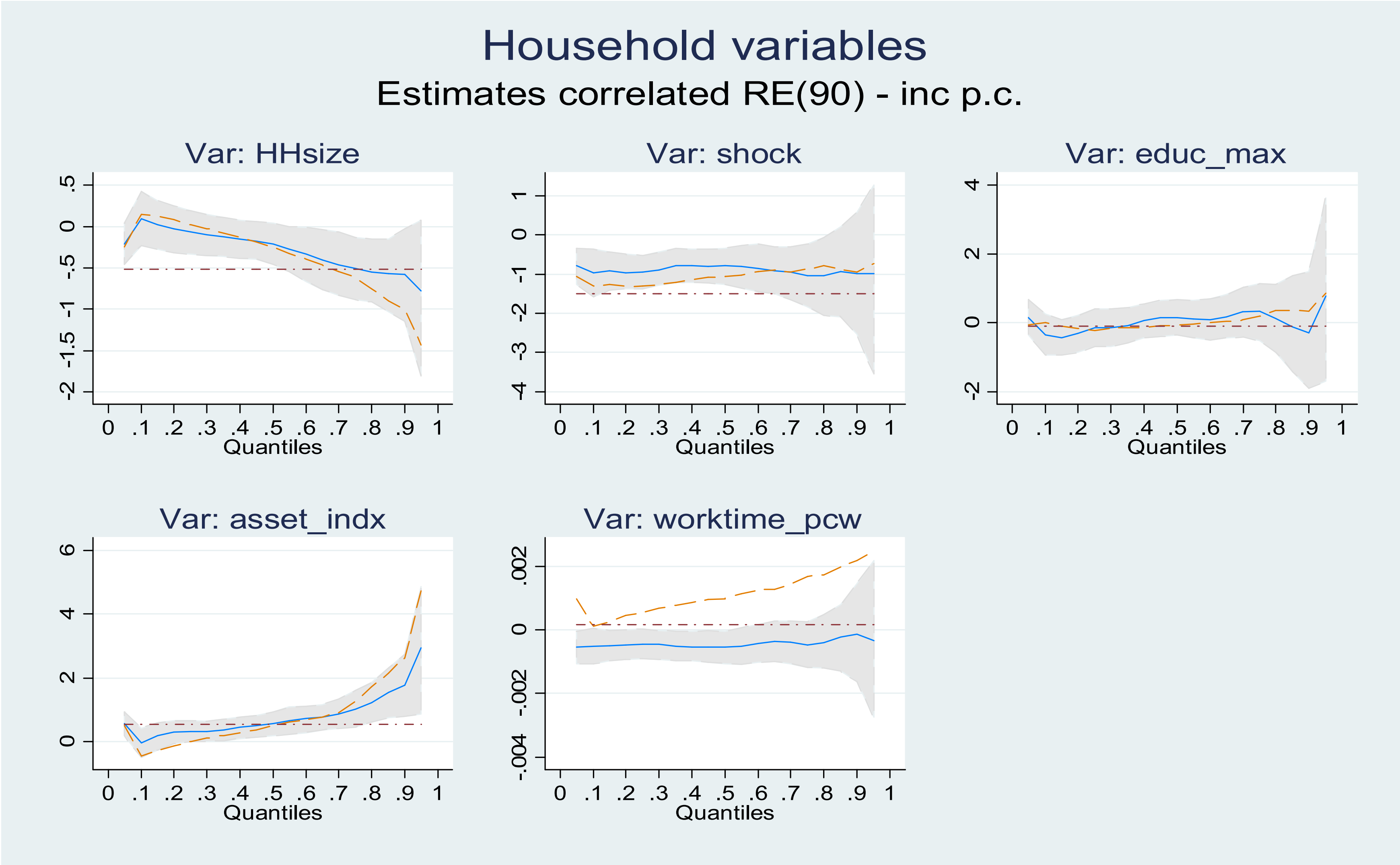
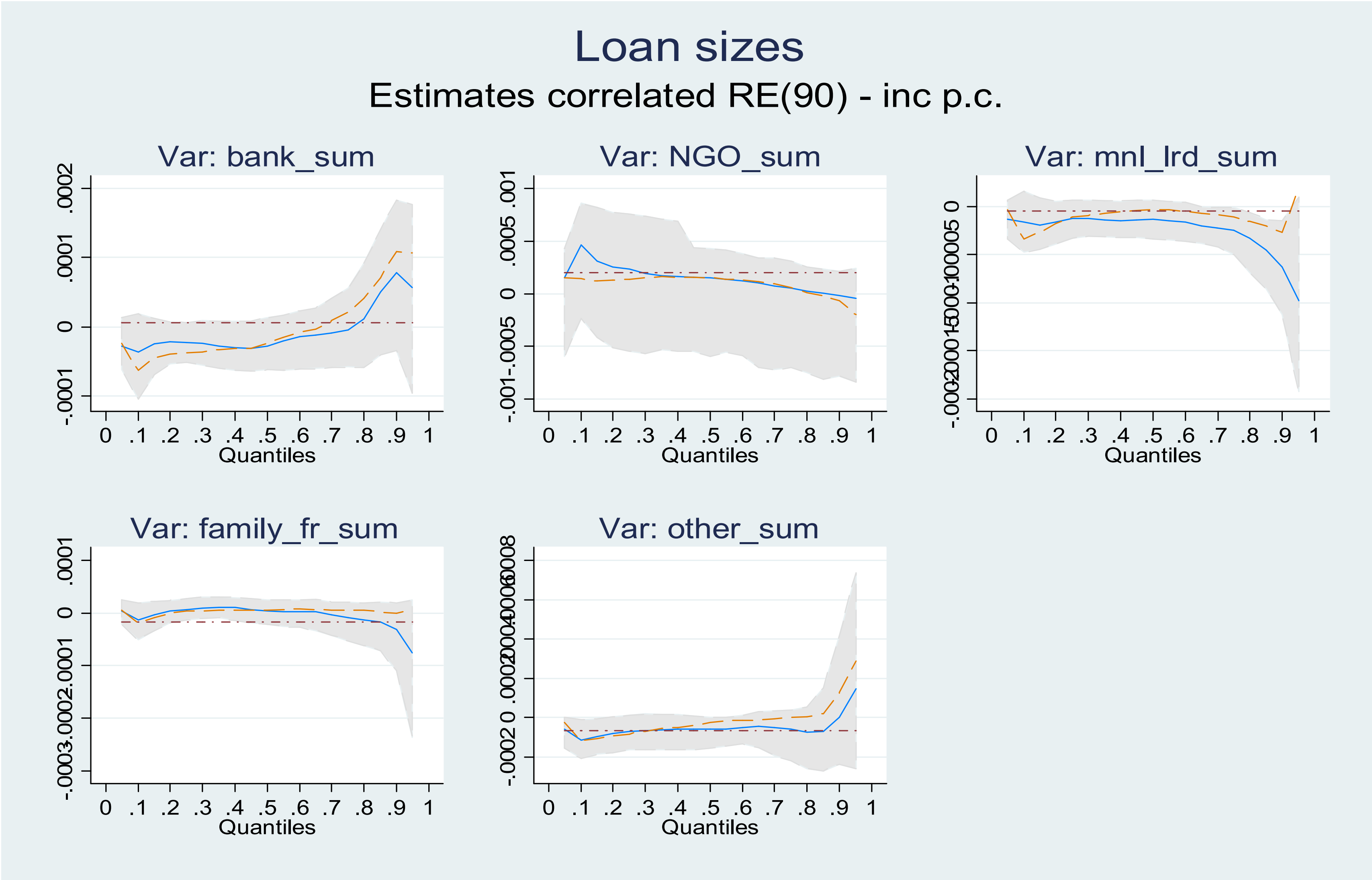


Figure 17 Loan size variables. Income per capita



CRE model estimates for income per capita and t values are found in Table 52. Regarding the SHG loan size, significant estimates range from the 40th percentile to the 85th with a slight rising trend as long as quantiles increase. This can be seen the positive slope in the graph (Figure 18). For each 100 rupees borrowed from SHG, the impact at the 50th percentile is an increase in annual income per capita of 6.9 rupees, ceteris paribus. This figure rises to 10.1 at the 90th percentile. This difference is quite noticeable as it represents roughly that the impact at the 90th percentile is around a 45% higher than at 50th percentile. Differences are to be tested for statistical significance below. With respect to income, graphs and table are included in the appendix and the pattern is similar: the impact is significant at the middle of the distribution. At the bottom or the top of the distribution this impact is not noticed.

Further tests are needed in order to assess whether the differences in estimates are statistically significant or not.

Figure 18 SHG loan sum. Income per capita

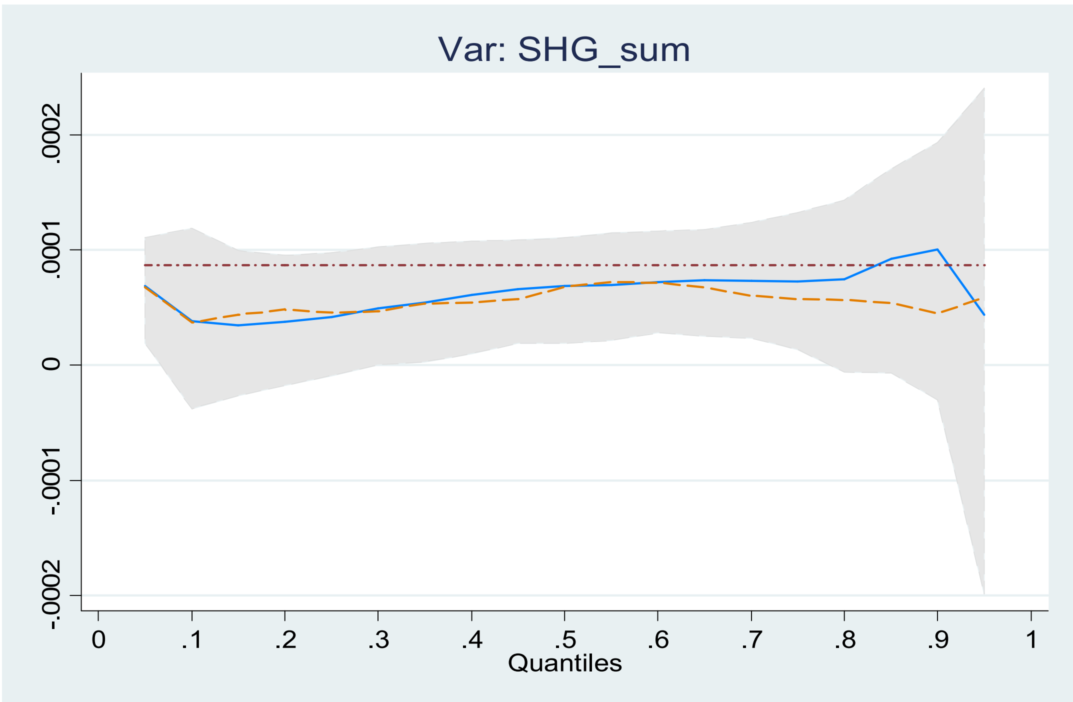


Table 52 CRE Income per capita

Correlated Random Effects Model Income PC																			
Quintiles	q5mn	q10mn	q15mn	q20mn	q25mn	q30mn	q35mn	q40mn	q45mn	q50mn	q55mn	q60mn	q65mn	q70mn	q75mn	q80mn	q85mn	q90mn	q95mn
year 2007	-21.60	777.40	760.41	666.10	582.21	563.69	486.50	348.02	177.38	-21.60	-189.06	-345.71	-455.81	-628.50	-758.13	-904.45	-1,132.51	-1,786.04	-2,756.53
	-0.10	3.11	3.47	3.43	2.85	2.78	2.22	1.66	0.85	-0.10	-0.82	-1.40	-1.74	-2.20	-2.43	-2.17	-2.35	-2.77	-2.28
HH size	-211.86	92.50	19.62	-27.51	-63.30	-98.37	-127.01	-155.81	-176.48	-211.86	-275.12	-334.48	-400.49	-465.31	-509.37	-549.07	-570.96	-582.03	-780.26
	-1.41	0.45	0.11	-0.16	-0.40	-0.66	-0.90	-1.16	-1.29	-1.41	-1.68	-1.72	-1.86	-2.01	-2.16	-2.42	-2.22	-1.72	-1.36
shock	-791.13	-959.61	-919.82	-961.54	-956.96	-893.12	-791.19	-777.69	-809.73	-791.13	-807.55	-864.64	-920.64	-954.16	-1,035.45	-1,034.97	-933.55	-987.46	-985.65
	-2.80	-2.63	-3.18	-3.75	-3.79	-3.55	-3.04	-2.95	-3.07	-2.80	-2.45	-2.42	-2.48	-2.28	-2.18	-1.74	-1.37	-1.04	-0.64
Max education	162.47	-351.29	-433.43	-299.88	-149.30	-134.35	-71.85	59.78	147.02	162.47	115.89	97.88	179.78	329.64	337.30	129.90	-112.44	-294.42	774.00
	0.52	-0.94	-1.39	-0.92	-0.45	-0.41	-0.23	0.20	0.47	0.52	0.35	0.27	0.47	0.74	0.68	0.22	-0.14	-0.28	0.49
Asset index	563.25	-42.90	178.54	304.30	326.92	319.97	366.76	445.92	497.48	563.25	654.41	719.57	764.46	871.99	1,030.44	1,220.76	1,531.46	1,778.25	2,935.70
	2.44	-0.15	0.69	1.41	1.66	1.64	1.80	2.15	2.32	2.44	2.55	2.87	3.14	3.06	3.03	3.16	3.32	3.01	2.37
worktime p_c_working	-0.5479	-0.5248	-0.4936	-0.4689	-0.4504	-0.4642	-0.5122	-0.533	-0.5487	-0.5479	-0.5274	-0.4242	-0.357	-0.3838	-0.4738	-0.4021	-0.2252	-0.1369	-0.3499
	-1.73	-1.50	-1.67	-1.66	-1.59	-1.65	-1.87	-1.90	-1.77	-1.73	-1.48	-1.12	-0.92	-0.95	-1.06	-0.75	-0.36	-0.14	-0.25
sum from bank	-0.0278	-0.0369	-0.0247	-0.0216	-0.0224	-0.0239	-0.0275	-0.0305	-0.0315	-0.0278	-0.0208	-0.0144	-0.0125	-0.00914	-0.00414	0.0115	0.0506	0.0778	0.0565
	-1.17	-1.03	-1.02	-1.13	-1.26	-1.23	-1.31	-1.41	-1.38	-1.17	-0.83	-0.55	-0.45	-0.29	-0.12	0.25	0.90	1.15	0.65
sum from NGO	0.1486	0.4637	0.313	0.2564	0.2323	0.1922	0.1722	0.1615	0.1547	0.1486	0.1358	0.126	0.1022	0.0766	0.0494	0.0217	0.00143	-0.0172	-0.0446
	0.49	1.14	0.82	0.73	0.69	0.61	0.56	0.53	0.53	0.49	0.45	0.41	0.32	0.24	0.15	0.06	0.00	-0.05	-0.11
sum moneylender	-0.0133	-0.0164	-0.0193	-0.0159	-0.0121	-0.012	-0.0136	-0.0145	-0.0139	-0.0133	-0.0141	-0.0163	-0.0195	-0.022	-0.0244	-0.0327	-0.0454	-0.0629	-0.0976
	-1.04	-0.88	-1.19	-1.18	-0.97	-1.03	-1.18	-1.27	-1.13	-1.04	-1.14	-1.33	-1.64	-1.77	-1.69	-1.74	-2.00	-2.02	-1.61
sum family friend	0.00309	-0.013	-0.00281	0.00348	0.00662	0.00961	0.0113	0.0101	0.00658	0.00309	0.00168	0.00241	0.00205	-0.00313	-0.00901	-0.0131	-0.0179	-0.0309	-0.0766
	0.21	-0.63	-0.16	0.27	0.54	0.77	0.91	0.77	0.46	0.21	0.11	0.14	0.11	-0.15	-0.39	-0.46	-0.55	-0.74	-0.92
sum others	-0.0574	-0.1124	-0.0947	-0.0793	-0.0694	-0.0653	-0.0628	-0.0587	-0.0572	-0.0574	-0.058	-0.0515	-0.0436	-0.0498	-0.06	-0.0712	-0.0697	0.0029	0.1474
	-1.14	-1.84	-1.72	-1.42	-1.27	-1.15	-1.05	-0.99	-1.05	-1.14	-1.21	-1.10	-0.78	-0.70	-0.68	-0.66	-0.52	0.01	0.45
sum SHGs	0.0687	0.0384	0.0347	0.0376	0.0418	0.0496	0.0542	0.0608	0.0662	0.0687	0.0698	0.0724	0.0736	0.0734	0.0728	0.0746	0.0926	0.1005	0.0439
	2.27	0.79	0.94	1.15	1.29	1.55	1.63	1.87	2.14	2.27	2.30	2.46	2.43	2.29	2.04	1.74	1.81	1.51	0.31
constant	3,759.72	-187.33	306.90	786.30	1,232.59	1,665.37	2,227.35	2,847.04	3,335.98	3,759.72	4,181.31	4,684.51	5,314.95	6,042.59	6,661.00	7,679.67	9,292.90	11,199.53	15,640.80
	6.72	-0.32	0.57	1.72	2.55	3.30	4.26	6.08	6.76	6.72	7.22	7.74	7.36	7.75	8.62	8.67	9.07	8.66	6.26

Nr. of observations: 2,907. In yellow, blue and orange estimates that are significant at least at 10% level for loan size from SHG, bank and moneylenders respectively.

The final tables in the appendix contain the coefficients for , λ_1, λ_2 for income per capita (Table 93 and Table 94 for 2005 and 2007 respectively) and income (Table 95 and Table 96). These measure how the unobserved heterogeneity shifts the estimates through their linear relation with the observables. In graphical terms, this can be explained as whether the gap between the dashed line (pooled cross sectional only estimates) and the solid line (CRE estimates) in the graphs is statistically significant. In both income and income per capita, the variables for which λ_1 and λ_2 show significance are shock, asset index and mostly work per working capita.

Given these results, two tests follow. The first test of the correlated random effects allows us to have some criterion in order to prefer CRE rather than the more common random effects model. If CRE is accepted, then the next step will be to test for statistical differences between the estimates of the SHG loan size in the models for income per capita an income.

Test of the correlated random effects and difference in estimates for SHG size

As stated in Abrevaya and Dahl (2008) the first test has to be formulated with a null hypothesis that all the coefficients for the correlated random effects are jointly and significantly equal to 0 or $\lambda_q^1 = \lambda_q^2 = 0$ for each of the quantiles q. Following this source, the test is run for the standard range of quantiles, 0.1 0.25 0.5 0.75 and 0.90. (See Table 53 below for both income and income per capita, tests of correlated random effects and test of equality across quantiles)

The test for CRE is implemented as a Wald test done over the variance-covariance matrix of quantile estimates obtained after the bootstrap method. The null is amply rejected when income and income per capita are the dependent variables, as can be seen in tables. Even in the case of the maximum p value the null can be rejected at 1.3% significance level. This outcome underpins the assumption that the unobservables are correlated with the observed variables x and, therefore, the correlated random effect approach is preferred over the traditional random effect model in which the unobservables are random and uncorrelated with the observables.

The second step would be to test the difference in the estimates of SHG loans between two different quantiles. The aim in this case is to establish whether the estimates of two different quantiles are statistically different or not. The outcomes are seen in table 24 and show a coherent pattern: the estimates of the variables are less likely to be statistically different as long as quantiles come closer. Tests are implemented again as Wald tests following the test suggested in Hao and Naimann, (2007) . They are done for all the pairs of percentiles 10, 25, 50, 75 and 90. In both income and income per capita the estimates are significantly different when the quantiles are more distant. Differences that were found significant were between quantiles 10-90, 10-75, 25-90, 25-75, 10-50 and 50-90, although the two latter pairwise combinations only at 10% significance level. The closer pairs 10-25, 25-50 and 75-90 do not show statistically significant differences.

Table 53 CRE test and equality test

Income		
Correlated Random Effects test		
Null: CRE coefficients jointly equal to 0		
Quantiles	Wald value	p-value
Q_10	245.184	0.000
Q_25	107.581	0.000
Q_50	519.849	0.000
Q_75	9,886.48	0.000
Q_90	55,918.27	0.000

Income per capita		
Correlated Random Effects test		
Null: CRE coefficients jointly equal to 0		
Quantiles	Wald value	p-value
Q_10	101.373	0.000
Q_25	200.397	0.000
Q_50	106.241	0.000
Q_75	71.88	0.000
Q_90	44.61	0.013

Quantile equality tests		
Null: Quantiles are equal		
Quantiles	Wald	Pvalue
test_1090	55.336	0.000
test_1075	49.791	0.000
test_1050	21.758	0.084
test_1025	5.242	0.982
test_2590	38.145	0.000
test_2575	34.849	0.002
test_2550	16.558	0.281
test_5090	22.331	0.072
test_5075	14.031	0.447
test_7590	9.331	0.809

Quantile equality tests		
Null: Quantiles are equal		
Quantiles	Wald	Pvalue
test_1090	32.005	0.004
test_1075	34.536	0.002
test_1050	21.282	0.095
test_1025	8.498	0.862
test_2590	34.302	0.002
test_2575	25.987	0.026
test_2550	11.023	0.684
test_5090	26.101	0.025
test_5075	13.625	0.478
test_7590	17.798	0.216

Outcomes withdrawing the negative income per capita observations

The presence of negative income per capita values might cast some doubts over the robustness of these outcomes to the withdrawal of these observations. As in previous chapters, the approach is to drop those households with negative income per capita and rerun all the models. These were the cross section quantile regression in years 2005 and 2007, quantile regression over the differenced variables (including or not additional variable levels in 2005) and, finally, the CRE approach. The latter is shown in Table 54.

The main conclusion is that the estimates are slightly higher and significant than the model with all households. Also, estimates at lower quantiles that did not show significance in Table 52 are now significant and the range of significant estimates comes from the 15^{th} percentile to the 80^{th}. For the rest of the models the outcomes are similar to those found with the whole set of households. The only difference is that in the regression over the differenced covariates including the 2005 level, no estimates are significant.

Table 54 CRE Income per capita. Negative income per capita values withdrawn

	CRE Income per Capita				
Source \ Percentiles	**0.1**	**0.25**	**0.5**	**0.75**	**0.9**
banks	-0.005 (-0.423)	-0.005 (-0.249)	0.017 (0.892)	0.066 (1.446)	0.1222* (1.818)
NGO	0.099 (0.332)	0.111 (0.308)	0.133 (0.402)	0.094 (0.252)	0.124 (0.324)
Moneylender	0.001 (0.118)	-0.010 (-0.927)	-0.012 (-0.967)	-0.025 (-1.445)	-0.042 (-1.166)
Family & Friends	0.0203* (1.720)	0.0243** (2.490)	0.011 (0.740)	-0.006 (-0.232)	-0.023 (-0.503)
Other	-0.004 (-0.213)	-0.004 (-0.158)	-0.013 (-0.414)	0.034 (0.472)	0.183 (0.682)
SHG	0.040 (1.330)	0.0599* (1.821)	0.0757** (2.549)	0.0805* (1.727)	0.098 (1.149)

t statistics in parenthesis; * p<0.10; ** p < .05; *** p < .01; Nr. of observations: 1,138

This outcome reinforces the positive impact for households in the middle of the income per capita distribution, and this would be the main conclusion. It also suggests that poorer households might be benefitting from microfinance. However, the negative per capita income observations cannot be overlooked and therefore further research is needed with other surveys in order to confirm this effect at the bottom of the distribution.

Estimates for the rest of the models when the negative income or income per capita values have been dropped can be found in the appendix, from Table 97 to Table 101.

Conclusion

The question of whether microfinance has a significant effect on the poor is quite present in microfinance impact evaluation literature. However we felt that the methodologies used so far, such as running the models for two different groups of borrowers (Coleman, 2006) or splitting the sample in two (Copestake et al. 2005) were not the best approaches. Instead, we propose to use quantile regression which estimates the impact at different points of the conditional distribution of the dependent variable. The technique is applied in the chapter in three different ways. The latter, CRE, estimates the impact taking advantage of the panel data condition of the dataset.

The overall outcome after the three approaches is that SHG borrowing shows a significant impact that takes place mostly at the middle of the income and income per capita distributions and this outcome is especially robust in the latter. Banks also show a quite consistent pattern of negative signs at the bottom and positive at the top of the distributions but significance is not always present. The rest of the credit sources do not show consistency in their estimates.

The difference between quantile regression and OLS is clearly seen in the graphs where OLS estimates show a flat line as takes the same value for the whole distribution. In fact the effect differs from quantile to quantile and these differences were found significant between more distant quantiles in the CRE model. This model complements the weaknesses of the previous two quantile approaches and comes to confirm the heterogeneity in the impact of microfinance.

Although the sample is taken from a poorer than average area, neither the poorest nor the richest in the sample are affected by the impact of microfinance. However the poorest should be the target of these programs and therefore this poses a question from a policy point of view, mainly if the poorest really need credit. This is quite relevant as other interventions more focused on the poorest could be financed with the funds that today support microfinance schemes.

CONCLUSION

The present work studies two datasets from Bangladesh and India and follows different quasi-experimental techniques to establish the impact of microfinance on several variables at household level. In the first two empirical chapters we follow mainstream microfinance impact evaluation literature. The third uses again the India dataset and uses an innovative approach to find whether the poor are benefitting from microfinance. Overall each of these chapters corresponds to the three different groups of studies into which quasi-experimental studies are split.

The first study revisits the Bangladesh survey in its second round. It contributes to literature in different ways. It is the first that takes into account the around 1,000 new households that were not surveyed in the first round and therefore are not part of the panel. The propensity score technique uses a wider range of matching algorithms and matching quality parameters than in Chemin (2008) and Duvendack and Palmer-Jones (2011). We also test the impact for other expenditures that are not always observed in impact evaluation studies and use different treatment-control group specifications.

In the second we contribute to literature analysing a dataset originally used for evaluating the impact of watershed programs. We took advantage of its rich microfinance information and we used a panel fixed effects approach for the analysis. We also include additional impact evaluation with an IV approach and MM estimator.

In the final empirical chapter we depart from mainstream microfinance evaluation literature and use a quantile regression to answer the question of whether microfinance has an effect on the poorest. The question has been raised in the past but we think that the methodologies used so far to answer it were not adequate. Our contribution is to use quantile regression and in particular the panel correlated random effects model adapted to quantile regression. This is methodologically a more consistent approach to solve this question.

In the first chapter we discussed the controversy between “randomistas” and academics that are more sceptical about the excellence of RCTs vs. quasi-experimental approaches.

From the review of the RCTs studies and other quasi-experimental sources in the particular field of microfinance we conclude that RCTs studies sometimes do not study exactly the impact of borrowing but the impact of having a saving account or receiving a grant (De Mel, McKenzie et al. 2008; De Mel, McKenzie et al. 2009; Dupas and Robinson 2009). In other occasions the randomization in the treatment and control group was imperfect and therefore some of the assumptions of RCTs needed to be reset to adapt to these departures from ideal randomization (Karlan and Zinman 2007; Banerjee, Duflo et al. 2009; Karlan and Zinman 2009).

So far the main sources have been quasi-experimental studies done on datasets obtained with surveys whose quality on many occasions was clearly improvable. In particular the Bangladesh survey that served as base for Pitt and Khandker (1998) and Khandker (2005) lacks good documentation and difficulties to reconstruct the data has been appointed in Chemin (2008) and Duvendack and Palmer-Jones (2011). And these flaws in the collection of data also cast some doubt on some of the outcomes reported so far.

We conclude that RCTs and quasi-experimental studies can complement each other in the future and from this debate about the pros and cons and empirical applications of both will enrich the impact literature and will contribute to the improvement in the assessments of the effects of microfinance programs. This will need to be accompanied by a leap in the quality of the surveys as, as contended in Copestake et al. (2011), sophisticated econometric techniques cannot always compensate the lack of adequate datasets.

In the first empirical chapter we revisit a common place in microfinance impact evaluation, the Bangladesh survey, in this case in its second round. The main study including this part is Khandker (2005) which finds a quite remarkable impact at individual level mainly on expenditures. Our aim was to tackle the evaluation impact using a technique that suits the characteristics of the dataset. We study the impact of borrowing from microfinance but also from all sources of credit, and we do not find any significance in the impact on current expenditures or food expenditures per capita at household level. But microfinance is seen to have a great and robust impact on extraordinary expenses and in particular in the expenses per capita on home repairs or extensions and investments of homes or land. These outcomes show quite stable in the presence of confounders that mimic the effect of unobservables. Income per capita outcomes were probably less reliable due to the difficulties in the construction of the variable. Estimates show overall negative impact but significance depended on the configuration of treatment and control groups.

The aim of the second paper was to replicate the approach in Tedeschi (2008) to our dataset from Andhra Pradesh. She borrowed from Coleman's model and applied it to a panel from Peru, concluding that the previous AIMS study on this same data was overstating the impact as it was not controlling properly for selection bias. The methods used was a panel fixed effects. The outcome shows a positive and significant impact effect of microfinance on income per capita. The presence of outliers and the possibility of endogeneity led us to complement the study with two further techniques. First, a MM method was used that is less sensitive to extreme values. The impact remains positive although to a lesser extent. Second, an IV approach tests for endogeneity but we fail to reject the null of exogeneity and therefore endogeneity is discarded.

But in these two approaches the impact is necessarily given by an estimate for the whole sample, a single figure that assumes that all the households in the sample are affected (or not affected) to the same extent by the microfinance intervention. We discuss that many sources have pointed out the fact that the ones taking advantage from microfinance were not the poorer households. On the contrary, better off households were benefitting from this instrument that is supposed to contribute to poverty alleviation and should target the poorest.

Thus, it was of great interest to complement the former analysis with a distributional study of impact effects of microfinance. This was done through a quantile regression that has been scarcely used in microfinance evaluation. The technique was run in three different ways, the latter of which followed the adaptation of the CRE model to quantile regression (Abrevaya and Dahl 2008). This allows us to take advantage of the panel characteristics of the dataset. The outcomes confirm that microfinance is not having an effect at the bottom of the income and income per capita distributions, where this impact is most needed.

We have to agree with Copestake et al. (2011) with respect to the difficulties when trying to get information out of the datasets. In the first case the Bangladesh data needed much work in the reconstruction of some variables that were poorly documented and had to be identified in the raw datasets. In the case of India, the dataset was by far better documented but had some flaws in the design as, for instance, at village level there are some questions in the second survey that were not formulated in the same way as they were in the first. Thus, this information cannot be used for the analysis.

In the debate RCTs vs. quasi-experimental techniques, it would be of extraordinary help to have a proper randomized study and use it as the benchmark to measure if quasi-experimental methods match its outcomes. This is what happened in the case of LaLonde (1986) in the field of labour economics. Also, to challenge works from the past using different approaches can be a great means of improving the evaluations.

With respect to microfinance in particular, the overall conclusion is that we find a positive and significant microfinance impact although not for all households According to our results, the impact is not noticed by the poorest which would be the main target for these microfinance projects. The story in the Bangladesh chapter comes to confirm this outcome. The impact is not significant in current expenditures or food expenditures. The poor are less likely to do the kind of "expenditures" where microfinance was found to have an effect, such as home extensions or investments in home/land.

This also poses a question about the fungibility of loans and how they are diverted to different ends than those applied for. In addition, these investments in houses/lands are supposed to have an effect on expenditures and income variables in the future, although this is out of the reach of these surveys. There has not been done yet a panel dataset covering a greater number of years in order to study what is the evolution of borrowing households in the mid term.

It is also important to investigate the household mechanisms that make it possible that microfinance has a positive impact effect (Orso 2011). A great help for this would be to have insider information such as that is contained in Collins et al. (2009). In it they studied many financial diaries that were given to poor households in India, Bangladesh and South Africa. The members were trained in how to use the diary and it was checked regularly. The book is an invaluable source of information and shows how people that live with $2-a-day are economically active. As long as their income is quite volatile they need to turn to several means to smooth consumption. Although they use informal sources frequently, from the diaries it can be inferred that they greatly value microfinance services. They consider MFIs as reliable institutions where, for instance, they can deposit their money and be sure that it will be there ready for withdrawal.

These kind of sources cannot support a statistically robust empirical study but can help researchers to understand, for example, whether the role of females at home change when they have access to loans and if this has an effect on children's education. Or they can suggest the collection of some particular information in the surveys that would have gone unnoticed without this inside view. Finally, understanding how the poor manage their meagre cash flows can help to test for impact on those variables that are important for the poor. In some studies a great number of variables are studied in an attempt to assess any possible impact effect of microfinance. This qualitative information will help to narrow down these variables.

Evidence of microfinance impact is so far mixed. Examples of failures to help the poor are in Andhra Pradesh state itself where there was a microfinance crisis and thousands of households went overindebted and could not cope with their payments. This raises the question of whether it makes sense to subsidize something that might make the poor worse off. Although with respect to this crisis not all the microfinance experts would agree.

We think that microfinance has a great potential to help the poor but whether it can help the poorest of the poor is yet to be seen. Evidence is not yet consistent enough to strongly argue in favour or against this point. In fact, sometimes the microfinance debate seems to be marked more by pre-established ideological position rather than by evidence. Assessing correctly the impact of microfinance is not a question of picking the sources or examples that best match prejudices in favour or against microfinance but a scientific task in which serious qualitative and quantitative research methods have to be combined to provide an accurate answer. It would not be sensible to waste efforts in empty debates when there are so many people needing a correct answer.

Bibliography

3ie, I. I. I. E. *Principles for Impact Evaluation* [Online]. Available: http://www.3ieimpact.org/doc/principles%20for%20impact%20evaluation.pdf [Accessed 13/10/2011].

Abadie, A. & Imbens, G. W. 2006. Large Sample Properties of Matching Estimators for Average Treatment Effects. *Econometrica,* 74**,** 235-267.

Abadie, A. & Imbens, G. W. 2008. On the Failure of the Bootstrap for Matching Estimators. *Econometrica,* 76**,** 1537-1557.

Abadie, A. & Imbens, G. W. 2011. Matching on the Estimated Propensity Score. *http://economics.stanford.edu/files/abadie5_25.pdf date: 25/07/2011*

Abrevaya, J. 2001. The Effects of Demographics and Maternal Behaviour on the Distribution of Birth Outcomes. *Empirical Economics*, 26, 247-257.

Abrevaya, J. & Dahl, C. M. 2008. The effects of birth inputs on birthweight: Evidence from quantile estimation on panel data. *Journal of Business and Economic Statistics,* 26**,** 379-97.

Angrist, J. D. & Pischke, J.-S. 2009. *Mostly Harmless Econometrics: An Empiricist's Companion,* Princeton, New Jersey, Princeton University Press.

Armendariz de Aghion, B. & Morduch, J. 2005. Chapter 8: Measuring Impact. *The economics of Microfinance.* Cambridge, Mass.: MIT Press.

Armendariz de Aghion, B. & Morduch, J. 2010. *The economics of microfinance (2nd ed.),* Cambridge, Mass., MIT press.

Athey, S. and G. W. Imbens (2006). "Identification and Inference in Nonlinear Difference-in-Differences Models." *Econometrica,* 74(2): 431-497.

Baker, J. L. 2000. *Evaluating the impact of development projects on poverty : a handbook for practitioners,* Washington, D.C., World Bank.

Banerjee, A., Duflo, E., Glennerster, R. & Kinnan, C. 2009. The Miracle of Microfinance? Evidence from a random evaluation. *(Working paper).* Massachusetts Institute of Technology.

Barnes, C., Keogh, E. & Nemarundwe, N. 2001. Microfinance clients and impact: An assessment of Zambuko Trust, Zimbabwe. *In:* Usaid (ed.) *USAID AIMS Report, October.*

Becker, S. O. & Caliendo, M. 2007. Sensitivity analysis for average treatment effects. *Stata Journal,* 7**,** 71-83.

Becker, S. O. & Ichino, A. 2002. Estimation of average treatment effects based on propensity scores. *Stata Journal,* 2**,** 358-377.

Bitler, M. P., Gelbach, J. B. & Hoynes, H. W. 2006. What Mean Impacts Miss: Distributional Effects of Welfare Reform Experiments. *The American economic review*, 96(4): 988-1012.

Bitler, M. P., Gelbach, J. B. & Hoynes, H. W 2008. "Distributional impacts of the Self-Sufficiency Project." *Journal of Public Economics* 92(3-4): 748-765.

Blundell, R. & Costa Dias, M. 2000. Evaluation methods for non-experimental data. *Fiscal Studies,* 21**,** 427.

Blundell, R., Dearden, L. & Sianesi, B. 2005. Evaluating the effect of education on earnings: models, methods and results from the National Child Development Survey. *Journal of the Royal Statistical Society: Series A (Statistics in Society),* 168**,** 473-512.

Bruhn, M. & Love, I. 2009. The Economic Impact of Banking the Unbanked: Evidence from Mexico. *Policy Research Working Paper 4981 The World Bank.*

Bryson, A., Dorsett, R. & Purdon, S. 2002. The use of propensity score matching in the evaluation of labour market policies. Working paper nr. 4. *Department of work and pensions.*

Buchinsky, M. 1998. Recent Advances in Quantile Regression Models: A practical Guide for Empirical Research. *Journal of Human Resources,* 33**,** 88-126.

Burgess, R. & Pande, R. 2005. Do rural banks matter? Evidence from the Indian social banking experiment. *American Economic Review,* 95**,** 780-795.

Calderón, T. 2006. Micro-bubble or Macro-immunity? Risk and Return in Microfinance: Lessons from Recent Crises in Latin America. *Microfinance Investment Funds.*

Caliendo, M. 2006. *Microeconometric Evaluation Labour Market Policies. Lecture Notes in Economics and Mathematical Systems No. 568,* Berlin, Springer-Verlag.

Caliendo, M. & Kopeinig, S. 2008. Some practical guidance for the implementation of propensity score matching. *Journal of Economic Surveys,* 22**,** 31.

Cameron, A. C. & Trivedi, P. K. 2005. *Microeconometrics: Methods and Applications*, Cambridge University Press.

Chamberlain, G. 1982. Multivariate regression models for panel data. *Journal of Econometrics,* 18**,** 5-46.

Chamberlain, G. 1984. Chapter 22 Panel data. *Handbook of Econometrics.*

Chamberlain, G. 1994. Quantile Regression, Censoring and the Structure of Wages. *Proceedings of the Sixth World Congress of the Econometrics Society, Barcelona*. C. Sims and J. J. Lafford. New York, Cambridge University Press pp 171-209.

Chemin, M. 2008. The Benefits and Costs of Microfinance: Evidence from Bangladesh. *Journal of Development Studies,* 44**,** 463 - 484.

Chen, M. & Snodgrass, D. 2001. Managing resources, Activities and Risk in Urban India: The Impact of SEWA Bank. *The AIMS Core Impact Assessments.* United States Agency for International Development (USAID).

Coleman, B. E. 1999. The impact of group lending in Northeast Thailand. *Journal of Development Economics,* 60**,** 105.

Coleman, B. E. 2002. *Microfinance in Northeast Thailand: Who Benefits and How Much?* [Online]. Asian Development Bank. Available: http://www.adb.org/Documents/ERD/Working_Papers/wp009.pdf [Accessed 20/10 2011].

Coleman, B. E. 2006. Microfinance in Northeast Thailand: Who Benefits and How Much? *World Development,* 34**,** 1612.

Collins, D., Morduch, J., Rutherford, S. & Ruthven, O. 2009. *Portfolios of the poor. How the world's poor live on $2 a day,* Princeton, Princeton University Press.

Copestake, J., Bhalotra, S. & Johnson, S. 2001. Assessing the impact of microcredit: A Zambian case study. *The Journal of Development Studies,* 37**,** 81.

Copestake, J., Dawson, P., Fanning, J. P., McKay, A. & Wright-Revolledo, K. 2005. Monitoring the Diversity of the Poverty Outreach and Impact of Microfinance: A Comparison of Methods Using Data from Peru. *Development Policy Review,* 23**,** 703.

Copestake, J., Goldberg, N. & Karlan, D. 2009. Crossfire: Randomized control trials are the best way to measure impact of microfinance programmes and improve microfinance product designs. *Enterprise Development and Microfinance,* 20**,** 167-176.

Copestake, J. G., Palmer-Jones, R., Duvendack, M., Hooper, L., Loke, Y. & Rao, N. 2011. What is the evidence of microfinance on the well-being of poor people?

London: EPPI-Centre, Social Research Unit, Institute of Education. University of London.

Dammert, A. C. 2009. "Heterogeneous Impacts of Conditional Cash Transfers: Evidence from Nicaragua." *Economic Development and Cultural Change,* 58(1): 53-83.

De Mel, S., McKenzie, D. & Woodruff, C. 2008. Returns to capital in microenterprises: Evidence from a field experiment. *Quarterly Journal of Economics,* 123**,** 1329-1372.

De Mel, S., McKenzie, D. & Woodruff, C. 2009. Are women more credit constrained ? Experimental evidence on gender and microenterprise returns. *American Economic Journal: Applied Economics,* 1**,** 1-32.

Deaton, A. 2010. Instruments, Randomization, and Learning about Development. *Journal of Economic Literature,* 48**,** 424.

Dehejia, R. & Wahba, S. 1999. Causal Effects in Non-Experimental Studies: Re-Evaluating the Evaluation of Training Programs. *Journal of the American Statistical Association,* 94**,** 1053-1062.

Dehejia, R. H. & Wahba, S. 2002. Propensity Score-Matching Methods for Nonexperimental Causal Studies. *The Review of Economics and Statistics,* 84**,** 151-161.

Djebbari, H. and J. Smith 2008. "Heterogeneous impacts in PROGRESA." *Journal of Econometrics* 145(1/2): 64.

Duflo, E., Glennerster, R. & Kremer, M. 2007. Chapter 61 Using Randomization in Development Economics Research: A Toolkit. *Handbook of Development Economics.*

Dunn, E. 2002. Research strategy for the AIMS core assessments. *AIMS working paper.*

Dunn, E. & Arbuckle, J. G. 2001. "The impacts of microcredit: A case study from Peru". *Assessing the Impact of Microenterprises Projects (AIMS) - USAID.* Washington.

Dupas, P. & Robinson, J. 2009. Savings Constraints and Microenterprise Development: Evidence from a Field Experiment in Kenya. *National Bureau of Economic Research Working Paper Series,* No. 14693.

Duvendack, M. & Palmer-Jones, R. 2011. High noon for microfinance impact evaluations: re-investigating the evidence from Bangladesh. Working Paper 27, DEV Working Paper Series, The School of International Development, University of East Anglia, UK.

Shahe Emran, M., Robano, Virginia and Smith, Stephen C., 2009 Assessing the Frontiers of Ultra-Poverty Reduction: Evidence from CFPR/TUP, An Innovative Program in Bangladesh. Available at SSRN: http://ssrn.com/abstract. *Accessed 10/05/2009*.

Firpo, S. 2007. "Efficient semiparametric estimation of quantile treatment effects." *Econometrica* 75(1): 259-276.

Fry, M. J. 1978. Money and capital or financial deepening in economic development? *Journal of Money, Credit and Banking,* 10**,** 464-75.

Gangl, M. 2007. RBOUNDS: Stata module to perform Rosenbaum sensititivity analysis for average treatment effects on the treated. *ssc downloaded 03/01/2010*.

Ghatak, M. 1999. Group lending, local information and peer selection. *Journal of Development Economics,* 60**,** 27.

Ghatak, M. 2000. Screening by the company you keep: Joint liability lending and the peer selection effect. *The Economic Journal,* 110**,** 601.

Goldberg, N. 2005. Measuring the Impact of Microfinance: Taking Stock of What We Know. *In:* USA, G. F. (ed.) *Grameen Foundation USA Publication Series.* Grameen Bank.

Guo, S. & Fraser, M. W. 2010. *Propensity Score Analysis,* Thousand Oaks, California, SAGE Publications.

Hahn, J., Todd, P. & Klaauw, W. V. d. 2001. Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design. *Econometrica,* 69**,** 201-209.

Hao, L. and D. Q. Naiman 2007. *Quantile regression*, SAGE Publications, Inc.

Harper, M. 2011. The Commercialization of Microfinance: Resolution or Extension of poverty? *In:* Bateman, M. (ed.) *Confronting Microfinance. Undermining Sustainable Development.* Kumarian Press.

Heckman, J., Ichimura, H., Smith, J. & Todd, P. 1996. Sources of Selection Bias in Evaluating Social Programs: An interpretation of Conventional Measures and Evidence on the Effectiveness of Matching as a Program Evaluation Method. *Proceedings of the National Academy of Sciences of the United States of America,* 93**,** 13416-13420.

Heckman, J. J., Ichimura, H. & Todd, P. E. 1997. Matching as an econometric evaluation estimator: Evidence from evaluating a job training programme. *The Review of Economic Studies,* 64**,** 605-654.

Heckman, J., Ichimura, H., Smith, J. & Todd, P. 1998. Characterizing Selection Bias Using Experimental Data. *Econometrica,* 66**,** 1017-1098.

Heckman, J. & Smith, J. 1995. Assessing the case for social experiments. *Journal of Economic Perspectives,* 9**,** 85-110.

Heckman, J., Smith, J. & Clements, N. 1997. Making the Most Out of Programme Evaluations and Social Experiments: Accounting for Heterogeneity in Program Impacts". *Review of Economic Studies***,** 487-535.

Heckman, J. & Todd, P. 2009. A Note on Adapting Propensity Score Matching and Selection Models to Choice Based Samples. *NBER Working Papers 15179.* National Bureau of Economic Research.

Hossain, M. 1988. Credit for the Alleviation of Rural Poverty: The Grameen Bank in Bangladesh. Washington, D.C.: IFPRI, Research Report No.65.

Hulme, D. 2000. Impact Assessment Methodologies for Microfinance: Theory, Experience and Better Practice. *World Development,* 28**,** 79-98.

Hulme, D. & Mosley, P. 1996. *Finance against poverty. Volume 2,* London, Routledge.

Hunter, B. H., S. Kennedy, et al. (2002). One size fits all? The effect of equivalence scales on Indigenous and other Australian poverty. CAEPR Working Paper No. 19/2002, The Australian National University.

Ichino, A., Mealli, F. & Nannicini, F. 2007. From Temporary Help Jobs to Permanent Employment: What Can We Learn from Matching Estimators and their Sensitivity? *Journal of American Academy of Business, Cambridge*.

Imbens, G. W. 2010. Better LATE than nothing: Some Comments on Deaton(2009) and Heckman and Urzua(2009). *Journal of Economic Literature,* 48**,** 299-423.

Kaboski, J. P. & Townsend, R. M. 2005. Policies and Impact: An Analysis of Village-Level Microfinance Institutions. *Journal of the European Economic Association,* 3**,** 1-50.

Kaboski, J. P. & Townsend, R. M. 2009 The Impacts of Credit on Village Economies. MIT Department of Economics Working Paper No. 09-13. Available at SSRN: http://ssrn.com/abstract=1395348 or http://dx.doi.org/10.2139/ssrn.1395348. Accessed 05/10/2008

Karlan, D. 2001. Microfinance impact assessments: the perils of using new members as a control group. *Journal of Microfinance,* 3**,** 75-85.

Karlan, D. & Goldberg, N. 2007. Impact Evaluation for Microfinance. *In:* Management, P. R. E. (ed.) *Doing Impact Evaluation no. 7*

Karlan, D. S. & Zinman, J. 2007. Expanding credit access: using randomised supply decisions to estimate the impacts. *CEPR Discussion Paper DP6407*

Karlan, D. S. and Zinman, J. 2009 Expanding Microenterprise Credit Access: Using Randomized Supply Decisions to Estimate the Impacts in Manila. Yale Economics Department Working Paper No. 68; Yale University Economic Growth Center Discussion Paper No. 976. Available at SSRN: http://ssrn.com/abstract=1444990 or http://dx.doi.org/10.2139/ssrn.1444990. . Accessed 05/07/2010

Kerr, J. 2002. "Watershed development, environmental services, and poverty alleviation in India." *World Development* 30(8): 1387-1400.

Khandker, S. R. 1998. *Fighting poverty with microcredit : experience in Bangladesh,* New York, Oxford University Press.

Khandker, S. R. 2005. Microfinance and poverty: Evidence using panel data from Bangladesh. *World Bank Economic Review,* 19**,** 263-286.

Khandker, S. R., Koolwal, GayatriÂ B. & Samad, H. A. 2010. *Handbook on Impact Evaluation,* Washington, The World Bank.

Khandker, ShahidurÂ R., Bakht, Z. & Koolwal, GayatriÂ B. 2009. The Poverty Impact of Rural Roads: Evidence from Bangladesh. *Economic Development and Cultural Change,* 57**,** 685-722.

Koenker, R. 2004. Quantile regression for longitudinal data. *Journal of Multivariate Analysis* 91(1): 74-89.

Koenker, R. 2005. *Quantile Regression*. New York, Cambridge University Press.

Koenker, R. & Bassett, G. J. 1978. Regression Quantiles. *Econometrica,* 46**,** 33-50.

Koenker, R. & Hallock, K. F. 2001. Quantile Regression. *Journal of Economic Perspectives,* 15**,** 143-156.

Kondo, T., Orbeta, A., Dingcong, C. & Infantado, C. 2008. Impact of Microfinance on Rural Households in the Philippines. *IDS Bulletin,* 39**,** 36-50.

Kotikula, A., Narayan, A. & Zaman, H. 2010. To What Extent Are Bangladesh's Recent Gains in Poverty Reduction Different from the Past? *World Bank Policy Research Working Paper #5199.*

LaLonde, R. J. 1986. Evaluating the Econometric Evaluations of Training Programs with Experimental Data. *The American economic review* 76(4): 604-620.

Lazarsfeld, P. F. 1959. Problems in methodology. *In:* R. K., M. L., Broom & L.S.,Cottrell Jr. (ed.) *Sociology today: Problems and Prospects.* New York: Basic Books.

Leuven, E. & Sianesi, B. 2003. PSMATCH2: Stata module to perform full Mahalanobis and propensity score matching, common support graphing, and covariate imbalance testing". *http://ideas.repec.org/c/boc/bocode/s432001.html* Version 4.0.4.

Madajewicz, M. 2003. Does the Credit Contract Matter? The Impact of Lending Programs on Poverty in Bangladesh. Working Paper, Columbia University

Manning, W., L. Blumberg, et al. (1995). "The Demand for Alcohol: The Differential Response to Price." Journal of Health Economics(14): 123-148.

Masset, E. and H. White (2006). Reducing Rural Poverty in India: An impact evaluation of rural investments in Andhra Pradesh. Design paper.

Meyer, B. D., W. K. Viscusi, et al. 1995. Workers' Compensation and Injury Duration: Evidence from a Natural Experiment. *The American economic review* 85(3): 322-340.

Morduch, J. 1998. Does microfinance really help the poor? New evidence on flagship programs in Bangladesh. *MacArthur Foundation project on inequality working paper.* Princeton University.

Morduch, J. 2000. The microfinance schism. *World Development,* 28**,** 617-629.

Mwabu, G. and T. P. Schultz 1996. Education Returns Across Quantiles of the Wage Function: Alternative Explanations for Returns to Education by Race in South Africa. *American Economic Review* 86(2): 335-339.

Nannicini, F. 2007. A Simulation-Based Sensitivity Analysis for Matching Estimators. *Stata Journal,* 7**,** 334-350.

Navajas, S., Conning, J. & Gonzalez Vega, C. 2003. Lending technologies, competition and consolidation in the market for microfinance in Bolivia. *Journal of International Development,* 15**,** 747-770.

Odell, K. 2010. Measuring the Impact of Microfinance: Taking another look. *Grameen Foundation.*

Orso, C. E. 2011. Microcredit and poverty: An overview of the principal statistical methods used to measure the program net impacts. *Polis Working Paper n. 180 February.*

Patten, R. H., Rosengard, J. k. & Johnston, J. R. D. E. 2001. Microfinance Success Amidst Macroeconomic Failure: The Experience of Bank Rakyat Indonesia During the East Asian Crisis. *World Development,* 29**,** 1057-1069.

Pitt, M. M. 1999. Reply to Jonathan Morduc's "Does Microfinance Really Help the Poor? New evidence from Flagship Programs in Bangladesh" Department of Economics, Brown University.

Pitt, M. M. & Khandker, S. R. 1998. The impact of group-based credit programs on poor households in Bangladesh: Does the gender of participants matter? *Journal of Political Economy,* 106**,** 958-996.

Poterba, J. M., S. F. Venti, et al. (1995). "Do 401(k) contributions crowd out other personal saving?" *Journal of Public Economics* 58(1): 1-32.

Reilly, B. 2010a. Instrumental Variables. Class notes Econometric Methods II. Course 2009-2010

Reilly, B. 2010b. Quantile regression. Class notes Econometric Methods II Course 2009-2010.

Rhyne, E. 2001. *Mainstreaming microfinance : how lending to the poor began, grew, and came of age in Bolivia,* Bloomfield, CT, Kumarian Press.

Rhyne, E. 2010. *A Tale of Microfinance in Two Cities* [Online]. The Huff Post. Available: http://www.huffingtonpost.com/elisabeth-rhyne/a-tale-of-microfinance-in_b_795840.html. Accessed 24/11/2011.

Robinson, M. S. 2002. *The microfinance revolution. Volume 2, Lessons from Indonesia,* Washington, D.C. World Bank. Open Society Institute.

Roodman, D. & Morduch, J. 2009. "The impact of Microcredit on the Poor in Bangladesh: revisiting the Evidence". *Working Paper No. 174.* Washington D.C.: Center for Global Development.

Rosenbaum, P. R. & Rubin, D. B. 1983a. Assessing Sensitivity to an Unobserved Binary Covariate in an Observational Study with Binary Outcome. *Journal of the Royal Statistical Society. Series B (Methodological),* 45**,** 212-218.

Rosenbaum, P. R. & Rubin, D. B. 1983b. The Central Role of the Propensity Score in Observational Studies for Causal Effects. *Biometrika,* 70**,** 41-55.

Rosenbaum, P. R. & Rubin, D. B. 1985. Constructing a Control Group Using Multivariate Matched Sampling Methods That Incorporate the Propensity Score. *The American Statistician,* 39**,** 33-38.

Rosenbaum, P. R. 1987. Sensitivity Analysis for Certain Permutation Inferences in Matched Observational Studies. *Biometrika,* 74**,** 13-26.

Rosenbaum, P. R. 2002. *Observational Studies,* New York, Springer.

Rubin, D. B. & Thomas, N. 1996. Matching using estimated propensity scores: relating theory to practice. *Biometrics* 52**,** 249-264.

Shadish, W. R., Cook, T. D. & Campbell, D. T. 2002. *Experimental and quasi-experimental designs for generalized causal inference,* Boston, Houghton Mifflin.

Shaefer, H. L. & Edin, K. 2012. Extreme Poverty in the United States: 1996 to 2011. *National Poverty Center Policy Brief, 28.*

Sharma, M. & Buchenrieder, G. 2002. Impact of Microfinance on Food Security and Poverty Alleviation: A Review and Synthesis of Empirical Evidence. *In:* Zeller, M. & Meyer, R. L. (eds.) *The Triangle of Microfinance.* Baltimore: The Johns Hopkins University Press.

Sianesi, B. 2004. An Evaluation of the Swedish System of Active Labor Market Programs in the 1990s. *The Review of Economics and Statistics,* 86**,** 133-155.

Smith, J. A. & Todd, P. E. 2005. Does matching overcome LaLonde's critique of nonexperimental estimators? *Journal of Econometrics,* 125**,** 305-353.

Song, J. and J. Manchester 2007. New Evidence on Earnings and Benefit Claims Following Changes in the Retirement Earnings Test in 2000. *Journal of Public Economics*, Vol. 91, 669-700.

Stewart, R., van Rooyen, C., Dickson, K., Majoro, M. & de Wet, S. 2010. What is the impact of Microfinance on poor people? A systematic review of evidence from Sub-Saharan Africa. London: EPPI-Centre. Social Science Research Unit. University of London.

Stiglitz, J. E. 1990. Peer Monitoring and Credit Markets. *The World Bank Economic Review,* 4**,** 351-366.

Stiglitz, J. E. & Weiss, A. 1981. Credit rationing in markets with imperfect information. *The American economic review,* 71**,** 393-410.

Tedeschi, G. A. 2008. Overcoming Selection Bias in Microcredit Impact Assessments: A Case Study in Peru. *Journal of Development Studies,* 44**,** 504 - 518.

Tedeschi, G. A. & Karlan, D. 2007. Microfinance Impact: Bias from Dropouts. *In:* Forthcoming (ed.).

Todaro, M. P. & Smith, S. C. 2009. *Economic Development,* Harlow, Pearson Education Limited.

Verardi, V. and C. Croux (2008). "Robust Regression in Stata." *Stata Journal* 9(3): 439-453.

White, H. 2007. Evaluating Aid Impact. United Nations University World Institute for Development Economics Research (UNU-WIDER). Research Paper 2007/75.

Wooldridge, J. M. 2002. *Econometric analysis of cross section and panel data,* Cambridge, Mass., MIT Press.

Worrall, J. 2007. Evidence in Medicine and Evidence-Based Medicine. *Philosophy Compass,* 2**,** 981-1022.

Yunus, M. 2007. *Creating a World Without Poverty,* New York, Public Affairs.

Appendix 1: Chapter 2

List of variables in the logit calculating the scores:

The variables are:

- Household characteristics/composition variables:
    - Household head age and age squared
    - Children 1-5: number of Children between 1-5 years old
    - Children 6-15: number of Children between 6-15 years old
    - H.head married: Binary stating whether the household head is married or not
    - H.head Islamic: Binary stating if the household head follows Islamic religion
    - No toilet: Binary that takes the value of 1 if the household has no toilet and 0 otherwise
    - Dist Pucca rd: Distance from the household to a Pucca road
    - Dist busns centr: Distance from household to the closest business centre.
- Village variables
    - Village average wage male for no agricultural job
    - Village average price of rice
    - Village average price of flour
    - Average 3-months interest rate for private loans in the village
    - Average 3-6 months interest rate for private loans in the village
    - Hat/Bazar village: Binary stating whether the village has a hat or bazaar in it.
    - FoodXEducation: Binary stating if there is a food for education program implemented in the village.
- Household education variables
    - HH highest education: what is the maximum number of education years completed by any member of the household
    - HH head primary: Binary stating if the household head has some primary education
    - HH head secondary: Binary stating if the household head has some secondary education

  - HH head secondary+: Binary stating if the household head has some higher-secondary or tertiary education (Omitted variable: No education)
- Household labor variables
  - Main s-emp agric: Binary stating if the household head's main activity is agriculture and he/she is self-employed
  - Main s-emp no-agric: Binary stating if the household head's main activity is not agriculture but he/she is self-employed
  - Main wage agric: Binary stating if the household head's main activity is agriculture and he/she is an employee.
  - Main wage no-agric: Binary stating if the household head's main activity is not agriculture and he/she is an employee. (Omitted variable: unemployed).
- Household borrowing variables:
  - Source bank: Total amount borrowed from a bank.
  - Source mlender: Total amount borrowed from a moneylender
  - Source relative: Total amount borrowed from a relative

Figure 19 Histogram Income per capita by MFI borrowing status

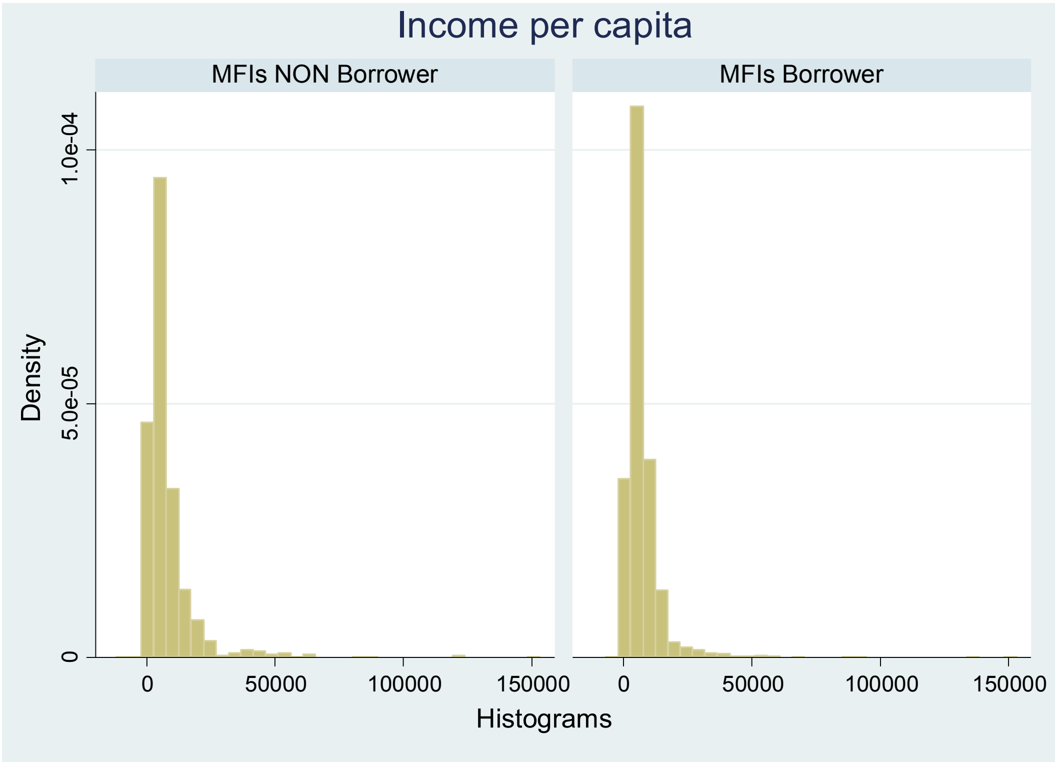


Figure 20 Histogram current expenditures per capita by MFI borrowing status

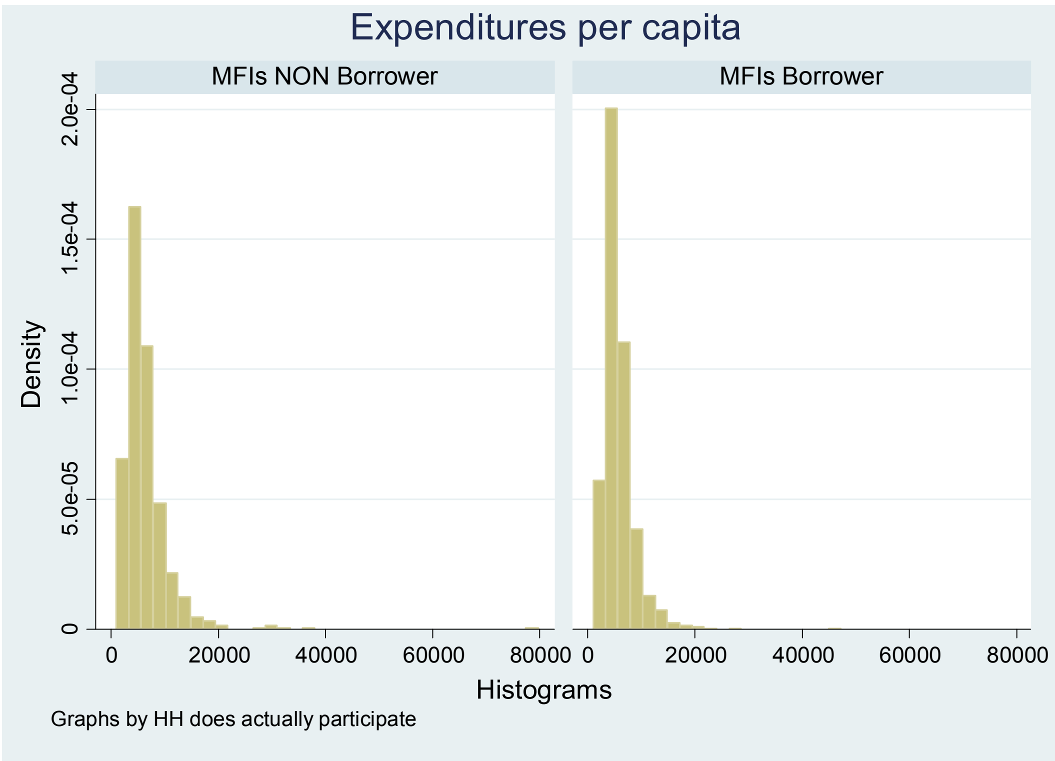


Figure 21 Common support Kernel graph

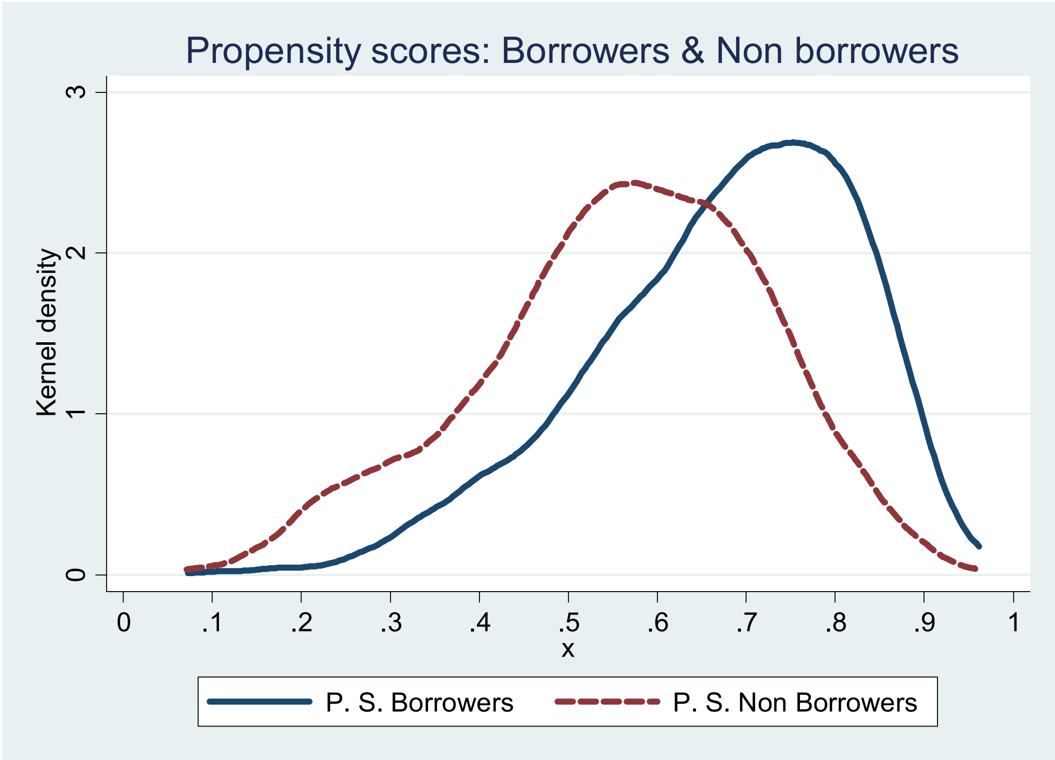
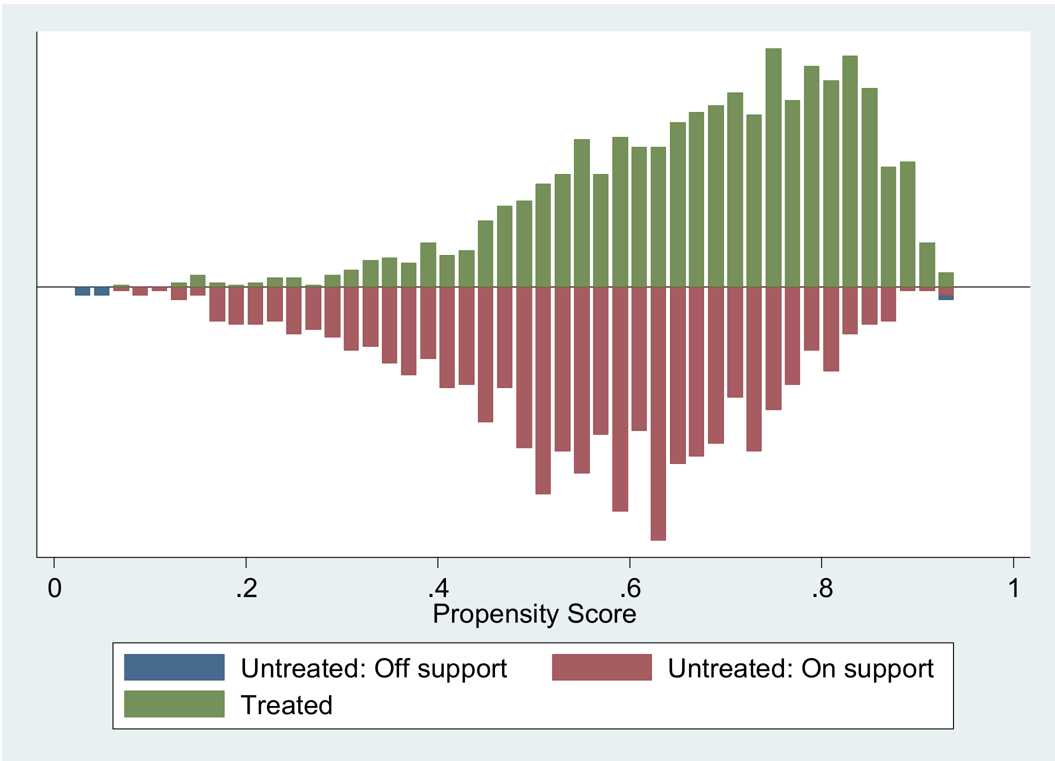


Figure 22 Common support histogram



Outcomes from the propensity score using pscore command

```
Note: the common support option has been selected

The region of common support is [.06616695, .935185]

Description of the estimated propensity score

in region of common support


                 Estimated propensity score

-------------------------------------------------------------

      Percentiles      Smallest

 1%     .1791818       .0661669

 5%     .3204578       .0721116

10%     .4030083       .0882637       Obs                2521

25%     .5241014       .0937959       Sum of Wgt.        2521


50%     .6524663                      Mean           .6322413

                        Largest       Std. Dev.      .1669881

75%     .7619108        .926154

90%     .8333885       .9311788       Variance        .027885

95%     .8637184       .9320907       Skewness      -.6093332

99%     .9006045        .935185       Kurtosis       2.962687

******************************************************

Step 1: Identification of the optimal number of blocks

Use option detail if you want more detailed output

******************************************************

The final number of blocks is 7

This number of blocks ensures that the mean propensity score

is not different for treated and controls in each blocks

**********************************************************

Step 2: Test of balancing property of the propensity score

Use option detail if you want more detailed output

**********************************************************

The balancing property is satisfied

This table shows the inferior bound, the number of treated

and the number of controls for each block
```

Inferior of block of pscore	HH does actually participate 0	1	Total
.0661669	50	18	68
.25	91	47	138
.375	161	143	304
.5	277	321	598
.625	237	457	694
.75	97	511	608
.875	6	87	93
Total	919	1,584	2,503

Note: the common support option has been selected

****

End of the algorithm to estimate the pscore

****

Table 55 Mean test before - after matching

	Variable	Match	Mean		%bias	%reduct bias	t-test	
			Treated	Control			t	p>|t|
Home	**Hhead age**	Before	45.3	45.1	1.2		0.29	0.768
		After	45.3	45.7	-2.8	-135	-0.85	0.393
	Hhead age^2	Before	2,206	2,260	-3.9		-0.97	0.334
		After	2,209	2,251	-3	22	-0.93	0.350
	Children 1-5	Before	0.73	0.79	-7		-1.74	0.083
		After	0.72	0.70	3	57.3	0.87	0.387
	Children 6-15	Before	1.44	1.19	21.2		5.21	0.000
		After	1.45	1.45	0.4	98	0.12	0.907
	Hhead married	Before	0.89	0.87	7		1.72	0.086
		After	0.89	0.89	-0.8	88.7	-0.23	0.815
	Hhead islam	Before	0.88	0.91	-10.2		-2.44	0.015
		After	0.88	0.88	-1.8	82.1	-0.48	0.629
	No toilet facility	Before	0.25	0.27	-2.9		-0.71	0.477
		After	0.25	0.24	2.8	2.8	0.81	0.415
	Distance to pucca road	Before	1.20	1.38	-12.2		-2.98	0.003
		After	1.21	1.30	-6.5	46.3	-1.94	0.053
	Distance to business centre	Before	3.55	3.81	-10.4		-2.57	0.010
		After	3.48	3.59	-4.2	59.5	-1.22	0.221
Village	**Av. wage male no-agri**	Before	72.49	75.50	-14.1		-3.45	0.001
		After	73.07	73.68	-2.8	79.8	-0.83	0.406
	Av. rice price	Before	15.29	15.01	12.1		2.92	0.004
		After	15.35	15.51	-6.8	43.9	-1.85	0.065
	Av. flour price	Before	12.74	12.80	-6.3		-1.58	0.115
		After	12.75	12.75	-0.5	92	-0.15	0.881
	Int. rate mlender 3 months	Before	26.23	27.34	-2.6		-0.64	0.525
		After	23.44	22.55	2.1	19.4	0.71	0.478
	Int. rate mlender 3-6 months	Before	23.35	24.12	-2.2		-0.53	0.597
		After	21.31	20.64	1.9	13.4	0.60	0.551
	Hat/Bazar in village	Before	0.43	0.33	20.8		5.04	0.000
		After	0.43	0.41	5	76	1.37	0.171
	FoodxEduc in village	Before	0.16	0.14	3.9		0.95	0.343
		After	0.16	0.15	1.9	51.7	0.53	0.597
education	**Hhead highest educ**	Before	1.25	1.49	-24.8		-6.25	0.000
		After	1.26	1.30	-4.1	83.5	-1.23	0.218
	Hhead primary educ	Before	0.25	0.26	-2.1		-0.51	0.613
		After	0.26	0.23	4.9	-137.7	1.42	0.156
	Hhead lower secondary educ	Before	0.12	0.18	-18.4		-4.58	0.000
		After	0.12	0.12	-1.3	92.7	-0.41	0.681
	HH head above secondary educ	Before	0.01	0.05	-19.6		-5.14	0.000
		After	0.02	0.02	-0.8	95.9	-0.32	0.750
Job HH head	**HHead self employed agric**	Before	0.19	0.25	-16.7		-4.11	0.000
		After	0.19	0.18	1.9	88.5	0.58	0.564
	HH head slf-employed no-agric	Before	0.40	0.27	28.9		6.93	0.000
		After	0.41	0.42	-2.4	91.7	-0.64	0.522
	HH head wge-employed agric	Before	0.14	0.16	-5.4		-1.33	0.183
		After	0.14	0.12	3.4	37.1	1.02	0.306
	HH head wge-employed non-agric	Before	0.13	0.08	14.4		3.41	0.001
		After	0.13	0.13	-2.1	85.4	-0.53	0.593
credit	**Bank/coop loan size**	Before	694	2,217	-10.7		-2.89	0.004
		After	690	760	-0.5	95.4	-0.29	0.775
	Informal loan size	Before	716	1,552	-6.7		-1.85	0.065
		After	726	710	0.1	98	0.09	0.927
	Relative loan size	Before	1,670	3,952	-17		-4.60	0.000
		After	1,705	1,777	-0.5	96.8	-0.26	0.799

Table 56 Alternative logit models for different treated / control groups

Variables	Models: Borrowers vs. non borrowers	Models: MFI borr vs. MFI noborr
Age of the hh head	0.117*** (5.40)	0.150*** (6.05)
Age of the hh head squared	-0.001*** (-5.05)	-0.001*** (-5.71)
Nr. of children under 6 years	0.035 (0.58)	-0.035 (-0.53)
Nr. of children between 6-15 (inclusive)years old	0.216*** (4.59)	0.171*** (3.33)
HH head Currently married	0.121 (0.78)	0.159 (0.92)
Religion of the hhead is islam	-0.498*** (-2.87)	-0.570*** (-3.13)
No toilet facility	-0.473*** (-3.97)	-0.392*** (-3.06)
Distance to Pucca Road	-0.052 (-1.40)	-0.066* (-1.65)
Distance to business centre	0.022 (0.96)	0.005 (0.20)
Village avg. wage male for no agric job	-0.002 (-0.98)	-0.006** (-2.40)
Village avg. Price rice	0.056** (2.47)	0.073*** (3.03)
Village avg. price flour	-0.236*** (-4.91)	-0.186*** (-3.60)
HH head highest educ	-0.070 (-1.00)	-0.189** (-2.48)
HH head primary educ	-0.013 (-0.10)	-0.115 (-0.84)
HH head lower secondary educ	-0.390** (-2.28)	-0.497*** (-2.61)
HH head above secondary educ	-1.012*** (-3.23)	-1.355*** (-3.47)
HH head self employed agric	0.461*** (2.99)	0.284 (1.63)
HH slf-employed no-agric	0.854*** (5.61)	0.892*** (5.31)
HH wge-employed agricultural	0.232 (1.29)	0.258 (1.30)
HH wge-employed non agric	0.878*** (4.37)	0.921*** (4.27)
Constant	0.478 (0.55)	-0.927 (-0.96)
Observations Adjusted R-squared	2504 0.0759	1878 0.0933
LR chi2(20) Prob > chi2	213.25 0.000	221.3 0.000

Table 57 Logit regression for the three different specifications of treated-control. Negative income per capita values dropped

	dy/dx: spec 1	dy/dx: spec 2	dy/dx: spec 3
Age of the hh head	0.024*** (5.56)	0.019*** (5.07)	0.027*** (5.60)
Age of the hh head squared	-0.000*** (-5.22)	-0.000*** (-4.69)	-0.000*** (-5.21)
Nr. of children under 6 years	-0.020* (-1.85)	0.007 (0.68)	-0.005 (-0.39)
Nr. of children between 6-15 (inclusive)years old	0.035***	0.038***	0.037***

	(4.08)	(4.79)	(3.71)
HH head Currently married	0.012 (0.40)	0.024 (0.90)	0.027 (0.79)
Religion of the hhead is islam	-0.088*** (-2.79)	-0.086*** (-2.96)	-0.116*** (-3.27)
Avge wage male labor in NON agriculture	-0.001*** (-3.07)	-0.000 (-0.56)	-0.001** (-2.05)
Average price of rice	0.015*** (3.48)	0.011*** (2.74)	0.014*** (2.93)
Average price of flour	-0.030*** (-3.24)	-0.041*** (-5.09)	-0.037*** (-3.66)
Int. rate priv. loan moneylender (up 3 months)	-0.000 (-0.51)	-0.001 (-1.30)	-0.001 (-0.73)
Int. rate priv. loan moneylender (3-6 months)	-0.000 (-0.06)	0.001 (1.21)	0.000 (0.18)
Is there any Hat/Bazar in this	0.068*** (3.56)	0.021 (1.17)	0.052** (2.39)
Is there any Food for education	-0.000 (-0.02)	-0.021 (-0.91)	0.015 (0.52)
HH highest level of education	-0.050*** (-3.86)	-0.014 (-1.17)	-0.041*** (-2.75)
HH head with primary education or equiv	-0.034 (-1.45)	0.003 (0.14)	-0.015 (-0.56)
HH head with lower secondary education or equiv	-0.084*** (-2.63)	-0.062** (-2.15)	-0.090** (-2.44)
HHead education above secondary	-0.186*** (-2.89)	-0.158*** (-3.02)	-0.246*** (-3.26)
Main job self employment in agriculture	0.026 (0.88)	0.080*** (3.08)	0.066* (1.95)
Main self-emp job of head is non agriculture	0.181*** (6.36)	0.144*** (5.69)	0.179*** (5.59)
Main job waged agricultural job	0.038 (1.08)	0.040 (1.30)	0.057 (1.47)
Main wage-emp job of head is non agriculture	0.172*** (4.66)	0.150*** (4.48)	0.189*** (4.58)
No toilet facility	-0.080*** (-3.51)	-0.082*** (-4.05)	-0.077*** (-3.06)
Distance to the nearest pucca road	-0.008 (-1.12)	-0.008 (-1.20)	-0.009 (-1.20)
Distance to the nearest business centre	-0.010** (-2.28)	0.004 (0.95)	-0.000 (-0.05)
Source of loan: bank or cooperative	-0.000* (-1.87)		
Source of loan: informal i.e. moneylender	0.000 (0.01)		
Source of loan: any relative	-0.000*** (-3.48)		
Observations **Pseudo R-squared**	2472 310.63	2472 214.98	1851 228.57
chi2 **p**	0.000 0.095	0.000 0.0779	0.000 0.098

Table 58 Matching quality in model Borrowers vs. non borrowers

	Matching quality model Borrowers vs. non Borrowers				
	Pseudo-R squared before	Pseudo-R squared after	P-value after	Median absolute bias before	Median absolute bias after
NN	0.0759	0.0113	0.000	8.20	2.63
Radius .01	0.0759	0.0114	0.000	8.20	2.67
Radius .001	0.0759	0.0091	0.001	8.20	3.61
Radius .0001	0.0759	0.0079	0.565	8.20	3.31
Kernel .02	0.0759	0.0032	0.653	8.20	2.26
Kernel .04	0.0759	0.0032	0.648	8.20	2.13
Kernel .06	0.0759	0.0036	0.506	8.20	2.42
Mahalanobis	0.0759	0.0131	0.000	8.20	2.89

Table 59 Matching quality MFI borrowers vs. non borrowers at all

	Matching quality model MFI borr vs. MFI noborrws				
	Pseudo-R squared before	Pseudo-R squared after	P-value after	Median absolute bias before	Median absolute bias after
NN	0.093	0.010	0.027	10.13	3.31
Radius .01	0.093	0.009	0.034	10.13	3.25
Radius .001	0.093	0.007	0.281	10.13	2.68
Radius .0001	0.093	0.023	0.055	10.13	3.56
Kernel .02	0.093	0.004	0.891	10.13	3.30
Kernel .04	0.093	0.003	0.937	10.13	3.29
Kernel .06	0.093	0.003	0.960	10.13	2.73
Mahalanobis	0.093	0.014	0.000	10.13	3.09

Table 60 Rest of the algorithms MFI borrowers vs. MFI non-borrowers

	PSM MFI borrowers vs. MFI non borrowers. Rest of algorithms				
	Algorithm	ATT	t-statistic	Lower bound C.I.	Upper bound C.I.
Income p/c	**NN**	-1,297	-1.87	-2,834	-105
	Radius .01	-1,296	-1.86	-2,832	-89
	Radius .001	-1,151	-1.53	-2,658	256
	Radius .0001	-1,267	-0.92	-4,094	1,043
	Kernel .04	-980	-1.76	-2,133	45
	Mahalanobis	-72	-0.15	-862	749
Current expenditures p/c	**NN**	-42	-0.17	-551	381
	Radius .01	-41	-0.17	-573	379
	Radius .001	4	0.01	-522	490
	Radius .0001	84	0.18	-1,009	843
	Kernel .04	-80	-0.41	-469	269
	Mahalanobis	211	1.24	-29	524

Food expenditures p/c	**NN**	168	1.50	-37	386
	Radius .01	169	1.56	-24	382
	Radius .001	196	1.71	-20	434
	Radius .0001	219	1.07	-146	643
	Kernel .04	69	0.67	-148	263
	Mahalanobis	172	1.69	9	409
Extraordinary expenditures p/c	**NN**	563	3.69	264	853
	Radius .01	563	3.77	309	869
	Radius .001	548	3.24	275	1,011
	Radius .0001	781	1.61	38	2,116
	Kernel .04	523	3.65	260	823
	Mahalanobis	593	4.61	352	862
Home realated expenditures p/c	**NN**	734	3.38	333	1,166
	Radius .01	755	3.69	399	1,174
	Radius .001	560	2.33	129	1,074
	Radius .0001	800	0.98	-470	2,805
	Kernel .04	636	3.39	266	1,017
	Mahalanobis	549	2.27	92	1,114

Table 61 PSM Borrowers from any source vs. Non borrowers at all

	PSM Borrowers any source vs. Non borrowers at all. Rest of algorithms				
	Algorithm	**ATT**	**t-statistic**	**Lower bound C.I.**	**Upper bound C.I.**
Income p/c	**NN**	-1,297	-1.96	-2,628	-139
	Radius .01	-1,296	-1.98	-2,704	-120
	Radius .001	-1,151	-1.58	-2,795	45
	Radius .0001	-1,267	-0.93	-4,604	879
	Kernel .04	-980	-1.91	-2,033	-3
	Mahalanobis	-72	-0.17	-653	780
Current expenditures p/c	**NN**	-42	-0.18	-556	367
	Radius .01	-41	-0.17	-644	362
	Radius .001	4	0.01	-485	492
	Radius .0001	84	0.17	-999	863
	Kernel .04	-80	-0.40	-544	264
	Mahalanobis	211	1.21	-35	663
Food expenditures p/c	**NN**	168	1.46	-37	441
	Radius .01	169	1.49	-63	395
	Radius .001	196	1.73	-47	402
	Radius .0001	219	1.14	-96	656
	Kernel .04	69	0.66	-129	308
	Mahalanobis	172	1.71	18	485
Extraordinary expenditures p/c	**NN**	563	3.71	268	866
	Radius .01	563	3.83	277	850
	Radius .001	548	3.27	259	907
	Radius .0001	781	1.71	19	1,874
	Kernel .04	523	3.64	247	800
	Mahalanobis	593	4.56	343	830
Home realated expenditures p/c	**NN**	734	3.60	373	1,130
	Radius .01	755	3.94	458	1,179
	Radius .001	560	2.25	120	1,149
	Radius .0001	800	1.00	-400	2,814
	Kernel .02	646	3.47	297	1,010
	Kernel .04	636	3.43	216	970
	Kernel .06	612	3.25	222	930
	Mahalanobis	549	2.32	157	1,078

Table 62 PSM MFI Borrowers vs. Non borrowers at all

	PSM MFI Borrowers vs. Non borrowers at all. Rest of algorithms				
	Algorithm	**ATT**	**t-statistic**	**Lower bound C.I.**	**Upper bound C.I.**
Income p/c	**NN**	-968	-1.30	-2,778	230
	Radius .01	-957	-1.30	-2,667	219
	Radius .001	-875	-1.16	-2,780	371
	Radius .0001	-2,838	-1.67	-7,158	-302
	Kernel .04	-780	-1.54	-1,823	212
	Mahalanobis	133	0.28	-644	1,120
Current expenditures p/c	**NN**	-114	-0.48	-535	327
	Radius .01	-107	-0.47	-501	368
	Radius .001	-140	-0.54	-674	290
	Radius .0001	-395	-1.05	-1,100	372
	Kernel .04	-212	-1.18	-565	120
	Mahalanobis	7	0.05	-241	333
Food expenditures p/c	**NN**	138	1.10	-68	452
	Radius .01	142	1.24	-60	361
	Radius .001	104	0.80	-120	389
	Radius .0001	-137	-0.57	-629	317
	Kernel .04	12	0.11	-181	247
	Mahalanobis	67	0.61	-133	289
Extraordinary expenditures p/c	**NN**	179	1.09	-152	395
	Radius .01	197	1.15	-187	491
	Radius .001	194	1.06	-230	512
	Radius .0001	-204	-0.47	-1,108	593
	Kernel .04	346	2.71	90	616
	Mahalanobis	375	2.80	93	620
Home realated expenditures p/c	**NN**	317	1.34	-304	678
	Radius .01	326	1.38	-202	691
	Radius .001	207	0.50	-625	999
	Radius .0001	198	0.20	-1,784	2,134
	Kernel .04	490	2.45	110	872
	Mahalanobis	430	1.81	-22	924

Table 63 Sensitivity analysis Borrowers vs. Non Borrowers at all

		Borrowers (any source) vs. Non borrowers at all				
		estimates & sensitivity			Confounder effect	
		ATT(target)	ATT(conf)	% U	Outcome	Selection
Income p/c	Hhead Old	-983	-846	14.0%	0.58	0.75
	Electric	-983	-568	42.2%	3.66	0.63
	Agriculture job	-983	-942	4.1%	0.33	0.70
	Asset high	-983	-1,115	-13.5%	5.74	1.52
	Land high	-983	-786	20.0%	0.99	0.22
	Av. Rice price high	-983	-810	17.6%	1.42	1.20
	Development activities	-983	-796	19.1%	1.20	1.81
Extraordinary expenditures p/c	Hhead Old	516	528	-2.2%	0.78	0.74
	Electric	516	493	4.6%	1.84	0.64
	Agriculture job	516	517	-0.2%	1.36	0.69
	Asset high	516	529	-2.6%	1.97	1.51
	Land high	516	481	6.8%	1.49	0.22
	Av. Rice price high	516	494	4.2%	1.14	1.19
	Development activities	516	560	-8.6%	1.90	1.83
Housing expenditures p/c	Hhead Old	612	610	0.3%	1.25	0.74
	Electric	612	607	0.8%	1.49	0.63
	Agriculture job	612	620	-1.3%	0.81	0.68
	Asset high	612	626	-2.2%	1.43	1.49
	Land high	612	616	-0.6%	1.53	0.22
	Av. Rice price high	612	618	-1.0%	1.13	1.21
	Development activities	612	600	2.0%	1.22	1.79

Table 64 Sensitivity analysis MFI borrowers vs. Non Borrowers at all

		MFI borrowers vs. Non borrowers at all				
		estimates & sensitivity			Confounder effect	
		ATT(target)	ATT(conf)	% U	Outcome	Selection
Extraordinary expenditures p/c	Hhead Old	340	347	-2.0%	0.78	0.74
	Electric	340	344	-1.1%	1.84	0.64
	Agriculture job	340	325	4.3%	1.36	0.69
	Asset high	340	368	-8.3%	1.97	1.51
	Land high	340	340	0.1%	1.49	0.22
	Av. Rice price high	340	324	4.8%	1.14	1.19
	Development activities	340	315	7.3%	1.90	1.83
Housing expenditures p/c	Hhead Old	482	446	7.4%	1.54	0.71
	Electric	482	500	-3.8%	1.66	0.59
	Agriculture job	482	498	-3.3%	0.72	0.80
	Asset high	482	473	1.8%	1.32	1.68
	Land high	482	438	9.2%	1.90	0.21
	Av. Rice price high	482	503	-4.4%	1.77	1.24
	Development activities	482	499	-3.5%	1.05	1.98

Appendix 2. Chapter 3

Table 65 Mean test in 2005 Whole sample vs. Panel sample

	ttest All = 1482 vs. Panel = 1,461			
	Whole sample	Just panel (no migr)	t_value	p_value
HH head age	45.81	45.76	0.09	0.93
HH head age^2	2,237	2,232	0.10	0.93
HH head sex	0.90	0.90	0.12	0.90
HH head married	0.90	0.90	0.23	0.82
HH head muslim	0.96	0.96	0.05	0.96
HH size	4.84	4.86	0.26	0.79
HH nr of males	2.52	2.53	0.23	0.82
HH nr of females	2.32	2.33	0.17	0.86
HH nr child <=6	0.23	0.23	0.05	0.96
HH head education	1.40	1.40	0.10	0.92
Female max education	1.62	1.62	0.11	0.91
Male max education	2.11	2.12	0.24	0.81
HH max education	2.23	2.24	0.15	0.88
Scheduled caste	0.22	0.22	0.04	0.97
Scheduled tribe	0.13	0.12	0.09	0.93
Backward caste	0.46	0.48	0.02	0.99
Upward caste	0.18	0.18	0.02	0.99
HH head unemployed	0.10	0.10	0.06	0.95
HH head main job agric	0.77	0.77	0.09	0.93
HH head main job non-agric	0.13	0.13	0.05	0.96
HH head no job	0.10	0.10	0.06	0.95
HH head self employed	0.55	0.55	0.16	0.88
HH head wage employed	0.35	0.35	0.13	0.90
HH head job time	1,194	1,193	0.00	1.00
HH job time	3,427	3,439	0.12	0.90
Job time per capita	1,279	1,278	0.02	0.98
Walls of composite material	0.13	0.13	0.07	0.95
Time spent to get water	21	21	0.03	0.98
Value of Jewelry	5,186	5,174	0.03	0.97
Asset Index	0.72	0.72	0.02	0.98
Land area	3.36	3.37	0.08	0.94
Land value	31,922	32,080	0.10	0.92
Livestock value	3,344	3,367	0.12	0.91
Suffered shock	0.61	0.61	0.18	0.86
Borrowed from bank y/n	0.36	0.36	0.08	0.93
Borrowed from SHG y/n	0.29	0.29	0.07	0.95
Borrowed from NGO y/n	0.01	0.01	0.02	0.99
Borrowed from moneylender y/n	0.49	0.49	0.02	0.99
Borrowed from family-friends y/n	0.15	0.15	0.08	0.94
Borrowed from others y/n	0.09	0.09	0.06	0.96
Loan size bank	16,691	16,685	0.01	1.00
Loan size SHG	6,121	6,144	0.02	0.99
Loan size NGO	18,679	18,970	0.02	0.99
Loan size moneylender	19,592	19,689	0.08	0.94
Loan size family-friends	23,361	23,399	0.01	0.99
Loan size others	15,546	15,566	0.01	0.99
Total amount borrowed all sources	26,388	26,465	0.05	0.96
Income	26,551	26,552	0.00	1.00
Income per capita	5,833	5,768	0.14	0.89

Table 66 Mean & Median of outcome variables by year

		income		income per capita	
		mean	median	mean	median
year	2005	26,552	17,166	5,768	3,866
	2007	24,071	18,807	5,173	4,271

Figure 23 Box & Whisker Income by year

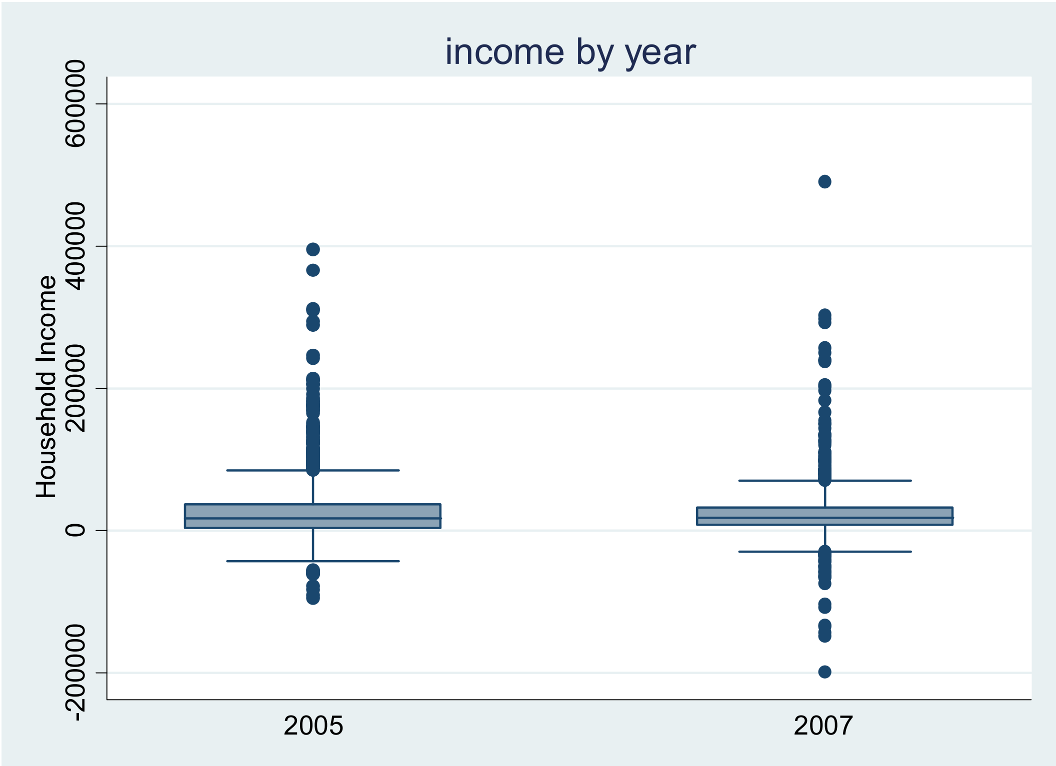


Table 67 Quantiles income distribution

		p1	p10	p25	p50	p75	p90	p99
year	2005	-35,604	-4,308	4,445	17,166	36,851	65,018	205,958
	2007	-48,700	-490	8,150	18,807	33,140	54,250	155,020

Figure 24 Quantiles income distribution

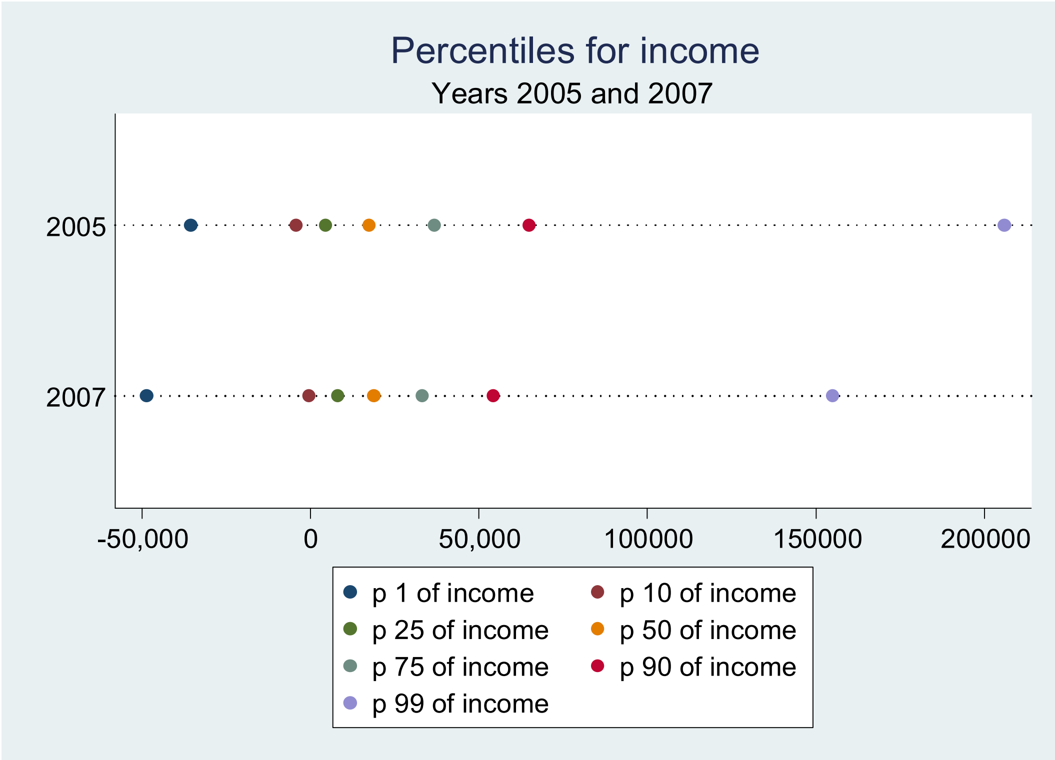


Table 68 Mean & Median income, by district & year

mean income	year		Δ2005-2007
median income	2005	2007	
1_PRAKASAM	33,012	30,448	-7.77%
	22,657	23,400	3.28%
2_KURNOOL	37,775	25,486	-32.53%
	27,108	19,007	-29.88%
3_ANANTAPUR	10,888	18,613	70.95%
	7,486	16,660	122.56%
4_MAHABUB NAGAR	37,057	22,781	-38.52%
	23,998	18,800	-21.66%
6_NALGONDA	20,864	25,096	20.28%
	16,976	20,000	17.82%

Figure 25 Mean & median income by district and year

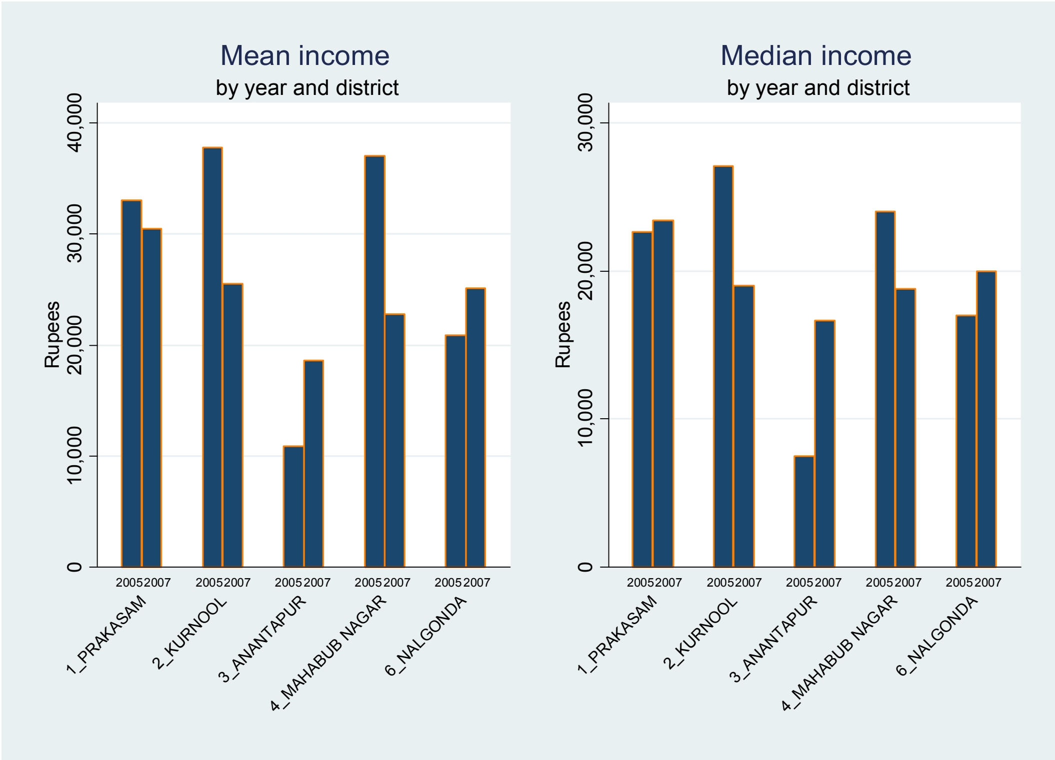


Table 69 Income per watershed area

mean income median income	2005	2007	Δ 2005 - 2007
non-watershed	21,034	22,776	8.3%
	13,768	18,750	36.2%
watershed	28,955	24,635	-14.9%
	19,831	18,807	-5.2%

Table 70 Median outcome variables by year and credit source

	Income per capita		Income	
	2005	2007	2005	2007
Bank	3,063	3,650	15,186	18,570
NGO	1,044	4,700	4,176	18,800
SHG	4,277	4,467	20,793	21,297
Moneylender	3,251	4,133	16,010	18,910
Family&fr	3,822	4,238	17,513	19,600
Others	2,515	2,488	12,716	15,040

Table 71 Income Mean & Median, by SHG-borrowing status

		Household income			
		mean		median	
		SHG non-borrow	SHG everborrow	SHG non-borrow	SHG everborrow
year	2005	25,840	27,448	16,646	18,390
	2007	22,005	26,678	18,000	21,000

Table 72 Pearson's & Spearman's correlation coefficients instruments - outcome variables. Dataset with negative income values.

			Income	Inc. p/cap
duration	Pearson	coeff	0.0224	-0.0134
		p-value	0.2268	0.4695
	Spearman	coeff	0.0291	-0.0054
		p-value	0.1156	0.7697
attendance	Pearson	coeff	0.0367	0.0062
		p-value	0.1475	0.7364
	Spearman	coefficient	0.0292	0.0001
		p-value	0.1148	0.995

Table 73 Pearson's & Spearman's correlation coefficients instruments - outcome variables. Dataset dropping negative income values

			Income	Inc. p/cap
duration	Pearson	coeff	0.0126	0.0015
		p-value	0.1932	0.9385
	Spearman	coeff	0.0194	0.0300
		p-value	0.2659	0.1284
attendance	Pearson	coeff	0.0231	0.0293
		p-value	0.1198	0.1379
	Spearman	coefficient	0.0298	0.0389
		p-value	0.1368	0.1201

Table 74 Regresssion instruments over quintiles of dependent variables

	Income per capita		Income	
	duration	%attendance	duration	%attendance
Year 2007 intercept	-0.449*** (-2.87)	-11.103*** (-5.73)	-0.445*** (-2.82)	-11.242*** (-5.81)
Age of the HH head	0.084** (2.02)	0.324 (0.59)	0.077* (1.83)	0.169 (0.31)
Age of the HH head sqared	-0.001** (-2.01)	-0.007 (-1.21)	-0.001* (-1.78)	-0.005 (-0.89)
Sex of the HH head	0.313 (1.24)	5.300* (1.74)	0.267 (1.06)	4.622 (1.53)
Soc group: Scheduled Caste	0.235 (0.97)	-1.058 (-0.33)	0.244 (1.01)	-1.555 (-0.49)
Soc group: Scheduled Tribe	1.909*** (4.83)	1.467*** (2.86)	1.941*** (4.93)	1.515*** (2.89)
Soc group: Backward Caste	0.059 (0.27)	-1.558 (-0.54)	0.075 (0.36)	-1.772 (-0.62)
Index of asset ownership	0.275 (1.39)	5.209 (1.21)	0.240 (0.96)	4.858 (0.58)
Nr of village households	-0.001*** (-4.25)	-0.014*** (-5.11)	-0.001*** (-4.09)	-0.013*** (-4.97)
Prakasam	-0.872*** (-3.69)	-18.937*** (-5.76)	-0.876*** (-3.72)	-18.570*** (-5.64)
Kurnool	-0.477** (-2.11)	-4.927 (-1.43)	-0.479** (-2.12)	-4.397 (-1.28)
Anantapur	0.733** (2.48)	0.427 (0.13)	0.800*** (2.71)	1.490 (0.45)
Mahabub-Nagar	0.553** (2.12)	0.678 (0.21)	0.552** (2.12)	1.032 (0.32)
quintile 1	-0.204 (-0.87)	-2.670 (-1.13)	-0.637 (-0.73)	-1.168 (-0.95)
quintile 2	0.013 (0.06)	-0.267 (-0.08)	-0.396 (-1.63)	-4.071 (-1.27)
quintile 3	0.127 (0.54)	-1.267 (-0.40)	0.214 (0.86)	1.311 (0.41)
quintile 4	0.296 (1.27)	-2.861 (-0.92)	0.068 (0.28)	1.851 (0.59)
Constant	0.527 (0.49)	52.128*** (3.71)	0.885 (0.82)	55.615*** (3.91)
Observations	2560	2560	2560	2560
Adjusted R-squared	0.078	0.083	0.084	0.089
F	10.161	13.956	11.013	14.865
p	0.000	0.000	0.000	0.000

Table 75 Relevance, orthogonality and exogeneity test. Dataset with no negative income values.

	Relevant	Orthogonality Hansen J		Exogeneity Wu-Hausman	
	F	Stat	p-value	Stat	p-value
Income	10.86	0.071	0.7899	0.005	0.9436
Income pc	10.86	0.004	0.9514	0.012	0.913

Table 76 Income outcomes. Dataset including negative income observations

	Income		
	FE with negatives	MM with negatives	IV with negatives
Year 2007 intercept	-3813.006*** (-2.17)	1944.223** (2.44)	-3772.210** (-2.11)
Age of the HH head	-28.085 (-0.11)	89.255*** (3.09)	-18.755 (-0.07)
Household size	4813.405*** (3.24)	2452.286*** (9.99)	4939.968*** (3.22)
Shock in last 12 mths	-4813.405** (-1.97)	-5283.869*** (-7.01)	-4544.985* (-1.75)
Asset index	3139.077 (1.61)	866.493* (1.67)	3087.938 (1.55)
SHG	0.405** (2.19)	0.278*** (2.60)	0.376* (1.82)
BANK	-0.000 (-0.00)	-0.146*** (-5.03)	-0.046 (-0.18)
NGO	0.817*** (4.95)	1.149*** (30.30)	0.820*** (4.74)
Moneylenders/others	-0.118 (-1.12)	0.003 (0.12)	-0.119 (-1.13)
Family/Friends	-0.093 (-1.12)	0.115*** (3.79)	-0.095 (-1.12)
Others	-0.250 (-1.04)	-0.161 (-1.17)	-0.233 (-0.92)
Constant		3588.757* (1.87)	
Observations	2906	2906	2906

Table 77 outcomes. Dataset NOT including negative income observations

	Income		
	FE NO negatives	MM NO negatives	IV NO negatives
Year 2007 intercept	-3799.656** (-2.16)	444.044 (0.61)	-2344.099 (-1.42)
Age of the HH head	-39.500 (-0.15)	42.287 (1.57)	-137.702 (-0.86)
Household size	4812.880*** (3.24)	2306.679*** (9.67)	4131.344*** (2.85)
Shock in last 12 mths	-4813.526** (-1.97)	-2799.056*** (-3.96)	-4110.045* (-1.66)
Asset index	3139.818 (1.61)	1463.784*** (3.03)	3974.474** (2.02)
SHG	0.405** (2.19)	0.255** (2.52)	0.396* (1.77)
BANK	-0.000 (-0.00)	-0.076** (-2.07)	0.387* (1.65)
NGO	0.816*** (4.95)	1.137*** (41.37)	0.858*** (7.03)
Moneylenders/others	-0.118 (-1.12)	0.019 (0.96)	-0.073 (-0.58)
Family/Friends	-0.093 (-1.12)	0.112*** (3.89)	-0.050 (-0.67)
Others	supplier (-1.04)	-0.047 (-0.63)	0.150 (1.14)
Constant	6649.815 (0.46)	6938.122*** (3.82)	
Observations	2560	2560	2560

Appendix 3. Chapter 4.

Table 78 Quintile mobility Income per capita Prakasam

Quintiles 2005	Income per capita Prakasam					
	Quintiles 2007					
	1	2	3	4	5	Total
1	7 15.85 16.96	7 15.76 14.38	13 28.9 20.84	4 9.29 7.21	14 30.2 16.14	**45** **100** **15.2**
2	8 21.55 19.79	3 8.59 6.73	8 21.31 13.18	6 15.59 10.38	13 32.96 15.11	**39** **100** **13.05**
3	8 12.9 19.64	23 36.07 46.85	14 21.61 22.16	14 21.73 23.99	5 7.69 5.85	**64** **100** **21.63**
4	12 18.04 28.33	5 6.95 9.31	15 22.41 23.7	15 23.1 26.3	19 29.5 23.12	**66** **100** **22.31**
5	6 7.81 15.28	11 13.61 22.73	13 15.25 20.11	19 22.62 32.11	34 40.71 39.79	**82** **100** **27.81**
Total	**42** **14.21** **100**	**49** **16.65** **100**	**62** **21.09** **100**	**58** **19.59** **100**	**84** **28.46** **100**	**296** **100** **100**

Table 79 Quintile mobility. Income per capita. Kurnool

Quintiles 2005	Income per capita Kurnool					
	Quintiles 2007					
	1	2	3	4	5	Total
1	2 8.63 3.55	10 37.61 16.61	7 26.11 13.23	2 7.54 3.13	5 20.11 8.39	**26** **100** **8.72**
2	9 19.57 14.5	10 21.09 16.79	7 14.13 12.9	13 28.84 21.59	8 16.37 12.31	**46** **100** **15.71**
3	17 21.68 27.11	15 18.98 25.49	17 21.63 33.33	13 16.7 21.1	16 21.01 26.66	**78** **100** **26.51**
4	7 11.87 10.64	12 21.11 20.34	12 21.52 23.79	18 32.06 29.05	8 13.43 12.22	**56** **100** **19.01**
5	28 31.17 44.2	12 13.64 20.77	8 9.58 16.75	16 17.53 25.12	25 28.08 40.41	**89** **100** **30.06**
Total	**63** **21.2** **100**	**58** **19.74** **100**	**51** **17.2** **100**	**62** **20.98** **100**	**62** **20.89** **100**	**295** **100** **100**

Table 80 Quintile mobility. Income per capita. Anantapur

Quintiles 2005	Income per capita Anantapur					
	Quintiles 2007					
	1	2	3	4	5	Total
1	38 36.86 51.58	18 16.88 28.12	15 14.03 24.54	13 12.88 25.39	20 19.34 45.14	**104** **100** **35.43**
2	14 17.93 18.38	22 28.58 34.88	19 24.7 31.64	16 21.29 30.76	6 7.51 12.84	**76** **100** **25.96**
3	10 16.8 12.88	14 24.27 22.15	15 25.52 24.45	11 18.49 19.97	8 14.93 19.09	**57** **100** **19.41**
4	6 16.87 7.94	7 20.12 11.28	8 23.98 14.1	9 25.43 16.87	5 13.6 10.68	**35** **100** **11.92**
5	7 32.06 9.22	2 10.45 3.57	3 14.65 5.26	4 17.28 7	5 25.56 12.25	**21** **100** **7.28**
Total	**74** **25.32** **100**	**62** **21.27** **100**	**59** **20.26** **100**	**53** **17.97** **100**	**44** **15.18** **100**	**293** **100** **100**

Table 81 Quantiles for plot area and asset index

	Plot area (acres)		Asset index	
	2005	2007	2005	2007
p1	0	0	0	0
p5	0	0	0	0
p10	0	0	0	0
p20	0	0	0	0
p25	0	0	0	0
p30	0	1	0	0
p40	1	1.5	0	0
p50	2	2	0	1
p60	3	3.5	1	1
p70	4	5	1	1
p75	5	5	1	2
p80	5	6	1	2
p90	8	9	2	2
p95	11	12	3	3
p99	20	20	4	4

Table 82 Land quintile mobility

Quintile $_{\_07}$-Quintile $_{\_05}$	Freq.	Percent
-2	2	0.14
-1	111	7.61
0	1,226	84.03
1	91	6.24
2	17	1.17
3	10	0.69
4	2	0.14

Table 83 Asset index mobility

$Index_{07}$-$Index_{05}$	Frequ	Percent
-5	1	0.07
-4	3	0.21
-3	14	0.96
-2	46	3.15
-1	205	14.04
0	720	49.32
1	315	21.58
2	129	8.84
3	25	1.71
4	2	0.14

Table 84 Cross sectional Q-reg Income per capita 2005

	Cross sectional Income PC 2005												
Quintiles	0.05	0.1	0.2	0.25	0.3	0.4	0.5	0.6	0.7	0.75	0.8	0.9	0.95
Age of the HH head	430.753*	214.017	123.706	108.944	146.529*	204.468**	198.740**	250.116**	313.635***	269.786*	350.011**	349.622	49.331
	(1.908)	(1.646)	(1.343)	(1.414)	(1.952)	(2.577)	(2.214)	(2.486)	(2.600)	(1.927)	(2.146)	(1.345)	(0.113)
Age of the HH head sqared	-4.553*	-2.145	-1.060	-1.003	-1.307*	-1.911**	-1.837*	-2.200**	-2.913**	-2.543*	-3.248*	-3.193	0.792
	(-1.844)	(-1.555)	(-1.102)	(-1.275)	(-1.657)	(-2.254)	(-1.917)	(-2.028)	(-2.234)	(-1.665)	(-1.801)	(-1.057)	(0.169)
Sex of the HH head	-1166.886	-548.057	-50.858	486.343	496.119	963.723**	758.931	408.762	740.135	1125.539	1708.353*	1997.150	1822.345
	(-1.208)	(-1.083)	(-0.108)	(1.132)	(1.223)	(2.033)	(1.349)	(0.559)	(1.010)	(1.367)	(1.813)	(1.514)	(0.784)
Household size	193.231	126.317	-59.454	-107.453	-126.284	-212.389**	-348.967***	-451.445***	-678.885***	-682.492***	-727.581***	-935.769***	-1664.923***
	(1.256)	(1.436)	(-0.650)	(-1.258)	(-1.439)	(-2.344)	(-3.354)	(-3.478)	(-4.155)	(-4.234)	(-4.526)	(-3.264)	(-3.939)
HH head married	1315.417	579.871	519.560	311.548	43.479	-182.455	-301.459	288.256	-1080.383	-1519.251	-2512.197*	-4046.279*	-3593.886
	(1.274)	(0.787)	(0.901)	(0.539)	(0.076)	(-0.267)	(-0.397)	(0.282)	(-0.964)	(-1.171)	(-1.785)	(-1.774)	(-0.934)
Suffered shock in the last 12m	-1622.589***	-1413.278***	-1466.592***	-1376.394***	-1554.953***	-1483.244***	-1829.511***	-2017.820***	-2023.076***	-2095.554***	-2443.127***	-2990.650***	-4262.231**
	(-3.310)	(-4.468)	(-5.104)	(-4.479)	(-4.719)	(-4.365)	(-5.277)	(-5.066)	(-3.492)	(-3.084)	(-3.497)	(-3.055)	(-2.444)
HH head self-employed	-1448.793**	-1217.018**	-1209.082***	-1141.988***	-1149.372**	-1090.925**	-823.336	-269.115	123.120	-132.691	-307.611	-156.351	823.716
	(-2.062)	(-2.483)	(-2.694)	(-2.645)	(-2.412)	(-2.019)	(-1.233)	(-0.340)	(0.131)	(-0.131)	(-0.277)	(-0.094)	(0.283)
HH head wage-employed	705.189	614.905	646.274	685.706	728.729	828.536	762.888	885.613	806.548	450.185	290.880	141.495	1239.243
	(1.091)	(1.309)	(1.268)	(1.303)	(1.251)	(1.430)	(1.092)	(1.075)	(0.831)	(0.441)	(0.248)	(0.080)	(0.427)
Worktime per working capita	0.426	0.234	0.747***	0.766***	0.773***	0.895***	1.256***	1.256***	1.607***	2.097***	2.528***	3.545***	5.123**
	(0.891)	(0.792)	(2.628)	(2.801)	(2.851)	(3.506)	(4.847)	(4.319)	(3.350)	(4.240)	(4.544)	(3.423)	(2.417)
Watershed program in village	533.897	375.174	308.779	402.859	323.270	247.501	187.487	399.747	-46.293	32.978	442.017	-192.296	1.382
	(0.997)	(1.059)	(1.008)	(1.318)	(1.070)	(0.775)	(0.557)	(1.009)	(-0.090)	(0.058)	(0.681)	(-0.160)	(0.001)
Prakasam	497.024	-46.715	161.078	110.477	83.951	386.435	281.650	317.603	678.673	413.328	607.020	1326.154	3383.594
	(0.483)	(-0.101)	(0.331)	(0.219)	(0.165)	(0.804)	(0.606)	(0.611)	(1.007)	(0.565)	(0.736)	(0.905)	(1.182)
Kurnool	385.696	1062.894**	1528.642***	1550.817***	1350.912***	1881.534***	1981.998***	2404.274***	3328.157***	3623.008***	4563.991***	6024.447***	9401.937***
	(0.371)	(1.998)	(3.442)	(3.309)	(2.708)	(3.588)	(3.425)	(3.468)	(4.122)	(3.959)	(3.943)	(3.927)	(3.070)
Anantapur	-1403.724	-1093.938*	-803.851*	-843.965*	-1214.028***	-1530.147***	-2001.226***	-2268.366***	-2292.549***	-2642.901***	-2536.374***	-2457.640**	-1981.940
	(-1.153)	(-1.747)	(-1.769)	(-1.798)	(-2.794)	(-3.485)	(-4.077)	(-4.110)	(-3.385)	(-3.529)	(-3.008)	(-2.161)	(-0.830)
Mahabub-Nagar	1318.381	563.943	649.527	653.393	411.302	921.450	1596.357***	1409.351**	2096.777**	2530.875**	3865.375***	6367.043***	12420.217***
	(1.337)	(1.163)	(1.316)	(1.373)	(0.843)	(1.567)	(2.645)	(2.012)	(2.170)	(2.219)	(2.910)	(3.479)	(2.712)
Borrowed bank ever	604.152	290.838	452.534	233.310	23.227	2.210	-21.025	36.295	47.757	402.265	207.921	109.008	-563.577
	(1.064)	(0.740)	(1.518)	(0.725)	(0.067)	(0.006)	(-0.045)	(0.067)	(0.079)	(0.586)	(0.263)	(0.089)	(-0.281)
Borrowed NGO ever	-3702.965	-6955.346**	-1544.770	-2005.891	-1870.814	-1497.065	-1447.692	945.619	-80.433	-86.994	-636.857	1756.174	-1226.438
	(-1.099)	(-2.020)	(-0.550)	(-0.766)	(-0.762)	(-0.698)	(-0.695)	(0.511)	(-0.041)	(-0.040)	(-0.242)	(0.553)	(-0.284)
Borrowed SHG ever	-801.812	-420.358	-573.771*	-629.285**	-596.318**	-327.408	-502.137	-311.908	-539.430	-931.911*	-1190.649*	-1192.519	-434.537
	(-1.417)	(-1.244)	(-1.920)	(-2.195)	(-2.029)	(-1.031)	(-1.530)	(-0.768)	(-1.047)	(-1.656)	(-1.946)	(-1.104)	(-0.190)
Borrowed mnl_lrd ever	-1086.266*	-717.575*	-319.246	-425.001	-256.631	-155.880	-468.128	-763.289	-619.089	-718.166	-392.085	624.962	-22.920
	(-1.927)	(-1.805)	(-1.075)	(-1.388)	(-0.820)	(-0.419)	(-1.095)	(-1.645)	(-1.141)	(-1.128)	(-0.569)	(0.549)	(-0.013)
Borrowed family_fr ever	340.780	88.578	-241.282	-120.148	-72.808	-6.256	-69.144	-77.848	115.258	316.224	643.320	1237.830	450.495
	(0.567)	(0.226)	(-0.769)	(-0.363)	(-0.222)	(-0.018)	(-0.190)	(-0.176)	(0.238)	(0.536)	(0.924)	(1.063)	(0.254)
Borrowed other ever	-1436.608	-543.755	-958.339*	-974.467*	-817.341	-921.609	-841.590	-394.076	-586.173	-825.556	251.833	1567.159	3339.071
	(-1.245)	(-0.696)	(-1.887)	(-1.799)	(-1.481)	(-1.590)	(-1.346)	(-0.551)	(-0.537)	(-0.660)	(0.180)	(0.713)	(0.874)
Sum from Bank	-0.124***	-0.026	-0.024	-0.006	0.004	0.016	0.015	0.039	0.093*	0.125**	0.146***	0.193***	0.260***
	(-2.830)	(-1.448)	(-1.380)	(-0.331)	(0.215)	(0.700)	(0.519)	(0.951)	(1.803)	(2.327)	(3.007)	(4.132)	(2.808)
Sum from NGO	0.317	0.384	0.047	0.326	0.315	0.300	0.290	0.257	0.251	0.247	0.236	0.173	0.162
	(0.951)	(1.159)	(0.160)	(1.037)	(0.962)	(0.842)	(0.867)	(0.694)	(0.585)	(0.519)	(0.449)	(0.267)	(0.219)
Sum from Moneylenders or Landlords	-0.014	0.016	0.014	0.010	0.005	0.011	0.027**	0.025**	0.020	0.018	0.013	0.001	0.218
	(-0.443)	(0.864)	(1.590)	(1.161)	(0.601)	(0.870)	(2.021)	(2.075)	(1.549)	(1.308)	(0.921)	(0.012)	(1.383)
Sum from Family or Friends	-0.035	-0.020	-0.006	-0.002	-0.003	0.006	0.022	0.024	0.018	0.013	0.006	-0.007	0.112
	(-1.118)	(-1.243)	(-0.349)	(-0.087)	(-0.169)	(0.268)	(1.137)	(1.342)	(1.410)	(0.831)	(0.321)	(-0.116)	(1.182)
Other sources: Coops, etc	0.028	-0.039	-0.030	0.005	0.024	0.017	0.058	0.048	0.059	0.067	0.036	0.052	0.020
	(0.492)	(-0.785)	(-0.655)	(0.111)	(0.581)	(0.402)	(1.358)	(0.975)	(0.959)	(0.998)	(0.476)	(0.413)	(0.113)
Sum from SHG or Vill. Orgs	0.031	-0.002	0.101**	0.099**	0.081**	0.091***	0.087**	0.079**	0.070*	0.065	0.059	0.041	0.018
	(0.233)	(-0.034)	(2.121)	(2.475)	(2.333)	(2.802)	(2.542)	(2.476)	(1.714)	(1.348)	(1.152)	(0.586)	(0.161)
Constant	-1.09e+04**	-4757.765	-2286.347	-1342.074	-1282.804	-2021.763	-137.268	-699.249	955.115	2648.844	1333.993	4755.921	11927.191
	(-2.126)	(-1.539)	(-1.041)	(-0.724)	(-0.747)	(-1.090)	(-0.061)	(-0.268)	(0.311)	(0.783)	(0.349)	(0.812)	(1.013)

t statistics in parenthesis; * p<0.10; ** p < .05; *** p < .01; Nr. of observations: 1,460. In yellow, blue and orange estimates that are significant at least at 10% level for loan size from SHG, bank and moneylenders respectively.

Table 85 Cross sectional Q-reg Income per capita 2007

	Cross sectional income PC 2007												
Quintiles	**0.05**	**0.1**	**0.2**	**0.25**	**0.3**	**0.4**	**0.5**	**0.6**	**0.7**	**0.75**	**0.8**	**0.9**	**0.95**
Age of the HH head	145.781	136.262	-28.007	2.075	20.465	69.745	72.022	100.537	154.416	237.246**	109.154	-82.468	-119.520
	(0.974)	(1.095)	(-0.283)	(0.028)	(0.294)	(0.975)	(0.862)	(1.149)	(1.557)	(2.099)	(0.840)	(-0.357)	(-0.379)
Age of the HH head sqared	-1.349	-1.277	0.382	0.088	-0.077	-0.456	-0.494	-0.634	-1.118	-1.830	-0.535	1.250	1.786
	(-0.896)	(-0.994)	(0.378)	(0.119)	(-0.109)	(-0.625)	(-0.562)	(-0.662)	(-1.056)	(-1.484)	(-0.378)	(0.520)	(0.556)
Sex of the HH head	-982.335	519.536	-363.434	-127.355	-181.315	-733.537	-951.739*	-1249.011**	-1482.098**	-1626.794*	-2656.695**	-1482.909	-3550.993
	(-0.400)	(0.668)	(-0.629)	(-0.254)	(-0.356)	(-1.359)	(-1.919)	(-2.339)	(-2.139)	(-1.763)	(-2.359)	(-0.818)	(-1.320)
Household size	258.924*	211.743**	118.813	54.877	21.235	-26.002	-188.079**	-325.169***	-438.399***	-563.640***	-677.093***	-1001.771***	-1182.444***
	(1.881)	(2.161)	(1.538)	(0.731)	(0.296)	(-0.357)	(-2.263)	(-3.807)	(-4.626)	(-5.356)	(-5.518)	(-5.818)	(-3.773)
HH head married	863.347	-242.642	107.206	169.394	138.941	531.130	227.443	772.167*	632.036	803.366	1692.274*	1561.439	656.709
	(0.432)	(-0.317)	(0.207)	(0.389)	(0.337)	(1.138)	(0.522)	(1.693)	(1.086)	(1.037)	(1.754)	(0.958)	(0.244)
Suffered shock in the last 12m	-1393.072**	-1060.048**	-929.577***	-671.330**	-706.198***	-689.977***	-567.358**	-592.910*	-574.147	-466.801	-639.765	-70.874	-35.060
	(-2.058)	(-2.554)	(-3.092)	(-2.418)	(-2.671)	(-2.883)	(-2.271)	(-1.960)	(-1.526)	(-1.094)	(-1.204)	(-0.084)	(-0.023)
HH head self-employed	-786.787	-730.017	-482.614	-342.647	97.169	236.024	13.984	584.843	20.935	-138.200	-145.536	1366.868	1998.393
	(-0.868)	(-1.122)	(-1.144)	(-0.798)	(0.230)	(0.520)	(0.027)	(0.877)	(0.025)	(-0.160)	(-0.149)	(0.841)	(0.588)
HH head wage-employed	2005.257**	1323.070**	904.005**	1083.581***	1278.885***	885.033*	430.421	699.059	-212.993	-637.517	-1005.841	-2183.687	-3184.536
	(2.175)	(2.016)	(2.137)	(2.604)	(2.945)	(1.950)	(0.835)	(1.077)	(-0.252)	(-0.738)	(-1.025)	(-1.407)	(-0.945)
Worktime per working capita	-0.527	-0.106	-0.067	-0.020	0.354	0.665***	1.010***	1.149***	1.291***	1.496***	1.885***	2.498***	1.899
	(-1.004)	(-0.345)	(-0.311)	(-0.079)	(1.450)	(2.773)	(3.303)	(3.352)	(3.591)	(3.338)	(3.782)	(3.356)	(1.386)
Watershed program in village	-17.608	-114.050	-127.520	-7.945	164.756	121.284	121.860	33.555	-116.426	-51.295	37.115	762.767	556.269
	(-0.032)	(-0.312)	(-0.458)	(-0.031)	(0.659)	(0.492)	(0.454)	(0.116)	(-0.350)	(-0.136)	(0.082)	(0.994)	(0.327)
Prakasam	891.593	964.659*	793.010**	556.646*	409.089	609.463	1145.012***	1616.688***	1735.196***	2064.592***	2327.508***	3251.727**	5749.816**
	(0.936)	(1.764)	(2.132)	(1.653)	(1.245)	(1.608)	(3.051)	(3.608)	(3.153)	(3.218)	(2.807)	(2.137)	(2.028)
Kurnool	490.137	162.921	383.589	559.290	430.992	547.225	382.682	814.615*	911.705*	994.059	955.331	-125.110	-124.038
	(0.543)	(0.280)	(0.816)	(1.376)	(1.104)	(1.522)	(1.063)	(1.910)	(1.734)	(1.481)	(1.167)	(-0.115)	(-0.073)
Anantapur	-829.622	-188.299	45.070	100.577	-39.089	22.729	-172.930	2.933	-329.544	-378.287	-526.724	-715.005	-210.714
	(-0.563)	(-0.255)	(0.089)	(0.235)	(-0.103)	(0.059)	(-0.467)	(0.007)	(-0.673)	(-0.660)	(-0.712)	(-0.529)	(-0.073)
Mahabub-Nagar	240.231	494.228	541.387	367.539	345.227	175.459	219.105	560.969	223.929	266.821	188.147	-209.472	-1055.472
	(0.232)	(0.960)	(1.448)	(0.999)	(0.957)	(0.476)	(0.527)	(1.281)	(0.448)	(0.426)	(0.237)	(-0.183)	(-0.584)
Borrowed bank ever	-421.255	-450.933	-708.889**	-783.999**	-728.519**	-705.683**	-193.970	-231.126	-50.209	-285.081	-125.512	6.827	-515.629
	(-0.735)	(-0.993)	(-2.139)	(-2.575)	(-2.441)	(-2.266)	(-0.597)	(-0.720)	(-0.154)	(-0.702)	(-0.224)	(0.007)	(-0.251)
Borrowed NGO ever	-8856.468	-4174.433	3353.900	2527.822	2119.213	1617.683	1002.245	1158.723	1838.424	1773.268	2160.077	-3603.960	-5538.249*
	(-1.172)	(-0.566)	(0.728)	(0.750)	(1.004)	(1.425)	(0.680)	(0.633)	(0.976)	(0.927)	(1.109)	(-1.470)	(-1.785)
Borrowed SHG ever	-204.593	203.918	419.082	196.206	316.182	380.432	462.781	428.823	164.526	195.022	498.051	497.682	328.675
	(-0.319)	(0.481)	(1.524)	(0.726)	(1.273)	(1.470)	(1.620)	(1.392)	(0.466)	(0.483)	(0.994)	(0.538)	(0.225)
Borrowed mnl_lrd ever	118.808	199.136	241.506	46.074	-3.066	-175.213	-14.546	-115.930	17.015	170.129	193.307	392.117	1349.574
	(0.229)	(0.505)	(0.827)	(0.167)	(-0.012)	(-0.669)	(-0.055)	(-0.351)	(0.045)	(0.362)	(0.353)	(0.445)	(1.005)
Borrowed family_fr ever	321.912	223.908	389.163	217.826	-14.913	64.092	320.652	516.781*	223.050	470.566	51.799	-259.927	904.681
	(0.518)	(0.510)	(1.180)	(0.762)	(-0.055)	(0.211)	(0.976)	(1.661)	(0.603)	(1.121)	(0.102)	(-0.298)	(0.574)
Borrowed other ever	-579.058	-417.528	6.091	192.804	453.108	188.633	-32.915	-23.990	-258.526	-209.681	-189.155	-1072.737	1788.883
	(-0.509)	(-0.606)	(0.012)	(0.394)	(0.944)	(0.449)	(-0.067)	(-0.044)	(-0.354)	(-0.213)	(-0.156)	(-0.637)	(0.665)
Sum from Bank	-0.182***	-0.136***	-0.039	-0.036*	-0.024	-0.029***	-0.032**	-0.017	0.003	0.012	0.024	0.131	0.168
	(-3.015)	(-2.686)	(-1.349)	(-1.721)	(-1.637)	(-2.977)	(-2.163)	(-0.827)	(0.132)	(0.347)	(0.461)	(1.337)	(1.501)
Sum from NGO	1.840	1.176	0.217	0.246	0.260	0.298	0.307	0.233	0.041	0.021	0.038	0.430	0.669
	(1.258)	(0.850)	(0.249)	(0.331)	(0.454)	(0.717)	(0.685)	(0.457)	(0.087)	(0.047)	(0.094)	(0.694)	(0.793)
Sum from Moneylenders or Landlords	-0.074**	-0.047**	-0.042***	-0.038**	-0.016	-0.007	-0.008	-0.005	-0.004	-0.007	-0.007	-0.018	-0.022
	(-2.150)	(-2.267)	(-3.408)	(-2.168)	(-0.993)	(-0.773)	(-1.007)	(-0.578)	(-0.567)	(-0.738)	(-0.669)	(-1.012)	(-0.744)
Sum from Family or Friends	-0.071	-0.008	0.006	0.007	0.019*	0.011	0.004	0.006	0.004	0.001	0.039	0.037	-0.032
	(-1.240)	(-0.187)	(0.338)	(0.583)	(1.706)	(1.179)	(0.363)	(0.468)	(0.200)	(0.022)	(1.336)	(0.839)	(-0.446)
="Sum from Coops.	-0.240	-0.243*	-0.116	-0.110	-0.116	-0.110	-0.080	-0.077	0.025	0.031	0.120	0.747*	0.663*
	(-1.191)	(-1.907)	(-1.018)	(-1.240)	(-1.501)	(-1.539)	(-1.055)	(-0.753)	(0.124)	(0.114)	(0.358)	(1.939)	(1.770)
Sum from SHG or Vill. Orgs	0.086	0.065*	0.052**	0.049**	0.045*	0.036*	0.027	0.039	0.043	0.063	0.018	0.001	0.248
	(1.201)	(1.820)	(2.192)	(2.202)	(1.883)	(1.748)	(0.966)	(1.060)	(1.066)	(1.513)	(0.375)	(0.006)	(0.822)
Constant	-4216.502	-3533.309	2033.323	1755.738	1083.358	679.940	2229.568	1751.628	2948.442	1689.423	5907.916*	12766.656**	19846.398**
	(-0.930)	(-1.262)	(0.878)	(0.939)	(0.598)	(0.392)	(1.194)	(0.969)	(1.454)	(0.686)	(1.941)	(2.280)	(2.416)

t statistics in parenthesis; * p<0.10; ** p < .05; *** p < .01; Nr. of observations: 1,446. In yellow, blue and orange estimates that are significant at least at 10% level for loan size from SHG, bank and moneylenders respectively.

Table 86 Differenced variables Q-regression Income per capita

	Δincome per capita												
Quintiles	**0.05**	**0.1**	**0.2**	**0.25**	**0.3**	**0.4**	**0.5**	**0.6**	**0.7**	**0.75**	**0.8**	**0.9**	**0.95**
Δ Age HH head	153.205 (0.79)	-34.266 (-0.43)	-47.508 (-1.19)	-40.228 (-1.02)	-26.553 (-0.67)	-37.395 (-0.90)	-43.475 (-1.12)	-10.435 (-0.38)	11.804 (0.45)	25.201 (0.78)	22.459 (0.62)	46.688 (0.68)	94.661 (0.77)
Δ HH size	-327.268 (-0.33)	-192.029 (-0.31)	-477.758* (-1.74)	-395.567* (-1.77)	-340.488 (-1.61)	-375.494*** (-2.71)	-346.533** (-2.03)	-397.241** (-2.10)	-413.483** (-2.21)	-480.388*** (-2.61)	-427.550** (-2.57)	-400.695 (-1.43)	-996.765** (-1.97)
Δ shock	-2097.421 (-1.10)	-830.262 (-0.82)	-1288.997** (-2.38)	-1134.576*** (-2.62)	-1035.645*** (-2.76)	-949.347*** (-3.21)	-909.122*** (-3.08)	-967.738*** (-3.17)	-1232.201*** (-4.05)	-1078.482*** (-3.45)	-850.437** (-2.22)	-1045.702 (-1.24)	-741.331 (-0.47)
Δ asset index	2776.333** (2.06)	1862.210*** (3.12)	946.127*** (2.88)	848.855*** (2.97)	759.792*** (3.11)	707.778*** (3.72)	607.662*** (2.94)	354.862 (1.53)	497.511** (2.21)	440.256* (1.74)	594.566** (2.27)	714.245 (1.63)	774.828 (0.97)
Δbank sum	-0.070 (-0.92)	-0.069 (-1.19)	-0.013 (-0.31)	-0.017 (-0.48)	-0.019 (-0.59)	-0.008 (-0.35)	-0.004 (-0.19)	0.011 (0.51)	-0.002 (-0.08)	-0.005 (-0.26)	-0.022 (-1.06)	-0.061 (-1.40)	0.007 (0.10)
Δ NGO sum	-0.006 (-0.01)	0.250 (0.50)	0.165 (0.37)	0.188 (0.44)	0.205 (0.46)	0.222 (0.52)	0.240 (0.59)	0.259 (0.76)	0.267 (0.89)	0.275 (0.93)	-0.073 (-0.24)	-0.033 (-0.11)	0.324 (0.85)
Δ Moneylender sum	0.007 (0.15)	0.000 (0.02)	-0.006 (-0.29)	-0.009 (-0.50)	-0.013 (-0.79)	-0.016 (-1.28)	-0.020* (-1.73)	-0.017 (-1.35)	-0.017 (-1.43)	-0.019* (-1.86)	-0.021** (-2.08)	-0.037*** (-3.00)	-0.058* (-1.90)
Δ family and friend sum	-0.026 (-0.35)	-0.016 (-0.39)	-0.021 (-1.02)	-0.008 (-0.43)	-0.001 (-0.07)	0.003 (0.21)	0.017 (1.17)	0.016 (1.01)	0.010 (0.62)	0.005 (0.28)	-0.002 (-0.09)	0.019 (0.84)	0.032 (1.21)
Δ Other sum	-0.124 (-1.32)	-0.068 (-0.92)	-0.030 (-0.64)	-0.021 (-0.51)	-0.025 (-0.69)	-0.041 (-1.04)	-0.012 (-0.24)	-0.003 (-0.04)	0.023 (0.27)	0.037 (0.38)	-0.024 (-0.25)	-0.035 (-0.28)	-0.068 (-0.43)
Δ SHG sum	-0.016 (-0.11)	0.021 (0.28)	0.074*** (2.63)	0.086*** (3.97)	0.085*** (4.08)	0.075*** (2.81)	0.080*** (2.69)	0.108*** (3.13)	0.125*** (3.65)	0.128*** (3.02)	0.095* (1.93)	0.127* (1.86)	0.184* (1.73)
Constant	-1.81e+04*** (-11.36)	-1.08e+04*** (-14.45)	-5742.010*** (-14.88)	-4276.075*** (-13.37)	-3158.761*** (-12.46)	-1379.836*** (-6.53)	16.893 (0.08)	1374.941*** (6.48)	2878.032*** (13.88)	3714.054*** (15.86)	4766.904*** (19.18)	8128.539*** (14.86)	12816.156*** (11.16)

t statistics in parenthesis; * p<0.10; ** p < .05; *** p < .01; Nr. of observations: 1,460. In yellow, blue and orange estimates that are significant at least at 10% level for loan size from SHG, bank and moneylenders respectively.

Table 87 Differenced variables Q-regression Income per capita (including 2005 level variables)

	Δincome per capita												
Quintiles	0.05	0.1	0.2	0.25	0.3	0.4	0.5	0.6	0.7	0.75	0.8	0.9	0.95
ΔHHsize	499.050 (0.914)	-18.295 (-0.056)	65.985 (0.257)	162.649 (0.690)	108.023 (0.538)	-53.773 (-0.342)	-141.700 (-0.812)	-212.277 (-0.898)	-472.528** (-2.074)	-453.729** (-2.179)	-438.986** (-2.276)	889.097*** (-3.370)	1121.689*** (-2.586)
Δshock	1141.024 (0.715)	-702.448 (-0.721)	-545.395 (-0.854)	-415.848 (-0.759)	-549.695 (-1.177)	-414.096 (-0.941)	-276.557 (-0.685)	-537.895 (-1.252)	-315.775 (-0.649)	-48.245 (-0.095)	-70.887 (-0.131)	473.779 (0.578)	103.558 (0.066)
Δeduc_max	-537.670 (-0.337)	-628.063 (-0.651)	-263.946 (-0.510)	-167.283 (-0.361)	87.207 (0.211)	202.849 (0.512)	132.052 (0.372)	-192.885 (-0.547)	49.205 (0.124)	10.448 (0.023)	-87.515 (-0.167)	-363.550 (-0.445)	100.166 (0.062)
Δasset_indx	-1319.414 (-1.175)	-270.363 (-0.437)	-396.208 (-1.027)	-255.458 (-0.767)	-54.492 (-0.193)	-62.984 (-0.244)	45.761 (0.173)	119.171 (0.454)	169.578 (0.582)	149.619 (0.473)	411.457 (1.289)	568.723 (1.391)	1945.990** (2.023)
Δworktime_pcw	-2.368 (-1.618)	-2.390** (-2.250)	-1.767*** (-2.964)	-1.422*** (-2.916)	-1.427*** (-3.337)	-1.128** (-2.530)	-0.868** (-2.283)	-0.806** (-2.278)	-0.446 (-1.081)	-0.359 (-0.750)	-0.323 (-0.598)	-0.311 (-0.353)	0.009 (0.006)
Δbank_sum	-0.127 (-1.154)	-0.040 (-0.859)	-0.055* (-1.737)	-0.068** (-2.233)	-0.073** (-2.516)	-0.056* (-1.725)	-0.022 (-0.709)	-0.019 (-0.629)	0.008 (0.254)	0.012 (0.376)	0.012 (0.339)	0.077 (1.032)	0.076 (0.859)
ΔNGO_sum	0.874 (0.686)	0.844 (0.616)	0.553 (0.544)	0.427 (0.525)	0.428 (0.649)	0.310 (0.490)	0.241 (0.356)	0.133 (0.188)	0.039 (0.045)	-0.034 (-0.036)	-0.079 (-0.076)	1.064 (1.029)	0.173 (0.180)
ΔmnΔlrd_sum	-0.159** (-2.115)	-0.119* (-1.871)	-0.028 (-0.737)	-0.019 (-0.803)	-0.016 (-0.993)	-0.020 (-1.323)	-0.003 (-0.194)	-0.008 (-0.537)	-0.005 (-0.313)	-0.001 (-0.057)	-0.002 (-0.112)	-0.002 (-0.064)	-0.032 (-0.674)
Δfamily_fr_sum	-0.247 (-1.372)	-0.044 (-0.524)	-0.034 (-0.932)	-0.039 (-1.241)	-0.034 (-1.137)	0.005 (0.191)	-0.006 (-0.256)	0.004 (0.169)	0.007 (0.262)	0.002 (0.058)	-0.001 (-0.032)	0.035 (0.697)	-0.001 (-0.017)
Δother_sum	-0.240 (-0.797)	-0.257 (-1.139)	-0.088 (-0.599)	-0.150 (-1.276)	-0.142 (-1.511)	-0.181* (-1.942)	-0.196 (-1.251)	-0.032 (-0.165)	0.110 (0.566)	0.150 (0.681)	0.203 (0.771)	0.670** (2.093)	0.659* (1.750)
ΔSHG_sum	-0.068 (-0.267)	0.057 (0.358)	0.044 (0.793)	0.036 (0.731)	0.081* (1.760)	0.076 (1.449)	0.096 (1.537)	0.110* (1.668)	0.127* (1.946)	0.124* (1.703)	0.105 (1.288)	0.110 (0.619)	0.507* (1.857)
Prakasam	-1450.924 (-0.637)	-1048.258 (-0.728)	277.556 (0.331)	787.065 (1.013)	72.700 (0.098)	300.513 (0.436)	-407.800 (-0.605)	-483.798 (-0.665)	137.348 (0.166)	437.536 (0.503)	768.174 (0.790)	969.805 (0.561)	2607.600 (0.838)
Kurnool	-8173.468*** (-3.708)	-6482.785*** (-3.895)	-3308.577*** (-3.009)	-2315.473** (-2.450)	-2161.909** (-2.488)	-1864.864** (-2.523)	-2160.927*** (-2.774)	-1608.995** (-2.143)	-895.390 (-1.126)	-772.595 (-0.928)	-202.799 (-0.236)	-1838.278 (-1.466)	-3036.536 (-1.420)
Anantapur	2232.896 (1.185)	2118.438* (1.903)	2853.865*** (3.511)	3023.148*** (4.357)	2266.524*** (3.398)	1937.954*** (2.872)	1211.181* (1.693)	982.776 (1.471)	870.024 (1.200)	678.976 (0.835)	566.489 (0.617)	-307.358 (-0.193)	-109.381 (-0.041)
Mahabub-Nagar	-4789.141 (-1.566)	-2923.287* (-1.902)	-1569.131 (-1.521)	-960.042 (-1.058)	-1174.365 (-1.542)	-1089.276 (-1.440)	-1050.226 (-1.340)	-1214.893* (-1.776)	-1026.392 (-1.494)	-1076.137 (-1.375)	-1028.461 (-1.237)	2047.834 (-1.533)	2642.983 (-1.239)
Household size	2470.065*** (6.124)	1237.112*** (4.623)	797.483*** (4.224)	633.640*** (3.840)	580.407*** (3.772)	475.843*** (3.111)	325.378** (2.168)	152.202 (0.991)	0.116 (0.001)	-112.243 (-0.686)	-378.325** (-2.120)	-475.936* (-1.947)	-1021.579** (-2.479)
Suffered shock	3540.871 (1.480)	1095.028 (0.721)	1306.184 (1.480)	1305.489* (1.804)	1043.319* (1.748)	1390.534** (2.292)	1458.080** (2.360)	1649.283*** (2.710)	2253.127*** (3.407)	2518.018*** (3.552)	2480.073*** (3.396)	3065.570*** (2.671)	2595.526 (1.383)
Max educ level in HH	-1694.595 (-1.610)	-1087.236 (-1.561)	-518.515 (-1.023)	-409.154 (-0.928)	-258.650 (-0.682)	-378.421 (-1.096)	-387.771 (-1.289)	-717.932** (-2.301)	-505.464 (-1.410)	-584.292 (-1.443)	-190.971 (-0.419)	-884.811 (-1.125)	-1602.632 (-1.174)
Index of asset ownership	-6321.153*** (-3.729)	-4157.024*** (-5.351)	-3025.570*** (-5.301)	-2254.618*** (-5.006)	-2116.391*** (-6.015)	-1731.820*** (-4.286)	-1123.201*** (-3.320)	-935.586*** (-2.765)	-798.957** (-2.240)	-885.555** (-2.220)	-150.664 (-0.375)	275.050 (0.391)	2507.631* (1.817)
Worktime per working capita	-2.323 (-1.106)	-2.798** (-2.470)	-2.102*** (-2.853)	-1.279** (-2.102)	-1.407*** (-2.705)	-1.341*** (-2.725)	-0.804* (-1.783)	-0.691 (-1.394)	-0.133 (-0.205)	0.193 (0.269)	0.460 (0.601)	0.418 (0.344)	0.445 (0.224)
Sum Bank	-0.241** (-1.981)	-0.184** (-2.542)	-0.111* (-1.929)	-0.107** (-2.172)	-0.100** (-2.416)	-0.064* (-1.762)	-0.045 (-1.276)	-0.041 (-1.228)	-0.020 (-0.505)	-0.011 (-0.228)	0.046 (0.853)	0.139* (1.724)	0.208 (1.583)
Sum NGO	0.921 (0.672)	0.825 (0.572)	0.424 (0.394)	0.250 (0.284)	0.250 (0.326)	0.112 (0.148)	0.013 (0.016)	-0.106 (-0.133)	-0.209 (-0.227)	-0.295 (-0.300)	-0.369 (-0.350)	0.750 (0.702)	-0.218 (-0.208)
Sum Moneylenders	-0.238* (-1.946)	-0.154** (-2.274)	-0.052 (-1.246)	-0.043 (-1.436)	-0.036 (-1.449)	-0.035 (-1.566)	-0.006 (-0.254)	0.011 (0.438)	0.022 (0.896)	0.023 (0.914)	0.017 (0.589)	0.024 (0.649)	-0.021 (-0.391)
Sum Family	-0.219 (-1.179)	-0.020 (-0.231)	-0.025 (-0.623)	-0.039 (-1.103)	-0.035 (-1.062)	-0.000 (-0.001)	-0.012 (-0.410)	0.001 (0.030)	-0.007 (-0.202)	0.004 (0.102)	0.004 (0.096)	0.029 (0.497)	-0.037 (-0.511)
Sum others	-0.221 (-0.682)	-0.231 (-0.991)	-0.127 (-0.812)	-0.165 (-1.290)	-0.166 (-1.601)	-0.186* (-1.868)	-0.178 (-1.100)	-0.028 (-0.142)	0.161 (0.765)	0.252 (1.042)	0.344 (1.209)	0.857** (2.428)	0.863** (1.992)
Sum SHG	-0.115 (-0.261)	0.051 (0.310)	0.012 (0.165)	-0.004 (-0.069)	0.034 (0.579)	0.011 (0.181)	0.006 (0.080)	0.009 (0.124)	0.001 (0.017)	-0.001 (-0.013)	-0.011 (-0.117)	0.018 (0.099)	0.292 (0.906)
Constant	-1.25e+04*** (-3.444)	-4011.519 (-1.641)	-2890.215** (-2.068)	-3060.906** (-2.575)	-1835.743 (-1.602)	-425.301 (-0.346)	832.902 (0.722)	3418.792*** (2.728)	3509.674** (2.451)	4423.679*** (2.930)	4806.005*** (2.839)	9465.362*** (3.926)	16234.617*** (3.962)

t statistics in parenthesis; * p<0.10; ** p < .05; *** p < .01; Nr. of observations: 1,460. In yellow, blue and orange estimates that are significant at least at 10% level for loan size from SHG, bank and moneylenders respectively.

Table 88 Cross sectional Q-regression Income 2005

	Cross sectional Income 2005												
Quintiles	**0.05**	**0.1**	**0.2**	**0.25**	**0.3**	**0.4**	**0.5**	**0.6**	**0.7**	**0.75**	**0.8**	**0.9**	**0.95**
Age of the HH head	2314.514**	1095.387	643.701	558.866	601.704*	735.509**	1109.654***	1015.935**	1312.856***	1237.406**	1271.883*	1779.096	-502.830
	(1.998)	(1.629)	(1.380)	(1.604)	(1.904)	(1.984)	(2.629)	(2.545)	(2.844)	(2.235)	(1.875)	(1.617)	(-0.249)
Age of the HH head sqared	-24.553*	-11.378	-6.182	-5.124	-5.305	-6.669*	-10.569**	-9.411**	-12.218**	-11.434*	-12.100	-17.050	12.499
	(-1.890)	(-1.602)	(-1.257)	(-1.403)	(-1.590)	(-1.682)	(-2.301)	(-2.209)	(-2.451)	(-1.868)	(-1.585)	(-1.337)	(0.568)
Sex of the HH head	-8174.409*	-3911.834	242.586	2032.344	3342.227*	5132.272***	4022.640*	4980.014	2294.605	4949.718	6827.299	6313.928	5568.262
	(-1.723)	(-1.373)	(0.119)	(1.047)	(1.898)	(2.624)	(1.652)	(1.556)	(0.603)	(1.258)	(1.582)	(0.986)	(0.633)
Household size	934.986	657.597	834.269**	1105.010***	1412.752***	1977.630***	2303.828***	3050.861***	4046.507***	4736.534***	5495.816***	6590.526***	6747.308***
	(1.388)	(1.487)	(2.259)	(2.788)	(3.200)	(4.300)	(4.586)	(5.500)	(5.561)	(6.581)	(6.493)	(5.691)	(3.216)
HH head married	9600.960*	5319.564	3244.971	2402.772	1187.465	396.676	-87.835	-1247.412	-3234.318	-7090.599	-9235.866*	-1.24e+04	-1.85e+04
	(1.894)	(1.477)	(1.359)	(1.071)	(0.563)	(0.149)	(-0.032)	(-0.350)	(-0.758)	(-1.523)	(-1.790)	(-1.101)	(-1.468)
Suffered shock in the last 12m	-7352.554***	-6293.960***	-6969.558***	-5771.908***	-6017.107***	-7200.718***	-8043.224***	-8719.498***	-1.01e+04***	-1.08e+04***	-1.11e+04***	-1.39e+04***	-1.75e+04**
	(-3.186)	(-3.838)	(-5.823)	(-4.351)	(-4.024)	(-4.310)	(-5.009)	(-5.264)	(-4.377)	(-3.608)	(-3.657)	(-3.211)	(-2.316)
HH head self-employed	-7001.562**	-6735.272***	-7086.209***	-5949.517***	-5146.302***	-3930.590	-3852.690	-4641.657	-795.639	1604.233	1232.858	4979.046	2547.581
	(-1.962)	(-2.871)	(-3.706)	(-3.188)	(-2.591)	(-1.558)	(-1.196)	(-1.285)	(-0.211)	(0.401)	(0.268)	(0.659)	(0.214)
HH head wage-employed	2325.099	1891.013	908.145	2484.016	2833.376	3579.266	2758.844	-529.876	1722.019	1984.812	-341.913	1390.493	4407.493
	(0.675)	(0.830)	(0.444)	(1.281)	(1.270)	(1.333)	(0.898)	(-0.151)	(0.443)	(0.471)	(-0.072)	(0.168)	(0.365)
Worktime per working capita	2.369	0.686	3.595***	3.699***	4.004***	3.974***	5.978***	6.685***	6.625***	6.720***	9.182***	12.899***	25.110***
	(1.113)	(0.490)	(3.133)	(3.587)	(4.120)	(3.469)	(5.051)	(4.661)	(3.858)	(3.184)	(4.043)	(2.997)	(3.016)
Watershed program in village	1274.823	1506.886	2459.407*	2304.861*	1460.603	2019.357	1038.680	1584.732	1193.712	1261.798	2398.746	1892.456	3609.291
	(0.504)	(0.938)	(1.920)	(1.763)	(1.111)	(1.348)	(0.684)	(0.866)	(0.553)	(0.504)	(0.872)	(0.398)	(0.452)
Prakasam	3099.933	-19.925	1943.513	1170.353	383.402	565.261	-70.332	1251.417	2308.903	2094.899	2787.234	6853.765	12750.224
	(0.718)	(-0.008)	(0.879)	(0.520)	(0.174)	(0.272)	(-0.031)	(0.558)	(0.812)	(0.673)	(0.762)	(1.226)	(1.194)
Kurnool	3053.161	5874.669*	9402.538***	7650.727***	6849.268***	9569.708***	9545.095***	11764.002***	14531.070***	16703.557***	19086.086***	28017.240***	43344.033***
	(0.785)	(1.863)	(4.180)	(3.656)	(3.009)	(3.542)	(3.528)	(4.091)	(3.886)	(3.654)	(3.505)	(3.910)	(2.988)
Anantapur	-6055.903	-4405.619	-1983.311	-3697.980*	-4203.145**	-5613.469***	-9105.316***	-1.00e+04***	-1.13e+04***	-1.12e+04***	-1.17e+04***	-1.34e+04***	-1.06e+04
	(-1.228)	(-1.530)	(-0.920)	(-1.773)	(-2.018)	(-2.739)	(-4.549)	(-4.327)	(-3.993)	(-3.596)	(-3.374)	(-2.882)	(-1.080)
Mahabub-Nagar	5720.845	3117.567	3813.411*	2564.815	2669.240	5504.384*	6213.207**	7496.899***	8309.919**	9804.220**	12698.136**	34244.615***	57207.284**
	(1.484)	(1.321)	(1.803)	(1.173)	(1.154)	(1.951)	(2.243)	(2.726)	(2.145)	(2.193)	(2.267)	(3.644)	(2.560)
Borrowed bank ever	1507.780	556.580	1759.092	1054.658	578.817	532.375	237.186	557.659	426.205	620.049	-507.260	1788.710	-2165.592
	(0.635)	(0.296)	(1.263)	(0.732)	(0.392)	(0.293)	(0.113)	(0.219)	(0.158)	(0.199)	(-0.139)	(0.357)	(-0.238)
Borrowed NGO ever	-1.26e+04	-2.71e+04*	-1.30e+04	-7096.936	-6236.018	-6117.633	-8683.007	3265.435	1599.341	-2461.946	-4701.163	3817.416	9305.632
	(-0.926)	(-1.960)	(-0.966)	(-0.552)	(-0.540)	(-0.647)	(-0.943)	(0.385)	(0.188)	(-0.260)	(-0.446)	(0.278)	(0.431)
Borrowed SHG ever	-2920.318	-1577.463	-2910.554**	-2629.468*	-2787.011**	-1739.348	-2560.148*	-2124.168	-2411.623	-3877.242	-4363.537*	-7958.204*	1434.464
	(-1.071)	(-0.919)	(-2.110)	(-1.924)	(-2.020)	(-1.159)	(-1.715)	(-1.217)	(-1.088)	(-1.606)	(-1.659)	(-1.796)	(0.158)
Borrowed mnl_lrd ever	-3421.441	-3514.759*	-1766.186	-866.571	-1028.875	-1527.687	-2858.725*	-2140.471	-2146.424	-2848.315	-1798.093	-1602.212	794.410
	(-1.310)	(-1.798)	(-1.270)	(-0.647)	(-0.737)	(-0.936)	(-1.755)	(-1.128)	(-0.907)	(-1.021)	(-0.593)	(-0.325)	(0.096)
Borrowed family_fr ever	2144.293	651.461	163.268	308.093	-368.685	14.227	33.393	-501.430	1331.758	1830.314	2682.211	6743.886	13619.398*
	(0.732)	(0.319)	(0.123)	(0.224)	(-0.263)	(0.009)	(0.021)	(-0.270)	(0.613)	(0.685)	(0.927)	(1.298)	(1.814)
Borrowed other ever	161.014	-1159.963	-4547.350**	-4048.690*	-4476.856*	-3047.717	-2981.595	-3331.660	-3125.174	-2834.296	40.494	-707.240	11182.058
	(0.035)	(-0.426)	(-2.051)	(-1.659)	(-1.698)	(-0.943)	(-1.155)	(-1.168)	(-0.679)	(-0.465)	(0.006)	(-0.075)	(0.733)
Sum from Bank	-0.381*	-0.172*	-0.111	-0.070	-0.033	0.051	0.071	0.127	0.462*	0.554**	0.727***	0.787***	1.082*
	(-1.900)	(-1.899)	(-1.259)	(-0.805)	(-0.347)	(0.417)	(0.502)	(0.603)	(1.923)	(2.379)	(3.469)	(3.222)	(1.925)
Sum from NGO	0.940	1.543	0.348	1.278	1.252	1.212	1.195	1.034	0.996	1.009	0.987	0.746	0.597
	(0.723)	(1.199)	(0.274)	(0.925)	(0.893)	(0.798)	(0.790)	(0.622)	(0.538)	(0.485)	(0.441)	(0.260)	(0.158)
Sum from Moneylenders or Landlords	0.042	0.058	0.065*	0.046	0.038	0.059	0.093*	0.079	0.065	0.071	0.052	0.050	0.752
	(0.346)	(0.782)	(1.804)	(1.290)	(0.871)	(1.075)	(1.805)	(1.484)	(1.172)	(1.103)	(0.636)	(0.166)	(0.815)
Sum from Family or Friends	-0.197	-0.113	-0.049	-0.047	0.048	0.009	0.041	0.081	0.068	0.058	0.032	-0.029	-0.128
	(-1.102)	(-1.289)	(-0.590)	(-0.502)	(0.481)	(0.095)	(0.454)	(1.047)	(1.195)	(0.914)	(0.392)	(-0.180)	(-0.598)
Other sources: Coops, etc	0.473	-0.262	-0.326	-0.003	0.069	0.090	0.020	0.241	0.220	0.293	0.233	0.033	-0.067
	(-1.315)	(-1.379)	(0.345)	(0.169)	(0.431)	(0.093)	(1.179)	(1.112)	(1.004)	(0.661)	(0.113)	(1.060)	(-0.275)
Sum from SHG or Vill. Orgs	0.080	-0.049	0.505*	0.586***	0.489***	0.542***	0.498***	0.475***	0.432**	0.398*	0.361*	0.263	0.161
	(0.153)	(-0.163)	(1.723)	(2.621)	(2.587)	(3.283)	(3.259)	(3.201)	(2.392)	(1.945)	(1.819)	(0.783)	(0.284)
Constant	-5.92e+04**	-2.41e+04	-1.82e+04*	-1.75e+04**	-1.76e+04**	-2.12e+04**	-2.47e+04***	-2.18e+04**	-2.62e+04**	-2.27e+04*	-2.53e+04*	-3.42e+04	-6487.253
	(-2.234)	(-1.528)	(-1.777)	(-2.243)	(-2.463)	(-2.444)	(-2.616)	(-2.228)	(-2.363)	(-1.749)	(-1.794)	(-1.513)	(-0.136)

t statistics in parenthesis; * p<0.10; ** p < .05; *** p < .01; Nr. of observations: 1,460. In yellow, blue and orange estimates that are significant at least at 10% level for loan size from SHG, bank and moneylenders respectively.

Table 89 Cross sectional Q-regression Income 2007

	Cross sectional Income 2007												
Quintiles	0.05	0.1	0.2	0.25	0.3	0.4	0.5	0.6	0.7	0.75	0.8	0.9	0.95
Age of the HH head	1538.463*	550.657	46.018	137.720	156.080	328.224	264.818	684.969	769.214	912.166*	574.103	-438.319	-717.949
	(1.881)	(0.874)	(0.127)	(0.458)	(0.541)	(0.984)	(0.770)	(1.624)	(1.577)	(1.824)	(1.031)	(-0.416)	(-0.568)
Age of the HH head sqared	-14.412*	-5.433	0.419	-0.986	-1.317	-2.559	-1.900	-6.006	-6.177	-7.598	-3.594	5.346	10.126
	(-1.799)	(-0.834)	(0.116)	(-0.327)	(-0.442)	(-0.734)	(-0.525)	(-1.328)	(-1.164)	(-1.385)	(-0.585)	(0.488)	(0.776)
Sex of the HH head	-640.130	370.891	457.722	219.555	539.282	-1304.771	-4393.494**	-4071.252*	-5514.693**	-6528.589**	-7628.547*	-4865.122	-6681.525
	(-0.100)	(0.130)	(0.190)	(0.113)	(0.296)	(-0.598)	(-2.366)	(-1.915)	(-2.137)	(-2.098)	(-1.924)	(-0.783)	(-0.734)
Household size	1638.189**	1421.625**	2372.251***	2663.539***	2738.369***	3413.624***	3772.650***	3855.724***	3904.161***	4087.013***	4574.495***	5825.029***	7310.938***
	(2.137)	(2.523)	(6.804)	(8.043)	(8.150)	(9.510)	(10.906)	(11.026)	(9.480)	(7.544)	(7.243)	(5.296)	(4.488)
HH head married	4310.474	2109.538	438.450	-299.428	290.422	-481.195	1204.413	2916.717*	4664.946**	4347.533*	6048.378**	2730.035	8312.805
	(1.045)	(0.784)	(0.215)	(-0.176)	(0.200)	(-0.280)	(0.768)	(1.753)	(2.324)	(1.877)	(2.038)	(0.462)	(1.008)
Suffered shock in the last 12m	-5714.021*	-5303.894***	-3808.294***	-3338.693***	-2969.765**	-2761.919***	-2520.872**	-2284.098*	-1904.321	-1757.371	-1103.086	-1593.883	-3673.562
	(-1.768)	(-2.771)	(-2.947)	(-2.921)	(-2.546)	(-2.656)	(-2.327)	(-1.709)	(-1.276)	(-0.986)	(-0.515)	(-0.412)	(-0.686)
HH head self-employed	-2110.566	-5956.922**	-1876.797	-923.688	-1111.677	928.499	192.809	741.780	-1273.316	-577.848	499.426	4466.040	8786.568
	(-0.439)	(-2.207)	(-1.035)	(-0.527)	(-0.593)	(0.443)	(0.091)	(0.275)	(-0.368)	(-0.155)	(0.130)	(0.661)	(0.627)
HH head wage-employed	11169.667**	2954.139	4743.031***	4622.016***	3888.627**	3927.611*	2478.796	628.109	-2577.316	-3685.374	-5403.508	-1.12e+04*	-1.56e+04
	(2.494)	(1.109)	(2.743)	(2.752)	(2.064)	(1.884)	(1.176)	(0.247)	(-0.743)	(-0.989)	(-1.362)	(-1.817)	(-1.263)
Worktime per working capita	-3.770	-1.464	-0.551	-0.353	0.664	2.365**	2.881***	4.367***	6.259***	6.685***	7.872***	10.595***	10.689**
	(-1.583)	(-1.041)	(-0.618)	(-0.363)	(0.622)	(2.461)	(2.671)	(3.083)	(4.481)	(4.034)	(3.911)	(2.976)	(2.573)
Watershed program in village	-849.378	-1073.051	452.062	815.810	377.216	740.962	1053.123	967.295	232.745	-51.533	1029.302	2291.042	3216.164
	(-0.340)	(-0.634)	(0.374)	(0.767)	(0.337)	(0.684)	(0.933)	(0.730)	(0.158)	(-0.030)	(0.512)	(0.616)	(0.589)
Prakasam	5712.357	4217.523	2882.498*	2912.677**	3287.523**	2300.146	4806.481***	5539.326***	7606.856***	9359.310***	8821.389***	12194.659*	24730.570***
	(1.430)	(1.586)	(1.850)	(2.048)	(2.216)	(1.394)	(2.864)	(3.120)	(3.334)	(3.726)	(2.680)	(1.852)	(2.615)
Kurnool	3549.656	432.766	1866.149	2404.359	2327.718	921.731	1899.473	2782.646	3186.565	3138.467	2962.820	-4137.124	-1536.486
	(0.846)	(0.160)	(1.025)	(1.465)	(1.328)	(0.544)	(1.282)	(1.473)	(1.392)	(1.018)	(0.932)	(-0.866)	(-0.211)
Anantapur	-1373.463	-651.798	196.244	921.609	1333.103	122.180	-8.470	-539.735	-1624.940	-2160.008	-3003.387	-2504.374	3103.037
	(-0.212)	(-0.179)	(0.087)	(0.514)	(0.785)	(0.070)	(-0.005)	(-0.311)	(-0.788)	(-0.826)	(-1.025)	(-0.461)	(0.284)
Mahabub-Nagar	1967.511	1887.742	1916.989	1788.076	2029.731	582.488	328.981	1198.054	247.624	-374.109	-639.124	-1636.711	-3909.862
	(0.401)	(0.721)	(1.190)	(1.182)	(1.269)	(0.335)	(0.180)	(0.606)	(0.120)	(-0.126)	(-0.201)	(-0.292)	(-0.526)
Borrowed bank ever	-3016.042	-1904.920	-2687.474**	-3347.684***	-2879.667**	-2968.673**	-2003.641	-1254.946	-294.046	-140.452	-1944.219	-823.242	-2573.193
	(-1.035)	(-0.968)	(-2.081)	(-2.676)	(-2.259)	(-2.213)	(-1.428)	(-0.853)	(-0.186)	(-0.076)	(-0.833)	(-0.172)	(-0.361)
Borrowed NGO ever	-2.95e+04	-2.12e+04	13255.644	11792.637	8893.905	6839.053	8630.516	5105.549	10314.695	7532.795	6581.551	-1.82e+04*	-2.55e+04*
	(-0.903)	(-0.680)	(0.650)	(0.756)	(0.833)	(0.950)	(1.139)	(0.641)	(1.303)	(0.980)	(0.851)	(-1.852)	(-1.764)
Borrowed SHG ever	559.509	1962.113	1991.441	1091.129	1707.145	1633.543	1876.421	1668.418	1569.314	2198.961	2339.643	5268.944	4102.933
	(0.187)	(1.052)	(1.587)	(0.942)	(1.519)	(1.420)	(1.493)	(1.129)	(1.005)	(1.155)	(1.119)	(1.185)	(0.688)
Borrowed mnl_lrd ever	2028.323	411.627	1396.721	94.930	-97.401	-103.178	386.167	-7.299	-242.815	157.719	-743.863	-1620.891	2906.365
	(0.879)	(0.223)	(1.139)	(0.083)	(-0.087)	(-0.092)	(0.305)	(-0.005)	(-0.167)	(0.081)	(-0.326)	(-0.411)	(0.534)
Borrowed family_fr ever	3648.285	2192.685	1931.598	720.622	-158.071	332.839	1350.549	1675.178	1403.151	2465.122	1603.623	-3104.365	3756.991
	(1.216)	(1.111)	(1.562)	(0.597)	(-0.125)	(0.245)	(0.989)	(1.092)	(0.862)	(1.264)	(0.787)	(-0.826)	(0.570)
Borrowed other ever	-2460.160	-1723.222	-321.310	225.764	523.371	531.830	-316.284	-377.391	-1671.738	-1486.103	-1560.052	-4382.053	991.067
	(-0.455)	(-0.513)	(-0.135)	(0.091)	(0.218)	(0.253)	(-0.157)	(-0.166)	(-0.563)	(-0.439)	(-0.294)	(-0.551)	(0.077)
Sum from Bank	-0.925	-0.575**	-0.245*	-0.217*	-0.171**	-0.167**	-0.154**	-0.049	0.018	0.096	0.169	0.532	0.947*
	(-1.548)	(-2.352)	(-1.739)	(-1.950)	(-2.001)	(-2.469)	(-2.195)	(-0.447)	(0.137)	(0.604)	(0.696)	(1.135)	(1.655)
Sum from NGO	7.668	5.971	1.533	1.402	1.516	1.570	0.922	1.375	0.138	0.080	0.647	1.760	2.822
	(1.235)	(0.958)	(0.368)	(0.385)	(0.513)	(0.639)	(0.380)	(0.599)	(0.066)	(0.038)	(0.333)	(0.616)	(0.684)
Sum from Moneylenders	-0.414*	-0.160*	-0.179***	-0.143**	-0.097	-0.043	-0.035	-0.061	-0.025	-0.035	-0.044	-0.064	-0.113
	(-1.924)	(-1.700)	(-2.677)	(-2.017)	(-1.448)	(-0.775)	(-0.907)	(-1.457)	(-0.558)	(-0.772)	(-0.906)	(-0.725)	(-0.924)
Sum from Family or Friends	-0.293	0.019	0.028	0.052	0.063	0.070	0.008	0.025	-0.002	-0.023	-0.065	0.059	-0.014
	(-0.919)	(0.078)	(0.324)	(0.753)	(0.929)	(1.242)	(0.132)	(0.394)	(-0.034)	(-0.268)	(-0.574)	(0.238)	(-0.043)
Other sources: Coops, etc	-0.943	-0.562	-0.594	-0.520	-0.476	-0.536	-0.354	-0.324	0.052	0.049	0.605	3.050*	2.745
	(-0.629)	(-1.037)	(-1.260)	(-1.202)	(-1.203)	(-1.355)	(-1.001)	(-0.657)	(0.053)	(0.038)	(0.392)	(1.767)	(1.625)
Sum from SHG or Vill. Orgs	0.395	0.223	0.244**	0.222**	0.198*	0.158	0.123	0.120	0.212	0.224	0.119	-0.164	0.673
	(1.218)	(1.250)	(2.311)	(2.135)	(1.858)	(1.495)	(0.887)	(0.725)	(1.249)	(1.286)	(0.689)	(-0.272)	(0.398)
Constant	-4.92e+04**	-1.32e+04	-6870.348	-6092.939	-6773.642	-1.09e+04	-8121.491	-1.82e+04**	-1.76e+04*	-1.98e+04*	-1.26e+04	20312.099	19850.874
	(-2.182)	(-0.933)	(-0.790)	(-0.776)	(-0.949)	(-1.416)	(-1.058)	(-2.054)	(-1.792)	(-1.766)	(-1.021)	(0.788)	(0.664)

t statistics in parenthesis; * p<0.10; ** p < .05; *** p < .01; Nr. of observations: 1,460. In yellow, blue and orange estimates that are significant at least at 10% level for loan size from SHG, bank and moneylenders respectively.

Table 90 Differenced variables Q-regression Income

	Δincome												
Quintiles	**0.05**	**0.1**	**0.2**	**0.25**	**0.3**	**0.4**	**0.5**	**0.6**	**0.7**	**0.75**	**0.8**	**0.9**	**0.95**
Δ Age HH head	315.870 (0.43)	-113.283 (-0.30)	-143.240 (-0.55)	-111.812 (-0.51)	-213.326 (-0.02)	-204.276 (-0.94)	-130.159 (-0.79)	-22.136 (-0.16)	126.738 (0.97)	81.397 (0.61)	79.001 (0.45)	1.923 (0.01)	56.032 (0.07)
Δ HH size	9773.562** (2.12)	5824.613** (2.14)	3804.112** (2.43)	4197.633*** (3.16)	3789.282*** (3.54)	3061.834*** (3.26)	2753.335*** (3.93)	2004.140*** (2.79)	2485.174*** (3.27)	2568.900*** (3.01)	3369.213*** (4.30)	4434.879*** (3.01)	8041.152** (2.40)
Δ shock	-8354.795 (-0.94)	-9150.711* (-1.80)	-5599.125** (-2.18)	-3959.185** (-2.32)	-4403.739 (-0.23)	-3408.622** (-2.27)	-3171.256*** (-2.89)	-4035.549*** (-3.25)	-5553.406*** (-3.50)	-5364.958*** (-3.36)	-5040.768*** (-3.13)	-5842.740* (-1.74)	-5657.751 (-0.85)
Δ asset index	12265.509*** (2.76)	7498.046*** (2.81)	3612.760** (2.39)	2979.523** (2.14)	3577.779 (1.50)	3122.889*** (3.16)	2178.397** (2.33)	1494.966 (1.63)	1414.739 (1.49)	1686.033 (1.48)	1908.256 (1.41)	1445.790 (0.75)	3255.832 (0.84)
Δbank sum	-0.237 (-0.66)	-0.350 (-1.35)	-0.091 (-0.43)	-0.104 (-0.56)	-0.111 (-0.04)	-0.063 (-0.51)	-0.014 (-0.14)	0.000 (0.00)	0.025 (0.26)	0.076 (0.73)	-0.047 (-0.37)	-0.100 (-0.61)	-0.107 (-0.34)
Δ NGO sum	0.118 (0.03)	0.333 (0.14)	0.664 (0.35)	0.777 (0.41)	0.764 (0.30)	0.842 (0.47)	0.912 (0.53)	0.943 (0.62)	1.031 (0.77)	1.069 (0.83)	-0.480 (-0.38)	0.157 (0.12)	0.728 (0.34)
Δ Moneylender sum	0.005 (0.02)	-0.051 (-0.31)	-0.015 (-0.17)	-0.042 (-0.60)	-0.061 (-0.02)	-0.077 (-1.18)	-0.094 (-1.60)	-0.118** (-2.46)	-0.078 (-1.50)	-0.086* (-1.72)	-0.076 (-1.40)	-0.128 (-1.64)	-0.325** (-2.08)
Δ family and friend sum	-0.131 (-0.43)	-0.138 (-0.74)	-0.102 (-0.95)	-0.085 (-1.03)	-0.038 (-0.01)	0.037 (0.53)	0.033 (0.52)	0.060 (0.94)	0.038 (0.58)	0.057 (0.73)	0.006 (0.07)	-0.032 (-0.25)	0.025 (0.12)
Δ Other sum	-0.333 (-0.71)	-0.298 (-0.93)	-0.132 (-0.54)	-0.066 (-0.30)	-0.194 (-0.14)	-0.116 (-0.56)	-0.084 (-0.40)	0.010 (0.04)	0.065 (0.21)	-0.001 (-0.00)	-0.331 (-0.74)	-0.045 (-0.07)	0.357 (0.45)
Δ SHG sum	0.124 (0.20)	0.227 (0.53)	0.472** (2.33)	0.406** (2.47)	0.366 (0.04)	0.270* (1.73)	0.267* (1.67)	0.303* (1.68)	0.453** (2.37)	0.435** (2.06)	0.474** (2.01)	0.430 (1.39)	0.958* (1.82)
Constant	-8.22e+04*** (-14.63)	-5.21e+04*** (-13.40)	-2.50e+04*** (-13.01)	-1.86e+04*** (-13.68)	-1.48e+04 (-0.24)	-5430.738*** (-4.77)	357.415 (0.43)	6411.616*** (6.88)	12815.272*** (14.31)	17035.290*** (13.92)	22202.370*** (17.03)	37718.722*** (15.16)	62894.393*** (12.84)

t statistics in parenthesis; * p<0.10; ** p < .05; *** p < .01; Nr. of observations: 1,460. In yellow, blue and orange estimates that are significant at least at 10% level for loan size from SHG, bank and moneylenders respectively.

Table 91 Differenced variables Q-regression Income (including 2005 level variables)

	Δincome												
Quantiles	0.05	0.1	0.2	0.25	0.3	0.4	0.5	0.6	0.7	0.75	0.8	0.9	0.95
Δ HHsize	5313.586**	4886.901***	4396.312***	3546.361***	3584.735***	4490.048***	4272.581***	3570.096***	4230.544***	4620.265***	5057.701***	5263.191***	5968.964**
	(2.223)	(2.655)	(3.149)	(2.983)	(3.193)	(4.154)	(4.191)	(3.310)	(4.431)	(4.763)	(4.640)	(3.227)	(2.278)
Δ shock	-422.647	-868.034	-3347.193	-1348.990	-1043.040	-247.764	-382.061	-1473.008	-704.374	-486.691	-985.225	1646.318	3307.443
	(-0.066)	(-0.196)	(-1.200)	(-0.588)	(-0.494)	(-0.125)	(-0.208)	(-0.747)	(-0.361)	(-0.232)	(-0.426)	(0.474)	(0.518)
Δeduc_max	-4518.451	-4359.638	1000.593	1144.812	611.133	-382.220	-260.621	-494.313	604.648	-204.671	-1185.526	946.375	1189.266
	(-0.730)	(-1.108)	(0.436)	(0.548)	(0.312)	(-0.204)	(-0.166)	(-0.313)	(0.376)	(-0.121)	(-0.598)	(0.255)	(0.193)
Δasset_indx	-8843.701	-3072.529	-1046.446	-84.008	136.702	-9.961	153.225	79.118	31.810	968.767	1403.535	944.888	7781.951*
	(-1.636)	(-1.107)	(-0.641)	(-0.054)	(0.107)	(-0.008)	(0.128)	(0.066)	(0.029)	(0.754)	(0.961)	(0.413)	(1.890)
Δworktime_pcw	-13.625**	-5.647	-7.387***	-6.260***	-5.735***	-4.767***	-3.637**	-3.857**	-2.703	-2.287	-1.485	-2.744	-5.038
	(-2.397)	(-1.373)	(-2.956)	(-2.589)	(-2.759)	(-2.591)	(-2.087)	(-2.279)	(-1.520)	(-1.229)	(-0.732)	(-0.922)	(-1.122)
Δbank_sum	-0.471	-0.706*	-0.328**	-0.347***	-0.378***	-0.290*	-0.211	-0.141	0.048	0.046	0.183	0.511	0.632
	(-0.930)	(-1.687)	(-2.406)	(-2.827)	(-2.879)	(-1.870)	(-1.402)	(-0.804)	(0.277)	(0.267)	(1.055)	(1.358)	(1.449)
ΔNGO_sum	-2.102	3.423	3.279	2.502	2.286	1.452	0.988	0.276	-0.077	-0.299	-0.367	4.260	-0.907
	(-0.306)	(0.607)	(0.726)	(0.744)	(0.786)	(0.534)	(0.340)	(0.093)	(-0.021)	(-0.079)	(-0.087)	(0.992)	(-0.195)
ΔmnΔlrd_sum	-1.164***	-0.884**	-0.152	-0.119	-0.092	-0.109	-0.032	-0.033	-0.027	-0.046	0.019	0.029	0.002
	(-3.622)	(-2.038)	(-0.820)	(-1.173)	(-1.246)	(-1.386)	(-0.426)	(-0.492)	(-0.390)	(-0.574)	(0.201)	(0.181)	(0.007)
Δfamily_fr_sum	-0.209	-0.233	-0.312	-0.225	-0.238	-0.119	-0.028	-0.005	0.005	-0.020	-0.033	0.191	0.084
	(-0.361)	(-0.782)	(-1.616)	(-1.245)	(-1.317)	(-0.686)	(-0.200)	(-0.036)	(0.043)	(-0.159)	(-0.214)	(0.819)	(0.295)
Δother_sum	-2.236**	-2.456*	-0.652	-0.804	-0.912	-0.946*	-0.742	-0.255	0.672	0.927	1.341	2.333*	2.221*
	(-1.988)	(-1.890)	(-0.735)	(-1.199)	(-1.633)	(-1.866)	(-0.951)	(-0.250)	(0.655)	(0.886)	(1.235)	(1.916)	(1.738)
ΔSHG_sum	0.114	-0.356	0.217	0.186	0.154	0.359	0.475*	0.539*	0.444	0.475	0.463	0.679	2.022
	(0.153)	(-0.646)	(0.788)	(0.834)	(0.745)	(1.545)	(1.695)	(1.817)	(1.524)	(1.628)	(1.485)	(0.839)	(1.490)
Prakasam	-5924.791	-3477.612	403.563	720.711	-585.130	2467.136	-740.930	-1997.468	2301.471	1936.412	2328.730	7092.658	10798.003
	(-0.669)	(-0.696)	(0.118)	(0.217)	(-0.181)	(0.806)	(-0.247)	(-0.651)	(0.725)	(0.589)	(0.582)	(0.787)	(0.807)
Kurnool	-3.57e+04***	-2.74e+04***	-1.59e+04***	-1.26e+04***	-1.10e+04***	-7936.285**	-1.05e+04***	-6729.667*	-4268.656	-2147.921	-3754.896	-1.32e+04**	-1.73e+04*
	(-3.527)	(-3.405)	(-3.409)	(-2.838)	(-2.748)	(-2.285)	(-2.776)	(-1.918)	(-1.305)	(-0.649)	(-1.030)	(-2.168)	(-1.818)
Anantapur	4718.269	12146.034**	11452.508***	12530.742***	10940.311***	10506.868***	5782.757*	3678.028	3688.708	3250.532	1536.489	-533.288	-2644.237
	(0.568)	(2.348)	(3.334)	(4.061)	(3.585)	(3.420)	(1.842)	(1.284)	(1.271)	(1.013)	(0.398)	(-0.074)	(-0.221)
Mahabub-Nagar	-3.19e+04**	-1.32e+04*	-8051.279**	-4691.102	-5522.402*	-4791.625	-4487.696	-2654.163	-4568.324	-4107.251	-5526.173	-1.20e+04*	-1.77e+04*
	(-2.492)	(-1.901)	(-1.986)	(-1.336)	(-1.702)	(-1.370)	(-1.182)	(-0.815)	(-1.631)	(-1.350)	(-1.545)	(-1.810)	(-1.732)
Household size	2204.642	541.743	-102.331	558.246	836.460	1753.400**	1920.961***	2072.967***	2839.414***	3143.037***	3528.574***	3368.718***	5120.399***
	(1.195)	(0.421)	(-0.126)	(0.798)	(1.199)	(2.504)	(3.024)	(3.078)	(4.820)	(5.097)	(5.146)	(2.951)	(2.687)
Suffered shock	13343.033	10391.228	3176.819	6245.380*	6355.795**	8398.985***	7997.362***	7103.865***	9778.156***	9813.930***	10021.621***	14390.857***	12025.784
	(1.383)	(1.560)	(0.837)	(1.947)	(2.220)	(2.930)	(2.818)	(2.744)	(3.569)	(3.366)	(3.023)	(2.805)	(1.535)
Max educ level in HH	-5379.977	-5577.327	-2180.543	-2621.792	-2370.456	-2830.310*	-1789.875	-2426.170*	-1686.386	-1847.496	-1711.574	-1932.624	-3873.647
	(-0.979)	(-1.509)	(-0.974)	(-1.352)	(-1.329)	(-1.892)	(-1.332)	(-1.669)	(-1.136)	(-1.169)	(-0.931)	(-0.603)	(-0.692)
Index of asset ownership	-2.95e+04***	-2.24e+04***	-1.32e+04***	-1.18e+04***	-9251.950***	-7819.691***	-5296.185***	-3446.852**	-3456.882**	-2514.786	-548.384	369.309	10917.274*
	(-4.016)	(-5.227)	(-5.995)	(-6.231)	(-5.211)	(-4.484)	(-3.356)	(-2.166)	(-2.249)	(-1.466)	(-0.278)	(0.112)	(1.764)
Worktime per working capita	-18.796**	-9.596**	-10.032***	-7.220***	-6.574***	-5.646***	-4.616**	-2.843	-1.317	0.053	1.123	2.351	0.338
	(-2.405)	(-2.016)	(-3.699)	(-2.757)	(-2.935)	(-2.912)	(-2.239)	(-1.406)	(-0.589)	(0.022)	(0.415)	(0.521)	(0.053)
Sum Bank	-1.116**	-1.292***	-0.596**	-0.571***	-0.533***	-0.460**	-0.384**	-0.201	-0.068	-0.025	0.216	0.831**	1.273**
	(-2.147)	(-2.713)	(-2.453)	(-2.765)	(-2.694)	(-2.470)	(-2.235)	(-1.028)	(-0.323)	(-0.110)	(0.864)	(2.146)	(2.031)
Sum NGO	-1.666	3.570	2.881	2.074	1.733	0.728	0.124	-0.651	-1.131	-1.397	-1.605	2.763	-2.781
	(-0.226)	(0.595)	(0.599)	(0.556)	(0.504)	(0.218)	(0.036)	(-0.188)	(-0.286)	(-0.349)	(-0.372)	(0.616)	(-0.551)
Sum Moneylenders	-1.657***	-1.030**	-0.198	-0.176	-0.167	-0.186*	-0.036	0.055	0.062	0.023	0.046	0.016	0.002
	(-3.180)	(-2.192)	(-0.997)	(-1.326)	(-1.527)	(-1.738)	(-0.354)	(0.544)	(0.622)	(0.217)	(0.372)	(0.071)	(0.009)
Sum Family	-0.004	-0.103	-0.243	-0.180	-0.207	-0.113	-0.043	-0.002	0.000	0.016	0.007	0.138	-0.141
	(-0.006)	(-0.325)	(-1.157)	(-0.918)	(-1.068)	(-0.601)	(-0.279)	(-0.011)	(0.002)	(0.102)	(0.036)	(0.502)	(-0.394)
Sum others	-2.690*	-2.133	-0.703	-0.847	-0.954	-0.850	-0.700	-0.236	0.678	1.439	1.897	3.411**	3.013*
	(-1.897)	(-1.620)	(-0.772)	(-1.198)	(-1.586)	(-1.547)	(-0.881)	(-0.228)	(0.640)	(1.273)	(1.601)	(2.478)	(1.880)
Sum SHG	-0.533	-0.227	-0.038	-0.110	-0.174	0.206	0.129	0.157	0.183	0.132	-0.051	0.137	0.866
	(-0.485)	(-0.354)	(-0.093)	(-0.299)	(-0.516)	(0.600)	(0.359)	(0.426)	(0.509)	(0.347)	(-0.125)	(0.160)	(0.548)
Constant	3615.004	4709.675	7036.789	2264.998	2525.246	1015.251	3075.214	5881.377	2743.843	1837.184	1767.786	13695.885	22286.221
	(0.292)	(0.540)	(1.324)	(0.468)	(0.585)	(0.235)	(0.697)	(1.360)	(0.606)	(0.369)	(0.313)	(1.550)	(1.578)

t statistics in parenthesis; * p<0.10; ** p < .05; *** p < .01; Nr. of observations: 1,460. In yellow, blue and orange estimates that are significant at least at 10% level for loan size from SHG, bank and moneylenders respectively.

Figure 26 Year 2007 Income

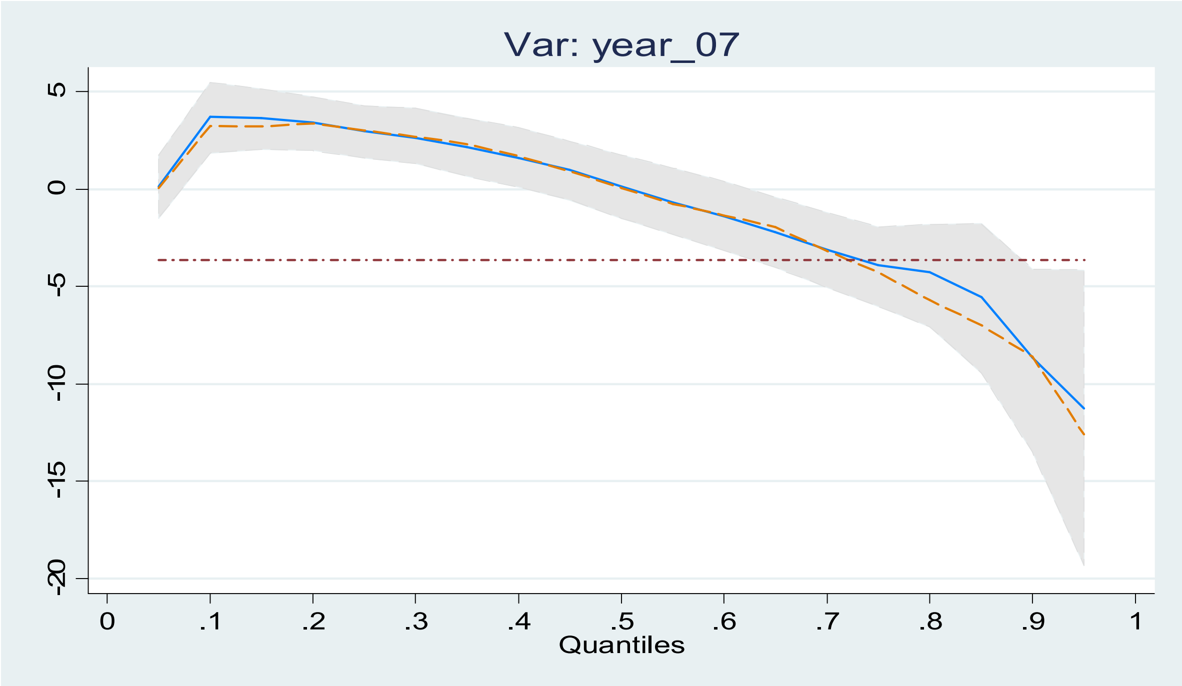


Figure 27 Household variables. Income

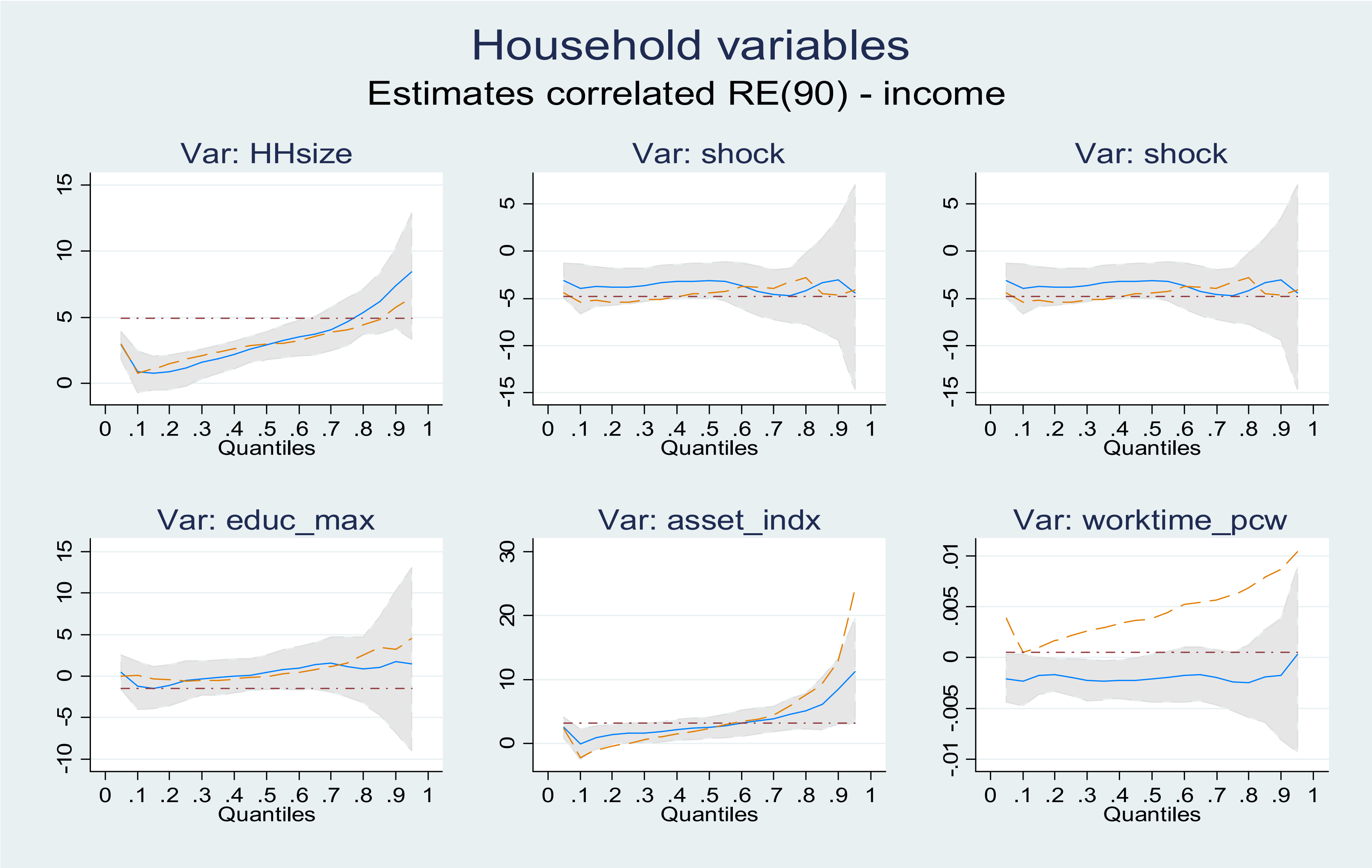


Figure 28 Loan size variables. Income

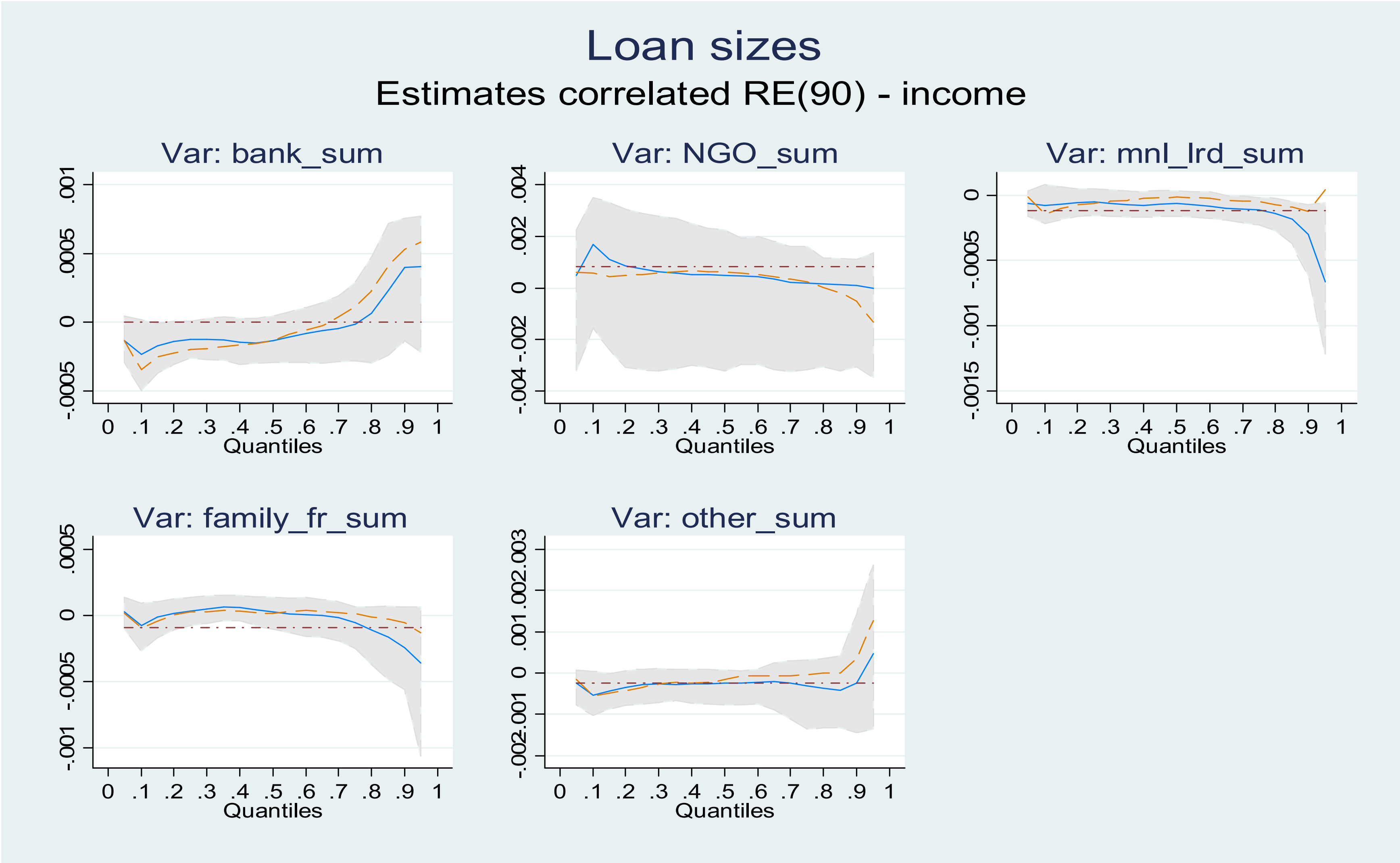


Figure 29 SHG loan size. Income

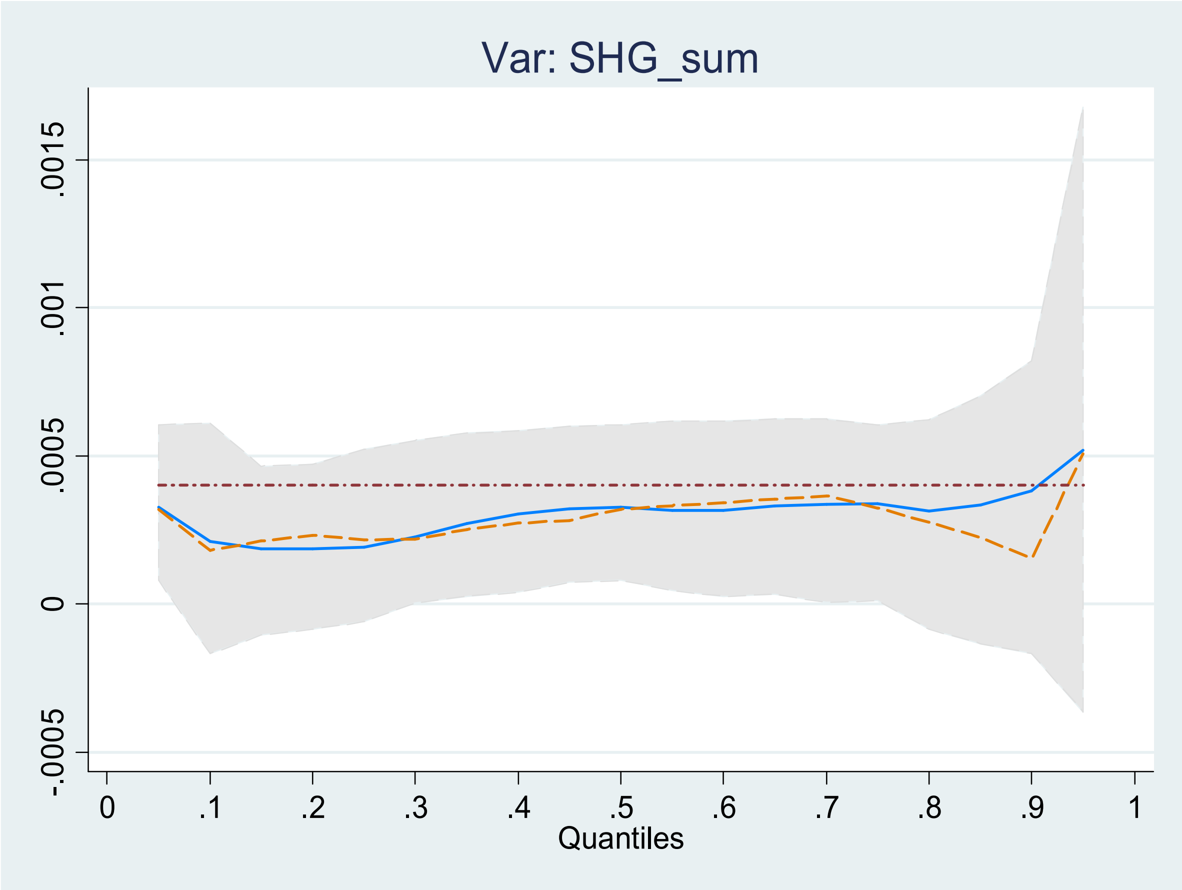


Table 92 CRE Income

	Correlated Random Effects Model Income																		
Quintiles	**0.05**	**0.1**	**0.15**	**0.2**	**0.25**	**0.3**	**0.35**	**0.4**	**0.45**	**0.5**	**0.55**	**0.6**	**0.65**	**0.7**	**0.75**	**0.8**	**0.85**	**0.9**	**0.95**
year 2007	129.01 0.13	3,697.81 3.43	3,640.82 3.91	3,403.93 4.09	2,971.14 3.60	2,638.06 3.07	2,166.77 2.38	1,613.13 1.78	986.36 1.08	129.01 0.13	- 699.15 - 0.69	-1,402.82 - 1.30	- 2,201.69 - 1.99	-3,126.47 - 2.70	-3,917.18 - 3.08	-4,256.77 - 2.67	-5,553.04 - 2.40	-8,657.48 - 3.02	-11,270.63 - 2.44
HH size	2,882.10 4.53	865.55 0.95	756.15 0.98	867.41 1.07	1,164.52 1.52	1,559.27 2.24	1,874.90 2.90	2,205.61 3.47	2,562.93 4.24	2,882.10 4.53	3,208.46 4.44	3,481.27 4.29	3,713.30 4.16	4,045.92 4.05	4,653.64 4.20	5,360.81 4.97	6,183.05 4.49	7,397.80 4.10	8,462.66 2.86
shock	-3,108.10 - 2.80	-3,987.75 - 2.56	-3,731.98 - 2.91	-3,779.82 - 3.18	-3,783.59 - 3.27	-3,669.95 - 3.50	-3,354.10 - 3.12	-3,228.38 - 2.92	-3,169.76 - 2.98	-3,108.10 - 2.80	-3,210.65 - 2.57	-3,665.70 - 2.47	- 4,251.09 - 2.71	-4,609.63 - 2.75	-4,695.60 - 2.61	-4,197.41 - 1.84	-3,356.46 - 1.12	-3,059.03 - 0.79	- 4,383.74 - 0.67
Max education	406.49 0.32	-1,240.48 - 0.70	-1,506.84 - 0.99	-1,133.37 - 0.76	- 558.50 - 0.40	- 329.19 - 0.26	- 175.17 - 0.14	25.15 0.02	105.31 0.09	406.49 0.32	764.29 0.54	977.76 0.60	1,406.11 0.81	1,535.28 0.77	1,109.07 0.50	880.47 0.35	1,046.53 0.30	1,744.24 0.36	1,522.88 0.23
Asset index	2,490.43 2.45	- 77.58 - 0.05	870.83 0.83	1,326.04 1.40	1,582.16 1.65	1,566.78 1.71	1,794.66 1.89	2,165.09 2.23	2,362.97 2.34	2,490.43 2.45	2,740.97 2.51	3,149.43 2.64	3,523.34 3.01	3,842.78 3.06	4,561.70 2.99	5,138.27 3.03	6,098.31 2.50	8,471.38 2.70	11,170.96 2.22
worktime p_c_working	-2.1 - 1.46	-2.2907 - 1.51	-1.7185 - 1.54	-1.6744 - 1.67	-1.9853 - 1.75	-2.2685 - 1.87	-2.317 - 1.99	-2.2741 - 1.95	-2.2596 - 1.76	-2.1 - 1.46	-1.9422 - 1.29	-1.752 - 1.10	-1.6631 - 1.07	-1.9573 - 1.16	-2.382 - 1.32	-2.4501 - 1.11	-1.8629 - 0.65	-1.7723 - 0.48	0.301 0.05
sum from bank	-0.136 - 1.30	-0.2334 - 1.50	-0.1731 - 1.57	-0.1383 - 1.45	-0.1232 - 1.43	-0.1256 - 1.37	-0.1292 - 1.30	-0.1428 - 1.38	-0.1496 - 1.47	-0.136 - 1.30	-0.1086 - 0.95	-0.0796 - 0.64	-0.0587 - 0.44	-0.0451 - 0.30	-0.0149 - 0.08	0.0632 0.27	0.2313 0.83	0.4006 1.36	0.4065 1.24
sum from NGO	0.4922 0.35	1.7049 0.94	1.1031 0.64	0.8628 0.54	0.7541 0.50	0.6353 0.45	0.5671 0.41	0.5239 0.38	0.5146 0.37	0.4922 0.35	0.4524 0.32	0.4321 0.31	0.3477 0.24	0.2204 0.15	0.1713 0.12	0.1614 0.11	0.1268 0.09	0.1099 0.07	0.000834 0.00
sum moneylender	-0.0635 - 1.05	-0.0771 - 0.84	-0.0687 - 0.88	-0.0572 - 0.90	-0.0528 - 0.86	-0.061 - 1.03	-0.0743 - 1.26	-0.0762 - 1.32	-0.068 - 1.13	-0.0635 - 1.05	-0.0734 - 1.23	-0.0854 - 1.46	-0.0985 - 1.74	-0.109 - 1.84	-0.1145 - 1.79	-0.141 - 1.86	-0.1846 - 1.88	-0.2974 - 1.78	-0.6625 - 1.80
sum family friend	0.0242 0.33	-0.0756 - 0.69	-0.0143 - 0.16	0.0179 0.27	0.0315 0.49	0.0493 0.81	0.0632 1.10	0.0606 1.02	0.0428 0.63	0.0242 0.33	0.0105 0.13	0.00452 0.05	-0.000964 - 0.01	-0.0187 - 0.20	-0.0548 - 0.51	-0.1084 - 0.74	-0.1647 - 0.90	-0.2481 - 1.29	-0.36 - 1.04
sum others	-0.2514 - 0.94	-0.5334 - 1.71	-0.4385 - 1.67	-0.343 - 1.36	-0.2837 - 1.07	-0.2651 - 1.01	-0.2733 - 1.06	-0.2698 - 1.02	-0.2572 - 0.96	-0.2514 - 0.94	-0.2424 - 0.96	-0.2225 - 0.85	-0.2097 - 0.65	-0.2439 - 0.57	-0.3189 - 0.62	-0.3751 - 0.68	-0.4204 - 0.68	-0.2481 - 0.29	0.4766 0.37
sum SHGs	0.3262 1.86	0.2111 0.89	0.1879 1.07	0.1871 1.12	0.1912 1.14	0.2271 1.35	0.2717 1.57	0.3034 1.74	0.3215 1.87	0.3262 1.86	0.3172 1.76	0.3176 1.77	0.331 1.83	0.3362 1.81	0.3383 1.85	0.3147 1.48	0.3355 1.34	0.3833 1.25	0.5197 0.85
constant	- 853.81 - 0.48	-2,489.09 - 1.21	-1,937.03 - 1.03	-1,675.29 - 0.99	-1,870.94 - 1.10	-2,260.97 - 1.37	-2,155.70 - 1.36	-1,667.03 - 1.06	-1,290.21 - 0.78	- 853.81 - 0.48	-1,082.50 - 0.54	-1,334.26 - 0.67	- 1,320.49 - 0.62	-1,388.02 - 0.64	-1,047.77 - 0.47	- 947.59 0.36	470.96 0.13	4,933.81 1.15	9,494.55 1.45

Nr. of observations: 2,907. In yellow, blue and orange estimates that are significant at least at 10% level for loan size from SHG, bank and moneylenders respectively.

Table 93 λ parameter. Income per capita 2005

λ parameters CRE 05 INCOME PC																			
Quintiles	**0.05**	**0.1**	**0.15**	**0.2**	**0.25**	**0.3**	**0.35**	**0.4**	**0.45**	**0.5**	**0.55**	**0.6**	**0.65**	**0.7**	**0.75**	**0.8**	**0.85**	**0.9**	**0.95**
HH size	-115.186	-39.600	-12.145	3.454	13.672	9.729	-17.184	-56.046	-94.293	-115.186	-127.228	-133.291	-115.694	-115.223	-146.725	-213.876	-313.577	-500.879	-845.037
	-1.077	-0.275	-0.102	0.031	0.118	0.080	-0.142	-0.486	-0.840	-1.077	-1.130	-0.979	-0.771	-0.745	-0.948	-1.205	-1.471	-1.948	-1.737
shock	-320.373	-399.672	-433.615	-400.847	-388.190	-400.997	-426.889	-449.009	-398.131	-320.373	-253.854	-194.352	-105.630	-40.677	129.365	287.225	355.928	649.292	1064.124
	-1.435	-1.278	-1.602	-1.748	-1.616	-1.712	-1.911	-2.215	-1.850	-1.435	-1.015	-0.699	-0.357	-0.121	0.319	0.597	0.657	1.050	0.867
Max education	295.686	543.444	490.940	379.518	287.269	276.255	275.411	259.203	268.420	295.686	348.125	397.483	382.916	348.682	412.396	610.945	665.793	526.865	-104.035
	1.490	1.593	1.839	1.572	1.156	1.075	1.064	1.074	1.249	1.490	1.579	1.639	1.510	1.202	1.198	1.454	1.260	0.809	-0.091
Asset index	-67.902	-497.525	-489.513	-478.345	-433.739	-349.089	-267.663	-199.548	-124.517	-67.902	-51.346	-39.982	-6.066	41.351	105.350	256.144	488.329	723.784	1717.427
	-0.417	-2.306	-2.355	-2.613	-2.521	-2.134	-1.642	-1.265	-0.774	-0.417	-0.307	-0.232	-0.033	0.195	0.407	0.809	1.346	1.489	1.703
worktime p_c_working	1.094	0.511	0.651	0.741	0.810	0.873	0.928	0.964	1.019	1.094	1.161	1.215	1.335	1.510	1.746	1.879	2.057	2.570	2.771
	4.270	1.648	2.722	3.128	3.213	3.625	4.032	4.279	4.257	4.270	3.967	4.053	4.108	4.046	4.535	4.705	4.629	3.963	2.748
sum from bank	-0.005	-0.029	-0.023	-0.021	-0.019	-0.015	-0.011	-0.008	-0.005	-0.005	-0.003	0.000	0.005	0.009	0.013	0.015	0.017	0.017	0.047
	-0.257	-1.145	-1.255	-1.264	-1.101	-0.803	-0.578	-0.420	-0.266	-0.257	-0.184	-0.013	0.274	0.498	0.647	0.589	0.515	0.395	0.645
sum from NGO	-0.034	-0.304	-0.189	-0.137	-0.113	-0.075	-0.056	-0.046	-0.041	-0.034	-0.027	-0.028	-0.018	-0.012	-0.019	-0.056	-0.119	-0.209	-0.373
	-0.105	-0.632	-0.423	-0.328	-0.277	-0.201	-0.160	-0.135	-0.126	-0.105	-0.087	-0.093	-0.066	-0.049	-0.077	-0.257	-0.538	-0.833	-0.849
sum moneylender	0.006	-0.019	-0.006	-0.002	-0.002	-0.002	0.000	0.002	0.003	0.006	0.007	0.007	0.008	0.009	0.012	0.018	0.026	0.036	0.113
	0.681	-1.080	-0.397	-0.210	-0.205	-0.156	-0.008	0.177	0.382	0.681	0.778	0.746	0.811	0.823	0.821	1.035	1.489	1.592	1.493
sum family friend	-0.005	-0.011	-0.013	-0.016	-0.015	-0.014	-0.014	-0.010	-0.006	-0.005	-0.005	-0.006	-0.007	-0.005	-0.003	-0.001	0.003	0.012	0.069
	-0.390	-0.608	-0.853	-1.098	-1.071	-1.037	-0.971	-0.671	-0.462	-0.390	-0.413	-0.456	-0.410	-0.296	-0.192	-0.056	0.084	0.226	0.758
sum others	0.023	0.004	-0.009	-0.011	-0.001	0.008	0.011	0.013	0.017	0.023	0.029	0.027	0.025	0.031	0.036	0.045	0.050	0.030	-0.038
	0.833	0.098	-0.288	-0.346	-0.044	0.331	0.621	0.697	0.722	0.833	0.963	0.826	0.588	0.542	0.487	0.487	0.517	0.355	-0.326
sum SHGs	0.016	-0.016	0.002	0.011	0.015	0.014	0.013	0.014	0.015	0.016	0.015	0.011	0.005	0.000	-0.005	-0.016	-0.040	-0.060	-0.053
	0.552	-0.223	0.045	0.323	0.573	0.568	0.468	0.475	0.537	0.552	0.535	0.394	0.182	-0.010	-0.177	-0.555	-1.179	-1.395	-0.573

Nr. of observations: 2,907. In red, estimates significant at least at 10% level..

Table 94 λ parameter. Income per capita 2007

λ parameters CRE 07 INCOME PC																			
Quintiles	**0.05**	**0.1**	**0.15**	**0.2**	**0.25**	**0.3**	**0.35**	**0.4**	**0.45**	**0.5**	**0.55**	**0.6**	**0.65**	**0.7**	**0.75**	**0.8**	**0.85**	**0.9**	**0.95**
HH size	61.423	106.715	122.302	107.771	70.108	50.285	46.221	55.520	58.772	61.423	75.495	72.737	36.983	11.143	0.147	-17.956	-60.067	-80.150	-12.877
	0.535	0.632	0.790	0.720	0.535	0.463	0.488	0.623	0.594	0.535	0.599	0.506	0.227	0.060	0.001	-0.096	-0.299	-0.307	-0.034
shock	-203.186	-252.606	-201.998	-215.120	-221.857	-248.756	-306.462	-296.918	-234.152	-203.186	-140.171	-17.114	126.428	218.185	266.355	182.579	-145.787	-294.089	-605.401
	-0.709	-0.798	-0.726	-0.857	-0.968	-1.132	-1.278	-1.191	-0.887	-0.709	-0.427	-0.049	0.355	0.574	0.625	0.338	-0.231	-0.334	-0.455
Max education	-480.117	-134.964	-90.636	-162.841	-216.710	-211.034	-276.710	-398.453	-473.206	-480.117	-436.916	-440.292	-521.542	-665.820	-724.144	-640.079	-422.652	43.075	-252.938
	-1.917	-0.463	-0.355	-0.585	-0.789	-0.872	-1.285	-1.916	-1.994	-1.917	-1.510	-1.399	-1.578	-1.778	-1.741	-1.412	-0.664	0.052	-0.200
Asset index	-96.066	-49.938	-114.805	-147.696	-101.144	-40.090	-35.571	-72.425	-94.737	-96.066	-82.583	-49.596	-26.971	-18.787	66.411	147.563	132.063	103.928	-57.810
	-0.484	-0.244	-0.602	-0.814	-0.586	-0.231	-0.204	-0.415	-0.520	-0.484	-0.372	-0.222	-0.124	-0.073	0.220	0.463	0.365	0.224	-0.063
worktime p_c_working	1.440	0.562	0.685	0.860	1.006	1.119	1.235	1.313	1.383	1.440	1.519	1.583	1.636	1.756	1.876	1.825	1.699	1.580	1.803
	5.402	2.081	2.655	3.600	4.267	4.689	5.247	5.605	5.246	5.402	5.346	5.245	4.983	4.920	4.651	3.860	2.955	1.831	1.260
sum from bank	0.010	-0.016	-0.011	-0.008	-0.007	-0.005	0.001	0.008	0.011	0.010	0.008	0.007	0.012	0.019	0.024	0.022	0.016	0.021	0.016
	0.598	-0.690	-0.621	-0.531	-0.474	-0.312	0.048	0.507	0.703	0.598	0.428	0.354	0.475	0.676	0.871	0.730	0.435	0.434	0.280
sum from NGO	0.090	-0.127	0.078	0.177	0.204	0.205	0.176	0.143	0.116	0.090	0.051	0.003	-0.023	-0.036	-0.037	-0.051	-0.076	-0.109	-0.396
	0.410	-0.161	0.123	0.381	0.575	0.767	0.726	0.658	0.549	0.410	0.240	0.012	-0.077	-0.106	-0.097	-0.114	-0.137	-0.186	-0.543
sum moneylender	0.005	-0.014	-0.011	-0.007	-0.002	0.002	0.003	0.005	0.005	0.005	0.005	0.005	0.006	0.007	0.008	0.015	0.026	0.043	0.067
	0.426	-0.896	-0.956	-0.710	-0.183	0.192	0.363	0.462	0.437	0.426	0.424	0.454	0.608	0.671	0.620	0.737	1.036	1.267	1.222
sum family friend	-0.002	0.002	0.002	0.002	0.000	-0.002	-0.004	-0.005	-0.004	-0.002	-0.002	-0.002	0.002	0.015	0.026	0.035	0.042	0.055	0.111
	-0.131	0.140	0.148	0.135	0.004	-0.197	-0.379	-0.380	-0.239	-0.131	-0.099	-0.093	0.086	0.576	0.953	1.142	1.272	1.434	1.218
sum others	0.018	-0.061	-0.035	-0.024	-0.025	-0.023	-0.014	-0.003	0.007	0.018	0.029	0.038	0.051	0.076	0.101	0.117	0.130	0.159	0.229
	0.312	-0.733	-0.514	-0.415	-0.456	-0.401	-0.228	-0.051	0.112	0.312	0.545	0.718	0.879	1.080	1.365	1.593	1.391	0.913	0.742
sum SHGs	-0.028	0.009	0.005	0.003	-0.003	-0.014	-0.020	-0.025	-0.027	-0.028	-0.027	-0.027	-0.029	-0.031	-0.033	-0.031	-0.038	-0.030	0.138
	-1.060	0.220	0.165	0.095	-0.132	-0.547	-0.803	-0.965	-1.031	-1.060	-1.031	-1.099	-1.173	-1.094	-0.849	-0.616	-0.626	-0.385	0.735

Nr. of observations: 2,907. In red, estimates significant at least at 10% level..

Table 95 λ parameter. Income 2005

λ parameters CRE 05 INCOME																			
Quintiles	**0.05**	**0.1**	**0.15**	**0.2**	**0.25**	**0.3**	**0.35**	**0.4**	**0.45**	**0.5**	**0.55**	**0.6**	**0.65**	**0.7**	**0.75**	**0.8**	**0.85**	**0.9**	**0.95**
HH size	-528.050	-515.429	-265.337	-176.156	-118.814	-126.618	-207.182	-344.978	-450.033	-528.050	-596.284	-637.990	-608.731	-590.973	-700.917	-869.159	-1097.679	-1394.202	-1470.419
	-1.435	-0.827	-0.580	-0.402	-0.289	-0.321	-0.507	-0.838	-1.153	-1.435	-1.467	-1.463	-1.336	-1.201	-1.279	-1.499	-1.498	-1.231	-0.646
shock	-1943.396	-1972.395	-2025.883	-1962.991	-1942.904	-1907.732	-2063.798	-2208.255	-2221.761	-1943.396	-1677.490	-1271.526	-854.918	-482.079	73.706	394.761	-389.167	-1034.675	945.236
	-2.093	-1.370	-1.686	-1.956	-1.985	-2.054	-2.280	-2.503	-2.537	-2.093	-1.583	-1.120	-0.686	-0.375	0.048	0.212	-0.158	-0.422	0.233
Max education	1318.774	2246.519	1893.922	1517.642	1206.589	1101.235	1073.717	1094.792	1260.872	1318.774	1488.543	1671.018	1541.225	1456.206	1618.784	1689.152	1761.500	863.889	-1878.888
	1.498	1.452	1.590	1.516	1.237	1.121	1.073	1.074	1.369	1.498	1.576	1.569	1.351	1.328	1.351	0.962	0.741	0.293	-0.426
Asset index	-257.794	-2629.307	-2412.416	-2135.150	-2007.487	-1701.077	-1354.918	-1149.809	-804.396	-257.794	88.962	135.439	179.414	391.973	466.416	897.883	2228.638	4293.223	9604.452
	-0.350	-2.290	-2.640	-2.490	-2.372	-2.246	-1.879	-1.706	-1.186	-0.350	0.116	0.181	0.235	0.449	0.437	0.643	1.208	1.792	2.222
worktime p_c_working	4.214	2.534	2.680	2.846	3.143	3.433	3.590	3.779	4.005	4.214	4.617	5.051	5.452	5.956	6.680	7.593	8.718	10.014	11.121
	4.027	1.990	2.923	3.384	3.595	4.395	4.804	4.438	4.316	4.027	3.792	3.995	4.335	4.533	4.623	4.721	4.824	4.359	2.996
sum from bank	-0.017	-0.085	-0.073	-0.072	-0.079	-0.073	-0.054	-0.034	-0.022	-0.017	-0.007	0.013	0.035	0.054	0.077	0.110	0.134	0.089	0.056
	-0.204	-0.718	-0.883	-0.978	-1.097	-0.888	-0.648	-0.418	-0.278	-0.204	-0.080	0.149	0.426	0.638	0.756	0.868	0.828	0.448	0.205
sum from NGO	-0.041	-1.074	-0.626	-0.401	-0.307	-0.191	-0.138	-0.097	-0.088	-0.041	-0.047	-0.092	-0.075	-0.053	-0.123	-0.339	-0.645	-1.097	-1.997
	-0.029	-0.505	-0.321	-0.216	-0.172	-0.117	-0.088	-0.064	-0.060	-0.029	-0.035	-0.071	-0.062	-0.050	-0.125	-0.382	-0.672	-0.903	-1.052
sum moneylender	0.037	-0.093	-0.029	-0.012	-0.002	0.007	0.013	0.021	0.030	0.037	0.043	0.045	0.047	0.049	0.048	0.058	0.075	0.155	0.668
	0.968	-0.922	-0.377	-0.217	-0.047	0.134	0.270	0.465	0.739	0.968	1.083	1.124	1.167	1.141	0.968	0.951	1.073	1.282	1.650
sum family friend	-0.014	-0.047	-0.053	-0.065	-0.060	-0.053	-0.050	-0.039	-0.022	-0.014	-0.014	-0.015	-0.014	-0.009	0.004	0.035	0.073	0.117	0.111
	-0.249	-0.497	-0.685	-0.960	-0.878	-0.810	-0.802	-0.592	-0.366	-0.249	-0.247	-0.243	-0.190	-0.118	0.050	0.289	0.461	0.668	0.368
sum others	0.061	0.017	0.006	-0.016	-0.005	0.025	0.037	0.035	0.042	0.061	0.085	0.091	0.101	0.138	0.197	0.249	0.285	0.221	0.011
	0.538	0.069	0.037	-0.108	-0.032	0.189	0.403	0.465	0.472	0.538	0.593	0.493	0.382	0.378	0.441	0.521	0.591	0.464	0.020
sum SHGs	0.078	-0.103	0.018	0.070	0.083	0.076	0.066	0.064	0.071	0.078	0.076	0.062	0.037	0.004	-0.031	-0.061	-0.129	-0.261	-0.507
	0.578	-0.260	0.068	0.427	0.653	0.635	0.519	0.486	0.524	0.578	0.583	0.518	0.312	0.034	-0.252	-0.468	-0.800	-1.469	-1.666

Nr. of observations: 2,907. In red, estimates significant at least at 10% level..

Table 96 λ parameter. Income 2007

λ parameters CRE 07 INCOME																			
Quintiles	0.05	0.1	0.15	0.2	0.25	0.3	0.35	0.4	0.45	0.5	0.55	0.6	0.65	0.7	0.75	0.8	0.85	0.9	0.95
HH size	541.187	614.137	764.726	851.257	752.564	655.958	673.193	677.911	617.943	541.187	437.238	351.779	267.384	155.708	-137.609	-502.752	-795.930	-1322.032	-2086.489
	0.965	0.842	1.147	1.187	1.127	1.120	1.272	1.314	1.201	0.965	0.690	0.475	0.330	0.166	-0.133	-0.504	-0.636	-0.871	-1.089
shock	-1139.053	-1095.136	-1048.822	-1066.993	-936.929	-985.874	-1110.872	-1220.609	-1234.430	-1139.053	-737.136	180.041	1023.900	1404.763	1481.451	1383.082	381.484	-451.959	-77.470
	-0.952	-0.785	-0.843	-0.925	-0.888	-0.955	-1.094	-1.164	-1.145	-0.952	-0.548	0.120	0.674	0.902	0.904	0.665	0.134	-0.121	-0.013
Max education	-1551.349	-557.052	-369.074	-548.356	-723.040	-813.699	-947.859	-1103.347	-1227.651	-1551.349	-1861.973	-2061.694	-2211.600	-2156.682	-1815.431	-1359.730	-1052.621	-1166.549	2136.720
	-1.629	-0.381	-0.290	-0.423	-0.605	-0.783	-1.026	-1.275	-1.421	-1.629	-1.610	-1.518	-1.479	-1.237	-0.939	-0.691	-0.382	-0.284	0.391
Asset index	-273.575	-323.420	-526.193	-695.923	-512.295	-139.721	-57.137	-272.698	-374.961	-273.575	-188.422	-207.289	-227.730	-59.841	472.857	1283.785	1492.606	993.021	2327.817
	-0.316	-0.308	-0.635	-0.930	-0.637	-0.171	-0.071	-0.327	-0.426	-0.316	-0.203	-0.192	-0.206	-0.052	0.356	0.879	0.744	0.401	0.655
worktime p_c_working	5.080	2.363	2.607	3.172	3.808	4.491	4.940	5.095	5.109	5.080	5.313	5.716	6.249	7.071	7.639	7.630	7.014	7.635	6.385
	3.990	2.190	2.785	3.458	3.685	3.940	4.557	4.740	4.423	3.990	4.076	4.184	4.272	4.392	4.600	3.739	2.599	2.105	1.237
sum from bank	0.022	-0.082	-0.050	-0.033	-0.033	-0.031	-0.025	0.002	0.026	0.022	0.011	0.014	0.035	0.081	0.109	0.119	0.107	0.161	0.239
	0.270	-0.773	-0.562	-0.428	-0.484	-0.434	-0.313	0.023	0.312	0.270	0.129	0.147	0.300	0.577	0.793	0.850	0.599	0.610	0.825
sum from NGO	0.670	0.169	0.979	1.344	1.412	1.356	1.152	0.956	0.758	0.670	0.514	0.269	0.091	0.001	-0.086	-0.334	-0.508	-0.930	-1.952
	0.644	0.049	0.353	0.660	0.917	1.188	1.125	1.076	0.812	0.644	0.519	0.227	0.063	0.001	-0.048	-0.169	-0.219	-0.345	-0.547
sum moneylender	0.025	-0.041	-0.059	-0.048	-0.022	-0.005	0.004	0.013	0.019	0.025	0.031	0.035	0.037	0.041	0.043	0.062	0.103	0.211	0.593
	0.430	-0.648	-1.225	-0.952	-0.407	-0.106	0.093	0.250	0.332	0.430	0.553	0.647	0.724	0.785	0.715	0.805	0.914	1.094	1.876
sum family friend	-0.018	0.019	0.022	0.019	0.003	-0.014	-0.031	-0.038	-0.032	-0.018	-0.002	0.009	0.022	0.053	0.090	0.142	0.234	0.320	0.468
	-0.213	0.260	0.375	0.340	0.058	-0.250	-0.574	-0.631	-0.435	-0.213	-0.019	0.090	0.214	0.470	0.691	0.781	1.110	1.820	1.221
sum others	0.078	-0.177	-0.161	-0.159	-0.141	-0.098	-0.052	0.003	0.041	0.078	0.124	0.187	0.298	0.449	0.659	0.738	0.899	1.159	0.979
	0.249	-0.615	-0.634	-0.616	-0.512	-0.345	-0.178	0.011	0.134	0.249	0.396	0.582	0.873	1.113	1.590	1.795	1.379	1.587	1.143
sum SHGs	-0.123	0.009	-0.014	-0.014	-0.011	-0.047	-0.092	-0.116	-0.126	-0.123	-0.108	-0.105	-0.111	-0.110	-0.116	-0.125	-0.162	-0.122	0.415
	-0.958	0.046	-0.087	-0.106	-0.083	-0.381	-0.764	-0.921	-0.979	-0.958	-0.845	-0.829	-0.902	-0.805	-0.693	-0.569	-0.586	-0.358	0.556

Nr. of observations: 2,907. In red, estimates significant at least at 10% level..

Table 97 Cross sectional Q-regression Income per capita 2005. Negative income per capita observations withdrawn.

	Q-regression cross sectional 2005 income per capita					Q-regression cross sectional 2007 income per capita				
Source \ Quintiles	**0.1**	**0.25**	**0.5**	**0.75**	**0.9**	**0.1**	**0.25**	**0.5**	**0.75**	**0.9**
banks	-0.014	0.017	0.047	0.146***	0.191***	-0.014*	-0.004	0.020	0.097*	0.203**
	(-0.677)	(0.891)	(1.195)	(3.176)	(3.024)	(-1.750)	(-0.298)	(0.814)	(1.793)	(2.418)
NGO	0.199	0.292	0.269	0.242	0.192	0.527	0.370	0.291	0.227	0.511
	(0.736)	(0.812)	(0.499)	(0.414)	(0.233)	(1.610)	(1.413)	(0.700)	(0.572)	(0.679)
Moneylender	0.006	0.005	0.019	0.006	0.025	0.012*	0.003	0.000	-0.004	-0.020
z-scores	(1.804)	(0.511)	(1.511)	(0.521)	(0.269)	(1.851)	(0.734)	(0.064)	(-0.280)	(-1.169)
Family and frieds	0.045	0.016	0.028**	0.012	0.025	0.016*	0.019**	0.012	0.024	0.029
	(0.779)	(1.168)	(2.125)	(0.553)	(0.389)	(1.661)	(2.436)	(1.128)	(0.808)	(0.716)
Other sources	-0.006	0.005	0.038	0.003	-0.009	0.010	-0.018	-0.025	0.210	0.742**
	(1.043)	(0.127)	(0.687)	(0.047)	(-0.064)	(0.312)	(-0.381)	(-0.254)	(0.707)	(2.115)
SHGs	0.005	0.097***	0.081**	0.060	0.039	0.036*	0.040*	0.029	0.027	0.038
	(0.952)	(3.058)	(2.469)	(1.114)	(0.639)	(1.646)	(1.663)	(0.867)	(0.704)	(0.296)

t statistics in parenthesis; * p<0.10; ** p < .05; *** p < .01; Nr. of observations: 1,256 for 2005 and 1,311 for 2007.

Table 98 Differenced variables Q-regression Income per Capita. Negative income per capita observations withdrawn.

	Δ income per capita with no 2005 levels					Δ income per capita with 2005 levels				
Source \ Quintiles	**0.1**	**0.25**	**0.5**	**0.75**	**0.9**	**0.1**	**0.25**	**0.5**	**0.75**	**0.9**
banks	0.083*	0.032	0.020	0.027	0.056	-0.140**	-0.102	-0.074*	0.055	0.173*
	(1.681)	(0.979)	(0.774)	(0.860)	(0.522)	(-1.967)	(-1.499)	(-1.825)	(0.685)	(1.840)
NGO	0.086	0.201	0.236	0.263	-0.343	1.698	0.034	-0.274	-0.522	-0.187
	(0.151)	(0.422)	(0.634)	(1.008)	(0.921)	(0.192)	(0.014)	(-0.065)	(-0.237)	(-0.059)
Moneylender	0.003	-0.006	-0.012	-0.012	-0.011	-0.067	-0.012	0.031	0.036	0.068*
	(0.135)	(-0.416)	(-0.994)	(-0.992)	(-1.738)	(-0.558)	(-0.389)	(1.366)	(1.184)	(1.678)
Family and frieds	-0.013	-0.004	0.016	0.012	0.004	-0.034	-0.039	-0.044	-0.025	0.008
	(-0.501)	(-0.282)	(1.459)	(0.749)	(0.137)	(-0.459)	(-0.986)	(-1.087)	(-0.498)	(0.117)
Other sources	0.074	-0.004	0.006	0.037	0.072	0.221	-0.042	-0.075	0.215	0.694
	(0.918)	(-0.087)	(0.312)	(0.930)	(0.962)	(0.681)	(-0.217)	(-0.267)	(0.552)	(1.639)
SHGs	0.101**	0.101***	0.075**	0.129***	0.125	-0.149	-0.048	-0.092	-0.078	-0.116
	(2.085)	(4.340)	(2.421)	(3.413)	(2.463)	(-0.704)	(-0.548)	(-1.293)	(-0.870)	(-0.619)

t statistics in parenthesis; * p<0.10; ** p < .05; *** p < .01; Nr. of observations: 1,138

Table 99 Cross sectional Q-regression Income 2005. Negative income observations withdrawn.

	Q-regression cross sectional 2005 income					Q-regression cross sectional 2007 income				
Source \ Quintiles	**0.1**	**0.25**	**0.5**	**0.75**	**0.9**	**0.1**	**0.25**	**0.5**	**0.75**	**0.9**
banks	0.033	0.052	0.251	0.755***	0.786**	-0.090**	-0.047	0.093	0.306	0.915*
z-scores	(-0.641)	(0.563)	(1.240)	(4.291)	(2.114)	(-2.190)	(-0.660)	(0.744)	(1.253)	(1.729)
NGO	1.423	1.149	1.063	0.995	0.774	3.002	2.462	1.622	1.187	2.138
z-scores	(0.506)	(0.470)	(0.449)	(0.431)	(0.221)	(1.468)	(1.556)	(0.881)	(0.602)	(0.683)
Moneylender	0.036	0.017	0.054	0.025	-0.020	0.064**	0.027	-0.007	-0.007	-0.071
z-scores	(1.641)	(0.436)	(1.020)	(0.430)	(-0.050)	(2.292)	(1.218)	(-0.254)	(-0.122)	(-0.811)
Family & Friends	0.113	0.073	0.108*	0.059	-0.029	0.072	0.104**	0.061	-0.042	0.101
z-scores	(0.616)	(1.242)	(1.856)	(0.719)	(-0.165)	(1.395)	(2.204)	(1.060)	(-0.439)	(0.440)
Other	0.074	0.045	0.229	0.022	0.092	0.022	-0.163	-0.092	1.589	3.057**
z-scores	(1.343)	(0.235)	(1.099)	(0.071)	(0.139)	(0.106)	(-0.665)	(-0.166)	(1.254)	(2.003)
SHG	0.216	0.572***	0.488***	0.388**	0.266	0.190*	0.193*	0.124	0.139	-0.266
z-scores	(1.369)	(4.121)	(4.113)	(2.157)	(0.927)	(1.847)	(1.789)	(0.672)	(0.961)	(-0.440)

t statistics in parenthesis; * p<0.10; ** p < .05; *** p < .01; Nr. of observations: 1,256 for 2005 and 1,311 for 2007.

Table 100 Differenced variables Q-regression Incom. Negative income observations withdrawn.

	Δ with no 2005 levels income					Δ with 2005 levels income				
Source \ Quintiles	**0.1**	**0.25**	**0.5**	**0.75**	**0.9**	**0.1**	**0.25**	**0.5**	**0.75**	**0.9**
banks	0.334	0.206	0.128	0.239	0.163	-0.641	-0.345	-0.498**	0.406	0.866*
z-scores	(1.319)	(1.067)	(0.847)	(1.416)	(0.909)	(-1.562)	(-1.180)	(-2.144)	(1.069)	(1.812)
NGO	0.275	0.785	0.900	1.054	-0.284	8.646	1.881	-0.294	-2.580	-2.463
z-scores	(0.065)	(0.274)	(0.568)	(0.840)	(-0.182)	(0.281)	(0.073)	(-0.022)	(-0.196)	(-0.148)
Moneylender	0.021	-0.053	-0.086**	-0.071**	-0.102*	-0.378	-0.101	0.112	0.127	0.457*
z-scores	(0.109)	(-0.992)	(-2.249)	(-1.988)	(-1.796)	(-0.633)	(-0.687)	(1.057)	(0.907)	(1.907)
Family & Friends	-0.043	-0.057	0.042	0.081*	0.090	-0.084	-0.270	-0.235	-0.079	0.035
z-scores	(-0.213)	(-0.958)	(0.936)	(1.678)	(0.653)	(-0.230)	(-1.168)	(-1.000)	(-0.363)	(0.115)
Other	-0.077	0.201	0.067	0.149	0.359	others "	0.196	-1.054	1.197	2.253
z-scores	(-0.259)	(0.998)	(0.457)	(0.937)	(0.753)	(0.539)	(-0.197)	(-0.482)	(1.321)	(1.195)
SHG	0.608**	0.426**	0.247*	0.422	0.267	-0.474	-0.360	-0.291	-0.296	-0.744
z-scores	(2.246)	(2.247)	(1.709)	(1.592)	(0.941)	(-0.535)	(-0.741)	(-0.705)	(-0.695)	(-0.994)

t statistics in parenthesis; * p<0.10; ** p < .05; *** p < .01; Nr. of observations: 1,138

Table 101 Table 23 CRE Income. Negative income values withdrawn

	CRE Income				
Source \ Quintiles	**0.1**	**0.25**	**0.5**	**0.75**	**0.9**
banks	-0.039	-0.021	0.073	0.333	0.5829**
z-scores	(-0.647)	(-0.212)	(0.775)	(1.556)	(2.340)
NGO	0.204	0.386	0.524	0.590	0.790
z-scores	(0.147)	(0.268)	(0.378)	(0.409)	(0.536)
Moneylender	0.002	-0.051	-0.067	-0.110	-0.187
z-scores	(0.042)	(-0.957)	(-1.298)	(-1.472)	(-1.077)
Family and frieds	0.1026*	0.1121**	0.055	-0.040	-0.165
z-scores	(1.876)	(2.502)	(0.756)	(-0.326)	(-0.772)
Other sources	-0.029	-0.039	-0.026	0.155	0.702
z-scores	(-0.234)	(-0.277)	(-0.136)	(0.509)	(0.602)
SHGs	0.191	0.2885*	0.3703**	0.325	0.294
z-scores	(1.346)	(1.847)	(2.030)	(1.325)	(0.877)

t statistics in parenthesis; * p<0.10; ** p < .05; *** p < .01; Nr. of observations: 1,138