Essays on microfinance and poverty dynamics

Gillaizeau, Marc V.

SWANSEA UNIVERSITY

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Marc Gillaizeau

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ABSTRACT

Microfinance celebrates 40 years of existence with an ever wider popularity in the community of development practitioners. It is one of the cornerstones of the newly designed Sustainable Development Goals. But popularity does not mean success. To this day, the actual empirical evidence on the welfare impacts of microfinance programs are mixed. Using panel data from Bangladesh, this thesis seeks to address three major gaps in the literature.

Impact evaluation studies typically focus on mean population outcomes. Chapter 2 makes use of quantile regression techniques in order to investigate potential distributional impacts of microfinance programs. There is compelling evidence that if microfinance benefits borrowers, the impacts are not the same for everyone. Such impact heterogeneity can have important welfare consequences.

Chapter 3 investigates whether spillover effects from microfinance programs exist, which could benefit the community as a whole on top of direct beneficiaries. After providing a new set of direct impact estimates that corroborate previous findings, estimations suggest there are potentially consumption gains to non-borrowers who live in villages where microfinance is accessible. A linear social interactions model succeeds in characterising spillover effects on consumption and on boys schooling as stemming from peer endogenous effects.

Chapter 4 looks into the benefits of microfinance in helping the poor deal with vulnerability, another dimension of welfare that relates to the ability to insure against risks. A measure of vulnerability as expected poverty is constructed from cross-sections of data directly. After seven years went by between the surveys borrowers, who were by far worse off than non-borrowers in their ability to face idiosyncratic shocks, do at least as well as non-borrowers. Empirical evidence suggests that households who borrowed are less likely to be considered as vulnerable.

DECLARATION

This work has not previously been accepted in substance for any degree and is not being concurrently submitted in candidature for any degree.

Signed [redacted]

Date 29.09.2017

STATEMENT 1

This thesis is the result of my own investigations, except where otherwise stated. Where correction services have been used, the extent and nature of the correction is clearly marked in a footnote(s).

Other sources are acknowledged by footnotes giving explicit references. A bibliography is appended.

Signed [redacted]

Date 29.09.2017

STATEMENT 2

I hereby give consent for my thesis, if accepted, to be available for photocopying and for inter-library loan, and for the title and summary to be made available to outside organisations.

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Abbreviations

MDG	Millenium Development Goal
MFI	Microfinance Institution
Tk	Taka
PK	Pitt and Khandker (1998)
AD	Abrevaya and Dahl (2008)
QR	Quantile Regression
QTE	Quantile Treatment Effect
2SQR	Two-Stage Quantile Regression
CRE	Correlated Random Effects
FGLS	Feasible Generalized Least Squares
CBN	Cost of Basic Needs
HIES	Household Integrated Economic Survey
UPL	Upper Poverty Line
LPL	Lower Poverty Line
ATE	Average Treatment Effect
ATET	Average Treatment EFfect on the Treated
ITT	Intention to Treat
SUTVA	Stable Unit Treatment Value Assumption
GE	General Equilibrium
PE	Partial Equilibriu
ROSCAS	Rotating Savings and Credit Associations
ICRISAT	International Crops Research Institute for the Semi-Arid Tropics
LPM	Linear Probability Model
CJS	Chaudhuri, Jalan and Suryahadi (2002)

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A Maxence et Léonce.

Introduction

Microfinance practices have received a growing interest in the program evaluation literature over the past twenty years. Indeed, this is legitimate as the provision of financial services to the poorest has become broadly adopted by development practitioners and is partaking at the very heart of the worldwide fight against poverty. The numbers provided by the State of Microcredit Summit Campaign Report witness the 'success' of microfinance: in 2010 more than 3,600 MFIs reported to have reached around 205 millions of people, amongst whom 137.5 million were living on less than $1.25 per day (the definition of extreme poverty). These numbers have dropped significantly in 2011 to 195 millions of clients served by MFIs, of whom 125 million extremely poor people, but the decrease is mostly driven by a worsening of outreach in India following the late 2010 financial regulation in the state of Andhra Pradesh; for instance, outreach improved in Sub-Saharan Africa between 2010 and 2011. Assuming that five persons live in one household on average, microfinance has potentially had an impact on 687.5 (621) million of extremely poor people in 2010 (2011), i.e. about half the estimated world population living in extreme conditions. The fact that such claimed outreach paralleled the achievement in 2010 of the first objective of the previous MDGs to halve poverty in the world in twenty years must make one wonder whether a causal link exists.

Notwithstanding the economic effects that are expected from microfinance interventions, their social impacts are also of prime interest when leading a thorough impact assessment; however hard or impossible to measure precisely, proxies and qualitative indicators can be used to assess the magnitude and significance of the latter. The overarching aim of the thesis is to show that there are numerous things to learn by investigating the impacts of microfinance programs above and beyond simple average effects on borrowers.

The same dataset is used throughout the thesis, and therefore Chapter 1 is dedicated to its description. We build our estimation dataset from raw household survey data that were collected in rural villages of Bangladesh in 1991-92 and in 1998-99. We detail the sampling procedure and discuss study design issues that can create potential statistical problems. More precisely, non-random program placement and self-selection provide ground for endogeneity issues between variables measuring the intensity of microcredit uptake and household characteristics or environmental features. Fortunately, the two waves of data embed a large panel of households who were successfully interviewed at two points in time, and the empirical analysis in every chapter draws heavily on this advantage.

Although descriptive in nature, Chapter 1 helps motivate the empirical strategy followed in Chapter 2 by examining the distribution of our most important outcome variables of interest: measures of household per capita expenditure (total, food and non-food). We compare the distribution of consumption data for various sub-samples: between borrowers and non-borrowers, and across time periods. In all cases, consumption data exhibit leptokurtic distributions with very long right tails. We observe substantial discrepancies in both location and shape of the distribution of expenditure between sub-groups of households and over time, suggesting that average impacts are probably missing something.

Thus motivated, Chapter 2 seeks to investigate whether microfinance programs have heterogeneous impacts for different categories of borrowers. Instead of doing so by comparing average impacts for sub-samples of households, as has been sometimes proposed in the literature, we decide to use quantile regression techniques to recover the marginal impact imputable to microfinance loans at different points of the conditional distribution of household expenditure.

Implementing such econometric techniques in the context of panel data with only two time periods and many panels is a real challenge, and our final specification consists of quantile regressions on pooled data with household correlated random effects, in order to mitigate as much as possible any possible correlation between the errors and the regressor of interest, i.e. microcredit. We find that although all categories experience consumption gains, the latter are much larger for relatively high-consumers (i.e. at top quantiles of the conditional distribution). We determine that this heterogeneity stems from impacts on non-food expenditure, and we discuss the possible implications in terms of social welfare.

Our interpretation of the main findings of Chapter 2 rely on the premise that everyone is affected by what others do. In that spirit, Chapter 3 explores the question of the potential spillover effects from microfinance programs, building on the idea that the context in which microfinance institutions operate should see such programs generate broader effects than just those expected on the borrowing population. After establishing the existence of important indirect consumption effects form microfinance, we follow the literature on linear social interactions model to examine whether whether such effects on the non-borrowing population could come from social interactions with the population targeted by the program. We also consider children education as a relevant outcome for the study of spillover effects, but evidence of direct and indirect impacts on boys and girls schooling is very scarce.

The last chapter of the thesis concerns itself with the potential for microfinance to work as a risk mitigation device. Indeed, anecdotal evidence from the surveys reveal that households in the sample under scrutiny are vulnerable to frequent adverse shocks with potentially drastic welfare consequences. We build a measure of household vulnerability as the expected probability of falling into poverty at a given time horizon, and use it as a dependent variable in a panel data analysis to infer on the impact of microfinance on this alternative measure of welfare. Our findings are unequivocal and show that household are less vulnerable when they have access to microfinance loans.

Chapter 1

Data Architecture

1 Introduction

In the introduction, we have reviewed the importance of microfinance in poverty alleviation. Indeed, the microfinance initiatives in developing countries started with the aim to provide the most destitute people with access to credit, following in the steps of pioneer Dr. Muhammad Yunus. Over the past three decades since the inception of the Grameen Bank, the microfinance industry has evolved and now more broadly refers to the provision of financial services to the poor, including credit, savings and insurance products by intermediaries such as NGOs, associations, government-sponsored financial institutions and private banks. As these practices flourished all over the developing world, becoming an important tool for economic and social development policies, precise and decisive evaluations of their impact have become more crucial.

With an aim to facilitate estimation of our empirical models in the ensuing chapters, it is necessary to understand the characteristics of the data. An important step in this endeavour is to explain the study design and how villages and households were selected before the surveys. Investigators in charge of the project sought to evaluate the impacts of microcredit thanks to planning data collection in a quasi-experimental framework. However, census data and household information from the surveys suggest that the intended quasi-experiment might not be powerful enough to enable clear identification of relevant program impacts, hence inviting the need for an alternative approach.

Along with presenting summary statistics of all variables used in regressions in the next chapters, the focal point of this chapter is to explore the distributional features of our main outcome variables of interest (i.e. household per capita expenditure as a proxy for household welfare). Consequently, the fact that the distribution of consumption in our sample is far from normal – with a long right tail – and displays discrepancies in location and shape between sub-groups of households (borrowers and non-borrowers) and over time, lays the foundations of our advocacy for the use of quantile regressions as useful tools to estimate potentially heterogeneous impacts of microfinance programs[1].

We plan this chapter as follows. In Section 2, we discuss the study design and focus on some characteristics of the data collection process. Section 3 and its various subsections focus on uncovering different distributional patterns amongst borrowing and non-borrowing households in terms of per capita expenditure, the main dependent variable of interest in Chapter 2 of the thesis. Finally, Section 4 briefly describes other variables used in regressions in the next chapters, and Section 5 concludes.

2 Study design

2.1 Data collection

The dataset consists of a survey of 1,798 households sampled from 87 villages in rural Bangladesh (about 20 households in each village) that were selected across 29 subdistricts (or *thanas*). Three villages were randomly chosen in each thana, while subdistricts had been sampled according to the presence of one of three leading microfinance programs at the time of survey, namely the Grameen Bank, the Bangladesh Rural Development Bank (BRDB) Rural Development-12 program (RD-12) and BRAC, resulting in three groups of eight thanas each exposed to a different microfinance initiative; five thanas were also selected where no microfinance programs existed, providing 15 control villages.

The first wave of data was collected during the years 1991 and 1992 through three rounds of survey corresponding to the three main rice seasons of the year ; 1,769 households of the original sample were successfully interviewed the three times.

A second wave of data was collected in 1998-99. This time, only one survey was administered in an effort to interview the same households as in 1991-92 as well as new households sampled from the same villages, new villages from the original thanas as well as three newly selected thanas, for a total of 2,599 households. There were 1,638 households from the first wave successfully re-interviewed in 1998-99, and 237 of these had split into 546 households for a total of 1,947 observations in 1998-99 for originally sampled households.

Finally, a third wave of data collection took place in 2010-11, in an effort to re-survey the same households as in 1998-99, but attrition and household split-offs resulted in 3,082 households being interviewed between March and September 2010-11. As of

[1] This is the topic of Chapter 2.

today, and to the best of our knowledge, this dataset remains one of the largest ever collected for the specific purpose of studying microfinance phenomena, and certainly spans the longest time period if we consider the three waves of data collection (20 years). The collected data contains a rich set of variables, including socio-economic aspects, credit history of the households and qualitative information. Raw data for the two first waves of survey (1991-92 and 1998-99) are available freely on the World Bank's website. However, the third wave of data still remains inaccessible to the public so far.

Our empirical strategy, which we describe in details in following chapters, seeks to exploit the "time" feature of the data and therefore makes use of a balanced panel structure, i.e. we keep in the sample only those 1,638 households that were successfully interviewed across all waves of data collection. We merge the 1998-99 data for those households that had split since 1991-92, and combine data across the three first rounds of interviews in order to have 2 observations per household: one in 1991-92, and one in 1998-99. Therefore, discussions on sampling and descriptive statistics consider this balanced sample only, unless indicated otherwise.

2.2 Sampling, program eligibility and microcredit uptake

2.2.1 Sampling

As explained above, households were randomly sampled within villages according to their eligibility status to the microfinance initiatives under scrutiny in 1991-92, i.e. Grameen Bank, BRAC and BRDB RD-12. Around 20 households were sampled from each village, and eligible households were oversampled compared to ineligible households (around 85% of households sampled in each village are program-eligible), as is customary in impact evaluation studies that seek to maximize the statistical power of program effects estimates for a lower cost. Weights to be used in the analysis are derived from this sampling procedure. They are not readily available on the World Bank's website with the rest of the raw data, but can fortunately be found on David Roodman's webpage at the Center for Global Development as a consequence of his numerous exchanges with Mark Pitt, when the author and his collaborator Jonathan Morduch were trying to replicate and criticise the seminal paper by Pitt and Khandker (1998)[2]. Interestingly, along with the household sampling weights are data about the initial census in the selected villages: the total number of ineligible and eligible households within each village – the latter subsample split in two groups of participants and non-participants

[2] This famous and most insightful replication and critic of Pitt and Khandker (1998) was published in Roodman and Morduch (2014). A preliminary version (Roodman & Morduch, 2009) also included a replication and critical review of the results in Khandker (2005), a study that uses a panel dataset similar to ours.

to microfinance in any of the programs considered at the time of survey – and the number of households finally sampled within each sub-group.

The final sample used in this thesis counts close to 19 households per village on average, with a minimum (maximum) of 13 (23) observations, for a total of 1,638 households. According to the initial sampling information, as of 1991-92 our dataset counts 824 eligible households who borrowed through microfinance programs (*treated* target households), 567 eligible households who did not participate to microcredit initiatives (*non-treated* target households), and 247 households not eligible to microcredit borrowing (non-target households). However, according to individual-level loan data, only 55.8% of those eligible households did borrow from either Grameen Bank, BRDB or BRAC, i.e. the subsample of participating target households is actually made of 761 households instead of 824 as stated by census data. Another 19 eligible households took up microcredit loans from other NGOs, as was also the case for one ineligible household.

Such census data are not available in 1998-99[3], and we must rely on individual-level information. By the late 1990s, every village in the sample had access to microfinance (including those initially sampled as control villages), be it through the MFIs initially considered or other initiatives. Therefore, 1998-99 data considers eligibility to any microcredit program instead of focusing on Grameen Bank, BRAC and BRDB only. The sample then consists of 839 eligible households participating to any microfinance initiative, 612 eligible non-participating households and 187 ineligible households, one of whom borrowed from a MFI.

Because of the aforementioned change in the definition of program eligibility between the two waves of interviews, we follow the 1998-99 definition and thus include microcredit from any source (Grameen Bank, BRAC, BRDB or any other MFI) in the credit variables used in the analysis.

2.2.2 Eligibility and participation

This sub-section explains how households were classified as program-eligible, which is relevant to the choice of an empirical strategy, as will be explained further. According to Pitt and Khandker (1998), at the time the study was designed the three MFIs under scrutiny followed a similar rule which deemed a household eligible to borrowing if their landholdings were no greater than half an acre (50 decimals). The study was planned so that this cut-off point be used as a source of identification in dealing with endogeneity issues. To make it viable for use in an instrumental variable approach, this landholdings-based eligibility rule should be consistent across household-level data

[3] As a consequence, the same sampling weights are used for both waves of data.

and the available census data on which sampling was designed. However, a closer look at the data shows that this rule was not strictly followed.

Table 1.1 **Sample composition in 1991-92, for various definitions of eligibility**

	Census data	Loan data	Land before	Land after	Arable land
Eligible borrowers	824	761	586	566	597
Eligible non-borrowers	567	630	589	587	625
Ineligible non-borrowers	247	247	288	290	252
Ineligible borrowers	0	0	175	195	164

Table 1.1 presents the sample composition in 1991-92 based on various definitions of eligibility to microfinance programs. According to census data, no microfinance loans were issued to households classified as ineligible, while 824 households out of 1,391 eligible ones did borrow. In the questionnaire module on credit history, households were asked to provide information on past loans and also to report their eligibility status. Based on said loan data, the eligible and ineligible populations are the same as with census data (none of the 247 ineligible households were issued microcredit), although there are discrepancies between self-reported credit history and participation as described in census data[4]. Indeed, the latter claim the sample includes 824 borrowing eligible households while loan data report only 761. The available information does not allow us to explain this difference, shedding doubts on the criteria that were used for the classification of households and hence for sampling.

Furthermore, we want to check the consistency in eligibility and participation status between census data and household survey data. It is not made clear in Pitt and Khandker (1998) whether total landholdings are considered to determine eligibility, or whether only arable land with productive potential (and higher value than, e.g., non-arable land) should be scrutinized. We consider three measures of landholdings. In 1991-92, households were asked to report landholdings *before* and *after* having had access to microcredit. Also, another module of the questionnaire was used to collect data on arable land[5].

The numbers in Table 1.1 show that landholdings data point to a substantial amount of "mistargeting" in the allocation of microcredit. For instance, landholdings reported as *before* the inception of microcredit programs lead to classifying 1,175 households as eligible (against 1,391 according to census data), amongst whom 586 borrowed, and 463 as ineligible with 175 borrowers, i.e. about 23% of borrowing households are not eligible. The fraction of mistargeted households is 25.6% when considering landholdings

[4] We consider borrowing from Grameen Bank, BRAC and BRDB only for the sake of comparability with census data that focused on these three MFIs only.

[5] We construct this variable for the first of the three rounds of interviews in 1991-92 only, as data for later rounds appeared to be incomplete and inconsistent.

after microfinance programs were made available, and 21.5% when looking at arable land. At any rate, whichever measure of landholdings is used, more than one in five borrowing households was mistargeted. This empirical statement further sheds a veil of shadows on the "actual" eligibility rule followed by loan officers when issuing credit, and on the source of information used to compile the census data that later influenced sampling.

Table 1.2 **Sample composition in 1998-99, for various definitions of eligibility**

	Loan data	Land before	Land after
Eligible borrowers	643	583	455
Eligible non-borrowers	808	895	612
Ineligible non-borrowers	187	100	383
Ineligible borrowers	0	60	188

Table 1.2 presents similar numbers for 1998-99, although the definitions change. Census data are not available and the eligibility status of the household is recovered from survey data on credit history. Also, questionnaire modules about landholdings were different in the late 1990s: "land before" corresponds to acquisitions of land before 1992 (not before access to microcredit as in 1991-92 questionnaires), and "land after" embeds total landholdings. Ownership of arable land cannot be computed for 1998-99 data in a reliable fashion as for 1991-92. Finally, program eligibility and participation relate to any MFI, as opposed to only three target initiatives in 1991-92.

Again, a substantial amount of mistargeting is unraveled when using landholdings data to assess whether a household is eligible to microcredit. Individual-level loan data show that 1,451 households were eligible to borrowing in 1998-99, amongst whom 643 did borrow, leaving 187 ineligible and non-borrowing households. Information on acquisitions of land before 1992 imply that 1,478 households are eligible, with 583 borrowers, while we find 60 mistargeted borrowing households amongst ineligible ones (160 in total). Based on total landholdings, only 1,067 households would be classified as eligible, and close to 30% of borrowing households would be considered ineligible.

Altogether, findings from Tables 1.1 and 1.2 show that information available from the survey does not allow researchers to replicate the eligibility rule – allegedly based on landholdings – used by MFIs as reported in census data. Maybe landholdings are only one of the factors that enter the decision to classify a household as eligible to receiving microcredit, in which case part of the decision process remains opaque, hence jeopardizing an identification strategy that relies on landholdings as an instrumental variable for eligibility – as in Pitt and Khandker (1998).

3 Descriptive statistics of outcome and treatment variables

A large part of our empirical investigations – in Chapter 2 and part of Chapter 3 – puts focus on household annual per capita expenditure as the outcome variable of interest, given its broadly recognized quality as a good proxy for household welfare in developing countries. We also break it down into food and non-food expenditures to explore whether the nature of the expense matters when households adjust their behaviour after receiving microcredit.

Our so-called "treatment" variable is the intensity of exposure to microcredit measured as the sum of all microfinance loans taken up over the previous six years. More accurately, cumulative borrowings for 1991-92 do include a few instances of loans more than six years old[6], and 1998-99 variables include all new loans taken up since the previous round of data collection in late 1992. The "treatment" variable in our regressions is either the total cumulative borrowings of a household or cumulative borrowings differentiated by gender (both appearing in the regression) to assess the existence of different impacts of microcredit following that loans are given out to men or women.

3.1 Means and dispersions of consumption and credit variables

Table 1.3 presents weighted summary statistics for our outcome variables of interest, namely household per capita total, food and non-food expenditures (the former being the sum of the two latter), along with weighted means and standard deviations for our measures of the intensity of exposure to microcredit programs corresponding to cumulative borrowings over the previous six years. Summary statistics are presented for the balanced panel dataset which is our final estimation sample, with 1,638 observations (households) for each time period. We count that 781 households borrowed at least once from microfinance programs in the first time period up to December 1992, and 840 households borrowed between then and the last round of data collection in late 1998/early 1999. Our sample includes 611 households eligible to group lending and 246 ineligible ones who did not borrow in 1991-92; these numbers are 612 and 186 for 1998-99, respectively.

Average female borrowings more than doubled between the early and late 1990s (from around Tk 6,700 to above Tk 15,000), and they are substantially greater than average male borrowings in both time periods (the latter decreased over time). This is hardly surprising given that microfinance programs have historically targeted women in priority, and that successful first loans typically open the right to borrow again, with subsequent loans that usually are of larger amounts. Table 1.3 shows that household per

[6] Out of 3,095 loans reported in 1991-92 across the three rounds of data collection, 122 were taken up before December 1986, i.e. more than six years before the last round of data collection.

capita total expenditure is on average greater for those households that are not eligible to group lending and do not borrow (Tk 5,664 per year in 1991-92 and Tk 6,989 per year in 1998-99) than for other categories of households for both time periods.

A similar pattern is observed for food and non-food expenditures. These summary statistics seem to confirm the idea that non-eligible households are on average better off than eligible households, in line with the idea that microfinance programs are supposed to serve the poorest of the poor. It also appears that households benefitting from microcredit loans achieve slightly higher levels of total per capita expenditure than eligible households who do not borrow (Tk 4,028 per year versus Tk 3,864 per year in 1991-92) although the spread decreases over time (Tk 5,040 per year versus Tk 4,971 per year in 1998-99). Similar observations arise when turning to food and non-food expenditures, with the exception of food expenditure in 1998-99 that sees eligible non-borrowing households achieve a slightly higher level on average than borrowing households (Tk 3,256 per year versus Tk 3,224 per year).

Table 1.3 **Summary statistics of household credit and expenditure variables**

	1991-92				**1998-99**			
	Borrowers	**Non-borrowers**		**Full sample**	**Borrowers**	**Non-borrowers**		**Full sample**
		Eligible	*Non-eligible*			*Eligible*	*Non-eligible*	
HH total microcredit cumulative borrowings	10,295.2			2,515.3	17,985.7			7,408.6
	(9,741.1)			(6,538.2)	(19,508.5)			(15,332.5)
Female microcredit cumulative borrowings	6,717.5			1,641.2	15,196.2			6,259.6
	(8,726.8)			(5,189.4)	(19,242.0)			(14,436.1)
Male microcredit cumulative borrowings	3,577.7			874.1	2,789.4			1,149.0
	(7,490.6)			(4,008.0)	(8,937.5)			(5,896.7)
HH per capita total expenditure, annual	4,028.0	3,864.0	5,664.2	4,513.7	5,039.9	4,971.0	6,989.2	5,373.0
	(1,645.7)	(1,683.6)	(3,553.9)	(2,605.2)	(3,324.2)	(3,599.6)	(5,387.5)	(3,963.2)
HH per capita food expenditure, annual	3,082.4	3,039.5	3,646.5	3,255.6	3,224.1	3,256.8	3,960.2	3,373.5
	(799.6)	(946.8)	(1,053.1)	(991.2)	(1,140.2)	(1,550.6)	(1,868.6)	(1,491.6)
HH per capita non-food expenditure, annual	945.6	824.4	2,017.7	1,258.1	1,815.8	1,714.2	3,029.1	1,999.4
	(1,194.1)	(984.5)	(2,980.4)	(2,011.9)	(2,727.8)	(2,541.6)	(4,693.2)	(3,156.7)
Observations	781	611	246	1,638	840	612	186	1,638

Note: Sample means and standard deviations (in parentheses) for household-level credit and expenditure variables. Statistics account for sampling weights. All amounts are deflated and given in 1992 Taka. The word household is abbreviated by 'HH'. The eligibility status of a household used to identify sub-samples is taken from census data for the first wave (1991/92) and from the questionnaire module on loans for the second wave (1998/99).

It must be mentioned, however, that comparisons in household per capita expenditure levels between borrowing households and eligible non-borrowing ones are to be considered with great care because of the substantial amount of mis-targeting observable in the data. Indeed, as discussed in the preceding section, some households are classified as eligible in 1991-92 census data while they hold more than half an acre of land, even though this cut-off point is presented by the researchers leading the study as an eligibility criterion shared by all microfinance programs under scrutiny. Assuming this criterion was set to identify *the poorest of the poor*, it implies that each sub-sample of borrowers and eligible non-borrowers includes a share of households relatively better off than the most indigent ones (according to landholdings as a proxy measure of wealth). Mis-targeting in microfinance programs regarding the "landholdings eligibility rule" makes it hard to assess the success of such programs in reaching the alleged targeted fringes of the population, at least in our sample, precisely because eligibility seems to be determined at the discretion of lending institutions.

3.2 A closer look at the distribution of consumption

The focal point of our study in Chapter 2 is to understand how the impacts of microcredit on household welfare are distributed amongst households who rank at various points of the outcome variable distribution. We complement our brief description of the means and variances of consumption variables with an exploration of their distributions across time periods and across household groups, namely borrowers and non-borrowers. We wish to readily emphasize that the purpose of the following discussion is *by no means* an attempt at unravelling *causal* effects, but merely at describing empirical facts about marginal distributions as stepping stones towards our econometric study, and also to strengthen our argument in favour of using econometric techniques with an object of interest other than conditional means.

3.2.1 Household per capita total expenditure

Panel A of Figure 1.1 presents kernel density estimates for household per capita expenditure in each time period for the full sample and the two sub-samples of borrowers and non-borrowers[7]. We can easily see that in every instance the distribution displays a sharp peak around its mode and is positively skewed with a long and thin right tail. For the full sample as for each sub-sample we also note that in the second time period the distribution of household total consumption, albeit still leptokurtic and right-skewed, is a bit more evenly spread around its central tendency than in 1991-92, with an increase of the proportion of households in the tails, most notably in the upper part of the distribution.

Bottom left graph of Panel A of Figure 1.1 shows that for borrowing households the distribution of total expenditure reaches slightly further left in 1998-99 than in 1991-92, indicating the occurrence of ever lower levels of consumption at the lowest quantiles. Borrowers at top quantiles tend to achieve higher consumption in the late 1990s than

[7] Expenditure variables are valued in 1992 Taka and hence are comparable across time periods.

in 1991-92, and the very highest achievers seem to realize comparable levels of expenditure in both time periods, although there appears to be slightly more of them in 1998-99[8].

In the group of non-borrowing households (bottom right graph of Panel A of Figure 1.1), the lowest levels of consumption are similar in both time periods although we find larger proportions of low-achievers in 1998-99. Similarly, there are more households achieving high levels of total expenditure in the top part of the distribution in 1998-99, while the very top achievers also realize higher levels of consumption in the late 1990s than in 1991-92.

These observations can be refined with the help of the quantile-quantile plots shown in Panel A of Figure 1.2. The bottom left graph of Panel A shows that the proportions of borrowing households with low levels of consumption are quite similar in both time periods, and departures of the plotted points from the identity line is clear evidence of the increased spread of the distribution of consumption from lower quantiles to around its median in the late 1990s. In the upper tail, points in the plot form a line with near-zero slope, representing the fact that extreme values in the high tail of the consumption distribution are closer together in the second time period than they are in the first. A kindred pattern can be observed for non-borrowers, although sharp discrepancies between distributions in both time periods appear at slightly higher quantiles still. Points at the top of the plot falling roughly on an upward sloping straight line paralleling the identity line suggest that the allocation of observations across higher levels of consumption in each time period is almost identical, even though the absolute levels of per capita expenditure in that part of the distribution are higher in 1998-99 than in the early 1990s, with a more frequent occurrence of extreme high values.

Panel A of Table 1.4 reports the values of the quartiles and of the lower and upper deciles for each sub-sample considered in the graphs and plots described above. The lower decile and lower quartile of per capita expenditure are very similar in both time periods for the full sample, with the former at Tk 2,537 and Tk 2,534 and the latter at Tk 3,099 and Tk 3,138 in 1991-92 and 1998-99, respectively. The positive difference in favour of the second time period sharpens at the median and higher quantiles, figuring the fact that the distribution is more spread to the right in 1998-99, as previously observed in Panel A of Figure 1.1. We also see that borrowers achieve higher total expenditure in 1998-99 than in 1991-92 at every reported quantile, with this difference sharpening from the bottom decile up, which suggests both a location shift (higher levels of consumption at each quantile) and a change in shape (quantiles are relatively further apart than they were) over time. Interestingly, non-borrowers at the bottom decile and first quartile achieve lower consumption in 1998-99 than in 1991-92 while the converse is true for non-borrowing households at the median and upper quantiles,

[8]The plotted densities seem to converge as we move to the far right end but the dashed line (1998-99) remains above the plain line (1991-92).

a stylized fact outlining that over time the distribution of consumption amongst non-borrowers spread out more than that of borrowers.

Table 1.4 **Sample quantiles and distribution tests for household per capita total expenditure**

(a) Panel A: Comparison across time, by borrowing status

Quantile	*Full sample*		*Borrowers*		*Non-borrowers*	
	1991-92	1998-99	1991-92	1998-99	1991-92	1998-99
10%	2,537.1	2,534.0	2,544.8	2,645.6	2,533.4	2,454.9
25%	3,098.9	3,138.1	2,992.5	3,208.4	3,148.5	3,071.4
Median	3,899.0	4,220.7	3,726.9	4,208.6	3,930.9	4,242.1
75%	5,065.5	6,094.5	4,675.1	5,725.2	5,220.2	6,457.5
90%	7,003.5	9,348.0	5,737.2	7,799.9	7,505.0	10,593.4
Wilcoxon signed rank test (p-value)	0.000		0.000		0.001	
Sign test, 2-sided (p-value)	0.000		0.000		0.021	

(b) Panel B: Comparison across borrowing status, by time period

Quantile	*1991-92*		*1998-99*	
	Borrowers	Non-borrowers	Borrowers	Non-borrowers
10%	2,544.8	2,533.4	2,645.6	2,454.9
25%	2,992.5	3,148.5	3,208.4	3,071.4
Median	3,726.9	3,930.9	4,208.6	4,242.1
75%	4,675.1	5,220.2	5,725.2	6,457.5
90%	5,737.2	7,505.0	7,799.9	10,593.4
Wilcoxon rank-sum test (p-value)	0.002		0.116	
Kolmogorov-Smirnov test (p-value)	0.002		0.009	
Median test (p-value)	0.043		0.199	

The bottom rows of Panel A of Table 1.4 provide p-values associated with distributions equality tests for matched paired data, a sensible choice given that comparisons over time consider repeated measurements for the same panel of households. The first is the Wilcoxon matched-pairs rank-signed test, which tests the null hypothesis that two distributions are equivalent by considering whether the median difference between pairs of observations is zero[9]. The second, coined the sign test, has a similar null hypothesis that is decided against in the event that there are unequal numbers of positive and negative pairwise differences[10]. Both are non-parametric tests in that they do not assume normality of the data, but their assumptions are still likely to be violated by

[9] The test assumes that pairs are randomly sampled and that pairwise differences are i.i.d. and follow a symmetric distribution.

[10] Unlike the Wilcoxon signed-rank test it does not account for the magnitude of the differences.

our ill-behaved consumption variables. We do not however see this as a major concern because the tests are carried out as part of our descriptive exercise and not for formal inference. The latter is also the reason why we consider the two-sided sign test only and not its one-sided alternatives. For the full sample as well as for sub-samples of borrowers and non-borrowers, the null that the distribution of household total per capita expenditure is similar in 1991-92 and 1998-99 is overwhelmingly rejected (the largest p-value we find is 0.021), hence comforting the idea that some distributional transformation happened over time in terms of household consumption, as was suggested by observations made so far from Panel A of Figure 1.1 and Panel A of Figure 1.2.

We now turn to comparisons between borrowers and non-borrowers in each time period, for which kernel density estimates can be found in Panel B of Figure 1.1. In 1991-92 the proportions of households with low levels of consumption are very close for both groups while both distributions still display leptokurtic shapes with positive skew. The right tail is however a bit thicker for non-borrowers than for borrowers, showing that more households from the former group manage to achieve higher levels of consumption. Interestingly in 1998-99 there are larger proportions of non-borrowing households than borrowing ones with low levels of per capita expenditure. Both distributions spread more to the right of their respective central tendencies but the upper tail remains thicker for non-borrowers. The leftmost quantile-quantile plot in Panel B of Figure 1.2 confirms the similarity of the consumption profiles of borrowers and non-borrowers at low quantiles in 1991-92, with discrepancies between both groups appearing little below Tk 5,000 of per capita expenditure. There are greater shares of high-consumers amongst non-borrowers even though high achievers are closer together than in the group of borrowers, as witnessed by the rightmost points of the plot that follow a steep upward sloping line.

In the second time period the largest discrepancies between the two groups appear in the top part of the distributions before slowly vanishing for high levels of consumption where the allocation of observations is similar for borrowing and non-borrowing households. Evidence from the quantile-quantile plots is confirmed by the numbers reported in Panel B of Table 1.4. The lower deciles and lower quartiles of total per capita consumption in 1991-92 are quite close for borrowing and non-borrowing households, although with a slight edge for the latter (except at the bottom decile), while the converse is true in 1998-99. The difference between both samples at the median and higher quantiles is systematically in favour of non-borrowers even though median consumption in both groups is almost identical in 1998-99 (Tk 4,207 for borrowers and Tk 4,242 for non-borrowers).

In the same vein as earlier (and with the same reserve) we carry out two non-parametric tests for the equality of distributions between both groups, as well as a Pearson's Chi-squared test for the equality of medians (the bottom rows of Panel B in Table 1.4 report p-values). The Wilcoxon rank-sum test and the Kolmogorov-Smirnov test both consider the null hypothesis that two independent samples were drawn from the same population, the former being more sensitive to discrepancies in location while the latter is also sensitive to differences in shape. Both tests invite us to reject the null that the distribution of per capita expenditure for borrowers is the same as that of non-borrowers in the early 1990s, while the third test is against the equality of medians for the two sub-samples.

Although we fail to reject the null that median consumption is similar for both groups in 1998-99, the Kolmogorov-Smirnov test suggests that both distributions are different and contradicts the Wilcoxon rank-sum test for which we just fail to reject the null at the conventional level of 10% confidence. Without putting too much emphasis in these tests, their conflicting conclusions are not necessarily inconsistent. We can imagine that the Wilcoxon rank-sum test, being more sensitive to location, considers that achieved levels of consumption in the group of non-borrowers are similar enough to those in the group of borrowers at many points of the distribution (see the reported values of quantiles in Panel B of Table 1.4 and the corresponding quantile-quantile plot in Panel B of Figure 1.2). On the other hand, the Kolmogorov-Smirnov test detects different enough locations and, especially, shapes to reject the null that both distributions are very close (as in the corresponding kernel density estimates in Panel B of Figure 1.1)[11].

3.2.2 Household per capita food and non-food expenditures

One main refinement of our subsequent empirical analysis is to consider the breakdown of total per capita expenditure into food and non-food expenditures as alternative outcome variables of interest. In the interest of saving space and avoiding redundancy we comment more briefly on the distributions of these two variables, and place the associated Kernel density estimates and quantile-quantile plots in Appendix A.

From graphs in Panel A of Figure A.1 we note that the distribution of food per capita expenditure is less skewed to the right than that of total consumption in either time period, although the mode remains in the upper part of the bottom half of the distribution. Observations are more evenly spread for borrowers than for non-borrowers, and for both sub-samples the graphs show the distribution "flattening" over time with a thickening of the tails around a central tendency that seems to change little. This is confirmed in the corresponding quantile-quantile plots (Panel A of Figure A.3) with points falling slightly below the identity line for the bottom half of the distribution and then settling above and not far from the identity line at higher quantiles. Non-parametric tests suggest that the distribution of food consumption in 1998-99 is indeed

[11] Put in yet another way, the Kolmogorov-Smirnov test detects large enough discrepancies to decide against the null.

different from that in 1991-92 for borrowing households whereas they fail to reject the null for non-borrowers (Panel A of Table 1.5).

Table 1.5 **Sample quantiles and distribution tests for household per capita food expenditure**

(a) Panel A: Comparison across time, by borrowing status

Quantile	*Full sample*		*Borrowers*		*Non-borrowers*	
	1991-92	1998-99	1991-92	1998-99	1991-92	1998-99
10%	2,192.2	1,952.2	2,213.2	1,966.6	2,183.6	1,938.6
25%	2,564.4	2,365.0	2,508.1	2,424.7	2,578.2	2,340.4
Median	3,093.8	3,067.8	2,946.1	3,022.1	3,137.3	3,124.3
75%	3,771.7	3,942.8	3,545.7	3,812.7	3,867.3	4,063.1
90%	4,524.0	5,010.0	4,188.6	4,664.9	4,658.1	5,501.4
Wilcoxon signed rank test (p-value)	0.081		0.000		0.321	
Sign test, 2-sided (p-value)	0.639		0.025		0.111	

(b) Panel B: Comparison across borrowing status, by time period

Quantile	*1991-92*		*1998-99*	
	Borrowers	Non-borrowers	Borrowers	Non-borrowers
10%	2,213.2	2,183.6	1,966.6	1,938.6
25%	2,508.1	2,578.2	2,424.7	2,340.4
Median	2,946.1	3,137.3	3,022.1	3,124.3
75%	3,545.7	3,867.3	3,812.7	4,063.1
90%	4,188.6	4,658.1	4,664.9	5,501.4
Wilcoxon rank-sum test (p-value)	0.005		0.382	
Kolmogorov-Smirnov test (p-value)	0.008		0.028	
Median test (p-value)	0.001		0.843	

Kernel estimates in Panel B of Figure A.1 display evidence that the allocation of households at low levels of food consumption is similar for borrowers and non-borrowers in 1991-92. However the mode for the latter appears to the right of its counterpart for the former group of households (we reject the null of equality of medians, Panel B of Table 1.5), and there are larger proportions of non-borrowing households than borrowing ones that achieve high levels of food consumption (left-hand graph in Panel B of Figure A.1). These discrepancies are backed by both distribution equality tests for which the null is rejected. For 1998-99 data the central tendencies for both groups of households seem much closer together (we fail to reject the null in the median test and Wilcoxon rank-sum test) and so does the overall shape (even though we reject the null hypothesis of the Kolmogorov-Smirnov test), with the distribution of food consumption for borrowers gathered a bit more tightly around its mode than that for non-borrowers which has thicker tails (right-hand graph in Panel B of Figure A.3).

Table 1.6 **Sample quantiles and distribution tests for household per capita non-food expenditure**

(a) Panel A: Comparison across time, by borrowing status

Quantile	*Full sample*		*Borrowers*		*Non-borrowers*	
	1991-92	1998-99	1991-92	1998-99	1991-92	1998-99
10%	272.7	407.2	269.8	450.9	272.7	384.6
25%	417.7	589.8	398.5	624.9	422.6	572.3
Median	677.3	964.3	608.6	973.1	712.4	954.2
75%	1,262.9	1,962.3	1,009.1	1,787.0	1,378.4	2,057.9
90%	2,580.6	4,358.3	1,844.6	3,740.3	2,947.8	5,157.5
Wilcoxon signed rank test (p-value)	0.000		0.000		0.000	
Sign test, 2-sided (p-value)	0.000		0.000		0.000	

(b) Panel B: Comparison across borrowing status, by time period

Quantile	*1991-92*		*1998-99*	
	Borrowers	Non-borrowers	Borrowers	Non-borrowers
10%	269.8	272.7	450.9	384.6
25%	398.5	422.6	624.9	572.3
Median	608.6	712.4	973.1	954.2
75%	1,009.1	1,378.4	1,787.0	2,057.9
90%	1,844.6	2,947.8	3,740.3	5,157.5
Wilcoxon rank-sum test (p-value)	0.001		0.013	
Kolmogorov-Smirnov test (p-value)	0.001		0.001	
Median test (p-value)	0.012		0.018	

Patterns in the distribution of non-food per capita expenditure closely match the observations made for total expenditure, only with much sharper discrepancies. Kernel estimates in Panel A of Figure A.2 show an extremely sharp peak in the distribution of non-food consumption around its mode in 1991-92 complemented by a far-reaching thin right tail. It seems the distribution shifts to the right in the second time period (which is evident from Panel A of Table 1.6), especially for borrowers, with the right tail getting slightly thicker, more so for non-borrowers. Quantile-quantile plots in Panel A of Figure A.4 look similar to those for total expenditure, only the discrepancies appear at the very low quantiles of the distributions and are sharper.

Panel A of Table 1.6 shows we can reject the null that the distribution of non-food expenditure does not change over time for the full sample as well as for the two sub-samples of borrowers and non-borrowers. In Panel B of Figure A.2 we see that both groups have quite similar non-food consumption profiles 1991-92 even though the peak around the mode is sharper for borrowers and higher levels of non-food expenditure are more often achieved by non-borrowers, as also shown by the near-flat pattern of points in the middle of the corresponding quantile-quantile plot (left-hand graph of Panel B of Figure A.4).

In the late 1990s data the central tendency of the distribution of non-food consumption for borrowers appears to settle to the right of that for non-borrowers, and larger proportions of households from the latter group achieve very low levels and high levels of non-food expenditure with a few exceptions at the topmost quantiles (points lying on the identity line in the top right corner of the right-hand plot in Panel B of Figure A.2). Our distribution equality tests and median tests suggest a rejection of the null in every instance, but we put little faith in their meaningfulness giving the highly irregular behaviour of the variable under scrutiny.

3.2.3 Summary of findings

In the context of our study, exploration of dependent variables beyond their average values and dispersions provides invaluable information. Our dependent variables exhibit leptokurtic and positively skewed distributions with departures from normality particularly salient for total and non-food consumption. Such shapes imply that empirical means are probably unfit to adequately summarise these variables, in turn questioning the relevance of measuring causal effects of microfinance programs with our data through the estimation of conditional expectation functions. Of course, in studies such as Pitt and Khandker (1998) and Khandker (2005) the authors transform all continuous variables into logarithmic forms in order to work with better behaved data, therefore improving chances to fulfil normality assumptions on the error terms and making average tendencies more representative of the distribution. One drawback is the need to input an arbitrary value to observations corresponding to non-borrowing households, and the choice of that value is not negligible because it can influence the interpretation of the estimated coefficients that represent elasticities. This concern is discussed in Roodman and Morduch (2014).

We feel that the non-standard distribution of consumption in our sample plays in favour of the argument of using quantile regression techniques that consider objects other than conditional expectation functions and do not rely on normality assumptions. Additionally, the robustness of said techniques to outlying observations makes it unnecessary to transform ill-behaved variables into logarithms and avoids having to choose an arbitrary value for the logarithm of zero in treatment variables (cumulative microcredit borrowings).

The set of kernel density estimates, quantile-quantile plots and corresponding tables also suggests that the distribution of consumption tends to spread out over time, with typically an upward shift in location and slightly thicker tails. This is true for the full sample and for each of the two sub-samples of borrowers and non-borrowers. Non-parametric tests confirm the intuition gotten from graphs and plots that consumption is distributed substantially differently across the sub-populations of borrowers and non-borrowers in each time period.

To that extent, quantile regressions are relevant tools because they can capture the influence of covariates not only on the location of the conditional distribution of the outcome variable at a given quantile, but also on its shape through allowing estimated coefficients to vary across quantiles. They will be invaluable in assessing whether apparent discrepancies in the location of distributions can be imputed (at least in part) to the effect of microcredit and, if so, whether this caused location-shift is homogenous across the distribution. We want to stress again that the aforementioned observations on the distribution of consumption conditional on the borrowing status of households and on the related diagnostic plots and non-parametric tests of distribution equality *do not* provide formal evidence about the causal relationship between microcredit and household welfare.

Finally, we note that kernel density estimates show that the distribution of food consumption does not exhibit as sharp a peak around its central tendency as that of non-food consumption, and that it is more evenly spread than the latter. Moreover, quantile-quantile plots confirm that differences in shape and location for the distribution of food per capita expenditure over time and across borrowing status are much less salient than is the case for total or non-food expenditure. Nevertheless, we still observe a "flattening" of the distribution around its centre in the second time period and thicker tails, even though a rightward shift of central tendency is not always obvious.

Interestingly, in both sub-groups of borrowers and non-borrowers there are markedly larger proportions of households achieving low levels of food consumption in 1998-99 as compared with 1991-92. It follows mechanically that clear discrepancies in the distribution of household non-food per capita expenditure over time and across household groups closely resemble those observed for total expenditure, only with sharper patterns. They are a manifestation of the fact that non-food expenditure includes several types of lumpy expenses, such as durable household goods or large amounts of money spent for religious and social ceremonies, the nature of which implies they are not systematically incurred by many households or maybe not often enough to be picked up by punctual surveys[12].

As a consequence, non-food expenditure data is made of many unique (or close to unique) observations that can represent some heterogeneity in extreme events facing households (such as marriage or death), but can also capture the notion that some of these expenses are important social markers (e.g. clothes and shoes) and hence might rank differently in the priorities of different households. In comparison, household behaviour with respect to the purchase of food items is more broadly consistent over time and sub-samples. The overall contrast between food and non-food expenditure in terms of distributional characteristics makes a good case for considering them as alternatives to total expenditure for our outcome of interest in the context of quantile regressions that will be sensitive to the distinct underlying shapes of the distributions.

4 Descriptive statistics for other household variables and village characteristics used in regressions

Table 1.7 shows weighted sample means and standard deviations for the fifteen variables included in each of our regressions as household-level covariates. Summary statistics are displayed for the sub-samples of borrowers and non-borrowers from microcredit programs as well as for the full sample, for each wave of data separately. Therefore, they can be read as summarizing our variables for the population under scrutiny, i.e. for all the households living in the villages selected for the study. We show summary statistics only for the balanced panel dataset used in regressions. The choice of variables follows previous studies using the same dataset (Chemin, 2008; Dalla Pellegrina, 2011; Khandker, 2005; Pitt & Khandker, 1998) and studies using similar quasi-experimental data collected in rural Bangladesh (Imai & Azam, 2012; Islam, 2011).

Around 95% of households in selected villages are male-headed in 1991-92 data against about 90% in the late 1990s. The share of male-headed households is very similar across the borrowers and non-borrowers sub-samples in the early 1990s, but is slightly higher for borrowing households (92%) than for non-borrowing ones in 1998-99. The heads of borrowing households are on average a bit younger and also less educated than their counterparts in non-borrowing households. Table 1.7 shows that the most educated man in a household achieved a higher class on average than the most educated woman in a household. The highest achievers in non-borrowing households are more educated (by about one additional year of schooling) than their counterparts in

[12] The inherent *lumpiness* of such expenses also means they are likely to take many almost unique values, i.e. two households might face similar events but do not necessarily incur the same financial consequences.

borrowing households, and this is the case for both waves of data and for men and women alike.

We see that households who borrow in the first wave of data hold less than 0.5 acre of land on average (46.6 decimals) while those who do not borrow hold 1.6 acres of land on average. The numbers for 1998-99 data appear inconsistent with that of the early 1990s (borrowing households hold more land than non-borrowing households on average), but this might stem from differences in the interview questionnaires between the two waves of data collection. In 1991-92, respondents were asked to report landholdings *before the inception of microfinance programs in their village*, while in 1998-99 they reported landholdings *before 1992*, which can explain under-reporting or at least less precise reports given the time that had elapsed. An alternative measure gathers information from reports of landholdings *after the inception of microfinance programs in the village* for 1991-92 data, and landholdings *after 1992* for 1998-99 data. However, we include a measure of household landholdings in our regressions to try and control for total wealth as an *ex ante* confounding factor in the determinants of microcredit borrowing and subsequent consumption decisions, so we stick to our measure of land *before* microfinance / *before* 1992[13].

Eligibility to group loans is the only variable for which the table shows unweighted summary statistics. About 85% of households in our estimation sample are eligible to group lending in 1991-92, according to village census data on which random sampling was then carried out. Such data is not available for the 1998-99 wave, we use respondents' answers to the question "Does any member of the household qualify to join any poverty alleviation credit programs?" for our classification of eligible households; 89% of households have at least one member who is eligible to such programs. One might think this number alone is a sign that the microcredit programs under scrutiny did not help many people to exit poverty, given that there are even more eligible households after the programs ran for at least 9 years in 72 of the 87 villages in our sample. However, most microfinance initiatives allow successful borrowers to take up new loans, so that some households can still be eligible to group lending even though they are better off than when they joined the program, for instance owning more land than was required to be eligible in the first place.

Our regressions (in the next three chapters) also include the household economic dependency ratio, which is defined as the number of non-active (non-working) members in the household divided by the number of active (working) members in the household. It is a measure of household structure taking into account the number of "earners" in the household as well as the size of the household. The data show it is quite similar for borrowing and non-borrowing households, albeit slightly higher for the latter group. Overall, the average economic dependency ratio is much higher in 1998-99 than in the

[13] It is also the measure of land used in Pitt and Khandker (1998) and Khandker (2005).

early 1990s. This could be due to a number of reasons, for instance more dynamic demographics resulting in larger households with more numerous very young, hence not working, children; or it could be due to the presence in households of ageing older people who stopped working. Another possibility is that access to microcredit worked in favour of children's schooling, and hence higher economic dependency ratios could reflect a substantial decrease in child labour. This last remark is pure speculation, although it does make for a good story. Indeed, Pitt and Khandker (1998) find that group lending is beneficial to children's schooling when loans are given out to women. However, Islam and Choe (2013) show that microcredit can actually have adverse effects on education.

Pioneering studies by Rosenzweig (1988a, 1988b) in rural India established the importance of extended family networks and inter-household transfers (especially through marital ties) as sources of *ex post* risk mitigation solutions. As a conclusion to his work on ICRISAT villages Townsend (1994) recognised that the extended family network should likely be the preferred unit of observation for the study of informal insurance mechanisms, an intuition later strengthened by Fafchamps and Lund (2003) who find that risk-sharing at the village level is not efficient and works in fact through family and friends networks. In their seminal work Pitt and Khandker (1998) acknowledge that family networks can be sources of transfers and informal insurance that can in turn influence a household's decision to borrow and their consumption profile. They include separate variables for whether the parents, brothers and sisters of the household head own land and add three similar variables for relatives of the household head's spouse. In order to limit the number of covariates in our specification we include only one variable for the *total number* of relatives of the household head who own land, along with a similar variable for his/her spouse[14].

Summary statistics show that heads of non-borrowing households and their spouses have on average more relatives who own land (around 5) than those of borrowing households (between 3 and 4), although the difference gets smaller for household heads and almost vanishes for head spouses in 1998-99 data. Our regressions also include the number of relatives of the household head and his/her spouse who live outside the *thana* (sub-district). Such variables account for the fact that geographic distance can potentially offer better – or at least more flexibility for – insurance within the extended family network as it exposes various components of the network to different and maybe little correlated states of the world. For instance, if the agricultural environment is different across sub-districts, members of the network facing a bad crop season have more chance to get help from members in a different area that might not be subject to the same adverse conditions. Additionally, Dalla Pellegrina (2011) shows that participation in the informal credit market depends positively on the distance to in-law relatives, dowries and marriage gifts, suggesting that the uptake of loans from

[14] Our variables account for all relatives: parents, siblings, offspring, uncles and aunts.

Table 1.7 **Summary statistics of household-level variables used in regressions**

	1991-92			1998-99		
	Borrowers	Non-borrowers	Full sample	Borrowers	Non-borrowers	Full sample
Age of HH head	40.20	41.66	41.30	47.70	48.90	48.40
	(11.75)	(13.25)	(12.91)	(12.60)	(13.51)	(13.15)
Education of HH head	2.157	3.007	2.800	2.286	3.117	2.774
	(3.126)	(3.856)	(3.708)	(3.229)	(4.065)	(3.765)
Gender of HH head	0.944	0.955	0.952	0.922	0.884	0.900
	(0.230)	(0.208)	(0.214)	(0.269)	(0.320)	(0.300)
Landholdings	46.45	153.5	127.4	41.30	36.83	38.67
	(84.49)	(351.3)	(311.6)	(135.1)	(148.8)	(143.3)
Eligibility of HH (1=yes)	0.993	0.552	0.660	0.995	0.685	0.813
	(0.0829)	(0.498)	(0.474)	(0.0696)	(0.465)	(0.390)
HH economic dependency ratio	1.698	1.792	1.769	2.490	2.624	2.569
	(1.369)	(1.520)	(1.485)	(1.761)	(2.000)	(1.906)
Highest education of men in HH	2.857	4.093	3.791	3.844	5.013	4.532
	(3.551)	(4.382)	(4.227)	(4.020)	(4.820)	(4.543)
Highest education of women in HH	1.316	2.200	1.984	2.383	3.378	2.968
	(2.543)	(3.446)	(3.270)	(3.308)	(4.127)	(3.841)
# of HH head relatives owning land	3.284	4.662	4.325	2.644	3.183	2.961
	(4.102)	(4.790)	(4.668)	(3.466)	(3.738)	(3.637)
# of HH head's spouse relatives owning land	3.875	5.292	4.946	3.779	3.901	3.851
	(4.887)	(5.844)	(5.656)	(4.550)	(4.269)	(4.386)
# of HH head relatives living outside thana	1.010	1.244	1.187	3.364	3.141	3.233
	(2.397)	(2.101)	(2.179)	(3.360)	(3.320)	(3.337)
# of HH head's spouse relatives living outside thana	1.868	2.428	2.291	4.330	4.470	4.412
	(3.975)	(4.214)	(4.162)	(4.892)	(4.811)	(4.843)
Loans from traditional banks (1=yes)	0.0241	0.121	0.0970	0.0627	0.0926	0.0803
	(0.153)	(0.326)	(0.296)	(0.243)	(0.290)	(0.272)
Loans from informal sources (1=yes)	0.0569	0.148	0.126	0.0580	0.0904	0.0771
	(0.232)	(0.356)	(0.332)	(0.234)	(0.287)	(0.267)
Loans from relatives (1=yes)	0.0439	0.140	0.117	0.170	0.227	0.204
	(0.205)	(0.347)	(0.321)	(0.376)	(0.419)	(0.403)
Observations	781	857	1,638	840	798	1,638

Note: Sample means and standard deviations (in parentheses) for household-level variables. Statistics account for sampling weights. The additional label '(1=yes)' indicates dummy variables. The word household is abbreviated by 'HH'. Landholdings are measured in decimals, where 1 decimal = 0.01 acre. The eligibility status of a household is taken from census data for the first wave (1991/92) and from the questionnaire module on loans for the second wave (1998/99).

moneylenders also has some sort of link with primary mutual insurance networks. Similarly, "wealthy" (or, rather, "landed") relatives can provide guarantees and help securing traditional bank loans that require collateral (Dalla Pellegrina, 2011).

Therefore our set of variables accounting for the number of relatives who own land and those who live outside the *thana* are relevant in controlling for the strength and depth of a household extended family network as a potential source of transfers and insurance, and they are also relevant in controlling for opportunities to access sources of credit other than microcredit, all of which are likely to influence borrowing and consumption decisions. Finally we control directly for alternative sources of credit with dummy variables taking a value of 1 if a household reports loans from *banks* (commercial, government-run and cooperative banks), *informal sources* (input supplier, shopkeeper, landlord, employer) and *relatives and friends* (including neighbours). Table 1.3 shows that in 1991-92 6% of households in our sample borrow from traditional sources, and around 10% from informal sources and relatives and friends; only 3% of households participating in microcredit programs borrow from banks (9% of non-participating households do), while about 5% of them get loans from other sources (15% and 12% of non-participating households borrow from informal sources and from relatives, respectively). In the second wave of data over 7% of households borrow from banks and informal sources while 19% borrow from relatives and friends. Around 6% of households involved in microcredit borrowing also borrow from banks and informal sources (the share is around 9% for non-participating households), and 15% of them get loans from relatives and friends (23% of non-participating households do).

Table 1.8 provides summary statistics for village-level variables included in some of our regressions that do not specify village fixed effects or village quantile effects. The set of village-level controls includes the levels of average wages for men and women; separate dummy variables for the presence of any primary school in the village, the presence of any food program (run by NGOs or the government), and the availability of electricity in the village; variables that measure the distance in kilometres to the nearest bank, the closest market and the nearest "pucca" road[15]; and prices for six popular food items that are rice, wheat flour, mustard oil, hen's eggs, milk and potatoes.

5 Conclusions

The aim of this chapter is to present the data used for empirical analyses in subsequent parts of the thesis. The data we gather from the World Bank website were collected for

[15] The term "pucca" comes from Hindi and means "solid, permanent". A "pucca" road is a road of good quality, i.e. a black-topped road. Similarly, "pucca" houses would refer to dwellings made of sturdy material such as brick or cement.

Table 1.8 **Summary statistics of village-level variables used in regressions**

	1991-92	**1998-99**
Average female wage	37.53	65.77
	(9.457)	(16.96)
Average male wage	16.50	37.85
	(9.594)	(12.84)
Primary school (1=yes)	0.678	0.897
	(0.470)	(0.306)
Food program (1=yes)	0.552	0.471
	(0.500)	(0.502)
Distance to nearest bank (km)	3.580	2.910
	(2.927)	(2.349)
Distance to nearest *pucca* road (km)	2.612	1.171
	(3.411)	(1.609)
Distance to nearest shop/market (km)	1.561	0.995
	(2.090)	(0.974)
Electricity in village (1=yes)	0.506	0.414
	(0.503)	(0.495)
Price of rice	11.14	12.65
	(0.856)	(1.705)
Price of wheat flour	9.575	12.77
	(1.004)	(1.047)
Price of mustard oil	52.50	58.25
	(5.810)	(5.973)
Price of hen's eggs	2.483	2.787
	(1.949)	(0.489)
Price of milk	12.52	15.68
	(3.031)	(4.576)
Price of potatoes	3.730	7.764
	(1.517)	(2.669)

Note: Sample means and standard deviations (in parentheses) for village-level variables. There are 87 villages in the sample. The additional label '(1=yes)' indicates dummy variables.

the purpose of evaluating the efficiency of microfinance programs on the livelihoods of the poorest of the poor. The study was designed so that the available information could be used to mimic the conditions of a controlled experiment from a statistical standpoint and therefore allow estimation of meaningful impacts, which is known as a quasi-experiment. Investigators in charge of planning data collection built on the fact that all MFIs providing credit in sampled areas deem a household eligible to group lending when they are landless (owning less than half an acre of land). That way, data on landholdings can be used to replicate this exogenous eligibility rule in a strategy to instrument for microcredit borrowings which are potentially endogenous to other household decisions such as consumption behaviour. The exact way identification can be achieved thusly is detailed in the methodological discussion of Chapter 2.

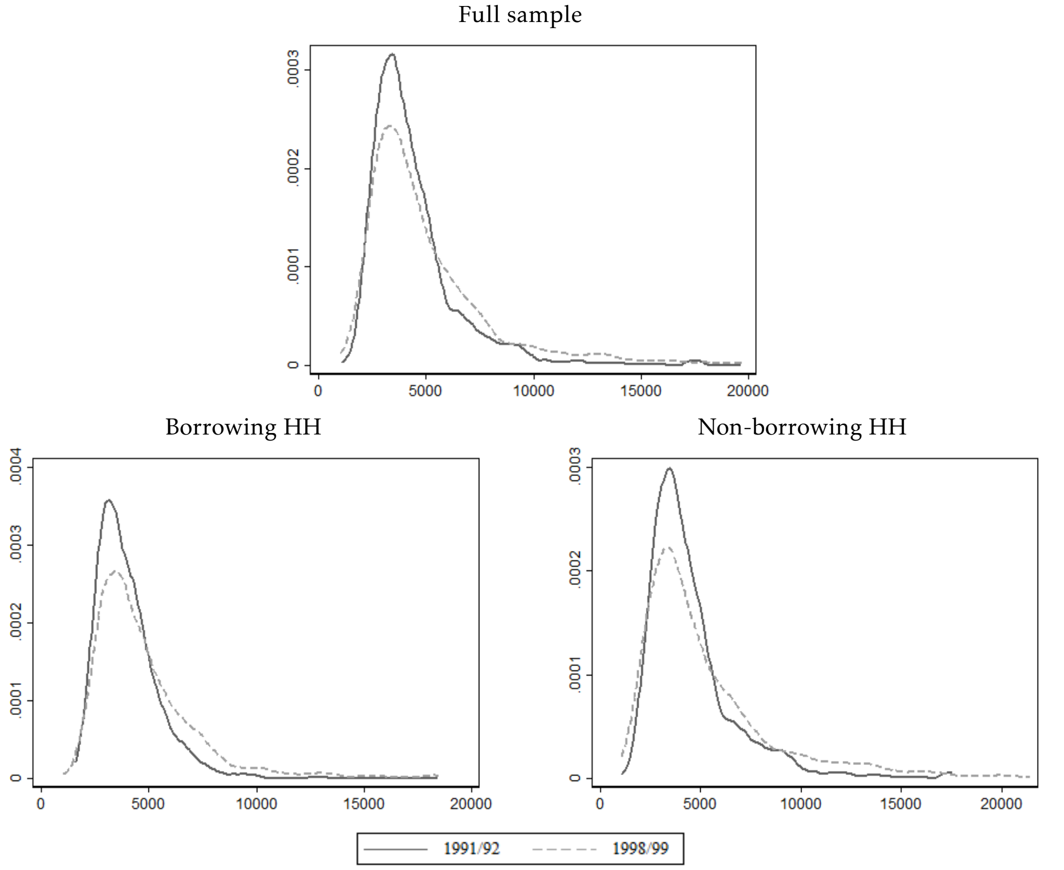
However, we show that defining eligibility based on landholdings data unravels a substantial amount of mistargeting in the sample, i.e. a non-negligible fraction of functionally landed households do actually receive loans from MFIs. This questions instrumental variable approaches used in Pitt and Khandker (1998) and Khandker (2005) and calls for an alternative empirical strategy to identify program impacts by building on the availability of panel data. Two waves of household survey are available, from which we construct a balanced panel dataset consisting of 1,638 households each observed twice over time, in 1991-92 and in 1998-99. We use said estimation sample in all empirical chapters of the thesis, unless indicated otherwise.

This chapter also aims at paving the way to defending the relevance of using quantile regression techniques – as implemented in Chapter 2 – by exploring the distributional features of our main outcome variables of interest, i.e. household per capita total, food and non-food expenditure. Indeed, they exhibit leptokurtic and right-skewed distributions. Dependent variables with such characteristics can be problematic in typical linear-in-means models, making it especially hard to fulfill necessary conditions regarding the distribution of residuals. Quantile regressions are robust to outliers and do not assume normally-distributed errors, thereby eliminating the need for logarithmic transformation and putting focus on objects better suited than expectations (which are sensitive to extreme observations) to represent such ill-behaved outcomes. Moreover, consumption variables are not distributed similarly for borrowers and non-borrowers, the latter typically achieving higher levels of consumption on average but not necessarily at every point of the distribution. Additionally, the distribution of consumption appears to evolve over time not only in terms of location but also with respect to shape, strengthening the idea that one could expect the welfare impacts of microcredit to be potentially different for various categories of beneficiaries. Quantile regressions can be used to estimate the influence of a covariate at multiple points of the distribution while allowing it to vary at different quantiles, hence measuring location- *and* shape-shifting impacts.

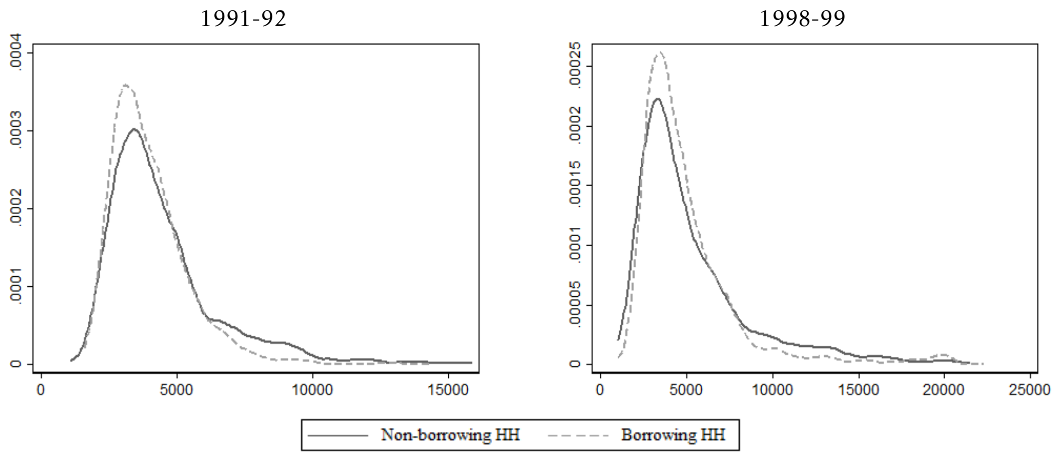
Finally, we provide descriptive statistics for household-level and village-level variables used in regressions to control for socio-economic characteristics and features of the environment facing the population under study in an effort to try and account for as many confounding factors as possible when estimating program impacts.

Figure 1.1 **Kernel density estimates for household per capita total expenditure**

(a) Panel A: By borrowing status



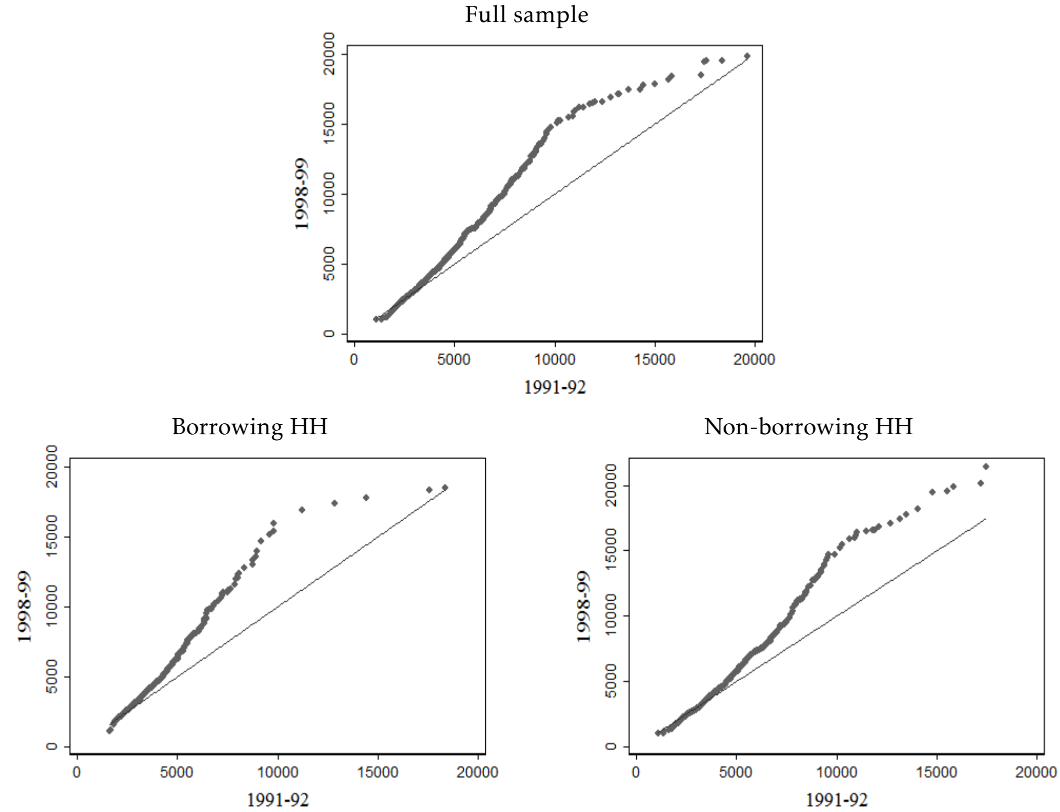
(b) Panel B: By time period



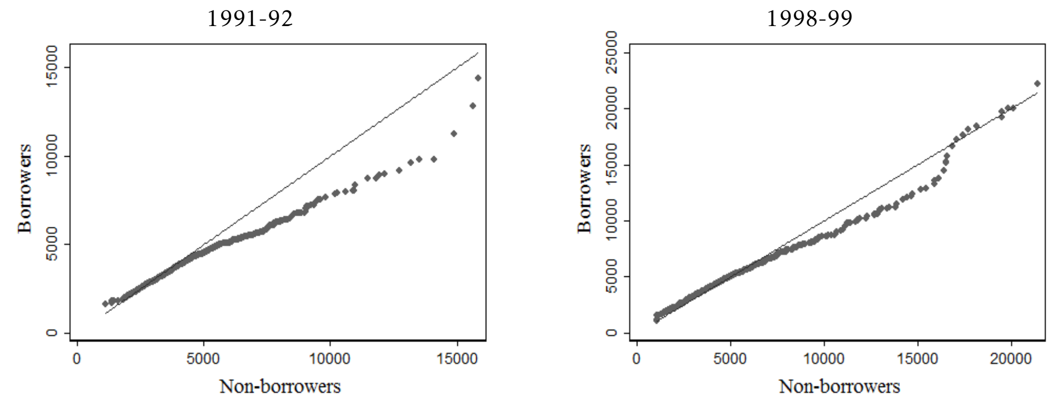
Note: Kernel density estimates using an Epanechnikov kernel function and sampling weights.Because of the long right tails, the top 1% observations of the grand distribution of the sub-sample considered in each graph are trimmed to improve visualisation.It does not hamper the overall shape of the density estimates.

Figure 1.2 **Quantile-quantile plots for household per capita total expenditure**

(a) Panel A: Comparison across time, by borrowing status



(b) Panel B: Comparison across borrowing status, by time period



Note: Quantile-quantile plots. The straight line is the 'identity' line, i.e. the benchmark case in which both distributions are identical. Because of a few very extreme observations, the top 1% observations of the grand distribution of the sub-sample considered in each graph are trimmed to improve visualisation. It does not hamper the overall shape of the plots.

Chapter 2

Distributional Consequences of Micro-Credit Borrowing on Household Consumption Behaviors: Evidence from a quasi-experimental setting

1 Introduction

Motivated by the findings in Chapter 1, we now turn to quantifying the *true* and *hidden* effects of microfinance initiative. The conceptual underpinning and the empirical construct, in this chapter, will draw on the broad implications of data characteristics presented in Chapter 1.

For myriad of economic theoretic and statistical reasons, evaluating the *true* impact of a program, such as a microfinance initiative, is not straightforward. This is because it is often mired by methodological and conceptual complexities. To the very least, the methodological weaknesses or conceptual issues or both can lead to misinformed estimates of microfinance program on household consumption behavior. At a finer level, conclusions arrived at within such an environment may lead to over or under-emphasis of policy concerning the true impact of microfinance program on household consumption.

Indeed, a quick survey of the extant literature reveals that program evaluation has long consisted in estimating average impacts, or program effects on the conditional means of outcome variables of interest. However, very often policymakers might be interested in the impact an intervention has on the distribution of said outcomes. The investigation of the distributional impacts of public policies or development programs has therefore become more and more popular since the late 1990's. We believe that an examination of the distributional impacts of microfinance initiatives is also relevant for today's policymaking. Although it is valuable to understand what impact microcredit has on e.g. the average consumption of borrowers compared to non-borrowers, one might be interested to know whether this impact is greater for those individuals who achieved a relatively low (or high) consumption in the first place. The quantile regression (QR afterwards) estimator introduced by Koenker and Bassett (1978) set the path for researchers to evaluate treatment effects at different points of the distribution of the outcome variable of interest. The authors provided a representation of quantiles as solutions to a linear programming problem, allowing one to estimate quantiles of a variable conditional on a set of covariates.

To the best of our knowledge, examples of studies using QR in the microfinance literature are sparse. In a recent research, Angelucci, Karlan, and Zinman (2015) use survey data on over 16,000 households from a randomised controlled trial (RCT) in Mexico to investigate the impact of expanding access to credit, and they complement their analysis of average intent-to-treat effects with QR to look at potential heterogeneous impacts, finding mild evidence of absence of negative impacts in the lower tails of the outcomes' distributions under scrutiny. Tarozzi, Desai, and Johnson (2015) also exploit a RCT design in rural Ethiopia to investigate the effects of expanded access to credit on economic activities such as investment and entrepreneurship, and the positive average intent-to-treat effects they find on cop-related costs, the value of livestock sales and total revenues from self-employment actually seem to be mostly driven by positive effects above the median, while the increase in crop sales is positive above the fourth decile. Augsburg, De Haas, Harmgart, and Meghir (2015) make the same observation from a RCT in Bosnia and Herzegovina to assess the impact of expanded microcredit using data on marginal applicants for loans, and find that positive impacts on firm profitability are only present at the top of the distribution.

These findings corroborate those by Banerjee, Duflo, Glennerster, and Kinnan (2015) that carry out a randomised experiment in the slums of Hyderabad, India, and by Crépon, Devoto, Duflo, and Parienté (2015) that follow the method by Chamberlain (1994) to estimate quantile treatment effects on various outcomes of an expansion of microcredit access in rural Morocco. In a study of the Thai Million Baht Fund, Boonperm, Haughton, and Khandker (2013) use panel data to estimate a fixed effects model and find out this nationwide promotion of microcredit led to significant increases in income and expenditures for its beneficiaries, and they rely on a panel data fixed effects QR model to assert these effects are pro-poor in the sense they benefitted disproportionately those individuals at the bottom of the income and expenditure distributions. Another example of QR in the context of microfinance is Polk and Johnson (2012) in an attempt to capture the effect of microcredit borrowing on poverty reduction in the Philippines.

Our study aims at estimating the distributional impacts of microcredit borrowing on household consumption outcomes using household-level panel data from villages in rural Bangladesh publicly available on the World Bank's website. We contribute to the existing literature by focussing on objects other than conditional means in our assessment of treatment effects, and we see our approach as a complement to more conventional impact studies that seek to measure average effects only. Even though conditional QR are often decried because of not allowing researchers to infer about the impact of covariates on the marginal distribution of the outcome of interest, we show that they remain relevant tools and provide insightful findings when correctly implemented and interpreted. Indeed, our empirical results lead us to conclude that microcredit helps all borrowing households generate similar food consumption gains but that overall welfare gains are heterogeneous and depend on a household's relative social status given its socio-economic characteristics. Moreover, we take on the econometric challenges inherent to the study of microfinance programs with non-experimental data by exploiting as best we can the panel structure of our dataset in order to tackle the issues of non-random program placement and self-selection in the context of QR.

We use QR on pooled data with village dummy variables and household-level correlated random effects, the latter following the work of Abrevaya and Dahl (2008). Ours is, to the best of our knowledge, the first study to make use of panel data QR with correlated random effects to investigate the impacts of microcredit in rural areas of a developing country[1]. Last but not least, in a field that tends to hold controlled experiments as a golden standard, we hope our work can demonstrate that observational data, albeit flawed in design, nevertheless harbor much valuable information.

The next section briefly reviews the empirical literature on microfinance and provides some context. Section 3 presents the study design, and Section 4 introduces QR and existing estimators that propose to deal with endogeneity or applicable to panel data. In light of previous studies that exploited the same data we do, we detail our empirical strategy in Section 5 before describing the variables used in regressions. Section 6 describes results from preliminary cross-sectional estimations. Finally, in Section 7 we present our main results from panel data QR while Section 8 exposes our final conclusions regarding their interpretation, before acknowledging the main limitations of our study. Section 9 concludes.

[1] One instance of a related research is Boonperm et al. (2013) who study the implications of the Thai Million Baht Village Fund Program. However they use a different estimator, and they exploit a natural experiment to investigate a phenomenon that is not exactly similar to the traditional group-lending microfinance programs we scrutinize.

2 Literature review

2.1 The importance of microfinance on household consumption behaviour

The positive impacts of microcredit on household-level economic development have been asserted in numerous studies in various contexts. Microfinance initiatives seem to achieve their prime goal of unlocking investment opportunities by favoring business creation and self-employment (Attanasio, Augsburg, De Haas, Fitzsimons, & Harmgart, 2015; Augsburg et al., 2015; Banerjee et al., 2015; Coleman, 2006) and gains from microcredit borrowing are realized in terms of income (Imai & Azam, 2012; Islam, 2011; Kaboski & Townsend, 2012) and profits (Attanasio et al., 2015; Tedeschi, 2008). Microcredit borrowing can also provide risk mitigation benefits through diminished income uncertainty (Morduch, 1998) and help the poor insure more or less directly against health shocks in the short run, to the extent that it mitigates the need for borrowers to sell livestock in response to such shocks (Islam & Maitra, 2012). Ultimately, borrowing through microfinance programs seems to trigger increases in household consumption expenditures (Annim & Alnaa, 2013; Chemin, 2008; Pitt & Khandker, 1998).

Access to and use of microcredit borrowing has also been shown to exhibit benefits for non-monetary outcomes such as school enrolment of children (Chemin, 2008; Pitt & Khandker, 1998), women's nutrition (Imai & Azam, 2012) and political empowerment (Basher, 2007). The observed consumption and asset accumulation benefits from microfinance persist over time (Khandker, 2005; Khandker & Samad, 2014), although they seem to be stronger and longer-lasting for those individuals who participate continuously to a program (Attanasio et al., 2015; Islam, 2011) as the impacts of previous loans are bound to decrease over time (Islam, 2011; Kaboski & Townsend, 2011). Microfinance initiatives significantly help reducing poverty and extreme poverty at the local level (Khandker, 2005; Khandker & Samad, 2014) while the expansion of access to credit for households living in rural and poor urban areas can play a role in reducing aggregate poverty (Burgess & Pande, 2005).

Nevertheless, the microfinance literature show us that the measured effects of microcredit on the livelihoods of the poor are heavily dependent on context (e.g. different countries; rural versus urban areas, etc) and are not consistent across the board. Potential benefits in terms of entrepreneurship are not always realized, as in studies by Coleman (1999) in Thailand or Copestake, Dawson, Fanning, McKay, and Wright-Revolledo (2005) in Peru. Nor does financial development in rural areas guarantee improvements regarding consumption, as the latter's response to microcredit can be mute (Crépon et al., 2015), or remain unclear as in Gloede and Rungruxsirivorn (2013) whose results state that microfinance fails to work as a consumption-smoothing device. Adverse impacts of microcredit are also not be ruled out, a striking example being the observed increase in child labor at the expense of schooling in the paper by Islam and Choe

(2013) where the authors find this phenomenon to be particularly significant for girls[2]. Moreover, the widespread belief that microcredit beneficiaries are systematically able to repay high interest rates thanks to high returns on investment activities is challenged by Schicks (2013) in a study that warns about the risks of over-indebtedness.

The quality of the data and the empirical strategy used to analyse them are also crucial to the results one might get. Duvendack and Palmer-Jones (2012) criticized the lack of replicability of Pitt and Khandker (1998) headline results that microfinance has significant positive impacts on household consumption expenditure. Roodman and Morduch (2014) provide an insightful criticism of the same seminal study by Pitt and Khandker (1998), questioning the reliability of the results because of a dubious instrumentation strategy.

Randomised controlled trials (RCTs) allow researchers to examine Intent-To-Treat effects, i.e. the impact of mere exposure to microfinance programs without considering actual uptake of loans. RCTs are often seen as a golden standard able to provide results that are more reliable and easier to generalise than those obtained from observational studies, although no methodology is perfect (Banerjee & Duflo, 2009). This is the case, for instance, of 6 studies recently published in 2015 in the American Economic Journal Applied Economics. Banerjee et al. (2015) find insignificant effects on total consumption, profits, women empowerment measures and Human Development Indices in the slums of Hyderabad, India. Most of these results are echoed in the Crépon et al. (2015) study in rural Morocco. Augsburg et al. (2015) unravel insignificant impacts of microfinance on profits and negative effects on total consumption in Bosnia and Herzegovina. If anything, these carefully led investigations shed even more doubts on the effectiveness of microfinance programs which, overall, remains inconclusive.

2.2 Mission drift and the financial challenges of microfinance institutions (MFIs)

Another widely recognized stylized fact about microfinance programs is that they can fail to reach the poorest fringe of the population (Coleman, 2006; Menkhoff & Rungruxsirivorn, 2011) and can also suffer from mistargeting, i.e. some households who are not eligible to microfinance based on landholdings requirements still get credit, as Morduch (1998) and Roodman and Morduch (2014) showed was the case in the dataset used by Pitt and Khandker (1998)[3]. Poor households who are the most vulnerable with respect to their capacity to smooth consumption appear to be excluded from microfinance programs or at least less likely to borrow (Amin, Rai, & Topa, 2003; Pearlman,

[2] This result is consistent with the idea that microcredit increases demand for labour in the household after a household business was setup thanks to the loan.

[3] See also sub-section 2.2.2 of Chapter 1 of this thesis.

2012). One could speculate that those beneficiaries who are already relatively better-off might also exhibit better ability to transform credit into profitable investments that could yield higher income and hence improve consumption; such ability being the reason they were better-off in the first place. If this is the case, average treatment effects could overestimate the true impact of microfinance.

These last remarks relate to the broader debate on the financial sustainability of MFIs, a concern raised early on by Morduch (1999a) that points out the reliance of MFIs on subsidies as their primary source of funds and questions their actual financial viability. For instance Morduch (1999b) shows that with proper provisioning and correct assessment of repayment rates the financial health of Grameen Bank would be jeopardized by small bumps in default rates on overdue loans or a small expansion of its activities. A more recent article by D'Espallier, Hudon, and Szafarz (2013) states that 23% of MFIs are now subsidy independent, but the reliance of the industry on cheap funds remains a timely issue as (Cull, 2015) confirms that few MFIs are actually profitable when accounting properly for the cost of capital, suggesting the sector is still heavily subsidised.

The funding structure of most MFIs that rely on donations and very low interest rates might be essential to their ability to reach remote markets, which is much more costly than traditional banking, and they raise the issue of subsidy-free financial sustainability at the expense of the social mission carried by MFIs. In their attempt to investigate potential trade-offs between financial sustainability and outreach, Cull, Demirguç-Kunt, and Morduch (2007) show that village banks, mostly present on the poorest segments of the market, experience the highest average costs and receive the most subsidies, even though lenders that make smaller loans (a characteristic of outreach to the poorer) do not seem to be less profitable. On the other hand, larger loans (likely less poor-oriented institutions) are associated with lower average costs (and cutting costs is crucial for MFIs to achieve financial sustainability) and individual lenders charge higher interest rates and are more profitable than village banks or solidarity group lenders.

Surprisingly, mission drift is salient for larger individual lenders and group lenders alike, preventing the authors to draw clear-cut conclusions regarding the consequences of healthier finances on MFIs practices. Nonetheless, the existence of such trade-offs is ascertained by Hermes, Lensink, and Meesters (2011) who show that deeper outreach is indeed strongly and significantly negatively related to cost efficiency: the more female borrowers and the poorer the borrowers, the less efficient the institution. To a similar extent, Hudon and Traca (2011) find that almost all subsidized institutions (in a sample of 100 representative MFIs from two microfinance rating agencies) are more productive than non-subsidized MFIs in terms of number of borrowers per staff. This intuition is confirmed by D'Espallier et al. (2013) who analyse data from over 1,000 MFIs around the world to find suggestive evidence that subsidies indeed seem to improve social performances.

On the other hand, unsubsidised lenders seem to have different strategies to achieve financial efficiency depending on context, but they usually come at the expense of their social mission: they tend to charge higher interest rates in Africa and Asia and to have shallower outreach in Eastern Europe and Central Asia, i.e. they target less poor borrowers, while Latin American MFIs reduce lending to women. The link between more subsidies and smaller loan sizes (an indicator of deeper outreach) is also corroborated by the findings in Armendáriz, D'Espallier, Hudon, and Szafarz (2011) who argue that mere uncertainty regarding the reception of future subsidies is harmful for outreach and pushes interest rates up.

Competition and fight for survival can sometimes be useful motors that trigger waves of innovation and push service providers to explore uncharted territories, which is relevant in the case of microfinance as well. However, as suggested by evidence, access to cheap funds seems crucial for MFIs to carry out their social mission. Nevertheless, Muhammad Yunus, among others, was one to voice his fears[4] in reaction not only to the events occurring in India at the time but to the underlying trend of transformations the sector experienced, referring to the emergence of IPOs launched by formerly non-for-profit MFIs, for instance Compartamos in Mexico and SKS in India. As more and more microfinance service providers undergo an institutional transformation towards more shareholder-oriented structures (D'Espallier, Goedecke, Hudon, & Mersland, 2017), increasing commercialisation and fierce competition have made financial sustainability a crucial determinant of the future of microfinance.

Although studying the mission drift of MFIs is outside the scope of that paper, a better understanding of how the gains from microfinance are distributed across destitute and relatively better-off beneficiaries could help the debate. If microfinance programs appeared to exhibit heterogeneous impacts that systematically benefit little or not at all the poorest of the poor, one could even start to wonder whether such initiatives can carry out the social mission they were set out to fulfill in the first place.

2.3 In search of the heterogeneous impacts of microfinance

The main contribution of this paper is to implement QR techniques to investigate the potentially heterogeneous impacts of microfinance programs. Generally speaking, existing studies on the impacts of microfinance focus on effects at the conditional mean of the outcome variable. However, one might wonder whether such *average* impacts are truly representative for the population under study. In their structural evaluation of the "Thai Million Baht Village Fund" program, Kaboski and Townsend (2011) develop

[4] Opinion piece in The New York Times, "Sacrificing microcredit for megaprofits", 2011, January 14.

a theoretical framework that allows for heterogeneous responses of consumption and investment to a surprise relaxation of credit constraints (expansion of access to credit) depending on a household's level of liquidity.

A few empirical studies also undertake to unravel whether microfinance loans recipients respond differently to newly gained access to credit. For instance, Islam (2015) finds that microcredit borrowing benefit more in terms of consumption to those households who hold less arable land. Copestake et al. (2005) find positive impacts on household income that are greater for the sub-sample of beneficiaries in the upper half of the income distribution (above the median). But suggestive evidence based on mere subsampling is not very satisfactory. A more suitable statistical technique to investigate this question could be quantile regressions, which allow one to examine the impact of changes in covariates at various points of the conditional distribution of the outcome variable.

Exploiting the "Thai Million Baht Village Fund" natural experiment to study data from nationally representative surveys, Boonperm et al. (2013) use panel data to estimate a fixed effects model and find out this nationwide promotion of microcredit led to significant increases in income and expenditures for its beneficiaries. They rely on a panel data fixed effects QR model (following Gamper-Rabindran, Khan, and Timmins (2010) in an application of the methodology by Chen and Khan (2008)) to assert these effects are pro-poor in the sense they benefitted disproportionately those individuals at the bottom of the income and expenditure distributions.

Angelucci et al. (2015) use survey data on over 16,000 households from a randomised controlled trial (RCT) in Mexico to investigate the impact of expanding access to credit, and they complement their analysis of average intent-to-treat effects with QR to look at potential heterogeneous impacts, finding mild evidence of absence of negative impacts in the lower tails of the outcomes' distributions under scrutiny. Tarozzi et al. (2015) also exploit a RCT design in rural Ethiopia to investigate the effects of expanded access to credit on economic activities such as investment and entrepreneurship, and the positive average intent-to-treat effects they find on crop-related costs, the value of livestock sales and total revenues from self-employment actually seem to be mostly driven by positive effects above the median, while the increase in crop sales is positive above the fourth decile. Augsburg et al. (2015) make the same observation from a RCT in Bosnia-Herzegovina to assess the impact of expanded microcredit using data on marginal applicants for loans, and find that positive impacts on firm profitability are only present at the top of the distribution. These findings corroborate those obtained in Banerjee et al. (2015) that carry out a randomised experiment in the slums of Hyderabad, India, and by Crépon et al. (2015) that follow the method by Chamberlain (1994) to estimate quantile treatment effects on various outcomes of an expansion of microcredit access in rural Morocco.

3 Data characteristics and previous studies

3.1 The dataset

As explicated in details in Chapter 1, our dataset consists of a survey of 1,798 households sampled from 87 villages in rural Bangladesh (about 20 households in each village) that were selected across 29 sub-districts (or *thanas*). We use three waves of data for our empirical investigation. The first wave of data was collected during the years 1991 and 1992 through three rounds of survey corresponding to the three main rice seasons of the year; 1,769 households of the original sample were successfully interviewed the three times. A second wave of data was collected in 1998-99. There were 1,638 households from the first wave successfully re-interviewed in 1998-99, and 237 of these had split into 546 households for a total of 1,947 observations in 1998-99 for originally sampled households. In sum, the sample we use includes 1,638 households that were successfully interviewed three times in 1991-92 and again in 1998-99. Details about the study design and data collection can be found in Chapter 1 of the thesis.

3.2 Quasi-experimental design and estimation issues: insight from previous studies

The sampling strategy presented in Chapter 1 makes it clear that potential statistical biases can easily arise in the analysis. First, program placement is not random. Only the selection of villages where programs are effective is random, while the *thanas* from which they are sampled are chosen precisely for the availability of microfinance in these areas. It is likely that MFIs do not randomly choose where to setup their offices. They might want to serve poorer areas where the need for credit is high and their social mission is crucial, or alternatively they could start in less destitute areas to ensure the financial viability of their operations before expanding to more remote and riskier segments of the market. Second, credit uptake is left to self-selection, posing the question of how households form their decision to borrow (or not to). Although we are able to control for a number of household characteristics and socio-economic variables, there almost surely remain some factors crucial to the borrowing decision and not observable to the researcher. One example that comes to mind is one's entrepreneurship ability, which must be determinant for the decision to borrow when most loans are given out for productive purposes. Moreover, existence of similar unobservable variables at the *thana*- and/or village-level is not to be ruled out.

Studies exploiting observational data often face the difficulty that valid excluded variables are not readily available to enable the use of an instrumental variable approach or a sample selection correction model to deal with endogeneity issues. With that in mind, data collection in this study was designed as a "quasi-experiment" in order to provide a candidate variable to be used as an instrument. Pitt and Khandker (1998) explain that this is the reason why households were sampled based on their eligibility status, which the authors consider to follow an *exogenous* rule based on land ownership. They carry out their analysis on the 1991-92 data. They define a household *choice* variable based on the eligibility status of a household and the availability of a microfinance program in the village. Indeed, an individual can only borrow if microcredit is available to her and if she is eligible. The *choice* variable is then interacted with the whole set of exogenous covariates to provide the instruments[5].

A detailed exposition of their approach is provided in Roodman and Morduch (2014), along with critics about its validity. The main concern is that identification hinges on the validity of the exclusion restriction for program-eligibility as defined by the supposedly *exogenous* rule about landholdings. We showed in Chapter 1 that according to this rule a substantial amount of mistargeting exists in the data (around 1 in 5 borrowing households is not eligible in 1991-92), which means that the *choice* variable introduced above could not instrument the amount of credit correctly as it would be zero for mistargeted households (ineligible borrowers)[6]. As it so happens, before estimation Pitt and Khandker (1998) actually classify households as eligible if they own no more than half an acre of land or if they effectively borrow, therefore not exploiting the *exogenous* discontinuity they defended in the first place (Roodman & Morduch, 2014).

Thanks to the second wave of data from 1998-99, panel data analysis techniques can be used to mitigate the endogeneity issues at hand. Let's assume an empirical model of the same form as Khandker (2005) where the outcome of interest C (say, consumption) for household i in village j for period t is determined by current and past characteristics including levels of borrowing S:

$$C_{ijt} = X_{ijt}\alpha + X_{ij(t-1)}\beta + S_{ijt}\delta + S_{ij(t-1)}\gamma + \eta^{c}_{ij} + \mu^{c}_{j} + \epsilon^{c}_{ijt} \tag{2.1}$$

where X is a vector of household- and village-level characteristics, η and μ represent unobserved time-invariant characteristics at the household and village levels, respectively, ϵ is the error term, and credit demand is given by:

$$S_{ijt} = X_{ijt}\lambda + \eta^{s}_{ij} + \mu^{s}_{j} + \epsilon^{s}_{ijt} \tag{2.2}$$

[5] The actual empirical strategy in Pitt and Khandker (1998) is more complex, as variables enter in logarithms (hence inputting a censoring value for when credit is zero), village fixed effects are introduced, and the authors differentiate between the impacts of microcredit issued to women versus men from the three possible lenders considered in the 1991-92 study, thereby estimating 6 coefficients of interest. They coin their estimator weighted exogenous sampling maximum likelihood-limited information maximum likelihood-fixed effects (WESML-LIML-FE).

[6] Roodman and Morduch (2014) use LOWESS regression plots to show that there is no apparent discontinuity in microcredit borrowings along the distribution of landholdings, although Pitt and Khandker (1998) deemed their approach to identification akin to that of a regression discontinuity design.

The superscripts c and s signify the fixed effects and error term belong to the 'consumption' or the 'credit demand' equations, respectively.

The above empirical model allows for the impact of microcredit on household-level outcomes to vary over time, with the inclusion of the lagged value of microcredit stock S[7]. Therefore, coefficient δ measure the impacts of the *current* stock of (female and male) microcredit on contemporaneous consumption while coefficient γ estimates capture the *long-term* impacts on current consumption of the *past* stock of household borrowings (for men and women borrowers). Furthermore, Khandker (2005) breaks down credit variables by gender, in line with baseline results from Pitt and Khandker (1998) that advertised different benefits from microcredit depending on the gender of borrowers, i.e. one regression yields two values for each coefficient δ and γ. In Khandker (2005), a first-difference version of Equation 2.1 is estimated directly via OLS to yield unbiased and consistent coefficient estimates, assuming that household- and village-level endogeneity stem from unobserved attributes that are time-invariant and hence controlled for by household-level fixed effects.

The author measures microcredit impacts on three outcome variables, namely household per capita total, food and non-food expenditure measured annually and expressed in 1992 Taka. He reports that an extra Tk 100 in microfinance loans given out to women in the previous time period yield an average marginal gain of about Tk 15 in household total annual expenditure, and gains of Tk 7 and Tk 8 in food and non-food expenditure, respectively[8].

As exposed in Khandker (2005), both equations must be estimated jointly, which would raise endogeneity issues in a cross-section framework such as that in Pitt and Khandker (1998). In a panel data setting, the introduction of household-level fixed effects pick up any unobserved household-level characteristics that is time-invariant, including such unobservable that might influence credit demand and consumption decisions. By the same token, time-invariant unobserved village-level attributes are also controlled for by household-level fixed effects assuming uncorrelated error terms between the consumption and credit demand equations (Khandker, 2005).

However, Khandker (2005) rightly points out that several such hidden characteristics may vary over time, hereby jeopardising the consistency of parameter estimates, as the author puts it: "The unmeasured determinants of credit at both households and village levels may vary over time, and if credit is measured with error (which is likely), the error gets amplified when differencing over time, especially with only two time periods.

[7] Note that credit stock is assumed to depend only on *contemporaneous* household characteristics, although Khandker (2005) argues that allowing for credit demand to depend on past characteristics would not change an estimation strategy based on fixed effects panel data analysis.

[8] Regressions are carried in logarithmic form, hence estimating elasticities, with dependent variables expressed in terms of *per capita* consumption, while marginal gains are computed for *total* consumption of an average-sized household.

This measurement error will impart attenuation bias to the credit impact coefficients, biasing the impact estimates towards zero." (Khandker, 2005). As a consequence of this statement the author advocates for the use of an instrumental variable approach, and proposes to use the same instruments as in Pitt and Khandker (1998), i.e. to interact the *choice* variable with each and every exogenous covariate to create the set of instruments in the first stage.

Khandker (2005) does note that the purpose of instrumentation is different: in the previous study it was aimed at getting rid of endogeneity; here it is used to deal with measurement error and potential time-varying heterogeneity. The author does not provide much more details about the instrumentation strategy and reports only the household-level fixed effects OLS estimation results, a model that is not out-performed by the IV approach according to the Hausman-Wu test, although results from the latter and from the 2SLS regression are not reported.

More details are to be found in the original version of Roodman and Morduch (2009) that is a well-known constructive critique of the Pitt and Khandker (1998) methodology, and in which they also replicate and criticise the study in Khandker (2005). For instance, although they replicate the pattern of results presented in Khandker (2005) quite nicely, they cannot reject the hypothesis that OLS and 2SLS differ, which makes them question the reliability of the conclusions of the paper about the very strong impact of microcredit on poverty reduction, the latter being computed from a numerical extrapolation based on the OLS results.

4 Overview of quantile regression methods

4.1 The linear quantile regression estimator

Let Y be a random variable with distribution function F_Y and $\tau \in [0,1]$. Define the τ-th quantile of F_Y, denoted by $q_Y(\tau)$, as:

$$q_Y(\tau) = F_Y^{-1}(\tau) = \inf\{y : F_Y(y) \geq \tau\} \tag{2.3}$$

The τ-th sample quantile of Y is then any solution to the following minimization problem (Koenker & Bassett, 1978):

$$\min_{\xi} \sum_i \rho_\tau(Y_i - \xi) \tag{2.4}$$

where $\rho_\tau(.)$ is the tilted absolute function (Koenker & Hallock, 2001), more commonly referred to as the "check" function:

$$\rho_\tau(u) = (\tau - \mathbb{1}[u < 0])u \tag{2.5}$$

and $\mathbb{1}[.]$ is the indicator function.

Now, consider that Y has a conditional distribution function $F_{Y|X}$ with its τ-th (conditional) quantile defined similarly as above by $Q_{Y|X}(\tau) = F^{-1}_{Y|X}(\tau)$. Assuming the τ-th conditional quantile of Y is linear in a set of covariates X, we can write:

$$Q_{Y|X}(\tau) = X'\beta_\tau \tag{2.6}$$

and an estimator for β_τ is given by:

$$\hat{\beta}_\tau = \underset{\beta_\tau}{\operatorname{argmin}} \sum_i \rho_\tau(Y_i - X_i\beta\tau) \tag{2.7}$$

which is the representation of linear conditional QR made popular by Koenker and Bassett (1978) for which a solution can be found through linear programming techniques as discussed for instance in Koenker and d'Orey (1987, 1994).

The main interest of QR lies in that it allows researchers to characterise the impact of a set of covariates at points of the conditional distribution of the response variables other than the mean as is typically done in "traditional" econometrics. As Koenker puts it: "from a policy standpoint it is important to have a clear indication of how the mean response to changes in [a covariate of interest] is 'allocated' to the various segments of the conditional distribution of [the outcome of interest], and this is what quantile regression analysis provides." (Koenker, 2005, Section 2.4.1, p.50)[9].

Another advantage of QR is that it is robust to outliers compared to OLS in the same fashion that the median is robust to outliers compared to the mean as a measure of central tendency. Moreover, QR are able to capture sources of heterogeneity stemming from the covariates when errors are not necessarily i.i.d. (Koenker, 2005). An intuitive way to visualise this property is to consider the following model that allows for a simple form of heteroscedasticity:

$$y_i = \beta_0 + \beta_1 x_i + (\gamma x_i)u_i \tag{2.8}$$

[9] The phrases in square brackets are amendments made to the original quotation to make it more general.

Equation 2.8 is what Koenker (2005) coins the *linear location-scale model*, where the u_i's are i.i.d. with distribution function F_u. The τ-th conditional quantile of Y is given by:

$$Q_{Y|X}(\tau) = \beta_0 + \beta_1 X + (\gamma X) q_u(\tau) \tag{2.9}$$

where $q_u(\tau) = F_u^{-1}(\tau)$ is the τ-quantile of the error term $u = \{u_i\}$ as defined before. We can then easily see that:

$$Q_{Y|X}(\tau) = \beta_0 + (\beta_1 + \gamma q_u(\tau))X = \beta_0 + \beta_\tau X \tag{2.10}$$

Equation 2.10 shows that QR of Y on X yield estimates of β_τ that vary with each estimated quantile level, reflecting the type of heteroscedasticity specified in (2.8). This example illustrates the ability of QR to capture some heterogeneity inherent to the data at little cost[10]. One might notice that in the case of homoscedastic i.i.d. errors (set $\gamma x_i = 1$ in (2.8)) the influence of $q_u(\tau)$ at various quantiles is absorbed in the constant, i.e. β_τ is the same for all $\tau \in (0,1)$ and the estimated conditional quantiles are parallel lines[11].

Note that it was assumed so far that the "true" conditional quantile function is linear, which of course is debatable. Angrist, Chernozhukov, and Fernández-Val (2006) show that QR can actually be seen as providing a minimum mean squared errors linear prediction of the conditional function, the implication being that QR is the best linear approximation to the conditional quantile function, much like OLS gives the best linear approximation to the conditional expectation function.

The robustness and relative simplicity of QR have made this technique a popular tool in empirical work since the mid-1990s. See for instance the book edited by Fitzenberger, Koenker, and Machado (2002), "Economic Applications of Quantile Regression", for a review of the uses and interest of this technique in modern econometrics.

4.2 Endogeneity in quantile regressions

Let us introduce a regression model that resembles that of our empirical study. We denote Y the observed outcome variable, say consumption, observed for a sample of N households, that can be written as:

$$Y_i = X_i \beta + C_i \delta + \epsilon_i \qquad \text{for } i = 1, ..., N \tag{2.11}$$

[10] Obviously, more complex forms of heteroscedasticity would not necessarily be adequately controlled for by specifying a simple linear conditional quantile function as in 2.9.

[11] This discussion and equations 2.8 to 2.10 draw heavily on Sections 1.4 and 1.5 of Koenker (2005), Chapter 7 of Angrist and Pischke (2008) as well as a brief exposition in Gamper-Rabindran et al. (2010).

where X is a set of covariates and ϵ represents a household-specific error term. The extra regressor C represents household microcredit borrowings in our case, and the coefficient of interest δ can be consistently estimated via OLS provided idiosyncratic errors are i.i.d., follow a Normal distribution with zero mean and constant variance, and are orthogonal to X and C. A household decision to borrow likely depends on measurable socio-economic factors X, but also on some unobserved households characteristics. Let credit C assume the following reduced-form representation:

$$C_i = Z_i + u_i \tag{2.12}$$

where Z is a set of measured household characteristics, for instance the variables in X, and u_i's are household-specific disturbances. Endogeneity concerns arise when the latter are correlated with the errors in (2.11), which violates the orthogonality condition on ϵ (i.e. that $E(\epsilon_i C_i) \neq 0$) and introduces bias in the OLS estimate of δ. When Z also includes at least one other exogenous variate that does not enter equation 2.11, then δ can be consistently estimated via an instrumental variable or control function approach.

To get a representation of endogeneity issues in QR, let us re-write Equation 2.11 as follows:

$$Y_i = X_i\beta(\tau) + C_i\delta(\tau) + \epsilon_i(\tau) \tag{2.13}$$

For a given quantile level τ, QR run on Equation 2.13 consistently estimates the τ-th conditional quantile of Y provided a zero conditional quantile restriction on the error term, i.e. that $Q_\tau(\epsilon|X,C) = 0$. The latter can be written in the form of a conditional expectation as in Kim and Muller (2013):

$$E(\phi_\tau(\epsilon_i)|X_i, C_i) = 0 \tag{2.14}$$

where $\phi_\tau(u) = \tau - \mathbb{1}[u \leq 0]$, for any quantile τ. This equality tells us that, at a given quantile, the *rank* of an observation in the conditional distribution of the response variable i.e. whether it is above or below the estimated conditional quantile function (the associated error term being positive or negative, respectively) must not be influenced by the values of the regressors, on average. Therefore, endogeneity at quantile τ is adequately described by $E(\phi_\tau(\epsilon_i)|X_i, C_i) \neq 0$ (Kim & Muller, 2013)[12]. The consequence is that, in our context, microcredit and household consumption might be endogenous at given quantiles of the conditional distribution of the latter, but not necessarily everywhere.

[12] Kim and Muller (2012) define endogeneity in a similar way based on the following orthogonality condition: $E[X_i\phi_\tau(\epsilon_i)] = 0$ (assuming only covariates X enter the regression equation).

An early suggestion to deal with endogeneity in QR was made by Amemiya (1982) in the context of median regression where the problematic regressor(s) is replaced by its fitted values from a linear-in-means first stage[13]. Kim and Muller (2004) generalise this approach to all quantiles and recommend using linear QR in the first stage, with both stages of estimation carried out at the same quantile level. The authors also show that any remaining bias from using OLS in the first stage is concealed to the constant, the approach still yielding consistent estimates for the slope coefficients which usually are of primary interest (Kim & Muller, 2012). This methodology follows the intuition of two-stage least squares and could be regarded as its quantile counterpart.

Abadie, Angrist, and Imbens (2002) discuss the use of instrumental variables in QR and provide identification results for a QR equivalent of the Local Average Treatment Effect when both treatment and the instrument are binary variables. Chernozhukov and Hansen (2006) propose an estimation procedure to recover quantile treatment effects when treatment is endogenous and a strong instrument is available under a rank similarity assumption. Other approaches suggest to address sample selection bias in the spirit of Heckman using control functions, such as in Buchinsky (1998, 2001) or Lee (2007), while Arellano and Bonhomme (2017a) proposes to use copulas to correct directly for selection bias in QR coefficients[14]. Other approaches include Chesher (2007) and Imbens and Newey (2009) who focus on identification under endogeneity in triangular models with non-separable disturbances, and also a recent paper by D. Powell (2016b) presenting an estimation procedure to recover quantile treatment effects on the unconditional distribution of the outcome variable, even when treatment is endogenous.

As of today, to the best of our knowledge, no method has emerged as superior to others, nor do standardised tests exist for the detection of endogeneity or for checking instrument validity in QR. A discussion of quantile models with endogeneity can be found in Chernozhukov and Hansen (2013).

4.3 Quantile regressions with panel data

Let us write a simple panel data regression model where outcome Y observed for household i at time t depends on a set of covariates X and a set of household-specific effects:

$$Y_{it} = X_{it}\beta + \alpha_i + \epsilon_{it} \tag{2.15}$$

Equation 2.15 is the typical representation of a "fixed effects" model[15]. Individual

[13] The asymptotics of this estimator were worked out in J. L. Powell (1983).

[14] Arellano and Bonhomme (2017b) provide a survey of such methods in the context of QR.

[15] We take this opportunity to clarify an often confusing semantic choice. In our discussion of panel data models, "random" and 'fixed' effects always refer to the way unobserved individual components are

effects, or unobserved individual heterogeneity, enter additively and are assumed time-invariant while their distribution is left unspecified, allowing the researcher to control non-parametrically for potential correlation between unobserved components α_i and one or several variables in X. In practice, in a balanced panel dataset with two time periods such as ours, model 2.15 can be estimated directly by running OLS on the first-difference version of 2.15, yielding a consistent estimate of parameter β (see e.g. Wooldridge, 2010b).

Unfortunately, no analogous transformation on quantiles exists that yields similar results for QR because linear functions do not pass through the quantile operator, i.e. the quantile of the difference does not equal the difference of the quantiles. First-differencing before implementing QR is not necessarily wrong, but the object of interest then becomes the conditional distribution of a differenced variable, making the interpretation of the results less intuitive and maybe even irrelevant (see e.g. Abrevaya & Dahl, 2008; Kato, F. Galvao, & Montes-Rojas, 2012; Koenker, 2004; Koenker & Hallock, 2000, among others).

The consequence is that potential correlation across individual-level repeated measurements and/or correlation between individual effects and regressors have to be handled *directly*. One could specify a QR version of (2.15) such as:

$$Y_{it} = X_{it}\beta(\tau) + \alpha_i(\tau) + \epsilon_{it}(\tau) \tag{2.16}$$

and consider the τ-specific individual effects as parameters to be estimated. However, this strategy is unreliable for many real-world applications because QR will suffer from the incidental parameters problem when the number of panels greatly exceeds the number of time periods (Graham, Hahn, & Powell, 2009; Kato et al., 2012; D. Powell, 2016a). For instance, studying a model of the form of (2.16) with our data amounts to estimating 1,638 individual effects on top of the common parameters pertaining to covariates, using only 3,276 data points (we have two time periods).

Understandably, there is a rich literature on QR with panel data. Koenker (2004) proposes to focus on the following conditional quantile functions for response variable Y:

$$Q_{Y_{it}}(\tau|X_{it}) = \alpha_i + X_{it}'\beta_\tau \qquad \text{for } i = 1,...,N \text{ and } t = 1,...,T \tag{2.17}$$

where the unobserved individual components α_i are restricted to have a location-shift

defined. Random effects representations assume the individual unobserved components are independent of the error term, uncorrelated to the regressors, and follow some pre-specified distribution function. In fixed effects models, the distribution of individual-specific unobservables is left unrestricted, allowing for arbitrary correlation with the error term and regressors, and individual effects can be seen as parameters to be estimated. We make this clarification because researchers with different backgrounds can understand the concepts of random and fixed effects in different ways.

effect only, i.e. they are the same across all quantiles[16] (note that the impact of covariates is allowed to vary across quantiles). The author makes a case to circumvent the high-dimensional nature of this specification (when the number of individuals is very large) by estimating the model for k quantiles simultaneously and imposing a penalty on the fixed effects which results in solving a problem of the form:

$$\min_{(\alpha,\beta)} \sum_k \sum_t \sum_i w_k \rho_{\tau_k}(Y_{ij} - \alpha_i - X'_{ij}\beta_{\tau_k}) + \lambda \sum_i |\alpha_i| \tag{2.18}$$

where w_k is a weight defining the contribution of the k-th quantile to the estimation of the individual effects, and λ is a regularisation parameter to be chosen by the researcher. The penalty term based on absolute values is called an l_1-penalty and is built to shrink the individual effects towards a common value to an extent determined by λ. For small values of λ tending to 0 the α_i's tend to their values in the unpenalised version of the estimator, whereas they tend to be zero for all i when λ tends to infinity. In practice the choice of λ remains at the discretion of the researcher (Koenker, 2004) and its impact on estimated coefficients can be quite substantial especially in datasets with numerous panels and few time periods (see for instance simulation studies in Geraci and Bottai (2007) and Bache, Dahl, and Kristensen (2013)). Although it does not directly address the incidental parameter problem, when a fair amount of shrinkage is allowed the penalty term will make the smaller village effects (in terms of magnitude) tend to zero which in turn can make for a sparser design matrix and improve the precision of the estimated slope coefficients (Koenker, 2004).

Canay (2011) also specifies pure location-shift individual effects and proposes to run a least squares regression to get estimates of the fixed effects, which are then subtracted from the response variable, the resulting transformed outcome being used as the dependent variable in linear QR. Ponomareva (2010) derives identification results of quantile coefficients in panel data with small T when fixed effects have a distributional shift impact under the condition that all covariates have continuous support; if (some) regressors are discrete identification is possible only when individual effects are independent from the error term (location-shift effect only). In a competing approach, Geraci and Bottai (2007) proposes a simulation-based estimation procedure for QR with random effects, assuming the quantile error term follows an asymmetric Laplace distribution and individual effects are independent from the error term and orthogonal to regressors.

The instrumental variable QR estimation procedure developed in Chernozhukov and Hansen (2006) is adapted by Harding and Lamarche (2009) to deal with endogeneity

[16] When the α_i's are τ-dependent as in Equation 2.16, they are implicitly allowed to have a distributional shift effect, i.e. to impact both *location and scale* of the conditional distribution.

in panel data models that include *quantile individual effects*[17] and interactive effects (Harding & Lamarche, 2014). Chetverikov, Larsen, and Palmer (2016) also develop an estimator to recover quantile treatment effects under endogeneity in panel data models when treatment is allocated at the level of a group.

In sum, the heart of the issue is to consider unobserved individual effects as parameters to be estimated while no transformation of the data can adequately 'get rid of them' in the context of QR[18]. Even though some methods are straightforward to implement such as that of Canay (2011) or the model considered in Kato et al. (2012), most of the aforementioned approaches rely on large T-large N asymptotics for consistency, while conditions for identification in a very small T setting are quite restrictive (as in e.g. Ponomareva, 2010). These considerations were crucial for our choice of a suitable empirical strategy given the nature of our data that have a large number of panels and only two time periods, as we detail in the next section.

5 Empirical methodology

In this section we discuss the details of our empirical strategy to exploit the panel dataset presented in Chapter 1 of this thesis.

We first carry out a cross-sectional analysis, performing regressions on both waves of data separately with an exploratory purpose, trying to unravel potential patterns of heterogeneity in how microfinance credit can influence household consumption behaviour. The first sub-section presents our baseline regression model and explains how we try to address the endogeneity issues pointed out in Section 3 of this chapter.

The second sub-section details how we intend to implement adequate panel data techniques in the context of QR, as the availability of longitudinal panel data – i.e. each household in the sample is observed twice over time – offers alternative ways of dealing with endogeneity, especially that stemming from self-selection. Given the limitations in addressing endogeneity using only cross-sections of our data, we consider the panel data approach our preferred specification in that it exploits more information and is more robust.

In each sub-section – whether discussing cross-sectional or panel data analysis – we take stock of what has been done in previous studies that used the same data before exposing our methodology.

[17] The authors point out that characterising individual effects as *fixed* when they are allowed to vary by quantile is "not fully appropriate" and therefore prefer to refer to them as *quantile individual effects*.

[18] We discuss models that assume additivity in the individual effects and the error term. For instance, Powell (2016b) develops a novel approach to estimating quantile panel data models with non-additive individual effects based on a rank similarity assumption, although the proposed moment-based estimator does not allow for the inclusion of covariates.

5.1 Cross-sectional data analysis

5.1.1 Insight from previous studies

This sub-section builds mainly on Pitt and Khandker (1998) (PK afterwards) and Roodman and Morduch (2009, 2014), and therefore equations and notations are very close to those found in their papers.

PK carry out their study on cross-sectional data and model response variable Y for household i in village j as a linear function of household microcredit C and household-level covariates X as follows:

$$Y_{ij} = C_{ij}\delta + X_{ij}\gamma + \alpha_j + \epsilon_{ij} \tag{2.19}$$

where α_j represents village j unobserved characteristics that influence Y_{ij} and ϵ_{ij} is a household-specific disturbance. Direct estimation of Equation 2.19 via OLS yields a consistent estimate of the parameter of interest δ under a zero conditional expectation restriction on the error term, i.e. that $E(\epsilon|X,C,\alpha) = 0$. We note that regressor C, the credit variable, stems from a household decision to borrow and that it can be modelled in a way analogous to Y in Equation 2.19 as proposed in PK:

$$C_{ij} = X_{ij}\gamma^c + Z_{ij}\lambda + \alpha_j^c + \epsilon_{ij}^c \tag{2.20}$$

where Z embeds household and village characteristics that affect the decision to borrow only (and not household behaviour Y_{ij}). Other terms are defined as in Equation 2.19 and subscript c signifies they pertain to the 'credit equation'.

We know from the the discussion in sub-section 3.2 that program placement in the sample is not random, which leads to possible correlation between α_j^c and α_j, hence violating the zero conditional expectation restriction on the error term in Equation 2.19 and yield biased OLS estimates. Similarly, credit program participation (and the amount borrowed) is left to self-selection, so we expect there exist variables entering the decision to borrow that are not observable and might influence other household decisions such as outcome Y. Again, the restriction on the error term in 2.19 is violated because of correlation between ϵ_{ij}^c and ϵ_{ij} and OLS yield a biased estimate of δ.

PK propose to implement an instrumental variable (IV) approach[19] to address endogeneity issues[20]. As explained in sub-section 3.2, the three microfinance initiatives

[19] Discussion of the estimation strategy in PK is very technical and can be hard to follow. Roodman and Morduch (2009, 2014) present it in a clear and more intuitive fashion and we build on their work for this paragraph, as well as on responses to their critique from Pitt (1999, 2014).

[20] In studies using the same dataset, Chemin (2008) uses propensity score matching (PSM) techniques that assume selection be stemming from observed characteristics only, while Dalla Pellegrina (2011) uses

considered in the study officially abode by the following rule: households must hold no more than half an acre of land to be eligible to microcredit borrowing. The interaction of the eligibility indicator e with a dummy variable p taking value 1 if a program is available in a village generates a household *choice* variable $c = p \times e$. It follows that a household decision to borrow depends first and foremost on c: microcredit borrowing is observed only when $c = 1$, i.e. when a *household is eligible* and lives in a village where a *program is available*. Because they choose a continuous measure of microcredit (household cumulative borrowings over the past 6 years), PK increase the variability and explanatory power of their binary candidate IV by interacting choice variable c with household and village characteristics to create their final set of instruments Z[21].

Identification hinges on two assumptions: the 'landholdings rule' of eligibility was set exogenously, and household landholdings are also exogenous. Morduch (1998) explores landholdings data to challenge the latter, while Roodman and Morduch (2009, 2014) show there is actually no clear discontinuity in microcredit participation along the distribution of landholdings, pointing to the substantial mistargeting in the data (a large proportion of ineligible households who do in fact borrow) and questioning the validity of the aforementioned eligibility rule as an exogenous cut-off point from which identification of the parameters of interest should stem[22]. The critiques by Morduch (1998) and Roodman and Morduch (2009, 2014) spawned a rich debate about the strategy used in PK with the latter being fiercely defended in responses by Pitt (1999, 2014).[23]. Notwithstanding the interest of the aforementioned discussions regarding econometric techniques and the virtues of transparent research to make replication easier, we do not comment further on these issues as our study involves a much different empirical setting, namely quantile regressions, and does not aim at replicating previous work but rather at taking stock of it in order to provide a sensible complementary analysis.

an IV setup to try to identify the impact of various sources of credit, including microcredit, on productive investment.

[21] This approach to IV estimation in a quasi-experimental setting, i.e. the *choice* variable as candidate IV, has become popular and was used for instance in Islam and Maitra (2012), Islam and Choe (2013) and Islam (2015) in studies of the impact of microfinance in Bangladesh (using a dataset different from ours), and in Boonperm et al. (2013) to analyse the impact of the nationwide Thai Million Baht Village Fund program.

[22] Roodman and Morduch (2014) explain that PK actually classified ineligible borrowers as eligible when estimating their model.

[23] Similarly, Duvendack and Palmer-Jones (2012) could not fully replicate the results of Chemin (2008) using PSM and built on their own estimates to challenge the findings of PK and Chemin (2008), an exercise that earned them two heated responses from the authors in question (Chemin, 2012; Pitt, 2012).

5.1.2 Baseline cross-section specification

We start by estimating a quantile version of Equation 2.19 as specified in PK, hence modelling the conditional quantile function of outcome variable Y as follows:

$$Q_\tau(Y_{ij}|C_{ij}, X_{ij} = C'_{ij}\delta_\tau + X'_{ij}\beta_\tau + \alpha_j(\tau) \tag{2.21}$$

The relationship in (2.21) is assumed to hold for all i and all j at every quantile τ provided the usual restriction on the error term mentioned in sub-section 4.2 of this chapter. Coefficient estimates are obtained via the textbook QR estimator of Koenker and Bassett (1978). Note that we allow for village-level unobserved components and leave their impact free to vary at each quantile, as is the case for other right-hand side variables. Regressor C stands for our measure of household microcredit borrowing and therefore δ_τ is the coefficient of interest. Other variates in X control for household characteristics and capture features of a village economic environment.

We estimate Equation 2.21 at every decile to recover the marginal impact of microcredit on the conditional distribution of consumption. Whether households rank above or below the conditional quantile function estimated at the τ-th quantile depends on their characteristics. That is, the rank of households in the conditional distribution is given *relative* to other households with similar characteristics. Therefore, δ_τ represents the shift in consumption imputable to microfinance loans on the consumption level of households who consume relatively little or much (depending on quantile level τ) given their socio-economic features. That way, Equation 2.21 allows us to investigate whether borrowers benefit from microfinance differently based on their pre-existing level of welfare.

We know from sub-sections 4.1 and 4.2 that in spite of its robustness properties, and even though it can handle some level of individual heterogeneity, the linear QR estimator can be subject to an endogeneity bias. Unfortunately, our options are limited by the structure of the data and the debatable quality of the candidate instrumental variable. Nevertheless, we carry out an attempt at correcting for the endogeneity of household microcredit and consumption through an IV approach.

5.1.3 IV approach for cross-sectional data

The candidate instrument is a household *choice* variable (c) defined as the interaction of two dummy variables: the eligibility status of the household (e) and the availability of microfinance programs in a village (p). All exogenous covariates are then interacted with c to create the set of instruments. While Roodman and Morduch (2014) consider PK's framework in the "traditional" IV setting to discuss the credibility of the necessary exclusion restriction regarding the candidate IV, Pitt (2014) points out that identification in PK does not rely on the instrument being excluded, but rather on the fact that the outcome variable is observed “for individuals that exogenously have no choice to obtain (credit) treatment” (Pitt, 2014). Essentially, their strategy is akin to instrumenting for microcredit with all exogenous covariates on a restricted sample for which $c = 1$, acknowledging the fact that credit is deterministically zero for observations that have $c = 0$ (Pitt, 2014).

In order to mimic PK’s instrumentation strategy we elect to implement the two-stage QR (2SQR) approach discussed in Kim and Muller (2012). We construct the household choice variable c defined above, and the first step consists of a least squares regression of household microcredit borrowings on the full set of exogenous variables for the subset of households for which $c = 1$. We then replace microcredit by its first-stage predicted values in QR at each decile, making sure to set predictions to zero where $c = 0$. In the case of gender-specific impacts, we define one choice variable for each gender built on the availability in a village of micro-lending to single-sex groups of the corresponding gender. In 1991-92, our first wave of data, 22 villages have female-only groups, 10 have programs providing credit to male-only groups and 40 have single-sex groups of both genders. Hence, there are two first-stage regressions in that case, one for each gender.

Recall that there is a substantial amount of mistargeting in the data, i.e. some borrowing households are not functionally landless although this is meant to be the main eligibility criterion. In order to make sure all observations with non-zero credit are included, eligibility is defined *de facto* as in PK (Morduch, 1998). In other words, in program villages households are deemed eligible if they own less than 0.5 acre of land *or* if they actually borrow, while only the former criterion applies in control villages[24].

Note that event though Kim and Muller (2004) recommend the use of QR in the first stage, we do not follow this approach because credit variables have a mass point at zero which rendered QR in the first step unfeasible because of the sample restrictions imposed by our instrumentation strategy. However, as pointed out in sub-section 4.2, Kim and Muller (2012) show that 2SQR with OLS predictions provides consistent slope estimates as any remaining bias in second stage estimation is limited to the constant. Therefore, we feel that our approach using linear-in-means estimators in the first stage is still sensible.

We must close this section with a few remarks regarding cross-sectional analysis of 1998-99 data. By the late 1990’s, every village in the sample has access to microfinance, including former control villages. Therefore, the *choice* variable described earlier cannot be defined and sample restrictions in the first stage are merely based on

[24] Morduch (1998) raises the issue that groups of eligible households in program and control villages might not be comparable as the former also includes a substantial proportion of landed households.

eligibility status defined de facto. Moreover, the three microfinance initiatives first under scrutiny in 1991-92 are not the only ones present in sampled villages in the second wave of data, and their activities are not limited within selected *thanas* anymore (see section 3 and our discussion of the study design). Although PK differentiate impacts of credit on households outcomes for different lenders (BRAC, Grameen Bank and BRDB), this distinction is not relevant in the 1998-99 data. For the sake of consistency, we do not allow for differential credit impacts between micro-lenders in 1991-92 either[25]. Finally, the 1991-92 variables identifying villages in which microfinance programs offer credit to single-sex groups of a given gender were not available in raw data from the World Bank's website and were obtained from a webpage at the Center for Global Development website that hosts data and resources relating to a revised version of Roodman and Morduch (2009) published as Roodman and Morduch (2014)[26]. They are not available at all for the 1998-99 data.

5.1.4 Empirical specification and estimation procedure

We estimate model (2.21) at each decile for both waves of data separately and include alternatively village covariates or a set of village dummy variables to control for non-random program placement[27]. The latter is more conservative but it implies the estimation of 86 village effects (there are 87 villages, one is left out as a reference point) which might increase the variability of other estimated coefficients, a consequence of the incidental parameters problem mentioned in sub-section 4.3. Therefore, we also implement the penalised QR method to restrict village fixed effects to have the same impact at each quantile of the conditional distribution (Koenker, 2004).

In fixed effects specifications, it is crucial for 2SQR estimates that first-stage regressions on restricted samples also include village dummy variables so as to control for possible unobserved community-level that could influence demand for credit in program villages. Indeed, we see that the outcome and credit demand equations - (2.19) and (2.20), respectively - each include their own set of village effects[28].

Along with impact estimates for household microcredit, we follow PK and accommodate the possibility of gender-based returns to microfinance loans by splitting microcredit borrowings into two variables, i.e. loans issued to female/male members of a

[25] Another reason for considering microcredit borrowings as a whole in 1991-92, instead of just these three MFIs, is that some borrowers already got micro-credit from various sources at that time.

[26] David Roodman himself obtained the information from a dataset sent to him by Mark Pitt in one of their communications.

[27] Controlling for observable village characteristics alone does not account for unobserved village characteristics that can correlate with the error term and it leaves the issue of non-random program placement partly unaddressed.

[28] This point was made in Morduch (1998) to argue that PK fail to account for it, and Pitt (1999) responded that the very design of their estimator, with restricted first-stage estimation samples, effectively allows for two different sets of village effects as reflected by equations (2.19) and (2.20).

household. Interestingly, PK was extremely influential in strengthening the belief that female borrowers do more for global household welfare than male borrowers. Anyhow, the microfinance initiatives under scrutiny when the study was set up only offered loans to single-sex groups, so it is sensible to differentiate the impacts of microcredit by gender in our dataset[29].

For inference, we use a block bootstrap procedure to generate 999 samples from our data by drawing villages randomly with replacement. The reason we draw at the village level rather than drawing individuals is to account for the within-cluster dependency structure of the data block bootstrap is often used to compute cluster-robust standard errors (see MacKinnon (2002) and Cameron, Gelbach, and Miller (2008), among others). We generate bootstrap samples separately for each wave of data. Then, we perform estimations on each bootstrap sample[30]. The quantile slope coefficients for microcredit variables shown in the results section are estimates from baseline regressions carried out on the original sample. The empirical distribution of our estimates is used directly to build 95% confidence intervals in the fashion of Efron and Tibshirani (1993), sometimes referred to as percentile bootstrap confidence interval[31]. Parente and Santos Silva (2016) developed a method to produce cluster-robust standard errors in QR, however they do not allow for sampling weights that are crucial to our analysis[32].

Another advantage of bootstrapping is that we can obtain an estimate of the variance-covariance matrix across quantiles, which opens the door for Wald-type tests of cross-quantile restrictions[33]. For each treatment variable under consideration we test the null hypothesis that the estimated coefficients are equal across all quantiles, and also whether they are jointly equal to zero. Rejection of the null for the first test indicates significant heterogeneity in the quantile process, while for the second test it means that the quantile process as a whole is not zero. In regressions with distinct credit variables for each gender, we test at each quantile whether the impact of female microcredit is statistically distinguishable from that of male microcredit. Finally, we carry out joint hypotheses tests to formally determine whether the quantile process as a whole

[29] Recent advances in theoretical microeconomics have put forward the issues of gender and bargaining power in the process of intra-household decision-making in developing countries (Fuwa, Ito, Kubo, Kurosaki, & Sawada, 2006; Xu, 2007). The influence of newly gained credit access on the balance of bargaining power within the household is studied in a theoretical model by Ngo and Wahhaj (2012), and Alam (2012) finds evidence that self-employment returns to microcredit significantly promote gender empowerment in rural Bangladesh.

[30] Note that for 2SQR we perform both stages of the estimation on each bootstrap sample.

[31] This approach provides better coverage than alternative bootstrap confidence intervals because the distributions of our bootstrap estimates are often asymmetric. Especially, such confidence intervals do not include "unrealistic" coefficient values (or rather, values that are not empirically observed in our bootstrap estimates).

[32] The same authors developed the **qreg2** command in Stata that performs QR with cluster-robust standard errors. Failing to account for sampling weights in our data produces substantially different coefficient estimates.

[33] See Abrevaya and Dahl (2008) for details on the testing procedure.

is similar for both credit variables, and also whether all quantile coefficients estimated on the two treatment variables are jointly equal to zero.

5.2 Panel data

In the previous section we discussed econometric issues pertaining to our dataset and more generally to the evaluation of microfinance programs based on observational data, namely *non-random program placement* and *self-selection*. We now present how we try to address these exploiting the fact that most households sampled in the study were interviewed at two points in time.

5.2.1 Baseline panel data model specification

The empirical framework in Khandker (2005) closely follows that of PK while drawing on the advantages of panel data. The author re-writes Equation 2.19 to include a time component as well as dynamics:

$$Y_{ijt} = C_{ijt}\delta + C_{ij(t-1)}\theta + X_{ijt}\gamma + X_{ij(t-1)}\beta + \alpha_{ij} + \eta_j + \epsilon_{ijt} \tag{2.22}$$

where Y_{ijt} is the outcome variable of interest for household i in village j at time t, and depends on contemporaneous and lagged values of covariates X and household microcredit borrowings C. Household and village unobserved components that influence outcome Y α_{ij} and η_j, respectively enter model (2.22) additively and are assumed to be time-invariant. In line with PK, Khandker (2005) defines a credit demand equation:

$$C_{ijt} = X_{ijt}\lambda + \alpha^c_{ij} + \eta^c_j + \epsilon^c_{ijt} \tag{2.23}$$

where superscript c signifies household and village time-invariant characteristics and the error term belong to the credit equation. As exposed in Khandker (2005), both equations must be estimated jointly, which would raise endogeneity issues in a cross-section framework such as that in PK. In a panel data setting, the introduction of household-level fixed effects can remove household and village-level endogeneity if the source of the latter are unobserved attributes that are time-invariant (Khandker, 2005). More details about the empirical strategy of Khandker (2005) can be found in the original version of Roodman and Morduch (2009).

For the purpose of our study, we modify Equation 2.22 and perform QR on panel data assuming that the conditional quantile of the outcome variable of interest respects the following linear specification:

$$Q_\tau(Y_{ijt}|C_{ijt}, X_{ijt} = C'_{ijt}\delta_\tau + X'_{ijt}\beta_\tau + \alpha_{ij}(\tau) + \eta_j(\tau) \tag{2.24}$$

The parameter of interest is δ_τ, the impact of microcredit on outcome Y. Note that this specification is very similar to Equation 2.21 specified in the cross-sectional case, only with the addition of household-specific unobserved characteristics α_{ij}. Their inclusion allows us to control for any time-invariant household feature that influences both the decision to borrow and the outcome of interest. Recall that in the context of QR there is no prior transformation of the data that can get rid of the quantile-specific individual and village effects and still yield estimates of the desired quantities. One might first-difference the data and run a linear QR on the transformed variables, but the object of interest is then different and nothing guarantees that quantile individual effects are appropriately dealt with. Indeed, Kato et al. (2012) remind us of what Koenker and Hallock (2000) noted: "Quantiles of convolutions of random variables are rather intractable objects, and preliminary differencing strategies familiar from Gaussian models sometimes have unanticipated effects." (see also our discussion of QR methods for panel data in sub-section 4.3). Therefore, individual effects have to be handled *directly*, in a fashion explained in the next sub-section.

One big difference with the OLS specification used in Khandker (2005) is that variables enter Equation 2.24 *in levels*. As a consequence we choose not to include dynamics in our model because of the potentially high collinearity between contemporaneous and lagged credit variables. Also, our measure of microcredit borrowings includes previous loans going as far back as six years, which means that our impact estimates necessarily embed some long-term effects of microcredit on household behaviour[34]. The inclusion of the lagged value of cumulative microcredit borrowing would imply to estimate the impact on contemporaneous household consumption of loans taken up 7 to 12 years ago.

5.2.2 Specification of household correlated random effects

The purpose of household-specific effects is to capture the potential correlation between microcredit and the error term stemming from unobserved attributes that also impact household outcomes. Assuming the self-selection process into microfinance programs is the same in both time periods and emerges from time-invariant hidden characteristics, this is akin to controlling for endogeneity in our credit variables. The large panel dimension of our data with only two time periods means that none of the methods mentioned in sub-section 4.3 is applicable to our problem as they rely on large N-large T asymptotics. The only candidate would then be the method developed

[34] By construction, long-term and contemporaneous effects are implicitly constrained to be equal.

in Ponomareva (2010) but our specifications include a mix of continuous and discrete variables as covariates, which violates the identifying assumptions of the estimator.

In their study of the impact of birth inputs on birthweight, Abrevaya and Dahl (2008) (AD henceforth) develop a quantile version of Chamberlain (1982) *correlated random effects* (CRE) model. They propose to model individual effects as a function of covariates as follows (we drop the subscript for village j):

$$\alpha_i = f(X_{i1}, X_{i2}) \tag{2.25}$$

So the individual effects in each time period depend on the values of the individual's observed characteristics of both time periods. Later in the discussion we sometimes refer to X_{i1} and X_{i2} as *fixed covariates*, because for instance X_{i1} takes the value of covariates X_i in period 1 for *both* time periods alike; similar reasoning applies to X_{i2}[35]. A closely related approach to controlling for numerous individual effects with two time periods in the context of median regression is considered in Chen and Khan (2008). There, function $f(.)$ in (2.25) is left unspecified and their two-stage estimation strategy involves a non-parametric first step. Their methodology was extended to other quantiles in an application by Gamper-Rabindran et al. (2010).

In its simplest form, function $f(.)$ in (2.25) is linear in its arguments, hence:

$$\alpha_i = c + X_{i1}\lambda_1 + X_{i2}\lambda_2 + v_i \tag{2.26}$$

where c is a scalar and v_i is a disturbance uncorrelated to X_{i1} and X_{i2}. Following the specification and notations in AD, we can then write the conditional quantile function of interest for each time period as follows (we absorb credit variable C into covariates X and drop the dependence on village j for ease of exposition):

$$Q_\tau(Y_{i1}|X_i) = \phi_\tau^1 + X_{i1}'\theta_\tau^1 + X_{i2}'\lambda_\tau^2 \tag{2.27}$$

$$Q_\tau(Y_{i2}|X_i) = \phi_\tau^2 + X_{i1}'\lambda_\tau^1 + X_{i2}'\theta_\tau^2 \tag{2.28}$$

These reduced-form equations represent approximations of the 'true' conditional quantiles by general functions of X_{i1} and X_{i2} (linear functions in that case). The quantile effects of interest are defined in AD in a way analogous to those identified in the linear-in-means case, that is:

$$\frac{\partial Q_\tau(Y_{i1}|X_i)}{\partial X_{i1}} - \frac{\partial Q_\tau(Y_{i2}|X_i)}{\partial X_{i1}} \tag{2.29}$$

[35] It is made clear in Bache et al. (2013) that correlated effects might be characterised as a function of only a subset of covariates which are assumed to be *sufficient* covariates.

and

$$\frac{\partial Q_\tau(Y_{i2}|X_i)}{\partial X_{i2}} - \frac{\partial Q_\tau(Y_{i1}|X_i)}{\partial X_{i2}} \tag{2.30}$$

The differences in (2.29) and (2.30) capture the effects of X_{i1} (first time period observables) and X_{i2} (second time period observables) on $Q_\tau(Y_{i1}|X_i)$ and $Q_\tau(Y_{i2}|X_i)$, respectively, above and beyond the contribution of each period's *fixed covariates* to the correlated effects (AD). In order for the estimation of (2.27) and (2.28) to yield the quantities of interest we follow AD and impose the additional condition that the quantile-specific effects of covariates – denoted β_τ – are the same for both time periods. We can therefore re-write the conditional quantile functions as:

$$Q_\tau(Y_{i1}|X_i) = \phi_\tau^1 + X_{i1}'\beta_\tau + X_{i1}'\lambda_\tau^1 + X_{i2}'\lambda_\tau^2 \tag{2.31}$$

$$Q_\tau(Y_{i2}|X_i) = \phi_\tau^2 + X_{i2}'\beta_\tau + X_{i1}'\lambda_\tau^1 + X_{i2}'\lambda_\tau^2 \tag{2.32}$$

AD propose a simple estimation procedure to recover estimates of the parameter of interest β_τ which consists in running the τ-th linear QR on the pooled sample while including the 'fixed' covariates as extra regressors to control for individual effects. An indicator variable taking value 1 for observations in the second wave of data (the 1998-99 sample) and zero otherwise (for the 1991-92) must enter each regression along with the constant to capture the overall distributional shift that can happen between time periods, i.e. to adequately account for $\phi_\tau^2 - \phi_\tau^1$. In traditional panel data econometrics parlance, this is a time effect[36].

The CRE approach is our preferred specification, as it is the only one allowing us to exploit all the information from our two waves of data while adequately controlling for household-level time-invariant unobserved confounding factors[37]. One drawback of this method is that the number of parameters to be estimated grows fast with the number of time periods. For instance, our regressions include 16 household covariates (including credit variables; 17 when differentiating by gender) and two time periods, which implies the addition of 32 (34) variables to model individual effects. Degrees of freedom are a crucial concern given the size of our dataset (especially when our specification includes the full set of village dummy variables), so we choose the so-called

[36] Note that in this setting the contribution of household covariates to the correlated effects is also identified, i.e. the method yields estimates of λ_τ^1 and λ_τ^2.

[37] In a working paper that recently came to our attention, Harding and Lamarche (2017) propose a quantile panel data estimator that combines semi-parametric correlated effects and location-shift individual effects. They propose a one-step estimator that basically follows that of Koenker (2004) where quantile-independent individual effects are shrunk towards a common value thanks to an l_1-penalty to mitigate the variability of the slope coefficients estimates and the quantile-varying part of the individual effects is captured through the inclusion of *fixed covariates* as in AD. We do not follow this approach that would imply the inclusion of 1,638 dummy variables for the individual fixed effects, which would greatly disturb slope coefficient estimates in spite of the penalty term (recall that Koenkers penalised estimator only mitigates the incidental parameter problem).

Mundlak's representation of correlated effects, i.e. they are modelled through the time-averaged values of individual covariates. This characterisation of CRE is mentioned in AD as a possible alternative to their model and is studied more extensively in Bache et al. (2013).

5.2.3 Estimation procedure

We get a first set of baseline results by estimating model (2.24) via pooled QR, ignoring individual unobserved effects. We merely stack observations together and run a linear QR at each decile, with the full set of household and village covariates, or alternatively village dummy variables instead of the latter to better account for non-random program placement. Given that we double the number of observations, direct estimation of time-invariant quantile village effects is less problematic than it is with cross-sectional data and our pooled QR can account for village-level unobserved heterogeneity. Similar to the cross-sectional case, village fixed effects are either left unrestricted to allow for quantile-varying village effects, or penalised following the approach of Koenker (2004) to coerce them to have the same impacts across quantiles and reduce the variability of quantile-varying slope coefficient estimates.

We then re-estimate model (2.24) via pooled QR with the inclusion of household CRE specified as a linear function of time-averaged values of household covariates. Our CRE pooled QR on panel data also include in turn village covariates, village quantile effects or penalised village effects. All our regressions consider the quantile-varying impacts of total household microcredit on consumption, alternatively allowing different impacts by gender of borrowers through the inclusion of both gender-based microcredit variables simultaneously.

Inference is carried out using block bootstrapping in a similar spirit to that of the cross-sectional case, with the difference that re-sampling is done at the household-level to account for the potential dependence of observations made on the same household at different points in time. We perform 999 replications, reported slope coefficient estimates are obtained from baseline regressions on the original sample while the graphs also present 95% percentile bootstrap confidence intervals.

6 Empirical results: Cross-sectional analysis

To minimise repetition, we do not report all the descriptive statistics of the variables and their distributional characteristics. Rather, we focus in this chapter exclusively on the analyses of QR results. The first part of this section comments on results obtained from "naive" regressions performed on both waves of data separately, while the second considers output of estimations carried out in an instrumental variable approach. Our interest lies in the distributional impacts of microcredit on household per capita expenditure, therefore only those coefficients attached to microcredit variables are plotted in graphs along with 95% confidence intervals for each of the specifications explained in details in the methodology section. The latter pertain to the way we account for village-level heterogeneity through the inclusion of village covariates, village dummy variables (to allow for quantile-varying village effects, also coined village quantile effects) or penalised village fixed effects *la* Koenker (2004). Plots are complemented by tables reporting the values of coefficients and their estimated bootstrap standard errors, as well as Wald statistics and p-values for a variety of tests. Regression tables reporting the full set of coefficients for some of our econometric specifications can be found in Appendices B, C and D (for a subset of quantiles).

6.1 Baseline estimates with village covariates

Tables 2.1 to 2.3 report the estimated values and bootstrap standard errors of the coefficients plotted in Figures 2.1 to 2.3, respectively, along with Wald statistics and p-values associated with tests of various restrictions.

The top left graph of Figure 2.1 shows that household cumulative borrowings have a small positive impact on household total per capita expenditure in 1991/92 which increases from 0.006 at the first decile to 0.008 at the 60th quantile, taking smaller values for deciles 7 and 8 (0.006 and 0.004, respectively), with the larger impact observed at the top of the conditional distribution (0.025). Coefficients are imprecisely estimated and the bottom rows of the first column of 2.1 show we cannot reject the null hypothesis that they are jointly equal to zero. Results for the second wave of data (right graph of Panel A) show that microcredit impacts household total consumption positively throughout the distribution, with quantile treatment effects (QTEs) between 0.010 and 0.014 for the bottom four deciles increasing steadily across the rest of the distribution to reach 0.053 at the top decile. Coefficient estimates from the second decile up are significant with 95% confidence. The large standard errors do not allow us to statistically distinguish the estimated coefficients from each other (Wald test with a p-value of 0.292), however they are not jointly equal to zero.

Graphs in Panel B of Figure 2.1 show that the impacts of female microcredit borrowings on household per capita total expenditure in the early 1990s decrease from 0.008 at the first decile to 0.002 at the 80th quantile and peak at the top decile (0.015), none of the estimates being statistically significant and all of them being jointly equal to zero across quantiles. The pattern for estimated QTEs in the late 1990s is similar to that observed for household total microcredit, with coefficient estimates ranging from 0.011 at the bottom decile to 0.054 at the 9th decile, all of which are statistically distinguishable from zero (at the 10% level for the first decile, and 5% level for others) and can be considered as not jointly equal to zero, although we cannot reject the null that they are of similar value across the distribution because of the imprecision of the estimates (Table 2.1, bottom cells of column 6).

The left graph of Panel C indicates a very slightly negative impact of microcredit issued to men on household total consumption in 1991/92 at the bottom of the distribution (-0.001) before it turns positive at the 30th quantile and increases steadily to reach 0.026 at the top of the distribution. The estimates are extremely imprecise and cannot be distinguished from zero, be it individually or jointly across all quantiles. The conclusions regarding significance are the same for the second wave of data with estimates ranging between -0.001 and 0.012.

We note that Wald tests reported in columns 4 and 8 of Table 2.1 do not allow us to reject the null hypothesis that microcredit has the same impact, at a given quantile, whether it is issued to women or men. Even though the estimates of the impact of female credit are always positive, and much larger than those of male credit in 1998-99, the conclusions of the tests are not surprising given the huge size of the estimated standard errors, which is well represented by the impressive width of the 95% confidence intervals. We are very close to rejecting the null with 90% confidence at the top decile in 1998-99 (p-value is 0.101) where the impact of female credit is positive (0.054) and significant. Similarly, the bottom row of column 8 in Table 2.1 shows that we can almost reject the null hypothesis that coefficients attached to female and male credit are jointly equal to zero across all quantiles at the 10% level for 1998-99 (p-value of 0.107).

Turning to Figure 2.2 and the relationship between microcredit and household food consumption, we observe that overall household borrowings have little positive to no impact in the early 1990s, with overwhelmingly imprecise estimates. In the second time period, household credit increases food consumption by 0.006 at the first decile and by 0.008 and 0.007 at the 3rd and 4th deciles, respectively. Statistical significance vanishes from the median up as estimates increase from 0.005 to 0.011 at the top decile, although we can reject the null that all estimates are jointly equal to zero (p-value of 0.096, bottom cell of column 5 in Table 2.2).

The pattern is quite similar for female microcredit (graphs in Panel B of Figure 2.2). In the early 1990s, it increases food consumption by 0.003 at the bottom of the distribution and by 0.009 at the median before the estimates settle back between 0.002 and 0.003 in the top three deciles, although statistical significance is nowhere present along the distribution. The impacts measured in 1998-99 are significant at the 10% level for bottom deciles up to the median (and even at the 5% level for deciles 2 through 4), with values ranging from 0.007 at the 10th quantile to 0.005 at the median, and increasing steadily from 0.008 to 0.013 (8th decile) in the top half of the distribution. Coefficient estimates at the top two deciles are also significant with 90% confidence. We can reject the null of coefficients being jointly equal to zero at the 10% level (p-value of 0.077, bottom cell of column 6 in Table 2.2).

When microfinance loans are issued to men, however, it seems that they have a negative effect on food consumption in 1991-92 (only two occurrences of positive values at the 2nd and 6th deciles), albeit very small in magnitude and not significant. The pattern is less decisive for the late 1990s, the right graph of Panel C in Figure 2.2 depicting an "N-shaped" curve. Estimates increase from just below zero at the 1st decile to 0.008 at the 3rd decile before plummeting to -0.006 at the 8th decile and finally take a small positive value at the top decile (0.001). None of these estimates is significant and they cannot be collectively distinguished from each other or from zero.

The 1991-92 estimates are very imprecise and we cannot reject the null that female and male credit have the same impact on food consumption at any quantile. The same is true at almost all quantiles for the second wave of data except at the 6th, 7th and 8th deciles where we reject the null at least at the 10% level (5% level foe the two latter), with QTEs of female credit between 0.008 and 0.013 and those of male credit below -0.002.

The estimated QTEs of microcredit on household per capita non-food expenditure are plotted in Figure 2.3. It appears that household credit has a positive impact on non-food consumption across the distribution in 1991-92, with a value between 0.002 and 0.003 in the bottom half of the distribution (significant at the 10% level for the bottom 3 deciles) and larger impacts thereafter ranging from 0.006 (6th decile) to 0.021 at the top decile. As for the late 1990s, household credit affects non-food expenditure positively and significantly across the board with estimated QTEs increasing steadily between the bottom (0.003) and top deciles (0.031). They are not statistically distinguishable from each other but we can reject the null that they are jointly equal to zero at the 10% level (p-value of 0.062, bottom row of column 5 in Table 2.3).

The impacts of female credit on non-food expenditure exhibit the same pattern as those of household total credit although they are never significant in the early 1990s. For the second wave of data, the estimated QTEs range from 0.003 at the 10th quantile to 0.034 and 0.032 at the 8th and 9th deciles, respectively, and are significantly different from zero across the distribution (although at the 10% level only for the bottom two deciles). We also reject the null that they are all jointly equal to zero at the 10% level (p-value of 0.054, bottom row of column 6 in Table 2.3).

Again, the aforementioned pattern of results for the early 1990s is observed in the case of male credit, with smaller impacts in the bottom half of the distribution and larger increasing impacts in the top half, albeit statistical significance is nowhere to be found. In 1998-99, the conclusions about significance are the same and estimates increase slowly from 0.001 at the bottom decile to 0.012 at the 8th decile before taking a negative value of -0.003 at the top decile.

Unfortunately, the estimates are too imprecise to allow us to statistically distinguish the impacts of female and male credit on non-food expenditure at any quantile, although estimates can sometimes differ in sign and even by one order of magnitude, for instance at the top decile for the second wave of data where the estimated impact of female microcredit is 0.032 against -0.003 for that of male microcredit.

Let us conclude this first sub-section by a few general remarks on the pattern of results obtained from cross-sectional regressions with village covariates. Overall, statistical significance for our coefficients of interest is almost never achieved for regressions carried out on 1991-92 data, only does it arise for the estimated impacts of household total and female borrowings in most of our regressions on the second wave of data. It appears that microcredit issued to men has virtually no significant impact on household per capita expenditure in either time period, although such conclusions are made with great care given the overall imprecision of our estimates as witnessed by the very large width of confidence intervals plotted on Figures 2.1 to 2.3. However, it is not incompatible with the broad picture one gets from glancing at the pattern of results depicted by plots of the estimated QTEs of household total and female microcredit.

Indeed, the resulting curves typically have similar shapes, and Tables 2.1 to 2.3 show that coefficient values are also very close in most instances. Therefore, this preliminary analysis hints that most of the observed impact of household microcredit on their consumption is actually driven by loans made out to women while those given out to men would have much smaller impacts, if any at all. This can also be an artefact of the data because our sample includes many more female borrowers than male (a consequence of the alleged targeting policy of microfinance programs), and also because average female borrowings are much larger than their male counterpart, possibly overwhelming the latter. At any rate, focussing on significant results, we observe that household total and female borrowings have positive impacts on household expenditure throughout the distribution, with typically larger impacts at higher quantiles of the conditional distribution. This pattern is especially clear in the case of household per capita total and non-food expenditures. It means that microcredit benefits more to those households who are better off in terms of consumption relative to households with comparable socio-economic characteristics. In other words, households who already achieve relatively high consumption given their socio-economic group are better able to increase their consumption thanks to microcredit. For instance in 1998-99, an extra Tk 1,000 in microcredit given out to women helps relatively low achievers (bottom decile of the conditional distribution of consumption) increase their per capita total expenditure by Tk 11 per year, while it helps relatively high achievers (top decile) increase theirs by Tk 54 per year. The overall picture is less clear in the case of food expenditure because estimates are very imprecise, although it seems that household total and female borrowings have positive impacts throughout the distribution with larger effects around the median in the early 1990s and a generally upward sloping quantile process in 1998-99; the estimated impact of male microcredit on food consumption takes on very small values that oscillate around zero in both time periods.

As our estimates stem from conditional QR, they do not allow us to infer about the absolute consumption-poverty status of these beneficiaries. Indeed, some households ranking relatively low in the conditional distribution of consumption can very well rank in the upper parts of its marginal distribution. Nevertheless, we can still say that microcredit does not seem to efficiently promote upward mobility within a given socio-economic group, given that it benefits most to those households who fare relatively well compared to similar households. Households who achieve low consumption relative to similar households experience smaller positive welfare benefits from microcredit than relatively high achievers, hence potentially resulting in sharper inequalities within a given group. This interpretation nonetheless opens the door to the possibility of upgrading to a “higher” social group for a household that receives microcredit while already achieving high consumption relative to its socio-economic reference group, although the odds of multiple subsequent such upgrades for the same household are very low as per the limited potential for within-class upward mobility.

This first set of results and its interpretation will be used as a baseline for our comments on subsequent sets of estimates. Given the great number of results we have to present, we choose to comment the next graphs and tables in a less detailed fashion by emphasising on overall patterns. The sub-section above is also seen as a “guide” to how we present and read our results. Replicating the exact same exercise for all of our results would be tedious and moot.

6.2 Cross-sectional analysis with unobserved village-level effects

6.2.1 Village Quantile Effects

The next set of results, presented in Figures 2.4 to 2.6 and Tables 2.4, 2.5 and 2.6, reports coefficient estimates obtained from regressions that include village dummy variables instead of covariates to try to control for village-level overall heterogeneity in a flexible way such that the issue of non-random program placement is addressed. Such village effects are left unrestricted and are free to vary at each quantile. The drawback of this specification is the number of parameters to estimate that becomes very large, and with the limited number of observations at our disposal we expect our regressions to suffer from the incidental parameter problem mentioned in Section 5. A direct consequence is an increased imprecision in parameter estimates, as is adequately reflected in the wide 95% confidence intervals of the graphs in Figures 2.4 to 2.6. We feel this justifies a shorter discussion for this set of results.

In the first time period, household total microcredit affects total consumption positively and almost constantly across the distribution (left graph of Panel A of Figure 2.4), and has small positive impacts on non-food consumption in the bottom half of the distribution and larger and increasing impacts in the top half (left graph of Panel A of 2.6), a picture broadly consistent with the results observed previously. The upward sloping quantile process identified earlier characterising the impact of household total and female microcredit on household total per capita expenditure in the second time period can be observed again (right graphs of Panels A and B of Figure 2.4), even though the effects are about twice as small in magnitude at the top decile (Table 2.4). This pattern is also persistent when the dependent variable is non-food expenditure (right graphs of Panels A and B of Figure 2.6).

The graphs in Panel C of Figure 2.4 show that male borrowings have positive distributional effects on total expenditure in the early 1990s (larger for the top than for the bottom deciles), and negative effects in the late 1990s with the most adverse impacts experienced at the bottom two deciles and between the 6th and 8th deciles. We can reject the null of equality of the impacts of female and male microcredit in 1998-99 at the 5% level for the bottom two deciles and the 70th quantile, and at the 10% level for the 3rd and 6th deciles (column 8 of Table 2.4). At the bottom of Figure 2.6, we can see that in 1991-92 male microcredit has close to no impact on non-food consumption through deciles 1 to 6, but larger and increasing estimated QTEs at the top three deciles. Conversely in 1998-99, male borrowings have close to zero effect on non-food consumption in the bottom three deciles before the quantile process falls below zero with the most adverse impacts at the top decile. Tests results show that the estimated impacts of male credit on non-food expenditure in the second time period can be considered statistically different from those of female credit at almost all deciles from the median up, except at the 7th decile (rightmost column of Table 2.6).

The overall patterns are harder to discern when food consumption is the dependent variable (Figure 2.5 and Table 2.5). An additional Tk 1,000 in microcredit for a household increases household per capita food expenditure in the early 1990s by Tk 3 per year at the bottom decile and by up to Tk 8 per year at the 3rd decile, with smaller effects at the top three deciles. In 1998-99, the impact of household total credit is small and positive for deciles two to four and at the top two deciles, with estimates slightly below zero at the bottom decile and at the median and 6th and 7th deciles. The effect is 0.005 at the top of the distribution (right graph of Panel A of Figure 2.5 and column 5 of Table 2.5). The quantile processes representing the distributional impacts of female microcredit (Panel B of Figure 2.5) are overall similar in shape to those described above, albeit with smaller effects in magnitude from the median up in the early 1990s, and fewer and less adverse negative effects in 1998-99.

It appears that male microcredit (Panel C of Figure 2.5) in the early 1990s helps household increase their food consumption throughout the distribution with a quantile process similar to that of total microcredit, with small impacts at extreme quantiles and rather large impacts around the median, ranging from 0.012 at the 3rd decile to 0.009 at the 4th and 6th deciles (column 3 of Table 2.5). Interestingly, in this specification with village quantile effects they are the only set of coefficients for which we are close to rejecting the null that they are simultaneously zero across the distribution (bottom row of column 4 of Table 2.5). In the second time period, however, loans issued to men have exclusively negative effects on food consumption, with small adverse impacts at extreme deciles (between -0.004 and -0.003) and larger adverse effects for the rest of the distribution (e.g. -0.013 at the 7th decile). We can never reject the null of equality between the estimated QTEs of male and female credit on food consumption, except at the median at the 10% level for the second time period.

The introduction of village quantile effects in our regressions greatly disturbs the accuracy of the estimated coefficients of interest and naturally often leads to the inability to reject the null for almost all of our statistical tests. Taking stock of this new set of results with great caution, we can only say that the appearing upward sloping quantile process associated with the impacts of household total and female microcredit on total and non-food consumption seems to be robust, especially for the 1998-99 data. The pattern of heterogeneity in the effects of total and female credit on food consumption remains hard to grasp. As for the influence of male credit on our various measures of household expenditure the overall picture is still unclear: with the previous specification distributional impacts often seemed to cluster around zero, and with village quantile effects they experience a seemingly systematic sign reversal, i.e. positive in 1991-92 and negative in 1998-99.

6.2.2 Penalised Village Fixed Effects

Our third and last specification also controls for non-random program placement by the introduction of village dummy variables, only this time the coefficients attached to the latter are constrained to be the same at all quantiles through an l_1-penalty (this estimator is developed in Koenker (2004)). This makes for less flexible village effects but on the other hand it efficiently mitigates the aforementioned incidental parameter problem. The results are reported in Figures 2.7 to 2.9 and Tables 2.7 to 2.9.

At a glance, overall patterns identified in our baseline regressions seem to be corroborated. Household total and female microcredit have positive distributional impacts on household per capita total and food consumption in 1998-99 (right graphs of Panels A and B of Figures 2.7 and 2.8), and for non-food consumption in both time periods (Panels A and B of Figure 2.9), with typically larger effects at higher deciles and the magnitude of maximum impacts at the top deciles in line with those obtained in our first specification (columns 1, 2, 5 and 6 of Tables 2.7 to 2.9). In the early 1990s, household total and female credit increase total consumption throughout the distribution in a heterogeneous fashion with maximum impacts at the 1st, 6th and 7th deciles (left graphs of Panel A and B of Figure 2.7). We find a fairly homogenous positive quantile process for the effect of microcredit on food consumption in 1991-92 with only one negative value at the top decile (all coefficients are insignificant, left graph of Panel A of Figure 2.8). In the same time period female credit shows a maximum impact on food consumption at the bottom decile and a small negative one at the top quantile. The overall picture for the impacts of male credit in 1998-99 show that they are mostly homogenously centred around zero across the distribution (right graphs of Panel C in Figures 2.7 to 2.9), which is also the case for impacts on food consumption in 1991-92. Credit issued to men has positive impacts on total and non-food consumption that are large at the top deciles in 1991-92 (left graphs of Panel C of Figures 2.7 and 2.9).

Statistical significance is strong for the estimated QTEs of total and female credit, especially in 1998-99 (columns 5 and 5 of Table 2.7) and in both time periods for non-food consumption (columns 1, 2, 5 and 6 of Table 2.9). We reject the null of the equality of coefficients on female and male credit from the second decile up for estimated impacts on total consumption in 1998-99, from the 5th decile up for impacts on food consumption, and from the 4th decile up for impacts on non-food consumption. We reject the null that quantile-coefficients are the same for estimated impacts of total credit and female credit on total and non-food expenditures in the late 1990s.

To sum up our findings from this cross-sectional exploratory analysis, we can say that the most robust pattern of results appears to be an exclusively positive and upward sloping quantile process characterising the distributional impacts of microcredit on household consumption, especially in the late 1990s. If microcredit indeed benefits more to those households who already achieve high consumption levels relative to their socio-economic characteristics, then there is a limit on the extent to which microfinance programs can help *upward mobility* within social groups, and such initiatives could even sharpen welfare inequalities amongst households with similar characteristics. Along with the mitigated picture about the impacts of male credit that oscillate around zero and are rarely significant (even when significance is strong for total and female credit), we interpret the fact that the distributional impacts of household total and female credit are very similar in shape as a potential sign that the positive influence of microcredit on household consumption (if it exists) is mostly driven by loans issued to women. This is confirmed in several instances by the rejection of the null in tests of the equality of the effects of female and male credit at a given quantile.

Obviously any clear-cut conclusion would be too premature at this point. We saw that our estimates are overall very imprecise, especially when we include village quantile effects, leading to seldom rejections of the null that the estimated coefficients of a given quantile process are all equal. However, the fact that we manage to reject the null of this test in a few instances even with such imprecise estimates (for instance for the impact of total and female credit on non-food consumption in 1998-99) is encouraging as it shows that there exist heterogeneous impacts of microcredit on consumption that are statistically distinguishable. This confirms the idea that QR techniques are relevant for the analysis of microfinance programs. They are also interesting in that they capture part of household-level unobserved heterogeneity, even though they are not an exact remedy to endogeneity issues stemming from self-selection, which we try to address through an instrumental variable approach in the next section.

6.3 Instrumental variable approach

We now briefly present results from QR on cross-sectional data performed in two steps in an attempt to correct for self-selection into group lending programs, following the methodology detailed in sub-section subsection:ivqrstrat. As such we carry on with the IV approach for the sake of completeness of our study, without hoping for it to be our preferred specification given the potential weakness of the instrumentation strategy. Therefore, we present only a subset of results, for the case where household per capita total consumption is the outcome variable of interest, in Figures 2.10 to 2.12 and Tables 2.10 to 2.12. Graphs and the associated results tables pertaining to other dependent variables can be found in Appendices E and F.

A first general remark is that estimates remain very imprecise and that accuracy does not respond as adversely to the introduction of village unobserved effects as is the case in specifications without instrumentation. In our specification with village covariates, we see in Panel A of Figure 2.10 that household total borrowings in the early 1990s generate consumption gains only at the top decile (0.015) while yielding losses in the rest of the distribution that are sometimes very large (coefficients range between -0.027 at the 3rd decile and -0.009 at the 8th decile), although none of these estimates is significant. Results from 1998-99 data show negative effects of household microcredit on consumption at the second and 7th deciles, with positive estimates for the rest of the distribution and the largest impacts to be found at the top decile (0.059 against 0.016 at the bottom decile, column 5 in Table 2.10).

Panel B of Figure 2.10 displays the distributional impacts of female credit on total consumption. In 1991-92, the quantile process is qualitatively similar to that associated to total credit with expenditure losses everywhere but at the top decile. In the second time period the quantile process is downward sloping, starting from consumption losses of 0.031 at the bottom tail of the distribution down to an adverse impact of -0.461 at the top decile. Panel C of Figure 2.10 displays negative impacts of male credit on total consumption in 1991-92 for the bottom four deciles, with a 0.004 gain a the median and up to 0.037 at the 9th decile. Male credit in the late 1990s impacts total expenditure positively expect at the 6th and 7th deciles, with a maximum impact of 0.559 at the top decile. We can never statistically distinguish between the impacts of female and male microcredit, although none of the individual coefficients are significant.

The introduction of village quantile effects does not offer a clearer picture, only confirming mostly negative estimated impacts from microcredit on total consumption in the top half of the distribution, except in the case of male credit in 1991-92. At any rate, again significance eludes our results (Figure 2.11 and Table 2.11).

Last, Figure 2.12 shows the results of implementing 2SQR with penalised village fixed effects. The left graph of Panel A displays the upward sloping quantile process identified earlier. Household microcredit in 1991-92 positively impacts total per capita consumption to a larger extent in the top half - where coefficients range between 0.022 and 0.041 for deciles 6 to 9 - than in the bottom half of the distribution up to the median with estimated coefficients between 0.009 at the bottom decile and 0.019 at the median (column 1 of Table 2.12). Estimated coefficients are significant with 90% confidence at the top three deciles. The upward sloping quantile process is also present for impacts estimated on 1998-99 data (right graph of Panel A of Figure 2.12. Recipients of microcredit in the second time period of our study experience statistically significant consumption gains that are larger the higher they rank in the conditional distribution of household total per capita expenditure, estimates increasing steadily from 0.020 at the bottom to 0.136 at the top decile (column 5 of Table 2.12). We are also able to reject the null that these coefficients are all equal across quantiles with 99% confidence (second to bottom row of column 5 of Table 2.12).

Female microcredit in the early 1990s also generates consumption consumption gains throughout the distribution, with significant estimates of 0.029 and 0.027 at the 2nd and 3rd deciles, respectively, and a maximum effect of 0.042 at the 7th decile (insignificant). The picture its impacts in 1998-99 resembles that observed for household total microcredit (Panel B of Figure 2.12). Households at the bottom of the conditional distribution experience gains of 0.021, the latter increasing throughout the distribution to reach 0.129 at the 9th decile. Here also all estimates are significant with 95% confidence (column 5 of Table 2.12), and the observed heterogeneity is statistically significant (second to bottom row of column 6 of Table 2.12).

Finally, the effects of male microcredit on total consumption in the specification with penalised village effects are also characterised by a positive upward sloping quantile process, although less precisely estimated than for household total and female microcredit (only one negative estimate at the bottom decile in 1991-92, Panel C of 2.12). Nonetheless, we find the largest impact at the top deciles in 1991-92 (0.031 and 0.056 for the 8th and 9th deciles, respectively) are statistically significant with 90% confidence (column 3 of Table 2.12). Only the estimated impact at the 8th decile in 1998-99 (0.072) is significant, but the quantile process is once again positive and upward sloping. Interestingly, although we can never distinguish between the impacts of female and male microcredit in a quantile by quantile fashion, we are able to overwhelmingly reject the null that both quantile processes are similar across quantiles (second to bottom row of column 8 of Table 2.12).

It is risky to identify any meaningful trend in the results commented above, mostly because many estimates experience sign reversals depending on how non-random program placement is addressed. They are usually negative when using village covariates or village quantile effects, and positive when implementing penalised QR. However, they only exhibit significance in the latter case, especially so for regressions using 1998-99 data. And, even when estimates are not significant, with penalised QR we systematically observe an upward sloping quantile process with larger consumption gains at top quantiles than at bottom ones. Actually, 2SQR results pertaining to food and non-food consumption (available in Appendices E and F) display similar trends as those presented here, with similar sign reversals across specifications, similar significance patterns and similar upward sloping quantile processes when penalised QR are implemented, more notably in 1998-99.

Altogether, given the flags of caution raised in section 5 about this instrumentation strategy, and the obvious instability of results across specifications, we do not feel this strategy is robust enough to allow us to strengthen our previous claims, no matter how tempted we are by some appealing results. However, it does not unequivocally hurt them.

7 Results from panel data regressions

We now turn to results from specifications that exploit the panel structure of the data. One natural advantage is that the number of degrees of freedom is now larger for each regression, which should help generate more precise estimates especially when our specification includes numerous village dummy variables. The first sub-section presents results from QR run on the full sample in the fashion of cross-section QR, with the addition of a time dummy variable on top of the full set of covariates. We coin these "pooled" QR in which we include either village covariates or village dummy variables, the latter controlling for any time-invariant village-level characteristics that we assume also determine non-random program placement, which is then adequately controlled for.

Also, in the panel data context we can employ the empirical strategy proposed in Abrevaya and Dahl (2008) and discussed further in Bache et al. (2013) that allows us to account for time-invariant unobserved household heterogeneity without having to include over 1,600 dummy variables, but with a richer specification than pure random effects allowing some arbitrary correlation between individual effects and covariates.

In other words, in that model unobserved household heterogeneity is correlated with household-level variables. Results from correlated random effects (CRE) regressions are discussed in the second sub-section. We present plots of our estimates in a slightly different order, sorted by dependent variable, and proceed to comment on results from each specification for each chosen measure of household expenditure.

7.1 Pooled quantile regressions

7.1.1 Total expenditure

Panel A of Figure 2.13 shows the distributional impacts of household total borrowings on household total per capita expenditure. When only observable village heterogeneity is controlled for (left graph of Panel A), estimated QTEs are all positive and increase steadily across the conditional, from 0.008 at the bottom decile to 0.039 at the top decile. Coefficients are statistically significant with 95% certainty, and we can reject the null hypothesis that they are jointly equal to zero, although the other Wald-type test does not allow us to distinguish between them (column 1 of Table 2.13).

The introduction of village quantile effects (middle graph of Panel A) narrows the amplitude of the quantile process that remains upward sloping throughout the distribution, with estimates ranging from around 0.006 at the bottom two deciles (where they are not significant) to 0.023 at the top of the distribution (column 5 of Table 2.13). The estimates are smaller in magnitude but remain imprecise, and therefore we cannot the null that they are equal at all quantiles and not distinguishable from zero.

Our last specification (right graph of Panel A of Figure 2.13) restrains the village effects to be the same across quantiles which has the benefit of reducing the variability of our estimates. The quantile process is exclusively positive and displays the same upward sloping pattern, and estimates are significant at the 95% confidence level, with magnitudes similar to the first specification with village covariates. Those households at the bottom of the conditional distribution experience a consumption gain of 0.008 while the top decile shifts up by 0.034 (column 9 of Table 2.13). We reject the null of equality of coefficients at all quantiles at the 5% level (penultimate bottom cell of column 9 of Table 2.13).

The distributional impacts on consumption of microcredit loans issued to women are qualitatively similar in sign and significance to those of total household microcredit, and a little larger in magnitude (Panel B of Figure 2.13). Regressions controlling for village covariates produce estimates ranging from 0.009 at the first decile to 0.049 at the 90th quantile, and if we cannot reject the null that they are equal at all quantiles we are however able to say they are not jointly equal to zero (column 2 of Table 2.13). The introduction of village quantile effects removes significance at the bottom two deciles (as was the case for total microcredit), but otherwise preserves the upward sloping quantile process (middle graph of Panel B of Figure 2.13) taking values of 0.007 at the bottom of the distribution and 0.025 at the top decile (column 6 of Table 2.13).

In the specification with penalised village effects the quantile process is very similar (right graph of Panel B of Figure 2.13), displaying an upward trend from a coefficient of 0.009 at the bottom decile to an estimate of 0.050 at the top one, and the gain in precision allows us to reject the null for both joint hypotheses tests, meaning that the observed heterogeneity is statistically meaningful (bottom two rows of column 10 of Table 2.13). We fail to reject the null for either test when village-level heterogeneity is left unrestricted and can vary by quantile ((bottom two rows of column 6 of Table 2.13)

The QTEs of male microcredit on total expenditure (Panel C of Figure 2.13) are the most imprecise. The first set of results shows a small negative impact of microcredit loans issued to men on consumption at the top of the distribution (-0.001) while the effects are otherwise positive, ranging between 0.001 and 0.009 between the 10th quantile and the median, and 0.010 or above for deciles 6 through 8. Only that coefficient estimated at the 3rd decile is significant with 90% confidence, while all coefficients are not statistically different from each other, nor are they jointly different from zero.

Similarly to what we pointed out earlier, village dummy variables shrink the magnitude of the estimated QTEs (middle graph of Panel C of Figure 2.13) and they now range between slightly below zero and 0.008, only one negative impact to be found at the top decile (column 7 of Table 2.13). Estimates with our third specification are similar to those obtained with the first one, where only the coefficient at the top decile is (very slightly) negative. Other estimates range between 0.004 and 0.008, with the largest (and significant) gains of 0.017 realised at the 8th decile (column 11 of Table 2.13).

In the regressions including village covariates only, we can reject the null that the impacts of female and male microcredit are equal at the upper end of the distribution (column 4 of Table 2.13). We fail to reject the null at any quantile when village effects are left unrestricted across quantiles, however we are at least 90% confident that male and female microcredit have different impacts on total expenditure from the 4th decile up when village effects are penalised, with the exception of the 8th decile (column 12 of Table 2.13). Only with the first and third specification can we reject the null that both quantile processes are jointly equal to zero (bottom rows columns 4 and 12 of Table 2.13), but never can we assert that they are significantly different from each other as a whole.

The pattern of results that arose from our preliminary cross-sectional analysis seems to be confirmed when running regressions on the pooled sample of panel data. Especially, household total and female microcredit have positive effects on household total expenditure at every point of the distribution with larger welfare gains for those households ranking higher in the conditional distribution. The impacts of male microcredit, however, appear to be small if they exist at all, albeit being positive at most quantiles under consideration.

7.1.2 Food expenditure

The impacts of microcredit on food expenditure are plotted in Figure 2.14 and reported in Table 2.14. Household total borrowings seem to have a small positive effect on food consumption which is fairly homogenous across quantiles (left graph of Panel A of Figure 2.14) and estimated to range between 0.005 and 0.008 (column 1 of Table 2.14). Indeed we cannot reject the null that coefficients are equal across quantiles, although they are not jointly zero (bottom rows of column 1 of Table 2.14).

The estimated QTEs with quantile-varying unobserved village effects remain positive and of small magnitude; the largest gains (at 0.005 and 0.006) are observed for the 2nd, 3rd and 4th deciles and are significant at the 5% level. The top deciles experience the smallest gains but estimates across the quantile process are not statistically distinguishable from each other. In the third specification the quantile process is again positive and relatively flat (right graph of Panel A of Figure 2.14) and while all coefficients are quite precisely estimated (significant with 95% confidence), they range from 0.005 to 0.008 and we cannot conclude that they differ significantly from each other (column 5 of Table 2.14). Households at the bottom two deciles experience the smallest gains and those at the third decile benefit most from microcredit.

When QTEs are differentiated by gender in our specification with village covariates, female microcredit yields positive food consumption gains that are a little greater in the top half of the distribution (left graph of Panel B of Figure 2.14) and contained between 0.006 and 0.010 at all quantiles (column 2 of Table 2.14). Introducing village quantile effects modifies the pattern and evens out the quantile process (middle graph of Panel B of Figure 2.14), yielding estimates ranging between 0.003 and 0.006 across the board with a smaller impact at the 8th decile only (0.001, column 6 of Table 2.14). As expected, restraining village effects across quantiles strengthens statistical significance as witnessed by narrower confidence bands in the middle graph of Panel B of Figure 2.14. If coefficients are similar in magnitude to those from the first specification, there emerges an upward sloping quantile process with a larger impact at the top decile (0.010) than at the bottom (0.005).

From the left graph of Panel C of Figure 2.14 we see that male microcredit generates food consumption losses at the bottom decile and top two deciles, with the most adverse effect found at the 90th quantile while the 2nd and 3rd deciles shift up by about 0.007. Interestingly, this is the only case when food consumption is the dependent variable that we can reject the null (with 90% confidence) that the estimated effects of male credit are equal across quantiles (column 3 of Table 2.14). Accounting for quantile-varying village heterogeneity shifts up the quantile process although consumption losses remain at the top three deciles. With penalised village effects the picture is rather similar to that of the first specification, with gains above 0.005 around the third decile and a small negative loss at the top quantile (column 11 of Table 2.14).

We reject the null that female and male credit yield similar food consumption gains at the bottom and top two quantiles when only village covariates are included (column 4 of Table 2.14). Estimates are too imprecise with village quantile effects to allow us to conclude against the null. With the third specification we conclude that female and male microcredit have different impacts on food consumption at the median, the 6th and 7th deciles (column 12 of Table 2.14). Overall the tests results suggest that female and male credit might yield different benefits in the top half of the distribution. We also note that with village covariates or penalised village effects we can reject the null that estimated coefficients at all quantiles are jointly equal to zero for each quantile process under scrutiny. This points to the idea that microcredit might have a homogenous impact across the distribution (as we cannot reject the null for the other test) but that this impact is significant (and small and positive according to the sign and magnitude of the estimates).

7.1.3 Non-food expenditure

Results from regressions focussing on the impact of microcredit on household per capita non-food expenditure are plotted in Figure 2.15 and coefficient values are reported in Table 2.15. We comment on these more concisely because the apparent pattern of results is rather unequivocal.

Indeed, household total borrowings affect non-food consumption positively at all quantiles for the three specifications, with all our estimates significant with 95% confidence (Panel A of Figure 2.15). The gains are typically small at the bottom decile (between 0.002 and 0.003 depending on the empirical model), ranging roughly between 0.002 and 0.006 in the bottom half of the distribution and up to the 6th decile, while larger benefits are experienced by those households ranking in the top three deciles (columns 1, 5 and 9 of Table 2.15). Those at the top of the distribution get the most from microcredit with QTEs estimated between 0.025 and 0.035. We reject the null that coefficients of the quantile process are jointly equal to zero across all specifications, but we can conclude in favour of significant impact heterogeneity in the model with penalised village effects (second to bottom row of column 9 of Table 2.15).

The pattern of results depicted in graphs of Panel B of Figure 2.15 for the impacts of female microcredit are very similar to the above, represented by an upward sloping quantile process with coefficient estimates that are typically slightly larger in magnitude than for total microcredit, especially so at the top decile where estimated QTEs are between 0.027 and 0.041. Households ranking at bottom quantiles up to the median experience gains between 0.002 and 0.007, while gains are typically greater than 0.010 at the 7th decile and above. Conclusions on the statistical tests are also qualitatively identical to those carried out on estimated impacts of total microcredit, and the observed heterogeneity is statistically significant in the specification with penalised village effects.

Overall, graphs in Panel C of Figure 2.15 show that households receiving microcredit issued to men also exhibit small positive gains at the bottom quantiles and larger benefits at the very top of the distribution (with estimates between 0.011 and 0.021 at the 9th decile, see Table 2.15). However, the estimates are inaccurate and we cannot reject the null that the observed quantile processes are significantly different from zero. In the third specification with penalised village effects, we can conclude that female and male microcredit have statistically different impacts on the distribution of non-food consumption at all deciles between the 40th and 80th quantiles. The test statistics also allows us to reject the null that all estimated coefficients on the female and male credit variables are equal.

The first set of results obtained from the exploitation of the full sample in pooled QR is encouraging and tends to confirm our observations from the cross-sectional analysis. Household total and female borrowings benefit all households in terms of total and non-food per capita expenditures, with significantly heterogeneous impacts that increase along the distribution. When food consumption is the outcome variable of interest, results seem to point to the existence of small positive impacts of microcredit that are probably homogenous across the distribution. The impacts of male microcredit are in general positive but smaller in magnitude than those of female credit. There is partial evidence that microcredit yields substantially different gains at given quantiles depending on the gender of the recipient, and that both quantile processes differ as a whole, although imprecise estimation of male microcredit impacts makes it hard to conclude formally.

7.2 Panel quantile regression with correlated random effects

We now turn to the final set of results in our study that are obtained through running QR on the pooled sample of data with household unobserved heterogeneity modelled as a linear function of time-averaged household covariates, allowing time-invariant household (random) effects to be correlated with observed household characteristics.

7.2.1 Total expenditure

Figure 2.16 gathers plots of the coefficients reported in Table 2.16. When village heterogeneity is controlled for through observed characteristics only, household total microcredit borrowings have a positive effect on household total expenditure across the distribution (Panel A of Figure 2.16). Those households at the bottom decile of the distribution experience an upward shift of 0.011 in total consumption, and this impact increases steadily throughout to reach a maximum of 0.061 at the 9th decile. All quantile slope coefficients are significant at the 5% level and we reject the null that they are all equal with 90% confidence (column of Table 2.16).

The upward sloping quantile process is robust to controlling for total village heterogeneity (middle and right graphs of Panel A of Figure 2.16), with estimated QTEs of similar magnitude although their values are typically lower at the top two deciles when village effects are left unrestricted (0.042 at the top decile with village quantile effects against 0.058 with penalised village effects, columns 5 and 9 of Table 2.16). In this latter case we cannot reject the null that estimated coefficients are equal across quantiles, whereas we can ascertain that heterogeneity in estimated impacts is statistically significant with at least 90% confidence in the other two specifications.

The impacts of female microcredit display a qualitatively similar pattern, as can be seen from the graphs in Panel B of Figure 2.16. The quantile process is upward sloping across the three specifications, positive gains at the bottom of the distribution range between 0.013 and 0.016 and the largest consumption gains at the top decile have values of 0.048 to 0.068, with estimated QTEs generally settling slightly higher than those obtained for household total borrowings. Although all individual estimates are statistically significant at the 5% level, we reject the null that they are equal across quantiles only in that specification with penalised village effects (bottom rows of column 10 of Table 2.16). However we can confidently conclude that the quantile process as a whole is statistically different from zero in every model considered.

Turning to the estimated QTEs of male microcredit shown in Panel C of Figure 2.16, our model with village covariates suggests small positive impacts at the bottom deciles that increase to a maximum of 0.049 at the 7th decile before plummeting back to 0.032 at the top quantile. Estimates around the median are statistically significant but we do not find evidence that the quantile process is indeed heterogeneous (column 3 of Table 2.16). The introduction of village dummy variables produces larger estimates at the bottom deciles (0.011) while column 7 of Table 2.16 shows homogenous impacts around the median (between 0.028 and 0.031) with the greatest benefits (above 0.040) found for those households at the 7th and 8th deciles. Statistical significance of the estimates extends around the median from the 3rd to the 8th decile, but not to the quantile process as a whole.

When village effects are restricted across quantiles the impacts of male microcredit in the bottom half of the distribution settle between 0.012 and 0.018, and those households in the top half of the distribution experience gains of 0.020 or greater, with benefits from microcredit being maximal at the 7th and 8th deciles still (0.033 and 0.037, respectively). Estimated QTEs are significant with 95% confidence except at the two extreme deciles, and we reject the null that they are jointly equal to zero, although we fail to conclude that there is significant heterogeneity in the quantile process.

Our previous observations are generally confirmed by this first set of results. However formal statistical evidence that female and male microcredit yield different benefits at any given quantile is non-existent. We also cannot reject the null that both quantile processes are statistically different, although they are not jointly equal to zero.

7.2.2 Food expenditure

The left graph of Panel A of Figure 2.17 shows that households ranking in the bottom half of the food consumption conditional distribution benefit from microcredit to a lesser extent than those at the 6th, 7th and 8th deciles. The smallest impact is found at the bottom decile, and estimates range between 0.006 and 0.015. They are all statistically significant and the tests at the bottom of column 1 of Table 2.17 bring us to the conclusion that they are not statistically different from each other but cannot be considered to be jointly zero.

When we specify village quantile effects the range of the estimates over the distribution is narrower (middle graph of Panel A of Figure 2.17), with statistically significant QTEs between 0.008 and 0.013. Conclusions from the joint hypothesis tests remain the same (column 5 of Table 2.17). In the case where village effects are identical across quantiles the quantile slope coefficients of interest are consistent with those previously observed. Households at the bottom decile benefit the least and those in the top half of the distribution experience the largest food consumption gains (right graph of Panel A of Figure 2.17). Estimated QTEs are statistically indistinguishable from each other but the quantile process is overall different from zero (bottom rows of column 9 of Table 2.17).

The graphs of Panel B of Figure 2.17 provide a very similar pattern of results for the impacts of female credit. The top of the distribution typically enjoy the largest food consumption benefits from female borrowings and estimates range from 0.009 to 0.015 across the three specifications under consideration. Heterogeneity of the QTEs can never be asserted formally and the quantile process as a whole is systematically different from zero (bottom rows of columns 2, 6 and 10 of Table 2.17). In no instance can we ever reject the null that male and female credit have similar impacts at a given quantile, even though the quantile process pertaining to the effects of male microcredit can be said to be statistically different from zero in one instance only (our third specification with penalised village effects).

Much like for previous results from pooled QR, while the graphs hint at slightly larger QTEs for households ranking in the top half of the distribution, the joint hypotheses tests invite us to conclude that microcredit does not generate heterogeneous food consumption gains across the distribution. Similarly, impacts from female microcredit seem visually more stable than those from male microcredit but we cannot reject the null that their respective quantile processes are identical.

7.2.3 Non-food expenditure

Our final set of results on the impacts of microcredit on household non-food expenditure is reported in Table 2.18 and plotted in graphs of Figure 2.18. Those graphs in Panel A of Figure 2.18 display an all-positive upward sloping quantile process representing the impacts of household total borrowings on non-food per capita expenditure. Households ranking in the bottom half the distribution up to the third decile experience an upward shift in non-food consumption ranging from 0.004 to 0.009; those in the top decile gain the most from microcredit borrowing, between 0.040 and 0.044 (columns 1, 5 and 9 of Table 2.18). All individual coefficients are statistically different from zero with 95% confidence. We systematically reject the null that estimates of the quantile process are jointly equal to zero, and when the regression includes penalised village effects we can reject the null that quantile slope coefficients are equivalent across all deciles (second to bottom row of column 9 of Table 2.18).

We draw similar conclusions in the case of female microcredit. The quantile processes depicted Panel B of Figure 2.18 are made of positive estimated QTEs, significant across the board. The smaller gains at the 1st decile are between 0.004 and 0.007, and the greatest benefits accruing to the top decile take values between 0.044 and 0.050. The statistical tests at the bottom of columns 2 and 10 of Table 2.18 ascertain the existing heterogeneity in the estimated QTEs in two of our specifications, while we reject the null that the quantile process as a whole is equal to zero for all three empirical models.

Finally, graphs in Panel C of Figure 2.18 show that male microcredit also yields greater benefits in the top half of the distribution. Estimates are strongly significant around the median and insignificant for the top two deciles (columns 3, 7 and 11 of Table 2.18). When village quantile effects are left unrestricted we can reject the null that the impacts of male microcredit on non-food consumption are equal across all quantiles, and also conclude against the hypothesis that they are jointly equal to zero.

In the next section we recap our main findings from our panel data analysis and draw our final conclusions on the heterogeneous impacts of microcredit on household expenditure.

8 Interpretation of the results

We set out to unravel the potential existence of heterogeneous impacts of microcredit programs on household welfare through the use of QR. The main challenges were to adequately account for non-random program placement at the village level and for self-selection into group borrowing at the household level. First differencing our variables is not an option because, unlike expectations, quantiles are not linear operators. Our final specifications best exploited the panel structure of the data by introducing household correlated random effects in order to model household-level unobserved heterogeneity without running into the incidental parameter problem. By allowing household effects to be correlated with household covariates, we control for time-invariant factors that potentially influence the outcome of interest as well as household characteristics, including microcredit variables. Non-random program placement is handled by village dummy variables which we allowed to have different effects at various quantiles or not. Here are the conclusions we are able to draw from our empirical study, focussing on results obtained in our correlated random effects framework.

8.1 Heterogeneous impacts

A clear pattern emerged in our cross-sectional study that remained robust and was even strengthened by our panel data analysis: the upward shift in household per capita expenditure caused by microcredit is larger at higher quantiles of the conditional distribution. Plots of the quantile process and the estimated coefficients reported in tables are unequivocal. For each quantile process associated with a treatment variable (household total microcredit, female microcredit or male microcredit) we carry out a test on the joint equality of coefficients across the nine deciles under consideration in order to formally assess the apparent heterogeneity in the estimated impacts.

We systematically reject the null of no heterogeneity for the impacts of total microcredit and female microcredit on total and non-food per capita expenditure when village effects are restricted to have the same impact at each quantile, even when correlated random effects are introduced, which hampers accuracy by drastically increasing the number of parameters to be estimated. As explained in the methodology section, this implementation of Koenker (2004) estimator aims at reducing the variability of the slope estimates due to the inclusion of many dummy variables. Leaving the latter unrestricted offers more flexibility in controlling for village heterogeneity but is costly in terms of precision of estimation, and hence we fail to reject the null in regressions with such specifications.

Similarly, the QTEs associated with male microcredit are in general very imprecisely estimated across all specifications, and there is too little evidence to support heterogeneous consumption gains from loans issued to men. Another interesting result is that we can never reject the null of no heterogeneity of the estimated impacts of microcredit on food expenditure. That is, the beneficial (or adverse) effects of microfinance loans in terms of food consumption are constant across the distribution. Nevertheless, the multiple occurrences of rejections of the joint hypothesis of equality of quantile slope coefficients on total and female credit variables in our second best specification (with correlated random effects and penalised village effects) constitute valuable evidence in favour of the claim that all recipients of microfinance loans do not necessarily benefit from such programs to the same extent, hence making a case for the interest of studying distributional impacts as a complement to average treatment effects that are usually the focus of program evaluation studies.

8.2 Sign and magnitude of estimated impacts

As an addition to testing the equality of slope parameters associated to a given credit variable across all quantiles, we also test whether these estimates are jointly equal to zero. The null is overwhelmingly rejected for the quantile processes attached to household total microcredit (and female microcredit) across all regressions from our panel data analysis. Moreover, all the corresponding parameters are estimated to be positive. Therefore, we can safely conclude that indeed microfinance loans help recipients improve their welfare as measured by household per capita expenditure, a finding in line with Khandker (2005) and Islam (2011) among others.

For instance, without differentiating by gender, an extra Tk 1,000 in microcredit to households who consume relatively little given their socio-economic characteristics (1st decile) helps them increase total per capita consumption by Tk 11 to Tk 14 per year. The annual per capita consumption gains from an extra Tk 1,000 in microcredit for median households settles between Tk 24 and Tk 31. Additionally, the benefits to relatively high-consuming households (9th decile) from receiving an extra Tk 1,000 in credit are between 42 and 61 Taka per year in per capita total expenditure.

Overall, the estimated gains from microcredit are quite small compared to the median levels of annual per capita consumption in the sample, which are Tk 3,899 in 1991/92 and Tk 4,220 in 1998/99. We provide a measure of central tendency for comparison with the estimated benefits from microfinance because the objects our methodology allows us to study are conditional quantile functions, which do not tell us about quantiles of the marginal distribution of consumption. Treatment effects on the latter might be of different magnitude. The main observation is that, according to our estimates, consumption gains realised thanks to microcredit at the top of the conditional distribution are about four times as large as those realised at the bottom.

8.3 Types of expenditure

We refine our analysis by considering two other dependent variables, namely household annual per capita food and non-food expenditure, which are the breakdown of total consumption. Non-food expenditure embeds items such as clothes, shoes, durable goods for the house and house repairs, as well as lumpy expenses incurred for religious ceremonies, weddings or funerals for example.

The distributional impacts of total household borrowings on per capita food expenditure typically settle between around 0.006 and 0.015 with the smallest estimates found at the bottom two deciles. However we already pointed out that the null of no heterogeneity for food consumption benefits can never be rejected, therefore we can only understand these values as providing bounds for QTEs that are most likely constant across the distribution. In other words, microcredit generates small food consumption gains that are similar for all beneficiaries and range between Tk 6 and Tk 15 per capita per year for an extra Tk 1,000 in microcredit.

It follows that the previously established heterogeneity of QTEs on total expenditure must stem from the impacts of microcredit on non-food per capita consumption. When the latter is the outcome of interest we are indeed able to reject the null of heterogeneity for the quantile processes associated to total and female microcredit in our specification with correlated random effects and penalised village effects. Our estimates suggest that relatively low-consuming households (bottom decile) gain about Tk 4 to Tk 7 per capita per year in terms of non-food expenditure for an extra Tk 1,000 of credit received, while their high-consuming counterparts realise gains between Tk 40 and Tk 46.

The signs and magnitudes of the measured impacts of microcredit on food and non-food expenditure are in line with the magnitudes and signs of those impacts measured on total consumption, the heterogeneity of the latter stemming from quantile-varying effects on non-food expenditure while QTEs are constant for food expenditure.

8.4 Gender of borrowers

We further investigate the impacts of microcredit borrowing by distinguishing treatment effects for female and male borrowers. It appears that in both cases microcredit generates smaller positive gains in the lower half of the distribution than in the upper one, with a very clear upward sloping quantile process representing QTEs of female microcredit while the picture is less clear-cut for the impacts of male borrowing. Additionally, the plots offer a visual representation of quantile processes for which it is easy to see that the pattern of distributional impacts pertaining to female microcredit is very close to that of household microcredit in general.

We can reject the null that quantile-varying coefficients are jointly equal to zero for the estimated impacts of female microcredit on the three outcome variables under scrutiny across all specifications with correlated random effects, although the null of no heterogeneity is only rejected for the estimated QTEs of female microcredit on total expenditure when we use penalised village effects and on non-food expenditure when we use village covariates or penalised effects.

Similar tests carried out on the estimated impacts of male microcredit seldom succeed in rejecting the null hypothesis, although in one instance we are able to reject the null in both tests (when the outcome variable is non-food consumption and village effects are left unrestricted). According to the shapes of quantile processes and the conclusions of the tests when they are considered separately it would be very tempting to conclude that the observed heterogeneous benefits from microfinance loans are actually driven by welfare gains generated in households with female borrowers, while those with male borrowers benefit little from it, if at all (given that for male microcredit QTEs we can rarely reject the null of no heterogeneity or the null that the quantile process as a whole is zero). Unfortunately these remain tentative conclusions because we can never reject the null hypothesis that, at a given quantile, the estimated coefficient on the female credit variable is similar to its male counterpart[38]. Similarly, we generally fail to reject the null that the quantile process as a whole is statistically different for each treatment variable.

Overall, the results seem confusing. On one hand, we have evidence to support the existence of positive and significantly heterogeneous impacts of female microcredit, and evidence that microcredit issued to men affects consumption behaviours little (individual quantile coefficients are often significant around the median). On the other hand, joint hypothesis testing does not allow us to conclude against the similarity of QTEs from female and male microcredit at any given quantile, nor can we rule against the statistical equivalence of the two associated quantile processes. It is more than likely that the large standard errors associated to our point estimates are guilty of making rejection of the null almost impossible, but if we are to believe the statistical tests presented here we cannot formally prove that consumption gains from microcredit depend on the gender of its recipients.

[38] In our regressions with correlated random effects, across all three specifications and with three different dependent variables, the null can be rejected only once in 81 tests.

8.5 Implications

From a policy-making point of view, our most significant finding is that indeed microfinance loans help their beneficiaries improve their welfare but not all to the same extent, and that this heterogeneity in consumption gains is especially important for non-food expenditure while the impacts of microcredit on food expenditure appear to be constant across the distribution. The rank of an observation in the distribution is a measure of unobserved heterogeneity and can be considered as its ability or "proneness" to achieve certain values of the outcome. This view goes back to Doksum (1974) and is used to discuss QR estimators in Chernozhukov and Hansen (2006) and D. Powell (2016a, 2016b). Therefore, households who typically achieve low levels of consumption given their socio-economic characteristics have a lower ability to generate consumption gains than those households who already achieve relatively high levels of consumption.

In line with our interpretation of the findings from cross-sectional regressions presented earlier, and argue that microcredit is inadequate to promote upward mobility within a group of household with similar characteristics. Significantly different impacts at various points of the conditional distribution of consumption suggests that microcredit could actually play a role in heightening inequalities amongst households that are alike with respect to their socio-economic features. Those who are relatively worse off will enjoy an increase in consumption thanks to help of microfinance loans but will struggle to catch up with those who are relatively better off as the latter category show better ability to reclaim benefits from microcredit.

It is of course reassuring to observe positive impacts of microcredit on food consumption, if only for the obvious reason that sustenance is the cornerstone of mere survival. In development economics, measures of consumption, and food expenditure especially, are often considered a good candidate to proxy household welfare in developing countries (Deaton, 1997), and to that extent our findings could lead to characterise microcredit as a fairly "democratic" tool in its capacity to improve welfare in a similar fashion for all categories of recipient households. Recall that tentative evidence from quantile-quantile plots and non-parametric tests suggested that the distribution of food consumption in the sub-sample of borrowers changes significantly over time, whereas it does not for non-borrowers.

Similarly, non-parametric tests shown in Panel B of Table 1.5 of Chapter 1 point to the fact that non-borrowing households exhibit food expenditure behaviours distributed significantly differently from those of their borrowing counterparts in 1991-92 (tests results go against the null of similarity of location or shape), but their results for the 1998-99 data are not so clear cut and hint at the possibility that both distributions are quite close in terms of location and shape.

In sum, it seems borrowers "catch up" on non-borrowers over time both in terms of levels of food consumption to reach a more homogenous distribution. This observation is in line with our estimates of the impact of microcredit on the location and shape of the conditional quantile function of food consumption. Our regressions provide evidence that all borrowers realise similar food consumption gains however they might rank in the distribution. Therefore, borrowing households catch up on comparable non-borrowing households at all levels of food consumption, and microcredit plays a significant role in this phenomenon.

Notwithstanding, our observation that heterogeneity in the distributional impacts of microfinance arises from its effects non-food expenditure can also have crucial welfare implications. We relate our interpretation of these findings to the existing literature on social spending in developing countries. For instance, Banerjee and Duflo (2007) explore survey data from 13 developing countries and find that households who rank amongst the poorest in the world consistently spend a substantial part of their financial resources on goods that do not strike as prime concerns for survival, mostly festivals and ceremonies (for instance weddings or funerals). The authors suggest this could stem from the well-known "keeping up with the Joneses" phenomenon. Indeed Bloch, Rao, and Desai (2004) claim that in rural India the holding of very lavish wedding ceremonies is used to signal the standing of the groom's family, in a way quite similar to what can be found in South Africa where funerals bear such social significance that some households feel compelled to borrow money in order for the service to match up to their apparent social status (Case, Garrib, Menendez, & Olgiati, 2013).

Status-seeking through increased spending on visible goods such as weddings, funerals and gifts has also been documented as being a consistent behaviour amongst poor households in rural China (Brown, Bulte, & Zhang, 2011). Our own measure of non-food expenditure includes a variety of items embedding clothes, shoes and house durables but also expenses for social events or religious ceremonies, all of which can to some degree be seen as potential positional goods. If food consumption is of primary importance for well-being, the ability to spend on conspicuous goods is relevant to social status and hence has a non-negligible role in household welfare in rural areas of developing countries.

Our empirical study shows that relatively high achievers who take up microfinance loans can increase non-food expenditure by up to 6 to 10 times what low-achieving comparable households are able to realise. In a conceptual framework where non-food consumption is an indicator of social spending and households with comparable socio-economic characteristics constitute a relevant reference group, we see that micro-credit can actually sharpen existing inequalities in social welfare amongst comparable households. Non-food expenditure gains realised from microcredit can strengthen the observed social status of those who already fare relatively well in signalling their standing within their reference group, while households who rank relatively low in terms of social status will not be able to catch up.

One could argue that this pattern of heterogeneous gains in non-food expenditure actually reflects unobserved differences in the consumption preferences of households, i.e. those who intrinsically put more value on social spending naturally decide to increase spending on conspicuous consumption when they have access to extra financial resources. Such preferences could also stem from the particular social environment households live in and therefore differ significantly across villages, for instance. However, we feel such an interpretation can be at least partially ruled out given that we control for observed ability through the inclusion of household covariates and also for the part of unobserved proneness that is time-invariant in our model with correlated random effects, as well as total village-level heterogeneity thanks to the inclusion of dummy variables.

Additionally, the aforementioned empirical studies show that the diversion of financial resources from private goods towards visible consumption is a consistent behaviour amongst poor and extremely poor households in developing countries (particularly so in rural India, Banerjee and Duflo (2007)). This observation is rather in favour of the idea that consumption preferences are likely similar for microcredit borrowers and non-borrowers, so that our interpretation holds even if we consider that microfinance programs succeed in targeting the poorest of the poor.

We also note that *rank-based* status models have been used to theoretise the welfare implications of changes in the location and scale of the income distribution when agents derive utility from relative income (Hopkins & Kornienko, 2004) and also from consumption of visible goods (Hopkins & Kornienko, 2009). Our work has no pretence to empirically apply or assess the validity of such models, but it is interesting to draw a parallel with QR that solve a minimisation problem whose objective function is based the *ranks* of observations[39].

The final conclusion is therefore that microcredit offers some opportunities of an upgrade in social status for those households who already have a relatively high standing given their socio-economic features, but microcredit borrowing has little virtue regarding within-group upward mobility. Although the estimated distributional impacts are overall positive and hence suggest that borrowers somehow catch up on non-borrowers in a general sense, the significant heterogeneity in coefficients at various quantiles highlights the fact that social welfare gains do not accrue to all borrowers to the same extent and are in favour of those who already have higher proneness in signalling social status.

[39] In theoretical models on relative income and conspicuous consumption the rank is typically the *exogenous* rank, the position of the observation in the marginal distribution. In quantile regressions, the minimisation problem is posed in terms of ranks of the observations below or above a given quantile of the *conditional* distribution.

Note that even though the inequality-sharpening feature of microcredit in terms of household welfare arises for borrowing households who increase non-food expenditure and fail to keep up with ever higher-achieving comparable borrowing households, adverse welfare consequences can also appear for the non-borrowing population. Indeed, the estimated QTEs of microcredit on non-food consumption are positive across the board, which means that, overall, borrowers catch up on non-borrowers, or leave them further behind depending on the initial situation. At any rate, provided that concerns of social status are shared amongst all households (which is likely in our setting), we cannot rule out the possibility that non-borrowing households (even high-achieving ones) may incur welfare losses as a consequence of microfinance loans helping borrowing households fill (or widen) the gap in terms of status-signalling expenditure.

8.6 Limitations of our study

There are two principal limitations in our study of the heterogeneous impacts of microcredit. The first one pertains to the nature of our data. Indeed, as was made clear in our description of the dataset, the non-experimental design of the study posits two main econometric challenges. Villages were initially sampled based on the presence of microfinance programs (or not) for at least three years, and it follows that such non-random program placement can bias estimates of treatment effects if microfinance practitioners decided to set up offices based on systematic differences in village characteristics. We address this issue by including village dummy variables.

Our most significant results typically arise in regressions where we compel coefficients on said binary indicators to have the same value across quantiles, which is akin to assuming that village heterogeneity has a pure location-shift effect on the distribution of consumption. We relax the assumption that the shape of the distribution is similar across villages by allowing coefficients to vary at each decile under consideration. However, doing so greatly increases the standard errors of our estimates and we tend to fail to reject the null hypothesis of no heterogeneity of microcredit impacts even though the range of values for our point estimates is consistent across specifications. The incidental parameter problem persists when using panel data because in the framework of QR we cannot first-difference variables to "get rid" of fixed effects (see sub-section 4.2). Moreover, we have relatively few observations and only two time periods, and these limitations close the door to many alternative estimators for panel data QR.

The other econometric issue is that of endogeneity of household borrowing and consumption behaviours, given that participation in microfinance programs is ultimately left to self-selection. Conceptually, endogeneity is hard to perceive in the context of QR because it is more pervasive than in the case of linear-in-means approaches: it can be strong at one quantile and less so at another. This in turn implies that instruments must have a rich relationship with the problematic regressors to adequately tackle the issue at every considered level of the conditional quantile function. In the cross-sectional case we attempt a two-stage QR approach to mitigate the issue but the results do not seem reliable in light of the reserves we voiced regarding the strength of the only instrument available. In regressions carried out on panel data we control for time-invariant household unobserved heterogeneity in the form of correlated random effects, which given our data is our best take on dealing with the potential endogeneity of household borrowing and household consumption. Non-random program placement and self-selection are popular issues in the study of microfinance programs with non-experimental data, and dealing with them is somehow made harder when using QR estimators.

The second limitation of our study is that our empirical strategy only provides information on the impact of covariates on the conditional distribution of outcome variables. As a consequence, our conclusions do not necessarily carry over to what happens on the marginal distribution of consumption. For instance, we cannot strictly match the measured heterogeneous impacts to parts of the population defined by a measure of absolute poverty. In order to infer anything about the effects of microcredit at various points of the marginal distribution of household expenditure one would need to use alternative estimators such as, for instance, that based on the re-centred influence function proposed by Firpo, Fortin, and Lemieux (2009), or the generalised QR estimator of D. Powell (2016b), or his QR for panel data estimator (D. Powell, 2016a). In our experience the assumptions on which they rely would make their implementation on our data problematic.

Nevertheless, from conditional estimates we are able to draw interesting conclusions about the welfare implications of microcredit, considering that matters of social status are relative in nature and are very important in the type of communities under consideration. In our interpretation of the estimated impacts of microcredit on food consumption we are also able to link the pattern of results to descriptive evidence on the discrepancies in the distribution of the outcome variable over time or between groups of households. We hope our work can show that results from conditional QR are informative when they are carefully interpreted.

9 Conclusions

We exploit a Bangladesh Institute of Development Studies/World Bank dataset to answer the question whether microcredit has heterogeneous welfare impacts, i.e. whether all borrowers experience the same gains (or losses) from microfinance loans. Our investigation is motivated first and foremost by the fact that most studies assessing the efficiency of microfinance initiatives in tackling poverty focus on their average impacts.

Our outcomes of interest are three measures of household per capita expenditure, namely total, food and non-food expenditure, which are broadly recognised as good proxies for households welfare. Descriptive evidence in Chapter 1 of this thesis showed that the distributions of household consumption among borrowers and non-borrowers differ substantially, and that their shapes evolve over time, further motivating the use of quantile regression techniques to get a more comprehensive view of microfinance program impacts.

Results from panel data QR with household correlated random effects lead to a number of interesting findings. First and foremost, all categories of households benefit from microfinance loans. Our impact estimates are positive across the board, and a household ranking at the median of the conditional distribution of total expenditure can expect a consumption gain of Tk 24 to Tk 31 per capita per year for an extra Tk 1,000 in microcredit.

However, we cannot confirm formally that gains from microcredit stem only from loans issued to women, as has been famously advertised in studies using the same dataset as ours, such as Pitt and Khandker (1998) and Khandker (2005). Visual evidence from plots of the estimated quantile processes pertaining to male and female credit invite us to believe that might be the case, given that the latter are very close in location and shape to estimated quantile treatment effects from household total microcredit. Additionally, in our preferred empirical specifications female microcredit always exhibits significant and positive impacts on consumption, while those associated male microcredit are typically small and not significant.

Nevertheless, formal testing does not allow us to claim that consumption gains from female and male microcredit are not the same at a given quantile. We estimate that an extra Tk 1,000 in female microcredit to a median household (in terms of conditional consumption) would boost their total expenditure by Tk 23 to Tk 33 per capita per year. Note that Khandker (2005) (using the same data as ours) advertises average returns to total annual household expenditure between Tk 15 and Tk 21 for an extra Tk 100 in female microcredit. To get a rough idea of how our results compare, we can divide the previously mentioned numbers by 10 and multiply them by 5.6, which is the average size of a household in our sample. It follows that average-sized households ranking at the middle of the conditional distribution experience gains of Tk 12.9 to Tk 18.5 in terms of total annual expenditure when receiving an extra Tk 100 in female microcredit. The magnitude of our estimates is therefore in line with previous studies

A related and crucial finding from our study is that these gains are actually *unevenly* distributed. Indeed, an extra Tk 1,000 in microcredit would yield total consumption gains of Tk 11 to Tk 14 per capita per year for relatively low-consuming households, while these benefits go up to between Tk 42 and Tk 61 for relatively high-consuming households, i.e. about *four times as much*[40]. This discovery does not contradict previous studies, as we saw that our estimated gains at the center of the distribution are in line with them, but rather complements them.

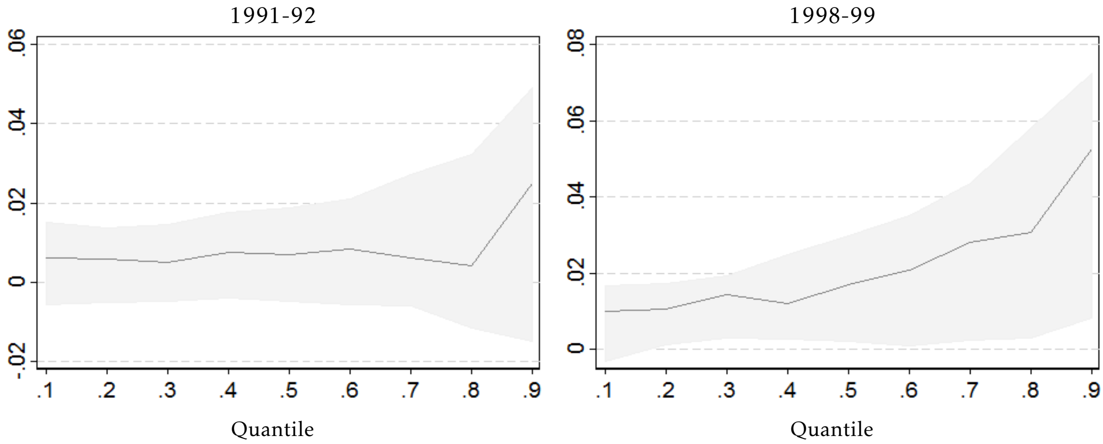
Moreover, we find that this heterogeneity stems from returns to non-food consumption. Indeed, all categories of households appear to benefit from microcredit to the same extent in terms of food consumption, with gains between Tk 6 and Tk 15 per capita per year for an extra Tk 1,000 in microcredit. However, the increase in non-food per capita expenditure generated from microcredit is 6 to 10 times greater at the top decile of the conditional distribution than it is at the bottom decile.

We link this last finding to the literature on the consumption habits of poor households in developing countries and on status-seeking. Our measure of per capita non-food expenditure includes a number of items that can be considered as conspicuous consumption goods, at least to some extent. Therefore, those households who have a relatively high ability in signalling their social status - compared to households with similar characteristics - reclaim larger non-food consumption gains from microfinance loans than comparable households with low levels of such ability. In communities where conspicuous consumption constitutes a way to ascertain one's social status, microcredit tends to sharpen social welfare inequalities in groups of households with similar characteristics, and most likely in the community as a whole. From a policy-making perspective, this is an important finding because it shows that positive welfare gains from microfinance programs in terms of consumption can hide more complex phenomena, such as potential social welfare losses due to the heterogeneity inherent to returns from microcredit.

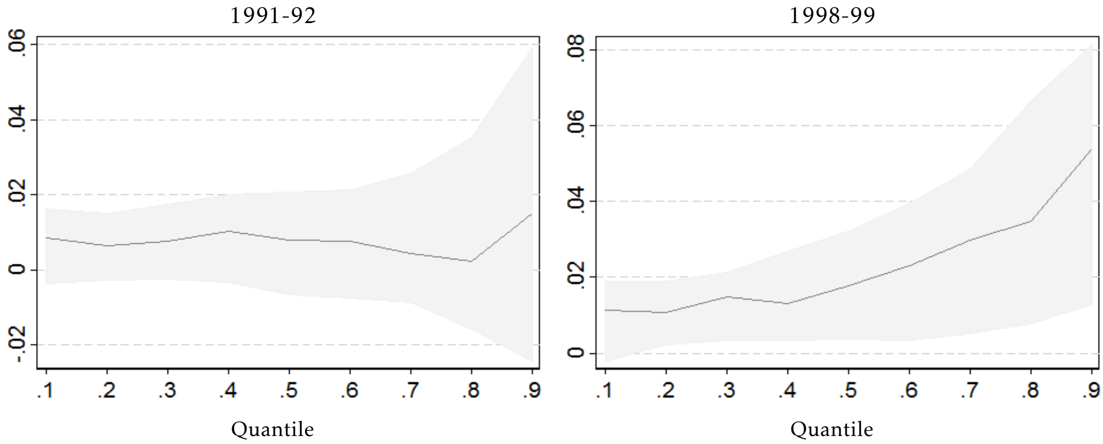
[40] Corresponding returns to female microcredit are Tk 13 to Tk 16 for households at the first decile and Tk 48 to Tk 68 for top-decile ones.

Figure 2.1 **Distributional impacts of microcredit on household total consumption: cross-section quantile regressions with village covariates**

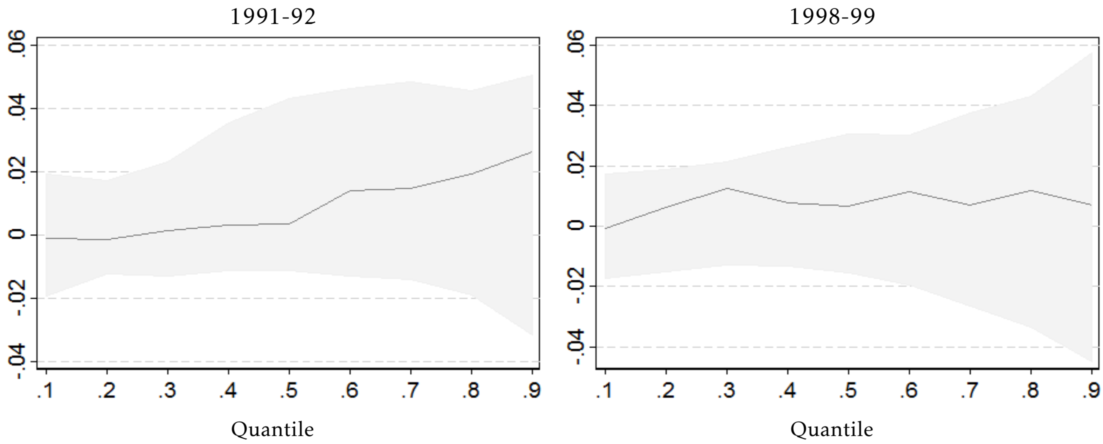
(a) Panel A: Household microcredit



(b) Panel B: Female microcredit



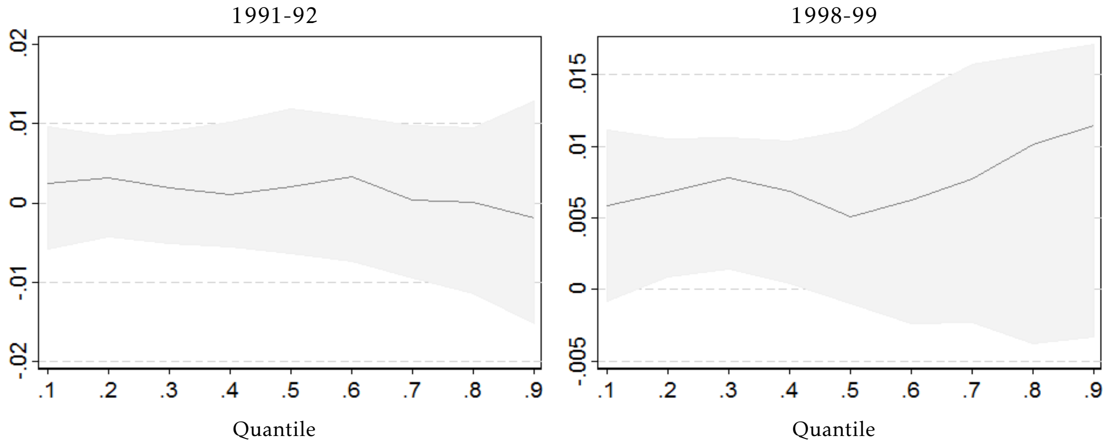
(c) Panel C: Male microcredit



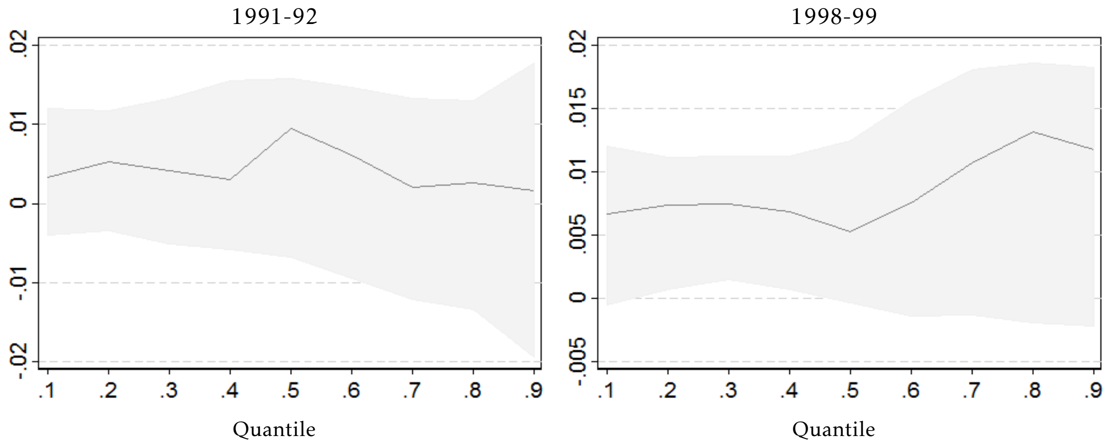
Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling villages with replacement to account for within-cluster dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Figure 2.2 **Distributional impacts of microcredit on household food consumption: cross-section quantile regressions with village covariates**

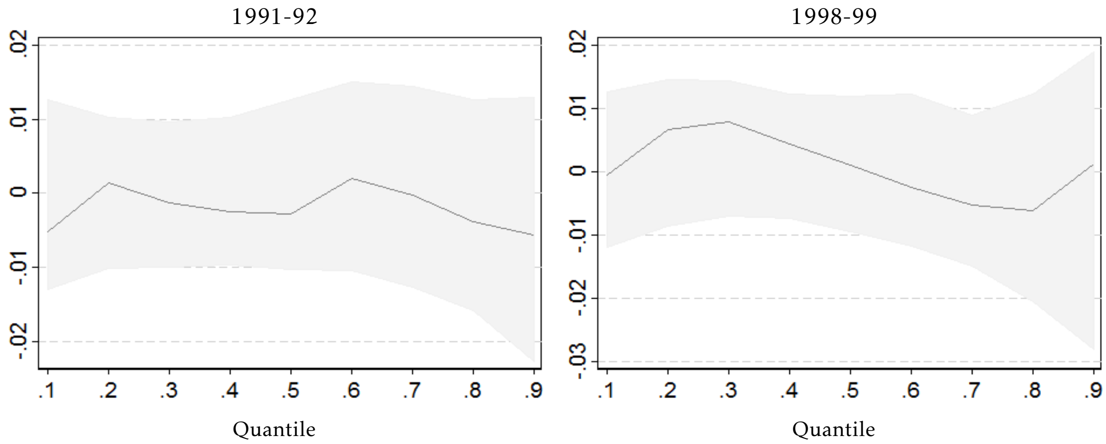
(a) Panel A: Household microcredit



(b) Panel B: Female microcredit



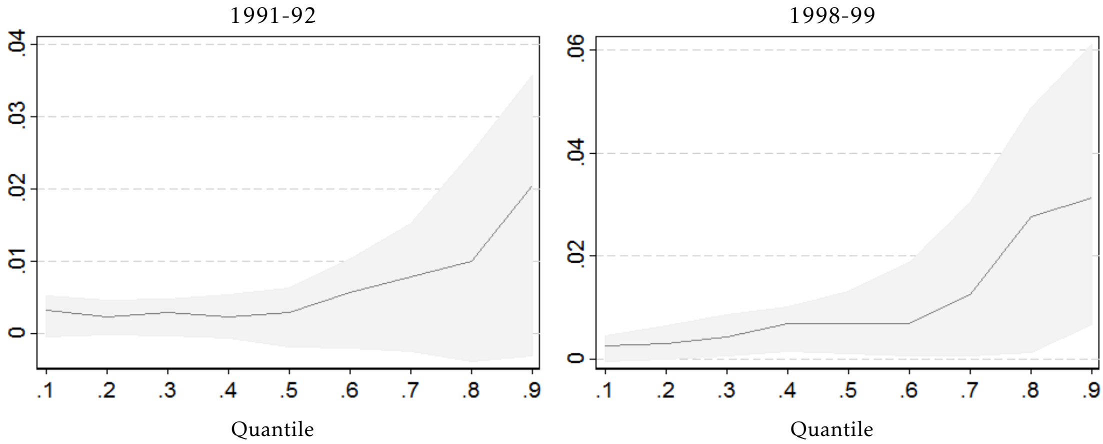
(c) Panel C: Male microcredit



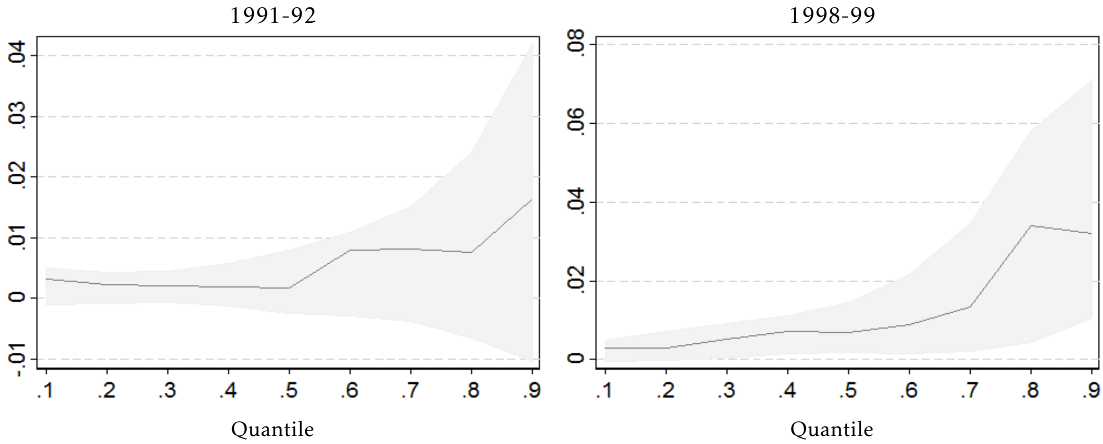
Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling villages with replacement to account for within-cluster dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Figure 2.3 **Distributional impacts of microcredit on household non-food consumption: cross-section quantile regressions with village covariates**

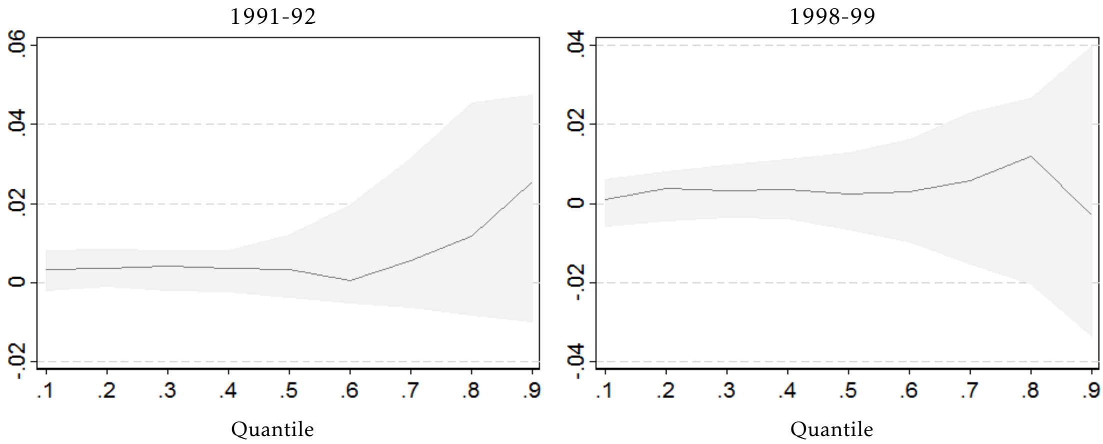
(a) Panel A: Household microcredit



(b) Panel B: Female microcredit



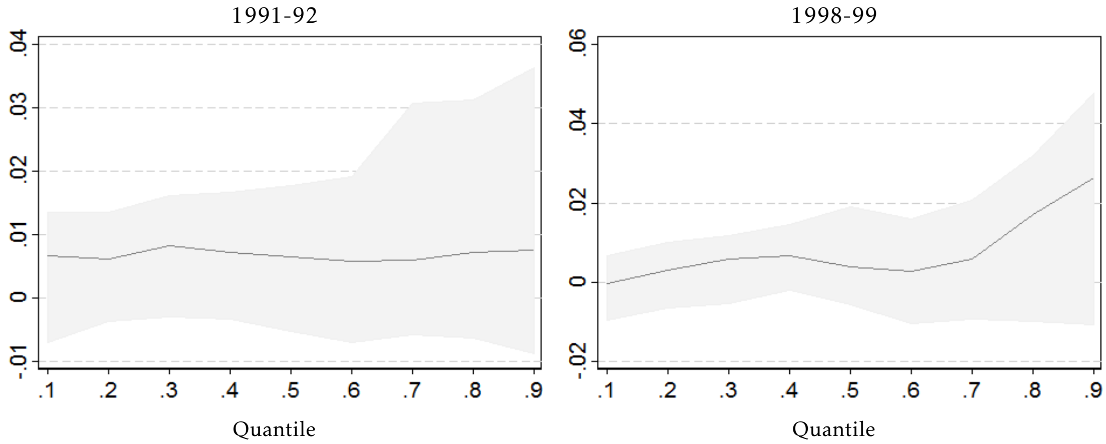
(c) Panel C: Male microcredit



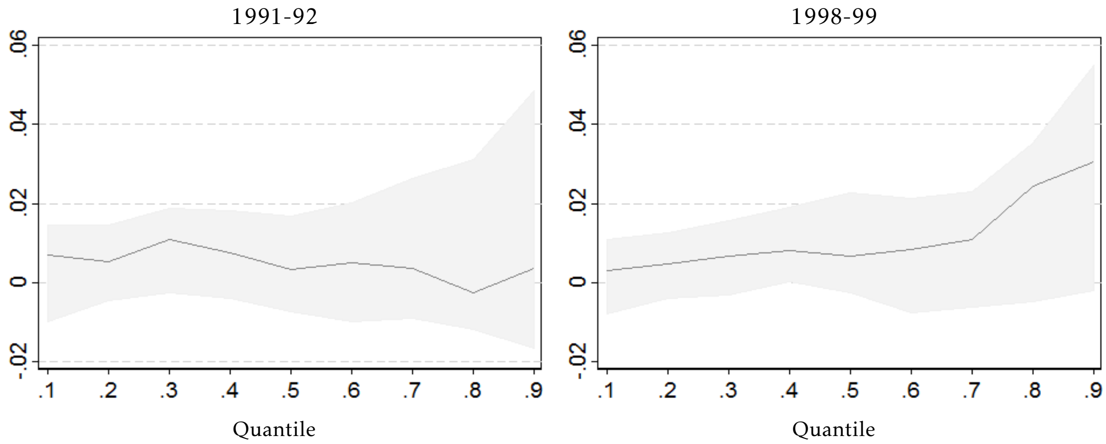
Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling villages with replacement to account for within-cluster dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Figure 2.4 **Distributional impacts of microcredit on household total consumption: cross-section quantile regressions with village quantile effects**

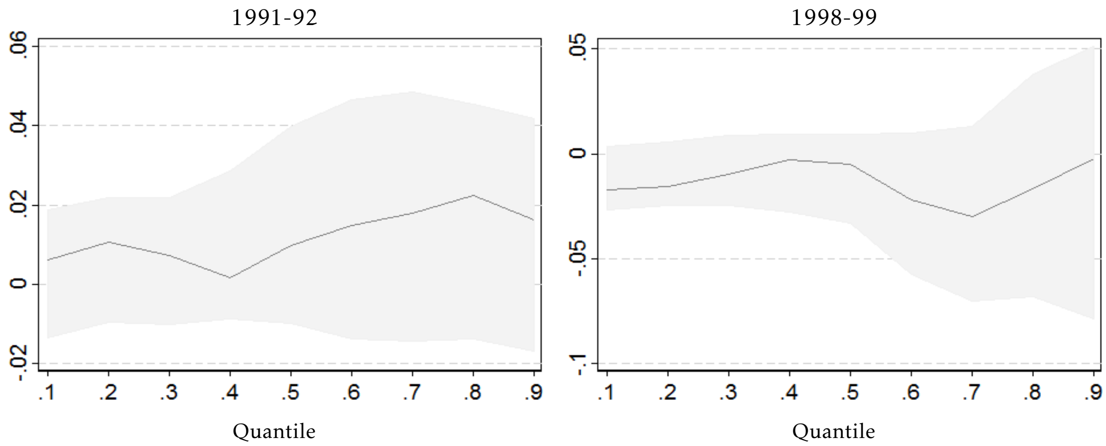
(a) Panel A: Household microcredit



(b) Panel B: Female microcredit



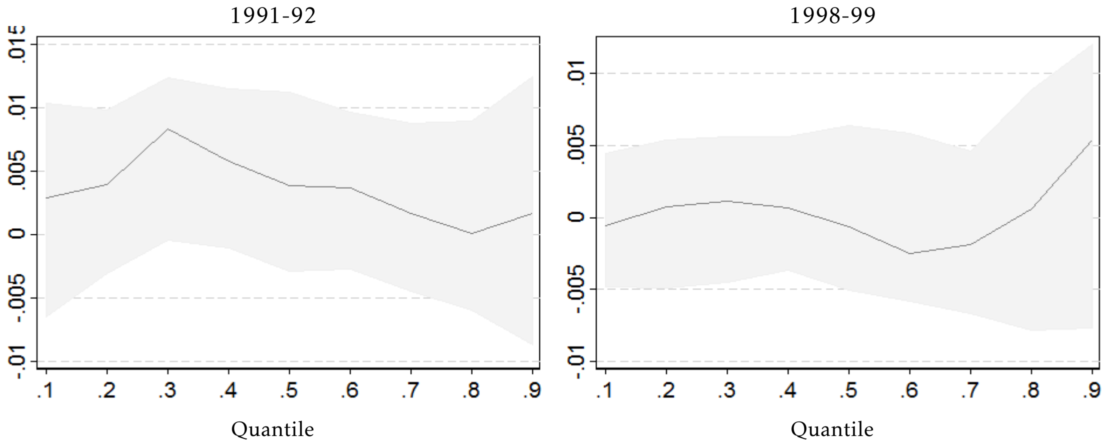
(c) Panel C: Male microcredit



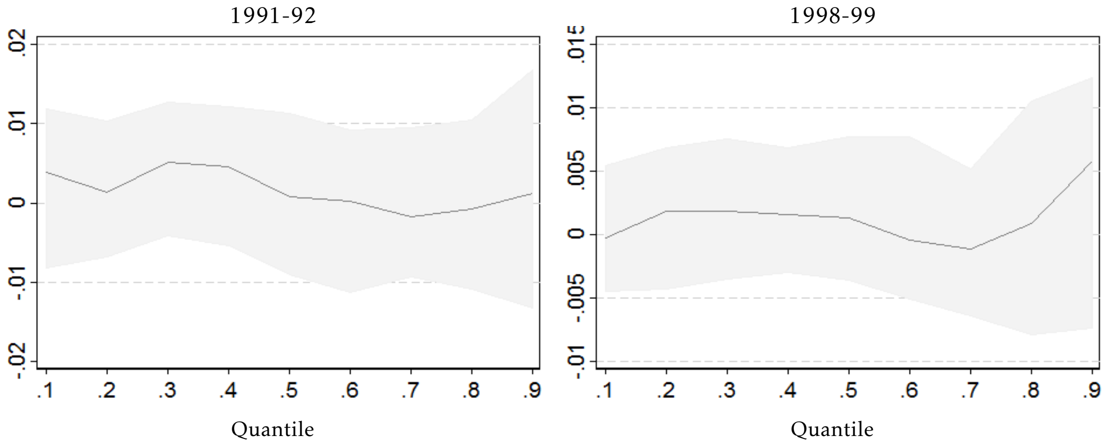
Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling villages with replacement to account for within-cluster dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Figure 2.5 **Distributional impacts of microcredit on household food consumption: cross-section quantile regressions with village quantile effects**

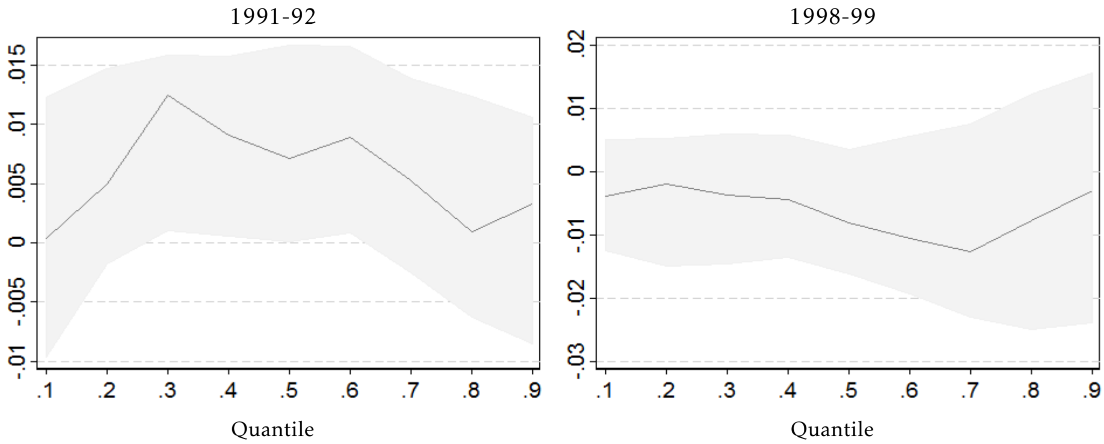
(a) Panel A: Household microcredit



(b) Panel B: Female microcredit



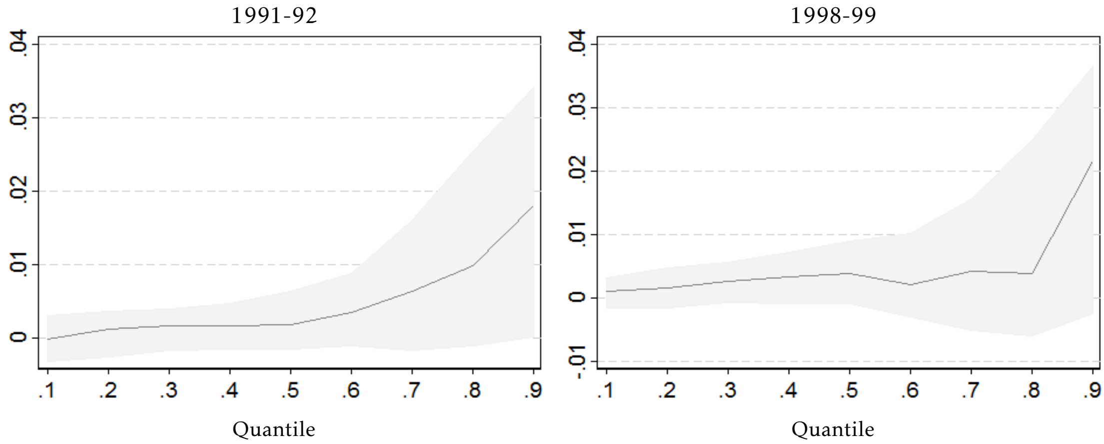
(c) Panel C: Male microcredit



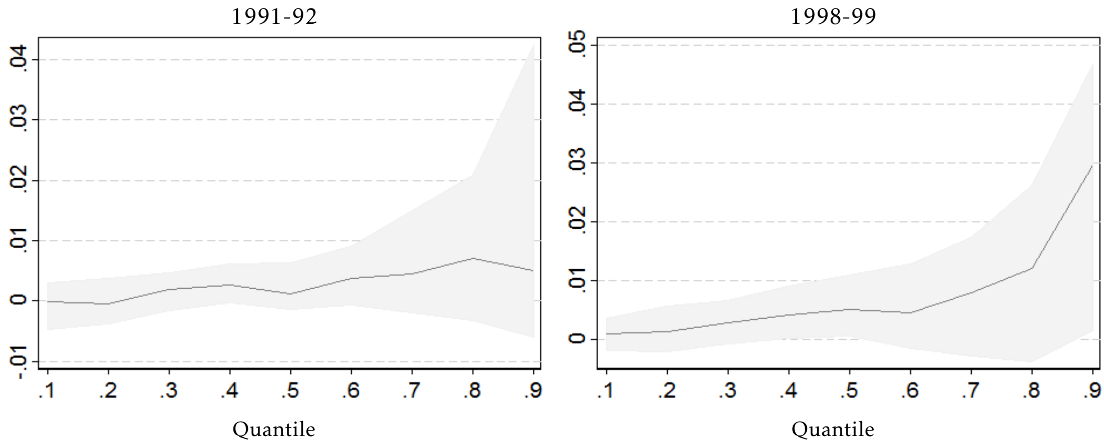
Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling villages with replacement to account for within-cluster dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Figure 2.6 **Distributional impacts of microcredit on household non-food consumption: cross-section quantile regressions with village quantile effects**

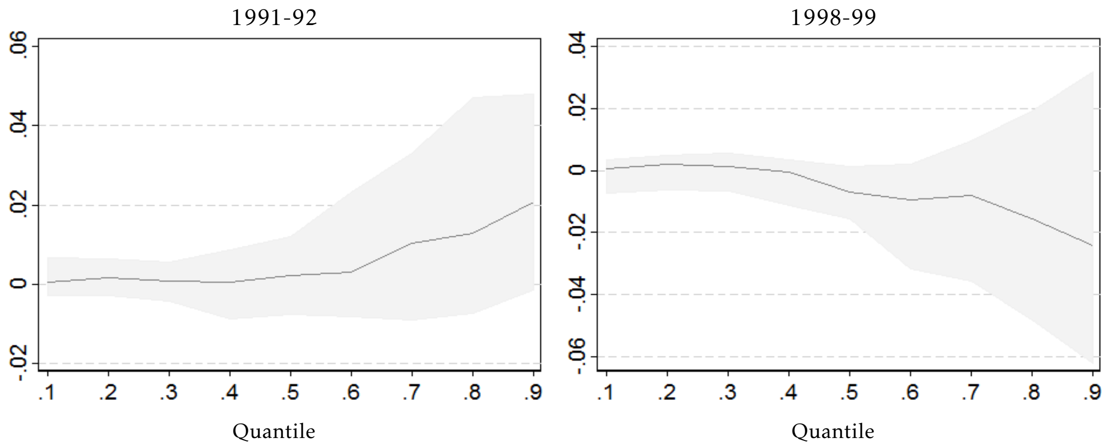
(a) Panel A: Household microcredit



(b) Panel B: Female microcredit



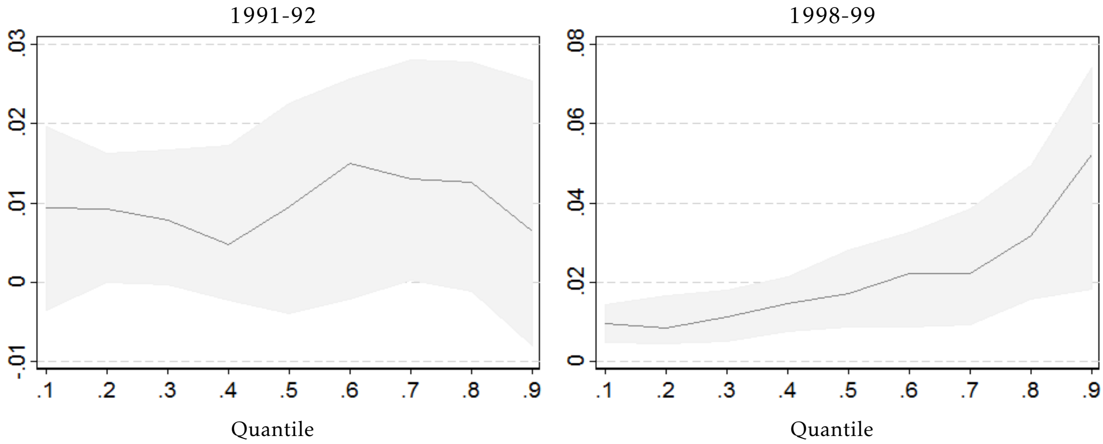
(c) Panel C: Male microcredit



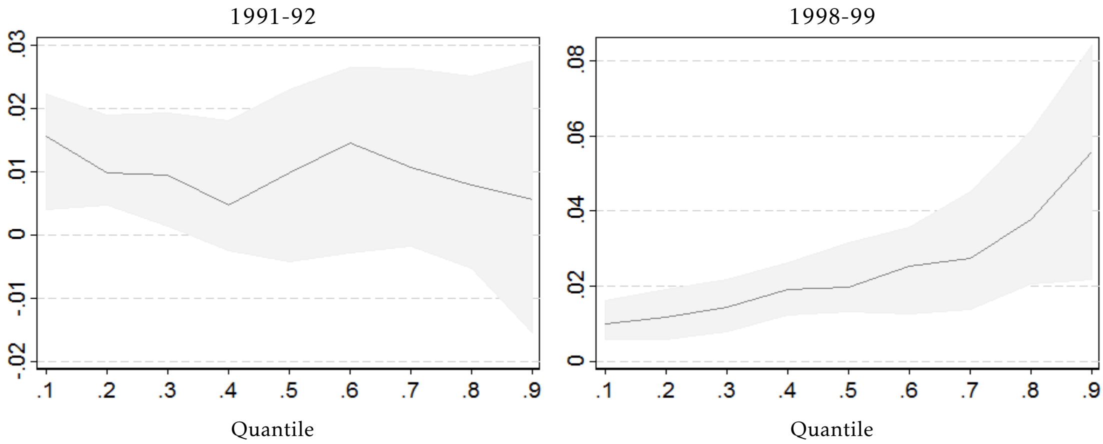
Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling villages with replacement to account for within-cluster dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Figure 2.7 **Distributional impacts of microcredit on household total consumption: cross-section quantile regressions with penalised village effects**

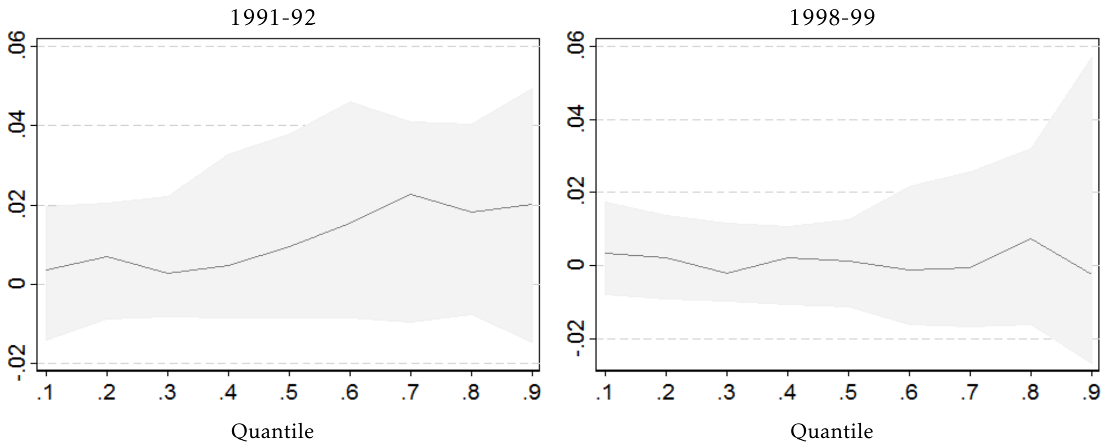
(a) Panel A: Household microcredit



(b) Panel B: Female microcredit



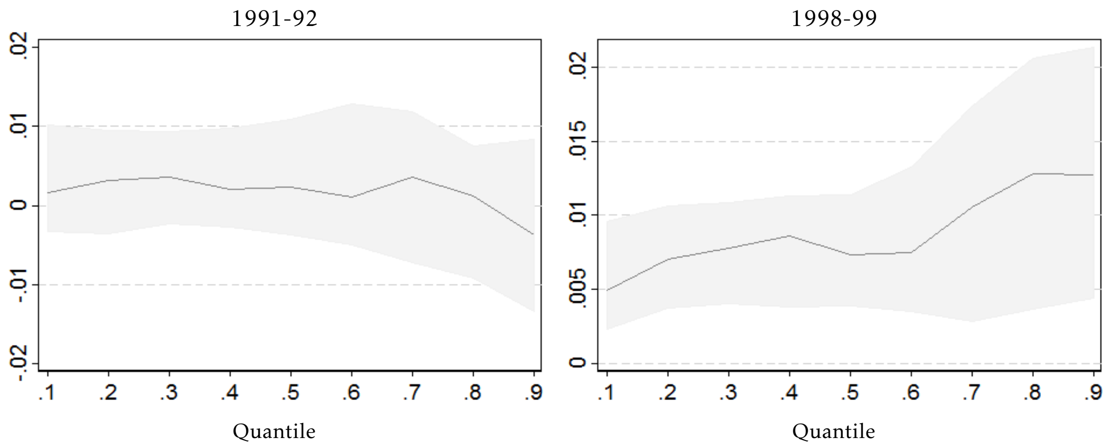
(c) Panel C: Male microcredit



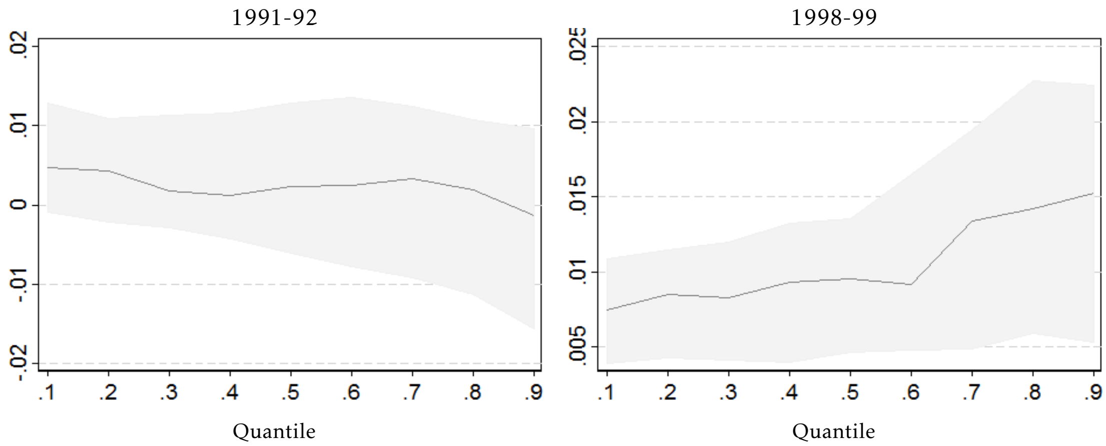
Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling villages with replacement to account for within-cluster dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Figure 2.8 **Distributional impacts of microcredit on household food consumption: cross-section quantile regressions with penalised village effects**

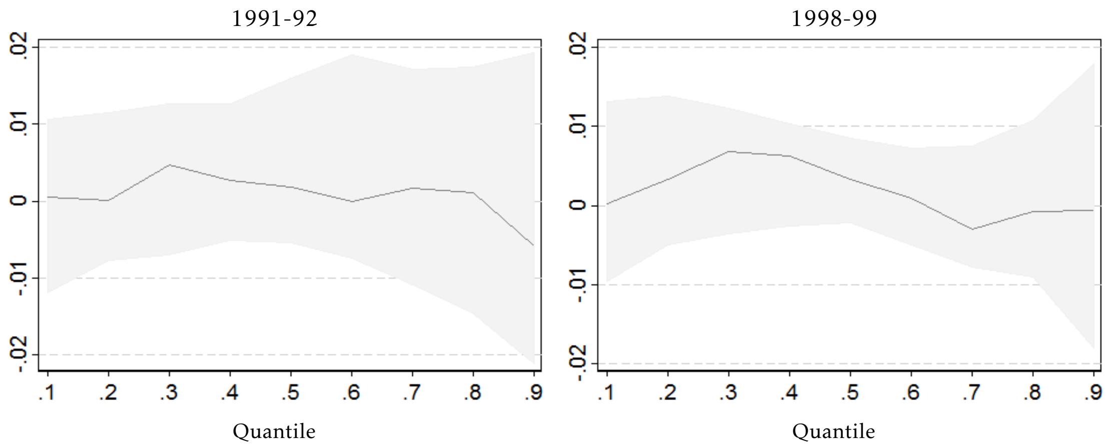
(a) Panel A: Household microcredit



(b) Panel B: Female microcredit



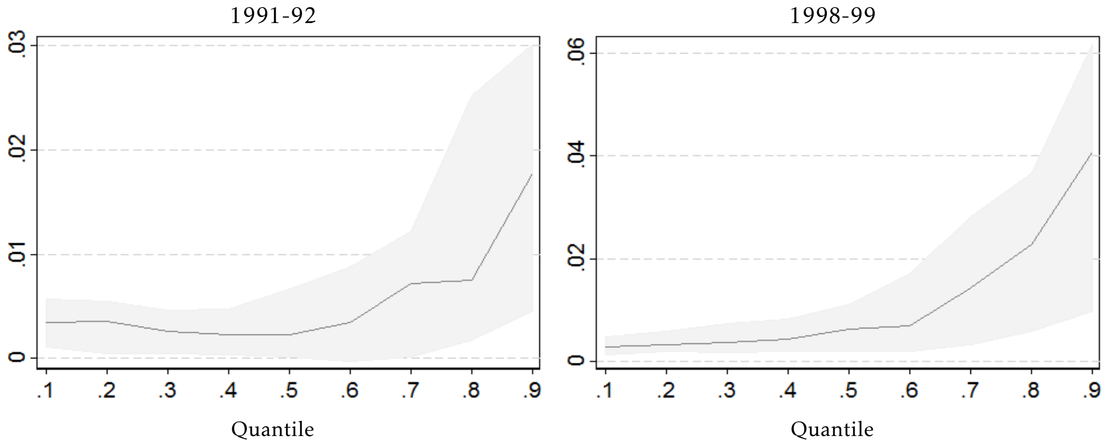
(c) Panel C: Male microcredit



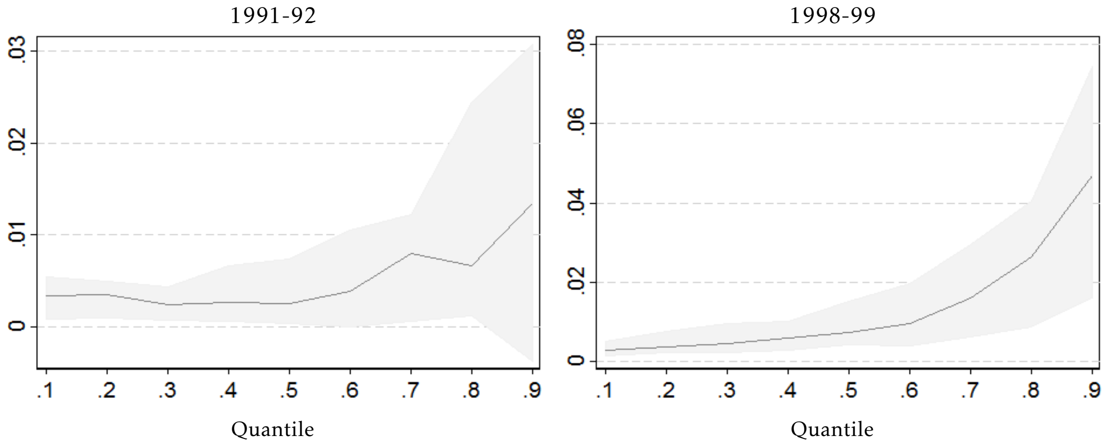
Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling villages with replacement to account for within-cluster dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Figure 2.9 **Distributional impacts of microcredit on household non-food consumption: cross-section quantile regressions with penalised village effects**

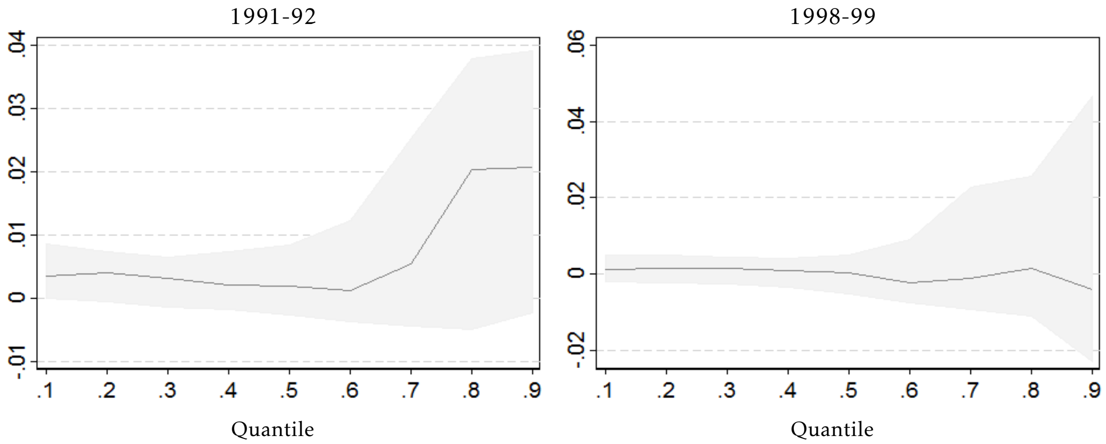
(a) Panel A: Household microcredit



(b) Panel B: Female microcredit



(c) Panel C: Male microcredit



Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling villages with replacement to account for within-cluster dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Table 2.1 **Distributional impacts of microcredit on household total expenditure, cross-section quantile r**

	1991-92				1998-	
Quantile	(1) **Household credit**	(2) **Female credit**	(3) **Male credit**	(4) **Female = Male (p-value)**	(5) **Household credit**	(6) **Female credit**
10	0.006	0.008	-0.001	0.300	0.010	0.011*
	(0.005)	(0.005)	(0.010)		(0.005)	(0.005)
20	0.006	0.006	-0.001	0.365	0.010**	0.011**
	(0.005)	(0.004)	(0.008)		(0.004)	(0.004)
30	0.005	0.008	0.001	0.521	0.014**	0.015**
	(0.005)	(0.005)	(0.009)		(0.004)	(0.005)
40	0.008	0.010	0.003	0.572	0.012**	0.013**
	(0.005)	(0.006)	(0.011)		(0.005)	(0.006)
50	0.007	0.008	0.003	0.774	0.017**	0.018**
	(0.006)	(0.007)	(0.014)		(0.007)	(0.007)
60	0.008	0.007	0.014	0.694	0.021**	0.023**
	(0.007)	(0.007)	(0.016)		(0.009)	(0.009)
70	0.006	0.004	0.015	0.575	0.028**	0.030**
	(0.008)	(0.008)	(0.017)		(0.011)	(0.011)
80	0.004	0.002	0.019	0.433	0.031**	0.035**
	(0.011)	(0.013)	(0.018)		(0.014)	(0.015)
90	0.025	0.015	0.026	0.712	0.053**	0.054**
	(0.016)	(0.022)	(0.022)		(0.016)	(0.017)
Coefficients equal across quantiles (p-value)	0.899	0.975	0.935	0.993	0.272	0.404
Coefficients jointly zero (p-value)	0.770	0.750	0.965	0.946	0.011	0.013

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns (4 of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4) and (8) show joint hypotheses tests of the equ microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

Table 2.2 **Distributional impacts of microcredit on household food expenditure, cross-section quantile r**

	1991-92				1998-	
	(1)	(2)	(3)	(4)	(5)	(6)
Quantile	**Household credit**	**Female credit**	**Male credit**	**Female = Male (p-value)**	**Household credit**	**Female credit**
10	0.002	0.003	-0.005	0.243	0.006*	0.007*
	(0.004)	(0.004)	(0.007)		(0.003)	(0.003)
20	0.003	0.005	0.001	0.563	0.007**	0.007**
	(0.003)	(0.004)	(0.006)		(0.002)	(0.003)
30	0.002	0.004	-0.001	0.401	0.008**	0.007**
	(0.003)	(0.005)	(0.005)		(0.002)	(0.003)
40	0.001	0.003	-0.002	0.415	0.007**	0.007**
	(0.004)	(0.005)	(0.005)		(0.003)	(0.003)
50	0.002	0.009	-0.003	0.115	0.005	0.005*
	(0.005)	(0.006)	(0.006)		(0.003)	(0.003)
60	0.003	0.006	0.002	0.631	0.006	0.008
	(0.005)	(0.006)	(0.006)		(0.004)	(0.004)
70	0.000	0.002	-0.000	0.801	0.008	0.011
	(0.005)	(0.006)	(0.007)		(0.005)	(0.005)
80	0.000	0.003	-0.004	0.481	0.010	0.013*
	(0.005)	(0.007)	(0.007)		(0.005)	(0.005)
90	-0.002	0.002	-0.006	0.572	0.011	0.012*
	(0.007)	(0.009)	(0.009)		(0.005)	(0.005)
Coefficients equal across quantiles (p-value)	0.979	0.714	0.798	0.835	0.884	0.837
Coefficients jointly zero (p-value)	0.975	0.660	0.844	0.855	0.096	0.077

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns (4 of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4) and (8) show joint hypotheses tests of the eq microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

Table 2.3 **Distributional impacts of microcredit on household non-food expenditure, cross-section qua variates**

Quantile	1991-92 (1) Household credit	(2) Female credit	(3) Male credit	(4) Female = Male (p-value)	1998- (5) Household credit	(6) Female credit
10	0.003*	0.003	0.003	0.920	0.003*	0.003*
	(0.001)	(0.001)	(0.002)		(0.001)	(0.001)
20	0.002*	0.002	0.004	0.583	0.003**	0.003*
	(0.001)	(0.001)	(0.002)		(0.002)	(0.002)
30	0.003*	0.002	0.004	0.482	0.004**	0.005**
	(0.001)	(0.001)	(0.003)		(0.002)	(0.002)
40	0.002	0.002	0.004	0.552	0.007**	0.007**
	(0.001)	(0.002)	(0.003)		(0.002)	(0.002)
50	0.003	0.002	0.003	0.710	0.007**	0.007**
	(0.002)	(0.003)	(0.004)		(0.003)	(0.003)
60	0.006	0.008	0.001	0.264	0.007**	0.009**
	(0.003)	(0.003)	(0.006)		(0.005)	(0.005)
70	0.008	0.008	0.006	0.814	0.013**	0.013**
	(0.004)	(0.005)	(0.010)		(0.007)	(0.008)
80	0.010	0.008	0.012	0.793	0.028**	0.034**
	(0.008)	(0.008)	(0.014)		(0.012)	(0.014)
90	0.021	0.016	0.026	0.636	0.031**	0.032**
	(0.010)	(0.013)	(0.015)		(0.013)	(0.015)
Coefficients equal across quantiles (p-value)	0.584	0.369	0.824	0.678	0.185	0.277
Coefficients jointly zero (p-value)	0.239	0.171	0.644	0.316	0.062	0.054

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns (4 of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4) and (8) show joint hypotheses tests of the equ microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

Table 2.4 **Distributional impacts of microcredit on household total expenditure, cross-section quantile effects**

	1991-92				1998-	
	(1)	(2)	(3)	(4)	(5)	(6)
Quantile	**Household credit**	**Female credit**	**Male credit**	**Female = Male (p-value)**	**Household credit**	**Female credit**
10	0.007	0.007	0.006	0.924	-0.000	0.003
	(0.005)	(0.006)	(0.008)		(0.004)	(0.005)
20	0.006	0.005	0.011	0.520	0.003	0.005
	(0.004)	(0.005)	(0.008)		(0.004)	(0.004)
30	0.008	0.011	0.007	0.693	0.006	0.007
	(0.005)	(0.006)	(0.008)		(0.004)	(0.004)
40	0.007	0.007	0.002	0.575	0.007	0.008**
	(0.005)	(0.006)	(0.010)		(0.004)	(0.005)
50	0.007	0.003	0.010	0.652	0.004	0.007
	(0.006)	(0.006)	(0.013)		(0.006)	(0.007)
60	0.006	0.005	0.015	0.540	0.003	0.008
	(0.007)	(0.007)	(0.015)		(0.007)	(0.008)
70	0.006	0.003	0.018	0.442	0.006	0.011
	(0.009)	(0.009)	(0.017)		(0.008)	(0.008)
80	0.007	-0.003	0.022	0.166	0.017	0.024
	(0.010)	(0.011)	(0.015)		(0.011)	(0.010)
90	0.008	0.004	0.016	0.566	0.026	0.031*
	(0.012)	(0.016)	(0.016)		(0.014)	(0.014)
Coefficients equal across quantiles (p-value)	1.000	0.862	0.840	0.963	0.510	0.712
Coefficients jointly zero (p-value)	0.947	0.775	0.769	0.889	0.326	0.221

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence
rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns (4
of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4) and (8) show joint hypotheses tests of the equ
microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

Table 2.5 **Distributional impacts of microcredit on household food expenditure, cross-section quantile effects**

Quantile	1991-92 (1) Household credit	(2) Female credit	(3) Male credit	(4) Female = Male (p-value)	1998- (5) Household credit	(6) Female credit
10	0.003	0.004	0.000	0.585	-0.001	-0.000
	(0.004)	(0.005)	(0.006)		(0.002)	(0.003)
20	0.004	0.001	0.005	0.511	0.001	0.002
	(0.003)	(0.004)	(0.004)		(0.003)	(0.003)
30	0.008*	0.005	0.012**	0.187	0.001	0.002
	(0.003)	(0.004)	(0.004)		(0.003)	(0.003)
40	0.006*	0.005	0.009**	0.402	0.001	0.002
	(0.003)	(0.004)	(0.004)		(0.002)	(0.002)
50	0.004	0.001	0.007**	0.327	-0.001	0.001
	(0.003)	(0.005)	(0.004)		(0.003)	(0.003)
60	0.004	0.000	0.009**	0.156	-0.002	-0.000
	(0.003)	(0.005)	(0.004)		(0.003)	(0.003)
70	0.002	-0.002	0.005	0.260	-0.002	-0.001
	(0.003)	(0.005)	(0.004)		(0.003)	(0.003)
80	0.000	-0.001	0.001	0.811	0.001	0.001
	(0.004)	(0.006)	(0.005)		(0.004)	(0.005)
90	0.002	0.001	0.003	0.807	0.005	0.006
	(0.005)	(0.007)	(0.005)		(0.005)	(0.005)
Coefficients equal across quantiles (p-value)	0.675	0.849	0.379	0.658	0.770	0.906
Coefficients jointly zero (p-value)	0.560	0.864	0.101	0.339	0.843	0.943

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns (4 of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4) and (8) show joint hypotheses tests of the equ microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

Table 2.6 **Distributional impacts of microcredit on household non-food expenditure, cross-section quant-tile effects**

Quantile	1991-92 (1) Household credit	(2) Female credit	(3) Male credit	(4) Female = Male (p-value)	1998- (5) Household credit	(6) Female credit
10	-0.000	-0.000	0.001	0.825	0.001	0.001
	(0.002)	(0.002)	(0.002)		(0.001)	(0.001)
20	0.001	-0.000	0.002	0.468	0.002	0.001
	(0.002)	(0.002)	(0.002)		(0.001)	(0.002)
30	0.002	0.002	0.001	0.714	0.003	0.003*
	(0.001)	(0.002)	(0.003)		(0.002)	(0.002)
40	0.002	0.003*	0.001	0.599	0.003	0.004**
	(0.002)	(0.002)	(0.004)		(0.002)	(0.002)
50	0.002	0.001	0.002	0.848	0.004	0.005**
	(0.002)	(0.002)	(0.005)		(0.003)	(0.003)
60	0.003	0.004*	0.003	0.926	0.002	0.005
	(0.003)	(0.002)	(0.008)		(0.003)	(0.004)
70	0.006	0.005	0.010	0.650	0.004	0.008
	(0.004)	(0.004)	(0.012)		(0.005)	(0.005)
80	0.010*	0.007	0.013	0.686	0.004	0.012
	(0.007)	(0.006)	(0.014)		(0.009)	(0.008)
90	0.018**	0.005	0.021*	0.374	0.022*	0.030**
	(0.009)	(0.013)	(0.012)		(0.010)	(0.011)
Coefficients equal across quantiles (p-value)	0.646	0.556	0.787	0.822	0.363	0.434
Coefficients jointly zero (p-value)	0.684	0.433	0.843	0.689	0.188	0.161

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns (4 of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4) and (8) show joint hypotheses tests of the equ microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

Table 2.7 **Distributional impacts of microcredit on household total expenditure, cross-section quantile r**
effects

Quantile	1991-92				1998-	
	(1)	(2)	(3)	(4)	(5)	(6)
	Household credit	**Female credit**	**Male credit**	**Female = Male (p-value)**	**Household credit**	**Female credit**
10	0.009	0.016**	0.004	0.199	0.009**	0.010**
	(0.006)	(0.005)	(0.010)		(0.002)	(0.003)
20	0.009*	0.010**	0.007	0.718	0.008**	0.012**
	(0.004)	(0.003)	(0.008)		(0.003)	(0.004)
30	0.008*	0.009**	0.003	0.428	0.011**	0.014**
	(0.004)	(0.005)	(0.008)		(0.003)	(0.004)
40	0.005	0.005	0.005	0.983	0.015**	0.019**
	(0.005)	(0.005)	(0.010)		(0.003)	(0.003)
50	0.009	0.010	0.010	0.982	0.017**	0.020**
	(0.007)	(0.007)	(0.012)		(0.005)	(0.005)
60	0.015*	0.015	0.015	0.958	0.022**	0.025**
	(0.007)	(0.007)	(0.014)		(0.006)	(0.006)
70	0.013**	0.011*	0.023	0.451	0.022**	0.027**
	(0.007)	(0.007)	(0.014)		(0.007)	(0.007)
80	0.013*	0.008	0.018	0.479	0.032**	0.038**
	(0.007)	(0.008)	(0.013)		(0.009)	(0.010)
90	0.006	0.006	0.020	0.405	0.052**	0.056**
	(0.009)	(0.010)	(0.015)		(0.016)	(0.016)
Coefficients equal across quantiles (p-value)	0.541	0.226	0.770	0.521	0.067	0.015
Coefficients jointly zero (p-value)	0.219	0.019	0.742	0.068	0.001	0.000

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence
rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns (4
of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4) and (8) show joint hypotheses tests of the equ
microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

Table 2.8 **Distributional impacts of microcredit on household food expenditure, cross-section quantile r**
effects

	1991-92				1998-	
	(1)	(2)	(3)	(4)	(5)	(6)
Quantile	**Household credit**	**Female credit**	**Male credit**	**Female = Male (p-value)**	**Household credit**	**Female credit**
10	0.002	0.005	0.000	0.450	0.005**	0.007**
	(0.003)	(0.003)	(0.006)		(0.002)	(0.002)
20	0.003	0.004	0.000	0.463	0.007**	0.009**
	(0.003)	(0.003)	(0.005)		(0.002)	(0.002)
30	0.004	0.002	0.005	0.648	0.008**	0.008**
	(0.003)	(0.003)	(0.005)		(0.002)	(0.002)
40	0.002	0.001	0.003	0.792	0.009**	0.009**
	(0.003)	(0.004)	(0.005)		(0.002)	(0.002)
50	0.002	0.002	0.002	0.935	0.007**	0.010**
	(0.004)	(0.005)	(0.005)		(0.002)	(0.002)
60	0.001	0.002	-0.000	0.766	0.008**	0.009**
	(0.005)	(0.006)	(0.007)		(0.002)	(0.003)
70	0.004	0.003	0.002	0.862	0.011**	0.013**
	(0.005)	(0.006)	(0.008)		(0.004)	(0.004)
80	0.001	0.002	0.001	0.923	0.013**	0.014**
	(0.004)	(0.005)	(0.007)		(0.004)	(0.004)
90	-0.004	-0.001	-0.006	0.669	0.013**	0.015**
	(0.006)	(0.006)	(0.010)		(0.005)	(0.005)
Coefficients equal across quantiles (p-value)	0.712	0.924	0.848	0.966	0.414	0.718
Coefficients jointly zero (p-value)	0.738	0.885	0.891	0.955	0.002	0.000

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence
rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns (4
of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4) and (8) show joint hypotheses tests of the equ
microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

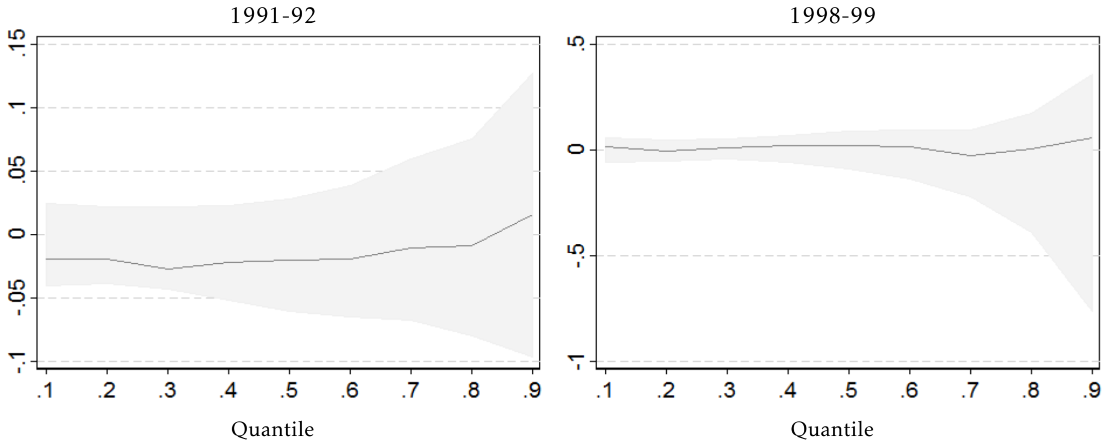
Table 2.9 **Distributional impacts of microcredit on household non-food expenditure, cross-section qua village effects**

Quantile	1991-92 (1) Household credit	(2) Female credit	(3) Male credit	(4) Female = Male (p-value)	1998- (5) Household credit	(6) Female credit
10	0.003**	0.003**	0.004**	0.988	0.003**	0.003**
	(0.001)	(0.001)	(0.002)		(0.001)	(0.001)
20	0.004**	0.004**	0.004	0.851	0.003**	0.004**
	(0.001)	(0.001)	(0.002)		(0.001)	(0.001)
30	0.003**	0.003**	0.003	0.745	0.004**	0.004**
	(0.001)	(0.001)	(0.002)		(0.001)	(0.002)
40	0.002**	0.003**	0.002	0.774	0.004**	0.006**
	(0.001)	(0.001)	(0.002)		(0.002)	(0.002)
50	0.002**	0.003**	0.002	0.849	0.006**	0.007**
	(0.002)	(0.002)	(0.003)		(0.002)	(0.003)
60	0.003*	0.004**	0.001	0.547	0.007**	0.009**
	(0.002)	(0.003)	(0.004)		(0.004)	(0.004)
70	0.007**	0.008**	0.005	0.723	0.014**	0.016**
	(0.003)	(0.003)	(0.007)		(0.006)	(0.006)
80	0.008**	0.007**	0.020	0.268	0.023**	0.026**
	(0.006)	(0.006)	(0.012)		(0.008)	(0.009)
90	0.018**	0.013	0.021*	0.524	0.041**	0.047**
	(0.006)	(0.009)	(0.010)		(0.013)	(0.014)
Coefficients equal across quantiles (p-value)	0.251	0.530	0.372	0.601	0.089	0.059
Coefficients jointly zero (p-value)	0.060	0.042	0.386	0.158	0.002	0.001

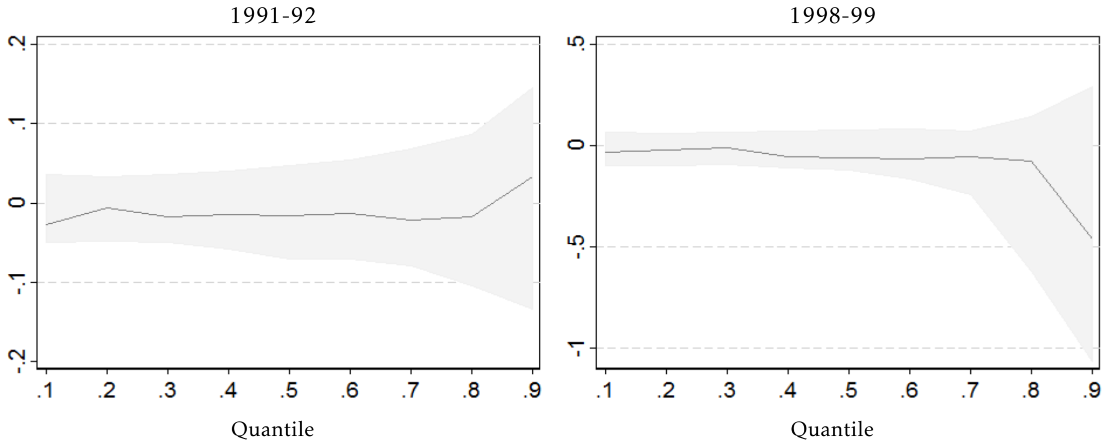
Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns (4 of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4) and (8) show joint hypotheses tests of the equ microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

Figure 2.10 **Distributional impacts of microcredit on household total consumption: cross-section two-stage quantile regressions with village covariates**

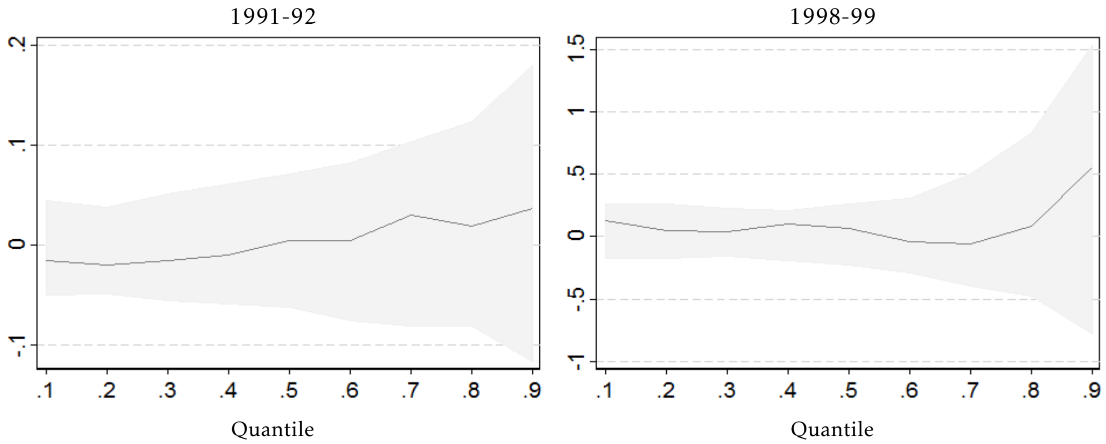
(a) Panel A: Household microcredit



(b) Panel B: Female microcredit



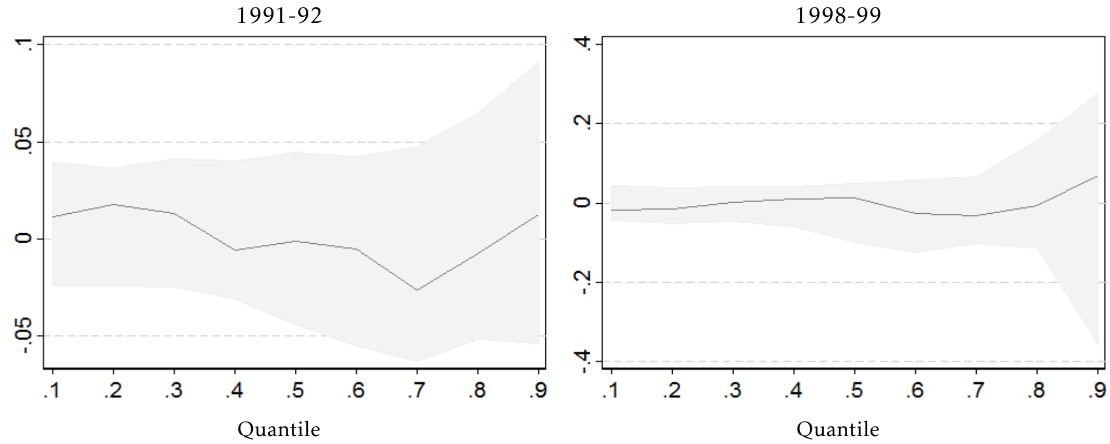
(c) Panel C: Male microcredit



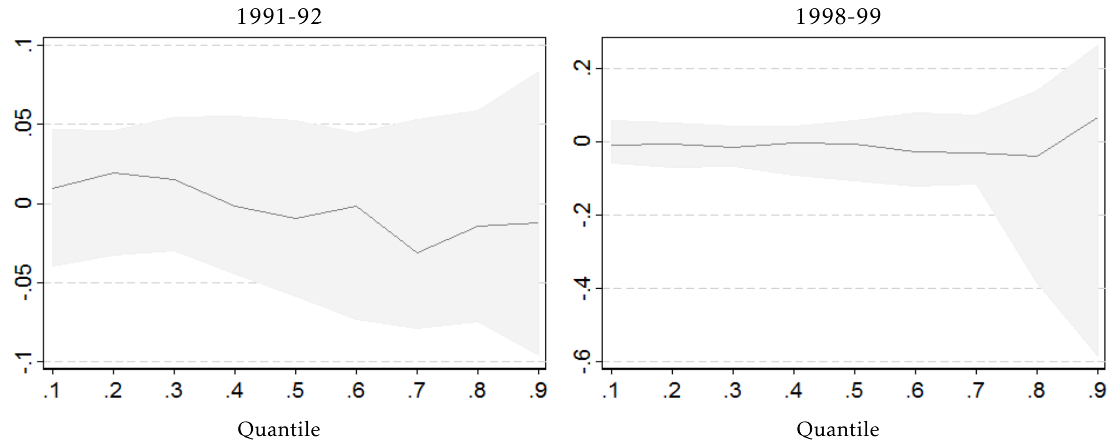
Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling villages with replacement to account for within-cluster dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. Microcredit is instrumented for following the approach in Pitt and Khandker (1998), using OLS in the first stage. Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Figure 2.11 **Distributional impacts of microcredit on household total consumption: cross-section two-stage quantile regressions with village quantile effects**

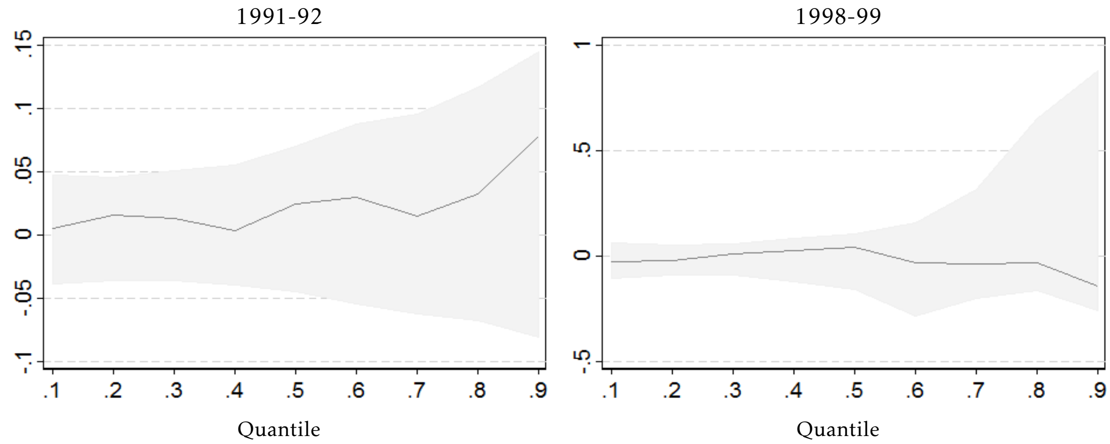
(a) Panel A: Household microcredit



(b) Panel B: Female microcredit



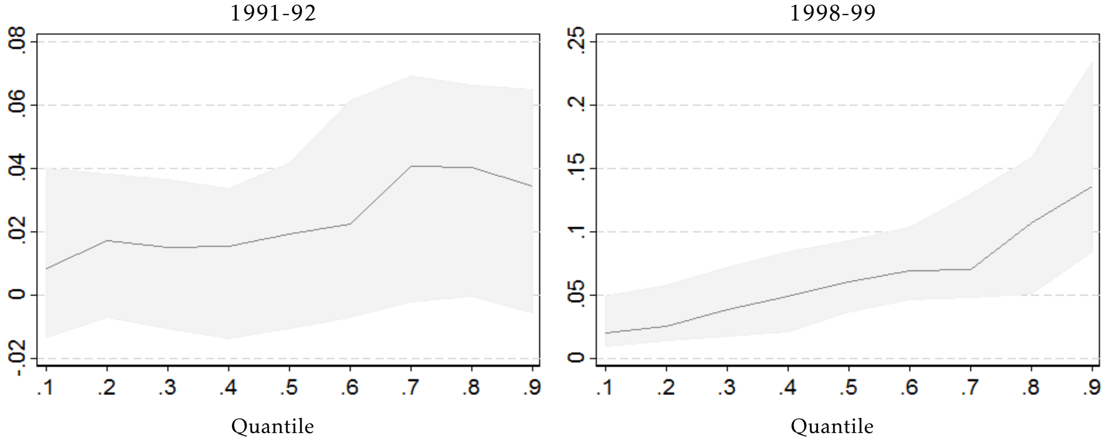
(c) Panel C: Male microcredit



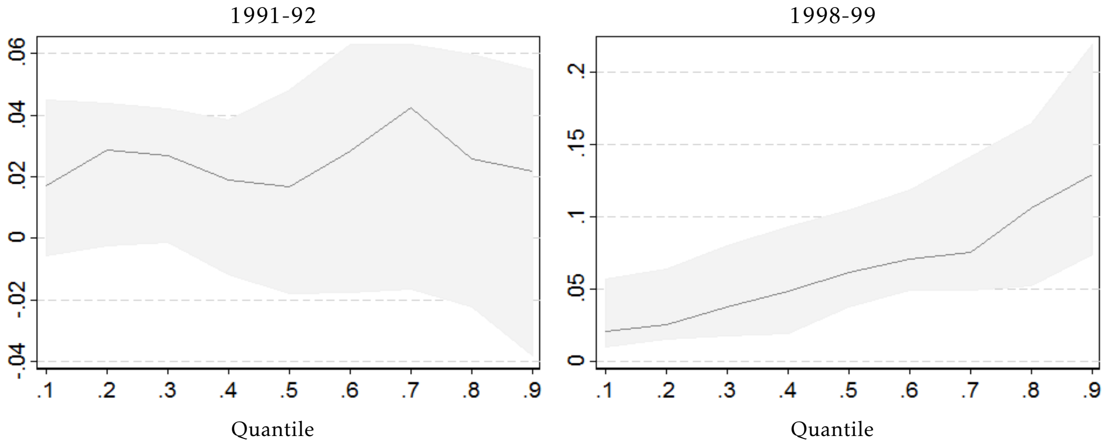
Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling villages with replacement to account for within-cluster dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. Microcredit is instrumented for following the approach in Pitt and Khandker (1998), using OLS in the first stage. Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Figure 2.12 **Distributional impacts of microcredit on household total consumption: cross-section two-stage quantile regressions with penalised village effects**

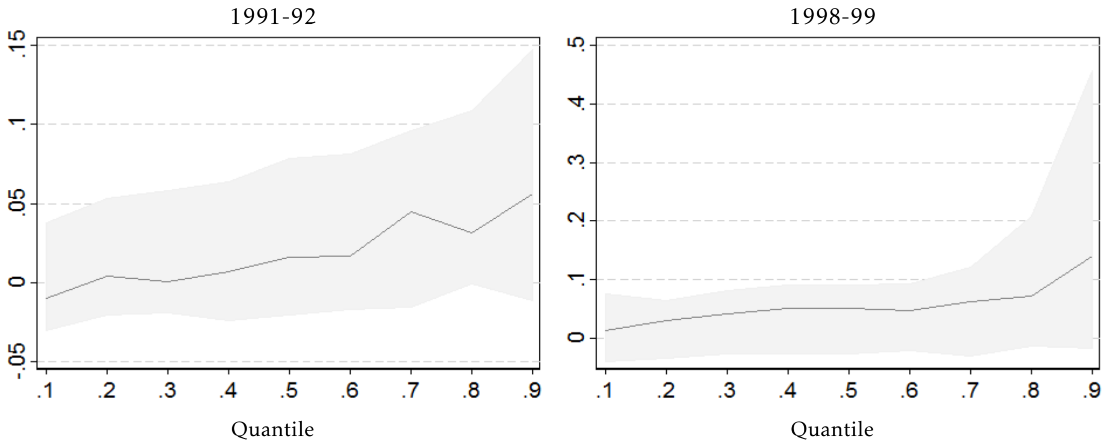
(a) Panel A: Household microcredit



(b) Panel B: Female microcredit



(c) Panel C: Male microcredit



Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling villages with replacement to account for within-cluster dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. Microcredit is instrumented for following the approach in Pitt and Khandker (1998), using OLS in the first stage. Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Table 2.10 **Distributional impacts of microcredit on household total expenditure, cross-section two-stage
covariates**

Quantile	1991-92 (1) Household credit	(2) Female credit	(3) Male credit	(4) Female = Male (p-value)	1998- (5) Household credit	(6) Female credit
10	-0.019	-0.027	-0.015	0.721	0.016	-0.031
	(0.017)	(0.022)	(0.024)		(0.030)	(0.043)
20	-0.019	-0.006	-0.020	0.662	-0.004	-0.022
	(0.015)	(0.020)	(0.021)		(0.025)	(0.040)
30	-0.027	-0.018	-0.016	0.955	0.012	-0.009
	(0.016)	(0.022)	(0.026)		(0.025)	(0.040)
40	-0.022	-0.015	-0.010	0.916	0.021	-0.053
	(0.019)	(0.025)	(0.029)		(0.032)	(0.046)
50	-0.020	-0.016	0.004	0.643	0.021	-0.060
	(0.023)	(0.029)	(0.034)		(0.044)	(0.052)
60	-0.020	-0.013	0.005	0.721	0.018	-0.066
	(0.026)	(0.031)	(0.037)		(0.059)	(0.060)
70	-0.011	-0.022	0.030	0.368	-0.025	-0.055
	(0.032)	(0.037)	(0.044)		(0.082)	(0.076)
80	-0.009	-0.017	0.018	0.601	0.006	-0.076
	(0.039)	(0.045)	(0.050)		(0.141)	(0.169)
90	0.015	0.033	0.037	0.970	0.059	-0.461
	(0.057)	(0.068)	(0.076)		(0.304)	(0.351)
Coefficients equal across quantiles (p-value)	0.996	0.934	0.963	0.995	0.957	0.753
Coefficients jointly zero (p-value)	0.940	0.928	0.950	0.990	0.975	0.788

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence
rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns (4
of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4) and (8) show joint hypotheses tests of the equ
microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

Table 2.11 **Distributional impacts of microcredit on household total expenditure, cross-section two-stage quantile effects**

Quantile	1991-92 (1) Household credit	(2) Female credit	(3) Male credit	(4) Female = Male (p-value)	1998- (5) Household credit	(6) Female credit
10	0.012	0.010	0.006	0.897	-0.019	-0.008
	(0.016)	(0.022)	(0.022)		(0.023)	(0.030)
20	0.018	0.019	0.016	0.900	-0.014	-0.004
	(0.015)	(0.019)	(0.022)		(0.023)	(0.031)
30	0.013	0.015	0.013	0.947	0.001	-0.015
	(0.016)	(0.021)	(0.022)		(0.022)	(0.029)
40	-0.005	-0.002	0.004	0.868	0.010	-0.002
	(0.018)	(0.025)	(0.024)		(0.026)	(0.032)
50	-0.001	-0.009	0.025	0.405	0.012	-0.005
	(0.023)	(0.028)	(0.030)		(0.036)	(0.042)
60	-0.005	-0.002	0.030	0.498	-0.028	-0.027
	(0.025)	(0.029)	(0.038)		(0.042)	(0.048)
70	-0.026	-0.032	0.015	0.332	-0.033	-0.029
	(0.028)	(0.032)	(0.040)		(0.044)	(0.052)
80	-0.008	-0.014	0.033	0.387	-0.007	-0.037
	(0.029)	(0.032)	(0.045)		(0.076)	(0.107)
90	0.013	-0.012	0.078	0.210	0.069	0.067
	(0.036)	(0.043)	(0.056)		(0.140)	(0.204)
Coefficients equal across quantiles (p-value)	0.617	0.723	0.786	0.903	0.781	0.977
Coefficients jointly zero (p-value)	0.638	0.763	0.841	0.890	0.823	0.984

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns (4 of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4) and (8) show joint hypotheses tests of the equ microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

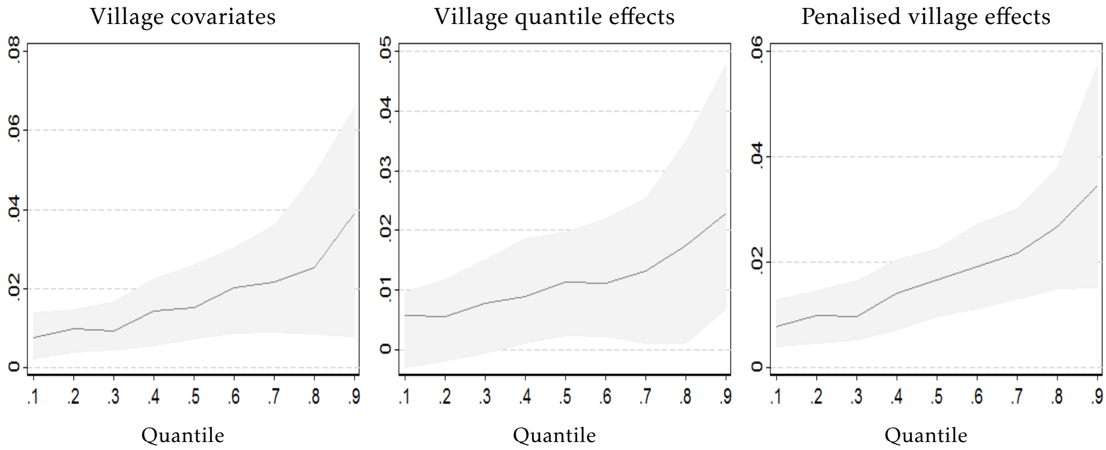
Table 2.12 **Distributional impacts of microcredit on household total expenditure, cross-section two-sta nalised village effects**

Quantile	1991-92 (1) Household credit	(2) Female credit	(3) Male credit	(4) Female = Male (p-value)	1998- (5) Household credit	(6) Female credit
10	0.009	0.017	-0.010	0.232	0.020**	0.021**
	(0.014)	(0.014)	(0.019)		(0.011)	(0.012)
20	0.017	0.029*	0.004	0.287	0.025**	0.026**
	(0.012)	(0.012)	(0.018)		(0.012)	(0.014)
30	0.015	0.027*	0.000	0.270	0.038**	0.037**
	(0.013)	(0.011)	(0.020)		(0.015)	(0.018)
40	0.016	0.019	0.007	0.650	0.049**	0.049**
	(0.013)	(0.013)	(0.022)		(0.016)	(0.020)
50	0.019	0.017	0.016	0.976	0.061**	0.062**
	(0.013)	(0.017)	(0.025)		(0.015)	(0.018)
60	0.022	0.028	0.017	0.737	0.069**	0.071**
	(0.018)	(0.021)	(0.026)		(0.015)	(0.019)
70	0.041*	0.042	0.045	0.938	0.070**	0.076**
	(0.019)	(0.021)	(0.028)		(0.021)	(0.026)
80	0.041*	0.026	0.031*	0.870	0.107**	0.106**
	(0.017)	(0.022)	(0.028)		(0.031)	(0.033)
90	0.035*	0.022	0.056*	0.453	0.136**	0.129**
	(0.018)	(0.025)	(0.039)		(0.041)	(0.040)
Coefficients equal across quantiles (p-value)	0.624	0.382	0.576	0.587	0.000	0.000
Coefficients jointly zero (p-value)	0.439	0.093	0.671	0.263	0.000	0.000

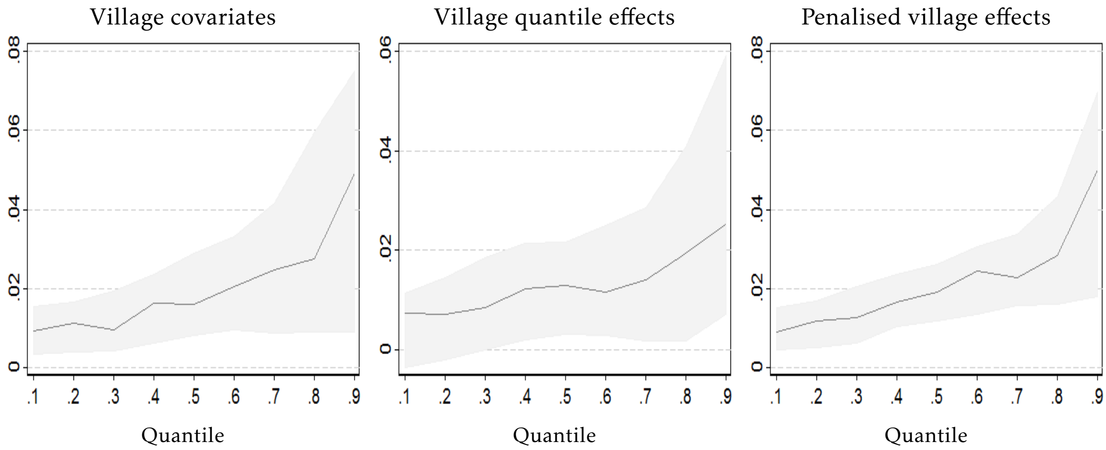
Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns (4 of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4) and (8) show joint hypotheses tests of the equ microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

Figure 2.13 **Distributional impacts of microcredit on household total consumption: pooled quantile regressions**

(a) Panel A: Household microcredit



(b) Panel B: Female microcredit



(c) Panel C: Male microcredit

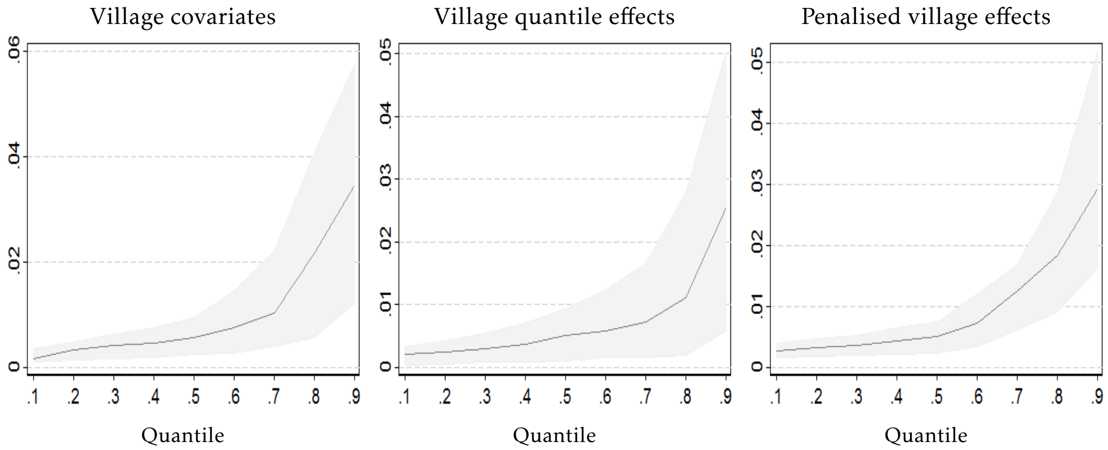
Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling households with replacement to account for dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Figure 2.14 **Distributional impacts of microcredit on household food consumption: pooled quantile regressions**

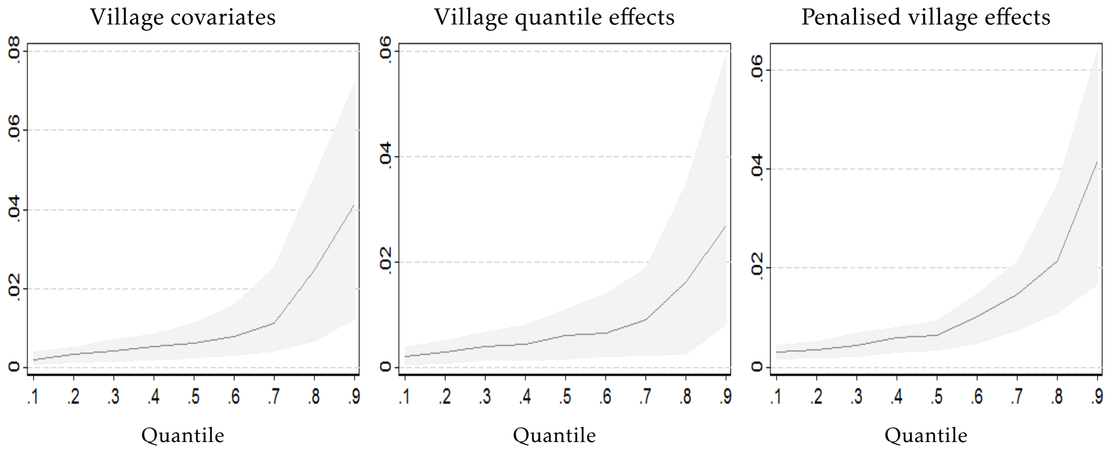
Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling households with replacement to account for dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Figure 2.15 **Distributional impacts of microcredit on household non-food consumption: pooled quantile regressions**

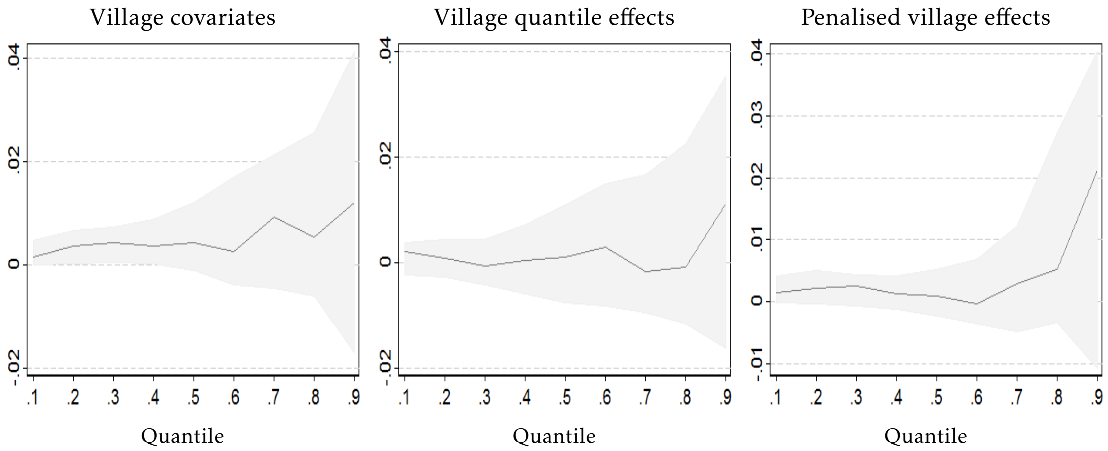
(a) Panel A: Household microcredit



(b) Panel B: Female microcredit



(c) Panel C: Male microcredit



Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling households with replacement to account for dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Table 2.13 **Distributional impacts of microcredit on household total expenditure, pooled quantile regres**

	Village covariates				Village quantile effects				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Quantile	**Household credit**	**Female credit**	**Male credit**	**Female = Male (p-value)**	**Household credit**	**Female credit**	**Male credit**	**Female = Male (p-value)**	**Househ credi**
10	0.008**	0.009**	0.001	0.134	0.006	0.007	0.004	0.621	0.008
	(0.003)	(0.003)	(0.005)		(0.003)	(0.004)	(0.005)		(0.00
20	0.010**	0.011**	0.008	0.599	0.006	0.007	0.003	0.480	0.010
	(0.003)	(0.003)	(0.005)		(0.003)	(0.004)	(0.005)		(0.00
30	0.009**	0.009**	0.008*	0.801	0.008*	0.009*	0.003	0.477	0.010
	(0.003)	(0.004)	(0.004)		(0.004)	(0.005)	(0.006)		(0.00
40	0.014**	0.016**	0.007	0.198	0.009**	0.012**	0.000	0.160	0.014
	(0.004)	(0.005)	(0.006)		(0.004)	(0.005)	(0.007)		(0.00
50	0.015**	0.016**	0.009	0.461	0.011**	0.013**	0.005	0.397	0.017
	(0.005)	(0.005)	(0.009)		(0.004)	(0.005)	(0.009)		(0.00
60	0.020**	0.021**	0.010	0.351	0.011**	0.012**	0.006	0.566	0.019
	(0.005)	(0.006)	(0.010)		(0.005)	(0.005)	(0.010)		(0.00
70	0.022**	0.025**	0.019	0.585	0.013**	0.014**	0.008	0.610	0.022
	(0.007)	(0.008)	(0.010)		(0.006)	(0.007)	(0.011)		(0.00
80	0.025**	0.027**	0.016	0.450	0.017**	0.019**	0.002	0.229	0.027
	(0.010)	(0.013)	(0.011)		(0.008)	(0.009)	(0.013)		(0.00
90	0.039**	0.049**	-0.001	0.044	0.023**	0.025**	-0.000	0.203	0.034
	(0.014)	(0.016)	(0.020)		(0.010)	(0.013)	(0.016)		(0.01
Coefficients equal across quantiles (p-value)	0.354	0.129	0.670	0.351	0.878	0.892	0.981	0.987	0.04
Coefficients jointly zero (p-value)	0.011	0.002	0.482	0.012	0.350	0.333	0.978	0.835	0.00

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns equality of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4), (8) and (12) show joint hypotheses test and male microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

Table 2.14 **Distributional impacts of microcredit on household food expenditure, pooled quantile regres**

	Village covariates				Village quantile effects				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Quantile	**Household credit**	**Female credit**	**Male credit**	**Female = Male (p-value)**	**Household credit**	**Female credit**	**Male credit**	**Female = Male (p-value)**	**Househ credi**
10	0.005*	0.006**	-0.002	0.068	0.003	0.003	0.004	0.827	0.005
	(0.002)	(0.002)	(0.004)		(0.003)	(0.003)	(0.004)		(0.002
20	0.007**	0.007**	0.007	0.938	0.006**	0.005*	0.008	0.522	0.005
	(0.002)	(0.002)	(0.004)		(0.002)	(0.003)	(0.003)		(0.002
30	0.008**	0.008**	0.007**	0.700	0.006**	0.005**	0.008*	0.496	0.008
	(0.002)	(0.002)	(0.003)		(0.002)	(0.003)	(0.003)		(0.002
40	0.006**	0.008**	0.004	0.227	0.005**	0.006**	0.004	0.701	0.006
	(0.002)	(0.002)	(0.003)		(0.002)	(0.002)	(0.003)		(0.001
50	0.006**	0.007**	0.002	0.209	0.004*	0.004*	0.004	0.951	0.006
	(0.002)	(0.003)	(0.004)		(0.002)	(0.003)	(0.004)		(0.002
60	0.007**	0.010**	0.002	0.133	0.005	0.005	0.002	0.503	0.007
	(0.003)	(0.003)	(0.004)		(0.003)	(0.003)	(0.004)		(0.002
70	0.006**	0.008**	0.001	0.166	0.003	0.003	-0.001	0.411	0.007
	(0.003)	(0.004)	(0.004)		(0.003)	(0.003)	(0.005)		(0.002
80	0.007*	0.010**	-0.002	0.017	0.000	0.001	-0.001	0.786	0.007
	(0.004)	(0.004)	(0.004)		(0.003)	(0.003)	(0.005)		(0.003
90	0.007	0.007*	-0.013	0.007	0.001	0.003	-0.006	0.182	0.007
	(0.004)	(0.004)	(0.007)		(0.004)	(0.004)	(0.007)		(0.003
Coefficients equal across quantiles (p-value)	0.901	0.820	0.042	0.160	0.625	0.628	0.589	0.760	0.49
Coefficients jointly zero (p-value)	0.011	0.009	0.023	0.002	0.298	0.308	0.373	0.344	0.00

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns equality of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4), (8) and (12) show joint hypotheses test and male microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

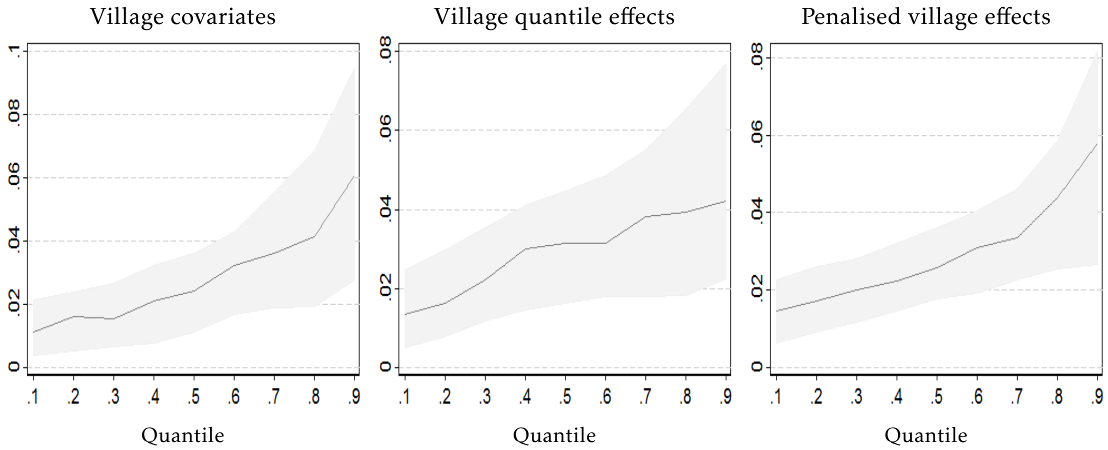
Table 2.15 **Distributional impacts of microcredit on household non-food expenditure, pooled quantile r**

Quantile	Village covariates (1) Household credit	(2) Female credit	(3) Male credit	(4) Female = Male (p-value)	Village quantile effects (5) Household credit	(6) Female credit	(7) Male credit	(8) Female = Male (p-value)	(9) Househ credi
10	0.002**	0.002**	0.001*	0.757	0.002**	0.002**	0.002	0.956	0.003
	(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.002)		(0.001
20	0.003**	0.003**	0.004**	0.856	0.002**	0.003**	0.001	0.282	0.003
	(0.001)	(0.001)	(0.002)		(0.001)	(0.001)	(0.002)		(0.001
30	0.004**	0.004**	0.004**	0.964	0.003**	0.004**	-0.001	0.050	0.004
	(0.001)	(0.001)	(0.002)		(0.001)	(0.001)	(0.002)		(0.001
40	0.005**	0.005**	0.004**	0.542	0.004**	0.004**	0.000	0.253	0.004
	(0.001)	(0.002)	(0.002)		(0.002)	(0.002)	(0.003)		(0.001
50	0.006**	0.006**	0.004	0.662	0.005**	0.006**	0.001	0.318	0.005
	(0.002)	(0.002)	(0.003)		(0.002)	(0.002)	(0.005)		(0.001
60	0.007**	0.008**	0.003	0.370	0.006**	0.007**	0.003	0.558	0.007
	(0.003)	(0.003)	(0.005)		(0.003)	(0.003)	(0.006)		(0.002
70	0.010**	0.011**	0.009	0.811	0.007**	0.009**	-0.002	0.147	0.013
	(0.004)	(0.005)	(0.007)		(0.004)	(0.004)	(0.007)		(0.003
80	0.022**	0.025**	0.005	0.127	0.011**	0.016**	-0.001	0.127	0.018
	(0.008)	(0.011)	(0.008)		(0.007)	(0.008)	(0.009)		(0.005
90	0.035**	0.041**	0.012	0.176	0.025**	0.027**	0.011	0.403	0.029
	(0.011)	(0.014)	(0.016)		(0.011)	(0.013)	(0.014)		(0.009
Coefficients equal across quantiles (p-value)	0.103	0.221	0.433	0.366	0.658	0.655	0.764	0.797	0.043
Coefficients jointly zero (p-value)	0.011	0.030	0.279	0.073	0.077	0.117	0.765	0.342	0.000

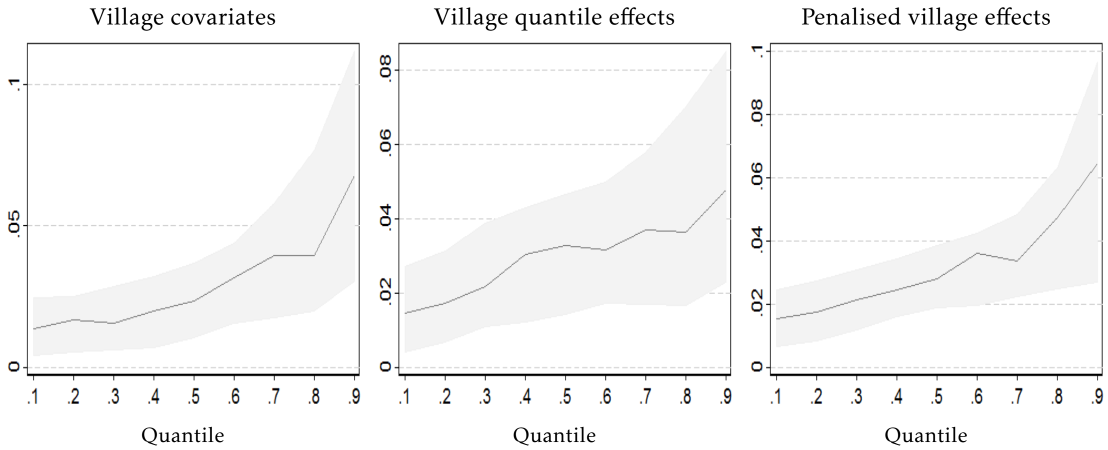
Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns equality of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4), (8) and (12) show joint hypotheses test and male microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

Figure 2.16 **Distributional impacts of microcredit on household total consumption: pooled quantile regressions with correlated random effects**

(a) Panel A: Household microcredit



(b) Panel B: Female microcredit

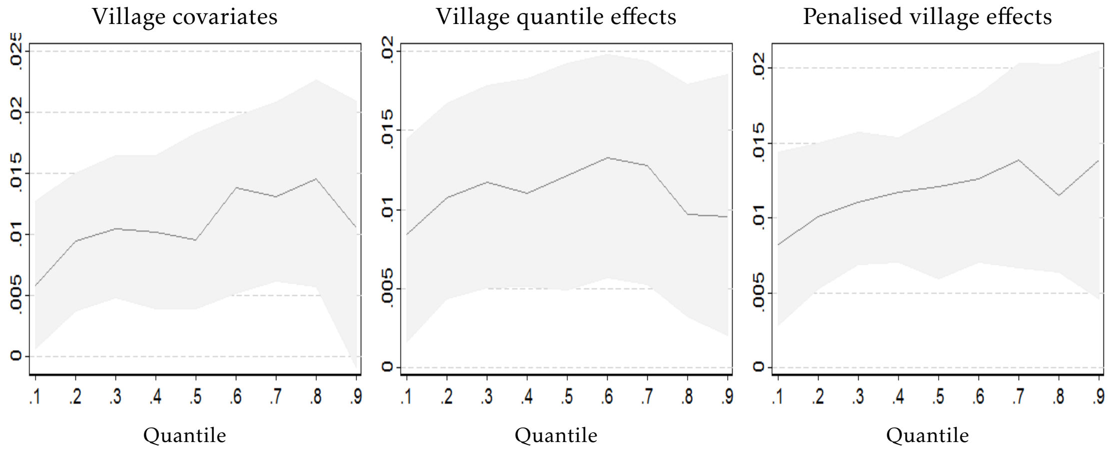


(c) Panel C: Male microcredit

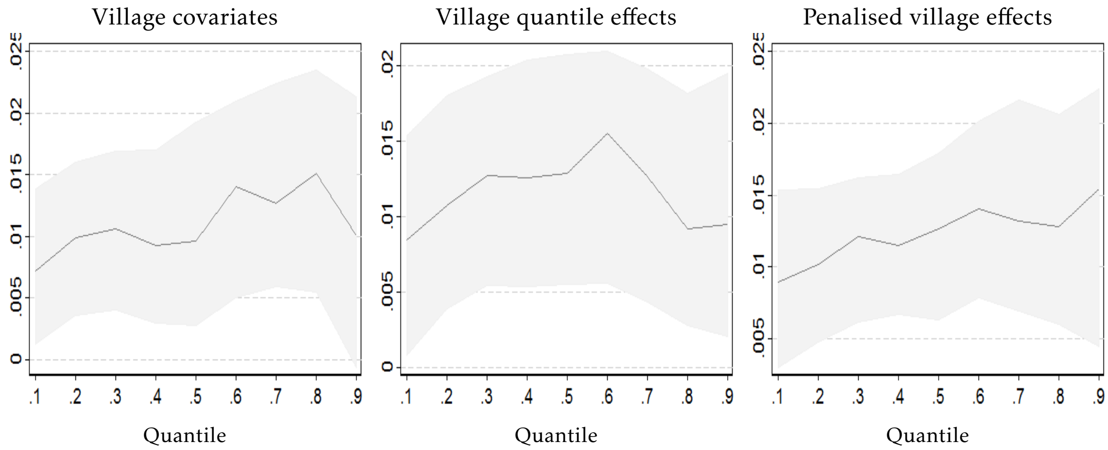
Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling households with replacement to account for dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. All regressions include the time-averaged values of all household covariates (including credit variables) to specify household *correlated random effects*, following the method of Abrevaya and Dahl (2008). Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Figure 2.17 **Distributional impacts of microcredit on household food consumption: pooled quantile regressions with correlated random effects**

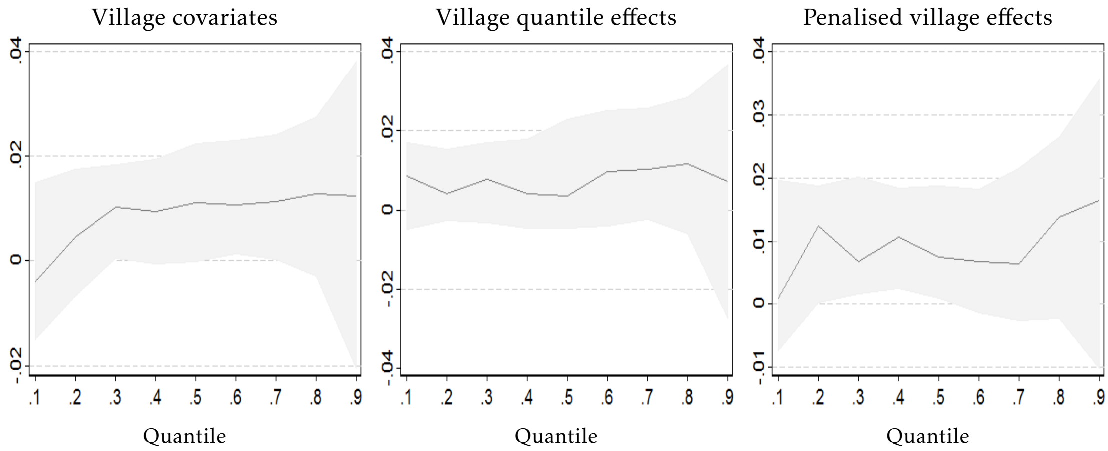
(a) Panel A: Household microcredit



(b) Panel B: Female microcredit



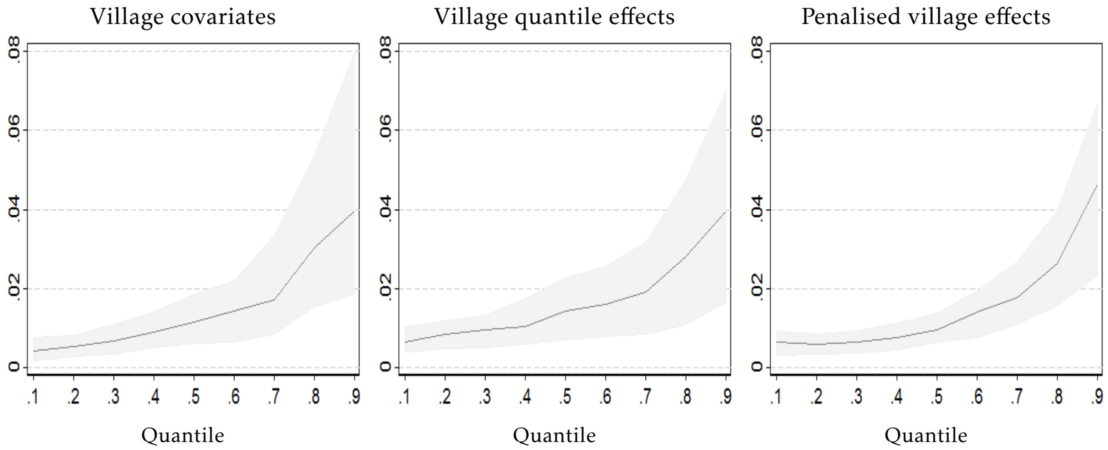
(c) Panel C: Male microcredit



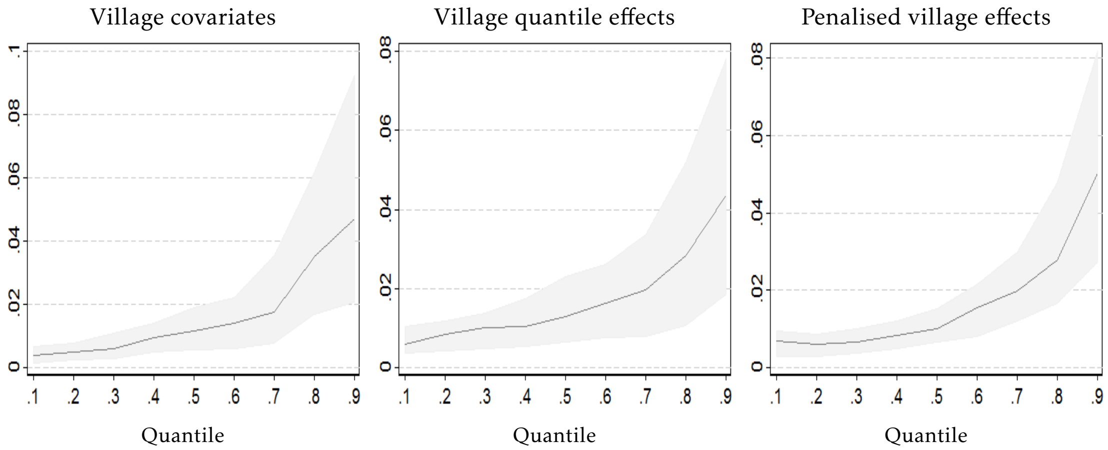
Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling households with replacement to account for dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. All regressions include the time-averaged values of all household covariates (including credit variables) to specify household *correlated random effects*, following the method of Abrevaya and Dahl (2008). Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Figure 2.18 **Distributional impacts of microcredit on household non-food consumption: pooled quantile regressions with correlated random effects**

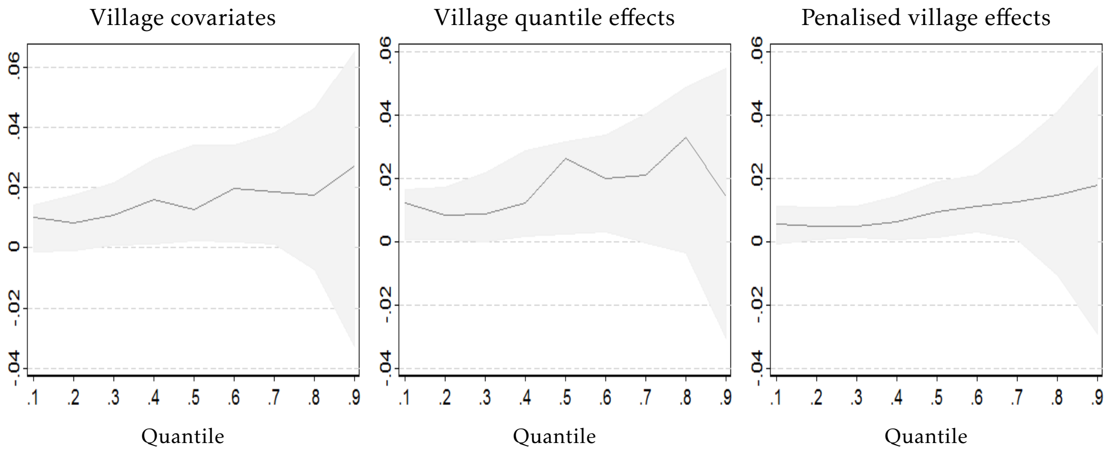
(a) Panel A: Household microcredit



(b) Panel B: Female microcredit



(c) Panel C: Male microcredit



Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling households with replacement to account for dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. All regressions include the time-averaged values of all household covariates (including credit variables) to specify household *correlated random effects*, following the method of Abrevaya and Dahl (2008). Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Table 2.16 **Distributional impacts of microcredit on household total expenditure, panel quantile regres random effects**

Quantile	Village covariates				Village quantile effects				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Household credit	**Female credit**	**Male credit**	**Female = Male (p-value)**	**Household credit**	**Female credit**	**Male credit**	**Female = Male (p-value)**	**Househ credi**
10	0.011**	0.013**	0.003	0.356	0.013**	0.015**	0.011	0.707	0.014*
	(0.005)	(0.005)	(0.011)		(0.005)	(0.006)	(0.008)		(0.004
20	0.016**	0.017**	0.006	0.297	0.016**	0.017**	0.011	0.540	0.017*
	(0.005)	(0.005)	(0.010)		(0.006)	(0.006)	(0.009)		(0.004
30	0.016**	0.016**	0.014	0.844	0.022**	0.022**	0.022*	0.972	0.020*
	(0.005)	(0.006)	(0.009)		(0.006)	(0.007)	(0.010)		(0.004
40	0.021**	0.020**	0.017*	0.831	0.030**	0.031**	0.031**	0.983	0.022*
	(0.006)	(0.007)	(0.011)		(0.007)	(0.008)	(0.010)		(0.004
50	0.024**	0.023**	0.023**	0.992	0.031**	0.033**	0.029**	0.754	0.026*
	(0.006)	(0.007)	(0.012)		(0.007)	(0.008)	(0.011)		(0.005
60	0.032**	0.032**	0.036**	0.726	0.032**	0.032**	0.028**	0.827	0.031*
	(0.007)	(0.007)	(0.013)		(0.007)	(0.008)	(0.014)		(0.006
70	0.036**	0.040**	0.049**	0.530	0.038**	0.037**	0.041**	0.819	0.034*
	(0.009)	(0.010)	(0.014)		(0.009)	(0.010)	(0.017)		(0.006
80	0.042**	0.040**	0.036*	0.860	0.039**	0.036**	0.040*	0.864	0.044*
	(0.013)	(0.014)	(0.020)		(0.012)	(0.013)	(0.022)		(0.008
90	0.061**	0.068**	0.032	0.382	0.042**	0.048**	0.037	0.722	0.058*
	(0.017)	(0.020)	(0.038)		(0.014)	(0.016)	(0.027)		(0.014
Coefficients equal across quantiles (p-value)	0.097	0.143	0.301	0.337	0.304	0.364	0.680	0.669	0.068
Coefficients jointly zero (p-value)	0.000	0.000	0.121	0.003	0.002	0.005	0.155	0.012	0.000

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns equality of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4), (8) and (12) show joint hypotheses test and male microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

Table 2.17 **Distributional impacts of microcredit on household food expenditure, panel quantile regres random effects**

Quantile	(1) Household credit	(2) Female credit	(3) Male credit	(4) Female = Male (p-value)	(5) Household credit	(6) Female credit	(7) Male credit	(8) Female = Male (p-value)	(9) Househ credi
	Village covariates				**Village quantile effects**				
10	0.006**	0.007**	-0.004	0.168	0.008**	0.008**	0.009	0.995	0.008
	(0.003)	(0.003)	(0.008)		(0.003)	(0.004)	(0.006)		(0.003
20	0.009**	0.010**	0.005	0.424	0.011**	0.011**	0.004	0.250	0.010
	(0.003)	(0.003)	(0.006)		(0.003)	(0.004)	(0.005)		(0.002
30	0.010**	0.011**	0.010**	0.956	0.012**	0.013**	0.008	0.385	0.011
	(0.003)	(0.003)	(0.004)		(0.003)	(0.004)	(0.005)		(0.002
40	0.010**	0.009**	0.009*	0.972	0.011**	0.013**	0.004	0.172	0.012
	(0.003)	(0.003)	(0.005)		(0.003)	(0.004)	(0.006)		(0.002
50	0.010**	0.010**	0.011*	0.812	0.012**	0.013**	0.004	0.206	0.012
	(0.004)	(0.004)	(0.006)		(0.004)	(0.004)	(0.007)		(0.003
60	0.014**	0.014**	0.011**	0.597	0.013**	0.015**	0.010	0.445	0.013
	(0.004)	(0.004)	(0.006)		(0.004)	(0.004)	(0.007)		(0.003
70	0.013**	0.013**	0.011**	0.857	0.013**	0.013**	0.010	0.780	0.014
	(0.004)	(0.004)	(0.006)		(0.004)	(0.004)	(0.007)		(0.003
80	0.015**	0.015**	0.013	0.800	0.010**	0.009**	0.012	0.802	0.011
	(0.004)	(0.005)	(0.008)		(0.004)	(0.004)	(0.009)		(0.003
90	0.011*	0.010*	0.012	0.883	0.010**	0.009**	0.007	0.877	0.014
	(0.005)	(0.006)	(0.015)		(0.004)	(0.004)	(0.015)		(0.004
Coefficients equal across quantiles (p-value)	0.625	0.707	0.855	0.913	0.945	0.785	0.871	0.919	0.939
Coefficients jointly zero (p-value)	0.004	0.006	0.379	0.025	0.017	0.015	0.584	0.065	0.000

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns equality of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4), (8) and (12) show joint hypotheses test and male microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

Table 2.18 **Distributional impacts of microcredit on household non-food expenditure, panel quantile re lated random effects**

Quantile	Village covariates (1) Household credit	(2) Female credit	(3) Male credit	(4) Female = Male (p-value)	Village quantile effects (5) Household credit	(6) Female credit	(7) Male credit	(8) Female = Male (p-value)	(9) Househ credi
10	0.004**	0.004**	0.010	0.151	0.007**	0.006**	0.012**	0.146	0.006
	(0.002)	(0.001)	(0.004)		(0.002)	(0.002)	(0.004)		(0.002
20	0.005**	0.005**	0.008*	0.475	0.009**	0.009**	0.008**	0.971	0.006
	(0.001)	(0.001)	(0.005)		(0.002)	(0.002)	(0.004)		(0.001
30	0.007**	0.006**	0.011**	0.381	0.009**	0.010**	0.009**	0.804	0.006
	(0.002)	(0.002)	(0.006)		(0.002)	(0.002)	(0.006)		(0.001
40	0.009**	0.009**	0.016**	0.370	0.010**	0.010**	0.012**	0.791	0.008
	(0.002)	(0.002)	(0.007)		(0.003)	(0.003)	(0.007)		(0.002
50	0.012**	0.012**	0.013**	0.876	0.014**	0.013**	0.026**	0.106	0.009
	(0.003)	(0.003)	(0.008)		(0.004)	(0.004)	(0.008)		(0.002
60	0.014**	0.014**	0.020**	0.495	0.016**	0.016**	0.020**	0.657	0.014
	(0.004)	(0.004)	(0.008)		(0.004)	(0.005)	(0.008)		(0.003
70	0.017**	0.018**	0.019**	0.920	0.019**	0.020**	0.021*	0.905	0.018
	(0.006)	(0.007)	(0.009)		(0.006)	(0.007)	(0.010)		(0.004
80	0.030**	0.035**	0.018	0.287	0.028**	0.028**	0.033*	0.760	0.026
	(0.009)	(0.011)	(0.013)		(0.009)	(0.011)	(0.013)		(0.006
90	0.040**	0.047**	0.027	0.506	0.040**	0.044**	0.014	0.233	0.046
	(0.015)	(0.019)	(0.025)		(0.013)	(0.014)	(0.021)		(0.011
Coefficients equal across quantiles (p-value)	0.131	0.057	0.590	0.153	0.230	0.218	0.054	0.081	0.012
Coefficients jointly zero (p-value)	0.002	0.002	0.155	0.005	0.000	0.000	0.002	0.000	0.000

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns equality of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4), (8) and (12) show joint hypotheses test and male microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

Chapter 3

Analysis of spillover effects from microfinance as social interactions

1 Introduction

To this day, existing empirical evidence on the efficiency of microfinance programs provide a rather mixed picture (Banerjee, 2013). Most studies have focussed on investigating the direct impacts of microcredit borrowings on its beneficiaries only. The reason for this is not only the relevance of measuring such impacts from a policy-making perspective, but also the necessity to impose strong statistical assumptions on empirical models to be able to actually recover average treatment effects (Imbens & Wooldridge, 2009). However, the importance of assessing potential indirect effects of such programs to the community as a whole has also been recognised as crucial in providing a comprehensive overview of the real impacts of development and poverty alleviation programs in developing countries (Angelucci & Di Maro, 2016; Philipson, 2000).

Some empirical studies have successfully shown that indirect program impacts are crucial to a better characterisation of treatment effects in the evaluation of policy interventions, as advocated by Angelucci and Di Maro (2016). Miguel and Kremer (2004) study a program providing de-worming drugs in Kenya and show children from non-targeted areas to benefit greatly from it. Another example is the study of PROGRESA, a program in rural Mexico consisting of cash tranfers to poor households conditional on good health and education practices. Bobonis and Finan (2009) and Lalive and Cattaneo (2009) unravelled positive indirect impacts from the program on the education of children from non-target households, while Angelucci and De Giorgi (2009) show that the initiative also triggered increases in the consumption of ineligible households.

Few studies have attempted to investigate the potential spillover effects stemming from microfinance initiatives, and the ones that do mostly focus on equilibrium effects on local markets (see e.g. Demont, 2016) or even on general equilibrium effects (Buera, Kaboski, & Shin, 2012). The present chapter endeavours to establish whether spillover effects from microfinance can work through other channels, and especially through social interactions.

The reason to believe that might be the case is that rural communities of developing countries (allegedly prime targets for microfinance initiatives) are typically very interconnected and display a high degree of solidarity. Networks of extended families, friends and neighbours can play a crucial role in the diffusion of information (Wydick, Karp Hayes, & Hilliker Kempf, 2011) and in the adoption of new technology (Conley & Udry, 2010). But the most important feature of this cooperation is that it also aims to fill the gap left by the lack of access to traditional financial services and risk-coping mechanisms, resulting in highly efficient informal insurance networks Rosenzweig (1988a); Townsend (1994); Udry (1994).

In such a context, it is legitimate to speculate that such a shock as the introduction of a microfinance program would impact the community as a whole. Availability of extra resources in the form of credit can influence and recast pre-existing informal networks and impact not only direct beneficiaries of loans, but also the people who interact with them.

The aim of this chapter is twofold. First, we want to establish whether microfinance programs generate welfare gains (or losses) for the local population as a whole, and more precisely at the village-level. Second, we try to determine whether the source (or one of the sources) of such indirect impacts is the existence of social interactions between beneficiaries and the non-target population. We focus on household per capita expenditure as a proxy for household welfare, and also on children education. The choice of the latter relates to the fact that previous studies using similar have advertised positive outcomes from microcredit in terms of schooling (Chemin, 2008; Pitt & Khandker, 1998), and because indirect impacts on education from development policies have been shown to arise in some cases, for instance for PROGRESA in Mexico.

Th next section reviews the existing literature in order to clarify the potential sources of spillover effects in policy interventions, and how they are relevant to the context of microfinance programs. Section 3 briefly describes the data and provides a detailed account of our empirical methodology used to recover direct program impacts and community-level indirect effects. We estimate well-defined positive welfare impacts from microcredit for borrowers and the community as a whole in terms of consumption, while results for children education are seldom significant. Finally, Section 4 presents our empirical strategy to investigate social interactions as a potential source of indirect impacts in microfinance programs, and Section 5 concludes.

2 Literature review

2.1 A first glance at spillover effects in microfinance programs: equilibrium effects

The incompleteness of markets for credit and other financial services makes the core reason why microfinance initiatives, with lending methods that differ from existing formal and informal institutions, fulfil part of the unmet demand for finance in rural and poor urban areas of developing countries (Morduch, 1999a). By delegating screening and monitoring costs onto borrowers (Madajewicz, 2011), classic microcredit contracts of group loans with joint-liability effectively mitigate adverse selection and moral hazard issues that are too salient for traditional banks to lend to the indigent, and ultimately improve borrowers' welfare (Ghatak & Guinnane, 1999; Stiglitz, 1990).

When microfinance or traditional banking services are not accessible, households rely heavily on informal sources for the provision of financial services[1]. Generally, local moneylenders are called upon in order to obtain investment funds that typically carry very high interest rates because of their somewhat monopolistic situation (see Hoff & Stiglitz, 1998, for a theoretical treatment), while social networks embedding the extended family, friends and neighbours supply insurance in the face of adverse circumstances (Fafchamps & Lund, 2003; Rosenzweig, 1988a; Udry, 1994).

A legitimate enquiry is therefore to assess how the presence of new actors - namely microfinance institutions (MFIs) - amongst existing formal and informal financial institutions might change market conditions. Indeed, one goal of MFIs is to directly compete with and ultimately crowd out moneylenders who, in their close to monopolistic position, are often seen to exhibit exploitative behaviours towards the credit-constrained indigent (Armendáriz & Morduch, 2010). Notwithstanding some level of crowding out, as Disney, Fichera, and Owens (2013) show to be the case in rural Malawi where members of microfinance programs borrow substantially smaller amounts from informal lenders, empirical evidence reveals that the various types of lenders actually continue to co-exist even after the inception of microcredit programs.

For instance, Islam (2015) use data collected in rural Bangladesh to show that if access to microcredit does indeed make borrowing from informal sources less frequent, the size of informal loans taken up by MFI borrowers remains unchanged. This phenomenon is due to the existing complementarity of various credit sources which is further documented in Jia, Luan, Huang, and Li (2015) about credit in rural China where

[1] Conning and Udry (2007) provide a comprehensive overview the main characteristics of rural financial markets.

microfinance loans are used for livestock and non-agricultural investment, while formal credit is typically used to fund crop production, and informal networks are relied on for consumption purposes[2]. Giné (2011) proposes that the co-existence of formal and informal credit sources is due to comparable low transaction costs for accessing informal credit and loans from formal sources alike. The limited ability of formal credit institutions to enforce contracts is the main reason why the latter sometimes ration credit for risky projects, which in turn allows the more flexible informal credit sources to survive an expansion of access to formal credit (Giné, 2011).

In sum, different providers of funds meet different needs. Nevertheless, the aforementioned evidence suggest that the entrance of MFIs on incomplete financial markets does disturb competition amongst lenders. The consequential changes on both the supply and demand sides are bound to have an impact on prices. Indeed, exploiting data from two surveys in Bangladesh, Berg, Emran, and Shilpi (2013) find that MFI members borrow less often from moneylenders and that informal interest rates increase in reaction to the inception of microfinance programs. The latter observation is corroborated by Mallick (2012) in an empirical study showing that moneylender interest rates are generally higher in villages with greater coverage by microfinance programs and where loans are mainly used for productive purposes. On the other hand, Venittelli (2017) claims that microfinance borrowers in Andra Pradesh, India, actually enjoy lower interest rates from moneylenders as a positive externality to their engagement in productive activities financed through group-lending or Self-Help Groups, which make them appear less risky in the eyes of informal lenders.

So, access to microfinance substantially modifies market conditions especially through its impact on the price of informal credit, and such general equilibrium effects can be one channel for the transmission of spillover effects from policy interventions (Angelucci & Di Maro, 2016). This approach is followed by Demont (2016) in a theoretical model showing that when moneylenders serve some of the safe borrowers in the absence of MFI, the entrance of the latter can push up informal interest rates and deteriorate outreach to creditworthy borrowers. Theoretical results are comforted by empirical evidence from India showing that individuals who were borrowing from moneylenders are less likely to do so after the entry of MFIs, and that moneylenders tend to charge higher interest rates where MFIs are present. Demont (2016) concludes that formal financial institutions are harmful to existing traditional ones and that, as a consequence, the vulnerability of worst-off individuals to adverse shocks increases.

In an attempt to assess general equilibrium effects generated by microfinance programs, some models analyse such initiatives from a more aggregate point of view,

[2] Dalla Pellegrina (2011) shows that microcredit in rural Bangladesh tends to be channelled towards investment in non-agricultural activities, whereas loans from traditional banks and informal sources typically support agricultural investment.

which usually allows to draw conclusions on the distributional implications of microfinance. For instance, Batbekh and Blackburn (2008) work with a Dynamic General Equilibrium model where two-period-lived agents with intergenerational ties have a choice between subsistence production, a small- and a large-scale investment projects. They show that traditional banks lend only for the large-scale investment project and are unable to correctly enforce loan contracts, therefore they ration credit and serve only those individuals with a minimum initial wealth, implying that some individuals cannot achieve their optimal occupation choice: only those who have at least the minimum amount of wealth can borrow, and their lineage definitely exits subsistence production while the others remain, i.e. initial inequalities persist. MFIs are modelled as offering unsecured loans to fund the small-scale project, and their presence results in this project becoming accessible and attractive such that more individuals access their optimal occupation choice and success in exiting subsistence production. The consequences of introducing microfinance in that setting in terms of income distribution are that poverty and inequalities are lower for the population as a whole, even though some poverty persists.

In Buera et al. (2012) the effects of microfinance as an economy-wide program are also investigated in a General Equilibrium (GE) framework. Here microfinance is considered as being regardless of initial wealth or collateral and is included in the model as an innovation in access to capital for a pool of talented entrepreneurs or future entrepreneurs, the others engaging in waged labour. Partial Equilibrium (PE) estimates tend to show that entrepreneurs benefit a lot from the innovation of microfinance and "basic" workers almost benefit nothing; understand PE as being the analogy to the usual empirical studies we encounter on microfinance. However, their main finding is that wages increase in GE, implying strong redistributive effects from entrepreneurs towards "less talented" people. This study gives valuable insight in line with the argument that in some parts of the world microfinance has developed so much that it might have reached the "critical" size where GE effects appear. Indeed, exploiting the 'natural experiment' setting offered by the ordinance issued in October 2010 in the state of Andra Pradesh, India – that saw the sudden ceasing of all microfinance activities in that state and created an economy-wide liquidity shock for lenders – Breza and Kinnan (2018) confirm the existence of positive externalities from microfinance on the welfare of non-borrowers through increased wages in GE.

The above discussion sheds light on the search for the equilibrium effects of microfinance programs. Access to microcredit tends to push up informal interest rates without however completely crowding out informal sources of credit. This change in the price of informal credit can have repercussions on any user of such sources of funds, and hence on microfinance participants and non-participants alike. Similarly, an economy-wide boost in productive investment triggered by the availability of microcredit can generate substantial increases in wages and subsequently create gains for the non-borrowing population thanks to larger earnings.

Unfortunately, the actual welfare implications of such general equilibrium effects are hardly assessed in the literature presented so far, but are rather assumed as logical consequences of empirical facts. We recognise the great difficulty of formally linking indirect impact of microfinance on informal interest rates or wages to other outcomes of interest such as household consumption or children schooling. We believe that the potential spillover effects of microfinance on such outcomes that are commonly used proxies for household welfare might be more easily quantified when said indirect effects play through the channel of social interactions, which is what we discuss next.

2.2 Spillover effects through social interactions

An empirical study by Flory (2011) exploits a natural experiment to assess the impact of an exogenous information campaign on the adoption of formal savings accounts in rural Malawi. The investigation shows that the intervention has a positive and significant effect on the proportion of former non-users of formal savings accounts (at the time of the baseline survey) who adopt such practices, the effect being stronger in more remote areas. The author then proceeds to show that as a consequence of the intervention the most vulnerable part of the population is more likely to receive inter-household transfers in treated locations. Thanks to the information campaign and its effects on the adoption of formal savings, the most vulnerable individuals also seem to experience better welfare outcomes such as a higher probability of exiting severe food insecurity and of reporting fewer instances of injury or sickness. The rationale for the interpretation of the results is that indirect treatment effects stem from the existence of informal insurance mechanisms that work through inter-household wealth flows.

In our view, such spillover effects stemming from social interactions are crucial in the context of microfinance programs. It is true that our study focusses on microcredit while that of Flory (2011) considers access to savings accounts, but we recognise the similarity in interventions that broadly speaking seek to provide financial services to the poor. Moreover, most institutional microcredit schemes also come with savings requirements, and the latter are also at the core of other widespread forms of microfinance in the developing world such as Self-Helf Groups and ROSCAs (Armendáriz & Morduch, 2010).

The existence of strong social ties in rural communities in developing countries has been extensively documented and is typically viewed as crucial for the provision of informal insurance in contexts where financial markets are incomplete. Some instances are Rosenzweig (1988a, 1988b) that show the importance of the family as a prime risk mitigation institution in rural India, Udry (1994) that documents bilateral informal loans with state-contingent repayments as the preferred risk-pooling device in Northern Nigeria, and Fafchamps and Lund (2003) that find risk-sharing, although not efficient at the village level, works primarily through family and friends networks. In sum, households who live in the poorest areas of the world consistently demonstrate solidarity in the face of hardship. It is therefore natural to think that radical changes in the financial environment of such communities, be it access to credit or savings accounts, can interact with pre-existing social networks of which one of the main purposes is insurance.

Building on the premise of the existence of risk-sharing networks in rural villages in Mexico, Angelucci and De Giorgi (2009) use a rich dataset to estimate the indirect effects of the famous Mexican program PROGRESA (renamed Oportunidades) on those households that are not eligible to the program. PROGRESA started in 1997 and consists of cash transfers to poor households in rural Mexico conditional on good health and schooling practices. Given the nature of the program implementation and the random selection of villages in the three waves of data under scrutiny, indirect treatment effects can be estimated by simply comparing the mean observed outcomes of ineligible households in treatment and control villages in the fashion discussed in Angelucci and Di Maro (2016).

The authors first investigate the idea that ineligible households in treatment villages should experience higher consumption, as a consequence of risk-sharing agreements anterior to the intervention. Indeed, albeit the full risk-pooling hypothesis is typically rejected at the village level in low-income countries (see Fafchamps & Lund, 2003; Udry, 1994, among others), Townsend (1994) shows such a model nonetheless provides a good benchmark of household consumption behaviours. Findings indicate the monthly food consumption per adult equivalent of ineligible households in treatment villages is indeed positively and significantly impacted, and also that nonpoor households receive more net transfers and loans (this last result is not very reliable given the poor quality of the data). As the researchers rule out alternative channels of transmission such as the labour or the goods markets, it seems that increased transfers to the ineligible would indeed be at the origin of positive spillover effects taking the form of increased consumption and supposedly greater insurance.

Obviously, PROGRESA differs in nature from microcredit initiatives. Nonetheless, we feel the above findings are relevant to the argument that indirect treatment effects might be expected from microfinance programs through similar channels of social interactions, to the extent that alleviating credit constraints radically modifies the amount of resources available to some part of the population, in a way comparable to conditional cash transfers that can be seen as positive income shocks to eligible households in treatment villages.

The simple estimator used in Angelucci and De Giorgi (2009) can easily be extended to accommodate conditioning variables and panel data, which is akin to estimating the intention-to-treat effects on the non-treated with a difference-in-differences estimator. This is the approach taken in Islam (2015) to investigate consumption spillovers from microfinance to ineligible households in rural Bangladesh, even though the results (discussed but not presented in the paper) suggest there are none. The potential existence of spillover effects from microcredit is also explored in Khandker (2005) , a follow-up paper of the pioneering study by Pitt and Khandker (1998). The author exploits panel data to estimate village-level overall indirect program effects that might accrue to borrowers or non-borrowers alike, and obtains conclusive evidence that the average level of borrowing by women in a village has a significant and positive impact on household expenditures.

The use of average borrowings in a village as an additional regressor resembles the methodology employed by Bobonis and Finan (2009) and Lalive and Cattaneo (2009) to study the importance of peer effects in the schooling of children from non-poor households living in areas where PROGRESA educational grants are available. Because the program is offered only to a fraction of the population (poor households) in randomly selected villages for which information on all inhabitants are available, the PROGRESA evaluation data offers a partial-population experiment (Moffitt, 2001) which allows the authors to use the proportion of children from treated households in a program-ineligible child's reference group as an instrument to disentangle contextual and endogenous social effects. Results from both papers show a positive indirect effect of the program on schooling levels of children from ineligible households through the influence of their grant-receiving peers' behaviour. The unlocking of investment opportunities thanks to microcredit programs is often presented as the key to trigger an overall improvement in the livelihoods of the poor, with impacts on income and consumption ultimately rippling to better health and education outcomes. If microfinance indeed effectively promotes children's schooling, the aforementioned research points to yet another area where to expect potential positive spillovers from microcredit.

To a similar extent, if one considers the idea that household welfare not only depends on absolute levels of income or consumption, but also on the relative value of such outcomes , then contextual effects could play a crucial role in the potential indirect effects of microfinance programs. Indeed, recent research has asserted the prevalence of status-seeking behaviours through spending on visible or conspicuous goods even amongst the poorest households of developing countries (see for instance Bloch et al. (2004) or Brown et al. (2011)). Extra resources made available thanks to microcredit might be diverted towards such expenses, and in a context where households value social status one could expect this behaviour to impact the socail welfare of all households in the community.

The latter class of spillover effects also relates, although not strictly equivalent, to what Angelucci and De Giorgi (2009) coin context equilibrium effects, which arise from the impact of a program on the social norms prevailing in the relevant unit of study for such interactions. The influence of social networks on existing behaviours is for instance famously recognised in Conley and Udry (2010) who show that farmers in Ghana are more likely to adopt a new technology when they witness the success of their 'information neighbours' who already did so (e.g. other farmers with neighboring crops).

In the context of microfinance, the dissemination of information through informal netwroks is crucial for the uptake of credit and has an especially large impact on community members' awareness of newly available microcredit sources (Okten & Osili, 2004). While Wydick et al. (2011) suggest that adoption of microfinance amongst members of the same church in rural Guatemala is partially driven by endogenous peer effects (i.e. people tend to behave similarly to others in their reference group), findings by Banerjee, Chandrasekhar, Duflo, and Jackson (2013) propose that participation is actually mostly driven by one's level of 'informedness' about accessible MFIs rather than by her following social norms. The authors use network data to show that the greater the 'centrality' of the injection point to relay information about MFIs, the higher the rate of participation in microfinance programs in a village - i.e. information diffuses more effectively when its source is an important member of the community, e.g. the chief or the doctor of the village. Interestingly, Banerjee at al. (2012) also reveal that even *non-participants* to microfinance do provide a substantial share of information spread through the network. This last point suggests that the relevant spheres for the study of social interactions in the context of microfinance are probably broader than those provided by a simple dichotomy between program participants and non-participants.

To sum up, social interactions play a central role in spreading awareness about available microcredit sources and how they can help, and also in influencing actual uptake of microfinance loans. We want to investigate whether such social interactions can generate spillover effects from microfinance programs on outcomes related to household welfare, such as consumption and education, as is the case for the Mexican conditional cash transfers program PROGRESA.

Because our dataset does not embed information on social networks nor on any variable that could be used to construct a social distance matrix, we cannot draw on modern approaches of social network analysis as in Banerjee et al. (2013) and must therefore resort to empirical models of social interactions. The complexity of estimating the importance of various types of social interactions on an individual's behaviour was famously discussed in the seminal paper by Manski (1993) that coined the phrase "reflection problem". We turn to these issues in a later section when presenting our methodology, and draw on the papers by Bobonis and Finan (2009) and Lalive and Cattaneo (2009), among others, to design an empirical strategy allowing us to investigate the indirect effects of microfinance programs through social interactions.

The rest of the paper is organised as follows. In the next section, after presenting the data we follow a strategy similar to Khandker (2005) to simply study the direct impact of microcredit on consumption and also to take a first look at the potential existence of spillover effects. We repeat the earlier study not in an attempt at replicating it, but rather because the subsequent study of spillover effects is more meaningful in light of baseline estimates form a typical impact evaluation. Moreover, we also consider chidren education as an outcome of interest, which was not done in previous studies on panel data similar to ours. We briefly compare our results to Khandker (2005) because our empirical model is slightly different and also because we implement some alternative approaches considered in the replication by Roodman and Morduch (2009). In section 4 we draw on studies of PROGRESA and the large literature on the econometrics of social interactions to devise an empirical strategy that allows us to disentangle contextual and peer effects and their respective contributions to the spillover effects of microcredit.

3 Impacts of microcredit on borrowing households and preliminary exploration of spillover effects

3.1 Some features of the data

3.1.1 Data collection

A rich dataset was created by the Bangladesh Institute for Development Studies (BIDS) and the World Bank in 1991-1992. It is a pool of surveys of 1,769 households in 87 randomly selected villages in a set of 29 rural sub-districts (upazillas) randomly chosen among the 391 Bangladesh counts. The interviews were undertaken in three rounds corresponding to the main rice seasons, the first round taking place between November 1991 and February 1992, the second one from March to June 1992 and the last one during the period July-October 1992. The original sample consists in surveys of just over 20 households in each one of three villages picked randomly from each subdistrict, and suffers very low attrition as only 29 households were not surveyed the three times. The control group is formed by the villages in five sub-districts that did not receive any microfinance program.

Credit programs from three providers of different types were evaluated: the Grameen Bank, the Bangladesh Rural Advancement Committee (BRAC) and the Bangladesh Rural Development Bank's RD-12 program. These MFIs targeted these programs towards the landless. As long as they meet the eligibility requirement of owning half an acre of land or less, people in treated villages can self-select into single-sex groups in order to get a loan; 10 villages have only male borrowing groups, 22 had only female groups and 40 had both.

The data collected contain a large set of variables, including socio-economic aspects, credit history of the households and qualitative information. Collection was implemented in the spirit of a quasi-experiment in which credit uptake is left to self-selection and where program placement is non-random (only the selection of villages within areas where the programs are effective is random). This design introduces potential statistical biases that need to be accounted for in the empirical strategy, and makes the case of how crucial the chosen methodology is in social program impact assessments. A follow-up survey was administered in 1998-1999 to the same households as well as to an extended sample of new households from the same villages, new villages from the original thanas, as well as 3 additional thanas, for a total of 2,599 households.

Our empirical strategy seeks to exploit the "time" feature of the data and therefore makes use of a balanced panel structure, i.e. we keep in the sample only those 1,638 households that were successfully interviewed across all waves of data collection. We follow Khandker (2005) and merge the 1998-99 data for those households that had split since 1991-92, and combine data across the three first rounds of interviews in order to have 2 observations per household: one in 1991-92, and one in 1998-99. Therefore, discussions on sampling and descriptive statistics consider this balanced sample only, unless indicated otherwise.

3.1.2 Program eligiblity and mis-targeting

Households were randomly sampled within villages according to their eligibility status to the microfinance initiatives under scrutiny in 1991-92, i.e. Grameen Bank, BRAC and BRDB RD-12. Around 20 households were sampled from each village, and eligible households were oversampled compared to ineligible households (around 85% of households sampled in each village are program-eligible), as is customary in impact evaluation studies that seek to maximize the statistical power of program effects estimates for a lower cost. Weights to be used in the analysis are derived from this sampling procedure. The final sample considered in this paper embeds close to 19 households per village on average, with a minimum (maximum) of 13 (23) observations, for a total of 1,638 households.

According to the initial sampling information, as of 1991-92 our dataset includes 824 eligible households that borrowed through microfinance programs ("treated" target households), 567 eligible households that did not participate to microcredit initiatives ("non-treated" target households), and 247 households not eligible to microcredit borrowing (non-target households). However, according to individual-level loan data, only 55.8% of those eligible households did borrow from either Grameen Bank, BRDB or BRAC, i.e. the subsample of participating target households is actually made of 761 households instead of 824 as stated by census data. According to Pitt and Khandker (1998), at the time of data collection the three MFIs under scrutiny followed a similar rule which deemed a household eligible to borrowing if their landholdings were no greater than half an acre (50 decimals). This should be consistent across household-level data and the available census data on which sampling was designed. However, a closer look at the data shows the eligibility status of households varies depending on the adopted definition, which unravels a substantial amount of mis-targeting in our sample, i.e. some allegedly landed and ineligible households do in fact borrow. Please refer to section 2 of Chapter 1 of this thesis for details.

3.2 Empirical strategy for program impact estimates

3.2.1 Continuous outcome

We estimate the impact of microcredit on three consumption measures: household per capita annual total, food and non-food expenditures, expressed in 1992 Taka for both waves of data. Our empirical framework closely follows that of Khandker (2005). We consider the following outcome equation:

$$Y_{ijt} = \phi_t + C_{ijt}\delta + X_{ijt}\gamma + \alpha_{ij} + \eta_j + \epsilon_{ijt} \tag{3.1}$$

where Y_{ijt} is the outcome realised by household i in village j at time t, and depends on contemporaneous values of covariates X and household microcredit borrowings C; ϕ_t is a time effect and is included in every regression, so we omit it from equations whenever possible. Household and village unobserved components that influence outcome Y α_{ij} and η_j, respectively enter model (3.1) additively and are assumed to be time-invariant. Khandker (2005) also defines a credit demand equation:

$$C_{ijt} = X_{ijt}\lambda + \alpha^c_{ij} + \eta^c_j + \epsilon^c_{ijt} \tag{3.2}$$

where superscript c signifies household and village time-invariant characteristics and the error term belong to the credit equation. In a panel data setting the introduction of household-level fixed effects can remove household and village-level endogeneity if the source of the latter are unobserved attributes that are time-invariant (Khandker, 2005). In practice, fixed effects estimates of the coefficients in model (3.1) are obtained through performing OLS on the first-differenced equation, which will be our baseline results.

Consistency of such fixed effects estimates relies on the assumption that the effects of household and village unobserved heterogeneity do not change over time. While we control for observed village features (which can vary over time) in our baseline regression, the potential existence of time-varying impacts of village-level characteristics unknown to the econometrician can be easily accommodated by introducing a set of village dummy variables in the regression equation after taking first differences. This new specification controls for the change in impact *over time* of *time-invariant* village-level unobserved heterogeneity, so that village covariates remain in the set of regressors as providers of information on *time-varying* village heterogeneity. We follow this method to obtain our second set of results .

Time-varying impacts of unobserved heterogeneity at the household-level are more problematic because they cannot be addressed in the aforementioned fashion. Indeed, we have only two observations per households, so re-inserting household dummy variables after first-differencing the estimation equation would result in an incidental parameter problem. Khandker (2005) suggests using an IV approach to try and deal with measurement error and potential time-varying heterogeneity and proposes to use the same instruments as in Pitt and Khandker (1998). The candidate instrument is a household *choice* variable (c) defined as the interaction of two dummy variables: the eligibility status of the household to microfinance loans (e) and the availability of microfinance programs in a village (p). All exogenous covariates are then interacted with c to create the set of instruments. Pitt and Khandker (1998) and Khandker (2005) classify households as eligible when they hold less than half an acre of land as per the alleged eligibility criterion followed by MFIs in the sample.

Because of the occurrence of mis-targeting mentioned earlier, the authors also reclassify ineligible borrowers as eligible in order to ascertain they are not excluded from the instrumenting equation. This latter ad hoc manipulation brings Roodman and Morduch (2009, 2014) to contest the validity of this strategy, arguing that the quasi-experiment setting offered by the exogenous variation in eligibility status around the cut-off point on landholdings is inadequately exploited. Moreover they warn about the risk of overfitting in the first stage given the multitude of instruments created by the interaction of the household choice variable with all covariates and village dummy variables.

Well aware of the issues raised so far, we choose to implement nonetheless a similar IV approach as it is, with this dataset, our only option to relax the assumption of time-invariant household unobserved heterogeneity. We use two different measures of eligibility to define our instruments, one based on census data (survey data for the second wave) and the other based on household landholdings. In each case we estimate the model with household fixed effects, household and village-level control variables, as well as time-interacted village unobserved effects in the form of village dummy variables that enter the second estimation stage after differencing other variables.

Finally, in every regression with cumulative microcredit borrowings as the treatment variable we also allow for impacts to differ depending on the gender of borrowers, and hence our specifications include in turn household total borrowings or two credit variables, one for women's borrowings and one for men's. Note that in the case of gender-based credit with use the same set of instruments for both variables. Indeed, when data collection started microcredit was offered to single-sex groups, with 10 villages having only male borrowing groups, 22 with only female groups and 40 with both. Such indicators of program placement could have helped us refine the intrumentation stage, but unfortunately this information is not available in the dataset made avilable by the World Bank. David Roodman does provide (on his webpage at the Center for Global Development) a dataset used in the replication of Pitt and Khandker (1998) that includes dummy variables identifying villages with male-only or female-only borrowing groups, however we choose not to use them because they conflict with our data (i.e. we find several occurrences of, for instance, men who borrow in villages with women-only groups) and are not available for the second wave of data.

3.2.2 Remark on logarithmic transformation

Previous studies by Pitt and Khandker (1998) and Khandker (2005) carry out regressions where consumption measures and explanatory variables of interest, i.e. microcredit borrowings, enter in logarithmic form. While transforming the dependent variable is motivated by trying to limit the influence of outliers and to "normalise" its distribution with the hope that residuals will then be better behaved, taking the logarithm of our explanatory variable of interest is rather a matter of interpretability of the results. Indeed, coefficients estimates from so-called 'log-log' regressions have a direct economic meaning as elasticities.

Roodman and Morduch (2014) debate the choice of the censoring value for credit variables in Pitt and Khandker (1998) and argue that imputing 0 when no borrowing occurs implicitly allocates 1 Taka of credit to non-borrowers (as $\ln(1) = 0$). As a consequence, the authors state that "moving from non-borrowing status, proxied by 1 taka, to minimal borrowing status 1,000 taka has the same proportional impact as moving from 1,000 to 1,000,000 taka of borrowing". In order to circumvent this conundrum they propose to enter an additional regressor alongside the log-transformed credit variable, namely a dummy variable equal to one if any borrowing occurs, which then renders the arbitrary choice for the censoring value harmless. However they acknowledge that available instruments are most likely insufficient to model both the decision to borrow and borrowing amounts as endogenous variables.

We choose to maintain the 'log-log' specification used in previous studies in order to produce directly interpretable estimates. The minimum amount for a loan observed in our data is 1,000 Taka, so we follow Roodman and Morduch (2009) in their replication of Khandker (2005) and input ln(1,000) as the value of microcredit variables for non-borrowing households[3].

We anticipate slightly on forthcoming sub-section 3.4 where we use the logarithms of village average and aggregate microcredit borrowings to have a first look at spillover effects. While we find microfinance programs and participants in every sampled village for the second wave of data, control villages in 1991-92 have no active programs and hence no occurrence of borrowing, positing again the problem of the logarithm of zero for which we choose to input the logarithm of the minimal observed amount of village average or aggregate credit. To draw a parallel with the previous situation, the occurrence of loan uptake in villages is a matter of program placement. As explained above, household fixed effects and time-varying village unobserved effects handle potential non-randomness in the latter, thereby mimicking the fix proposed by Roodman and Morduch (2014).

3.2.3 The case of schooling

We use two measures of children schooling as the outcome of interest: a child-level binary variable with a value of one if the subject is currently enrolled in school, and the proportion of children in the household who are currently enrolled in school. We define school enrolment for children of age 5 to 18 and for boys and girls separately, in line with previous studies using the same data (Chemin, 2008; Morduch, 1998; Pitt & Khandker, 1998).

Ideally, binary or fractional outcomes are modelled through probit or logit regressions. One challenge is then to address unobserved household-level heterogeneity, even if it is time-invariant, because the non-linearity of the link functions used in the probit and logit estimators do not allow differentiation to remove the fixed effects. The "brute force" method consisting of a mere inclusion of household dummy variables is not an option given the shortness of our panel (only two time periods) as it would result in heavily biased estimates, a consequence of the incidental parameter problem (see e.g. Greene, 2004a).

One option is to run a probit regression with random effects, at the cost of assuming strict exogeneity and also that unobserved household heterogeneity is uncorrelated to the regressors, which seems too restrictive in our case. The latter assumption can possibly be relaxed by the inclusion of time-averaged household-level variables in the spirit of a correlated random effects model *à la* Mundlak (1978) and Chamberlain (1980), as suggested by Wooldridge (2010b)[4].

[3] All monetary values are deflated and expressed in 1992 Taka.
[4] See Section 15.8 in Chapter 15 of his book.

Another possibility is offered by the form of the logistic link function that allows one to leave the relationship between unobserved effects and regressors unrestricted. However, a dummy variables approach will still yield biased estimates (Coupé, 2005; Katz, 2001), so the likelihood needs to be conditioned on those observations for which outcome changes over time, which can result in a significant share of observations being dropped from the regression.

We follow the approach proposed in Papke and Wooldridge (2008) and implement probit regression on pooled data while specifying a linear functional form for the relationship between unobserved household-level heterogeneity and the regressors in a Mundlak-Chamberlain fashion, i.e. by adding time-averaged values of household covariates as extra regressors. Standard errors clustered at the household level also offer heteroskedasticity-robust inference. There remains the issue that nonlinear estimators are sensitive to the number of covariates because each parameter is considered a quantity to be estimated (Greene, 2004a, 2004b), hence limiting our ability to control for time-varying effects of time-invariant unobserved village heterogeneity. Therefore, our pooled probit regressions include either village controls (observed time-varying heterogeneity) or village dummy variables (time-constant unobserved heterogeneity).

Albeit valid for the case of a binary outcome, the empirical methodology of Papke and Wooldridge (2008) is actually proposed in a fractional probit framework and therefore is also applicable in the case where schooling is measured at the household-level by the proportion of children currently schooled. They devise a two-step control function approach in the spirit of Heckman to deal with endogneous regressors where residuals produced from a linear first-stage are directly included in the second stage alongside the original endogenous variables.

Consider including our set of instruments Z in the credit equation (3.2):

$$C_{ijt} = X_{ijt}\lambda + Z_{ijt}\beta + \alpha_{ij}^{c} + \eta_{j}^{c} + \nu_{ijt} \tag{3.3}$$

Equation 3.3 is estimated via OLS and we recover predicted residuals $\hat{\nu}$ that are plugged in as an extra regressor in a second stage regression equation of the form of (3.1). This method also provides a straightforward test of exogeneity of the problematic regressors, i.e. the estimated coefficient attached to first-stage residuals will be statistically significant if there is endogeneity. As a consequence of using two-stage estimation, we carry out 1,000 bootstrap replications to produce standard errors that are fit for inference by re-sampling clusters (i.e. households). Note that we use this control function approach for both binary probit and fractional probit.

Given that our focus is primarily on the sign and significance of the estimated effects with no intent to make predictions, we complement our results from nonlinear models by the simpler linear probability model and run regressions following the same empirical strategy as that implemented when consumption is the outcome of interest. The linear-in-means framework allows for flexible control over unobserved effects while avoiding the incidental parameter problem, and for a straightforward application of the IV approach described in Section 3.2 which should still provide economically meaningful estimates even if the underlying relationship is actually non-linear (Angrist & Krueger, 2001).

Finally, note that the nature of our educational outcome variables forces us to consider only the units for whom they are relevant, namely those households with girls age 5-18 and those with boys age 5-18, depending on the dependent variable. As a consequence, our estimation sample is an unbalanced panel dataset because, as could be expected, household composition changes over the seven years separating our two waves of data. With that in mind, one important drawback of the correlated random effects approach is that cross-section units with only one observation are ignored (Bluhm, 2013; Wooldridge, 2010a).

There is no way around this conundrum if we want to account for household unobserved heterogeneity while adequately modelling a binary or fractional outcome. Actually, controlling for fixed effects in a linear model for panel data on an unbalanced sample with only two observations per cross-section at most would result in singletons having no explanatory power. It is therefore common practice to run linear panel data estimation with fixed effects on the complete unbalanced sample and let the statistical software deal with singletons, given that coefficient estimates on independent variables are unaffected. However, inference is influenced by the presence of singletons as it tends to yield smaller standard errors and hence improve statistical significance, as explained in a short manuscript by Correia (2015). We were also able to observe this very phenomenon when running preliminary regressions. Therefore, we always use the balanced sample when studying schooling outcomes, irrespective of the method.

To sum up, we model schooling for boys and girls separately as either binary or fractional. In each case we select households with observations for both time periods to get balanced panel data in order to implement pooled probit regressions with Mundlak-Chamberlain correlated random effects[5]. Even after selection, child-level binary outcome leave enough observations to include village dummy variables as an alternative to village covariates, although we do not include both together in order to limit the number of parameters to estimate.

We follow Papke and Wooldridge (2008) and use pooled fractional probit regressions with correlated random effects to model household-level schooling outcomes, however we control only for village observed heterogeneity because the balanced panel severely

[5] In the case of schooling as a binary outcome, the estimation sample is balanced at the household level, i.e. there is not necessarily the same number of children of age 5 to 18 in one household in both time periods. Correlated random effects are modelled at the household-level, so this is the relevant unit on which to balance the panel.

limits degrees of freedom. We apply their control function approach to try and account for the potential endogeneity of credit variables in the pooled fractional probit model as well as in the binary outcome case, using 1,000 bootstrap replications for inference (re-sampling households with replacement).

Finally, we complement our set of results with OLS fixed effects and 2SLS for panel data. These estimators can be used with both outcomes and allow us to control for household-level time-invariant heterogeneity as well as fixed village level unobserved factors whose impact can change over time. Note that when instrumenting for credit variables the first stage is always linear, irrespective of the estimator used in the second-stage. Therefore we use the same instrumentation strategy as that used in the case of continuous outcomes where both stages are linear, i.e. we interact the household *choice* variable with all household and village covariates as well as with the full set of village dummy variables to produce instruments[6].

3.2.4 Descriptive statistics for outcome and credit variables

Credit and consumption variables are described extensively in Chapter 1 of the thesis. Specifically, the distributions of the various expenditure variables are carefully explored and their analysis reveals that these variables are heavily skewed to the right and exhibit a peak around their mode. This is the case particularly for total and non-food expenditure, the lumpy nature of the latter influencing the former, while food consumption is less positively skewed and rather 'flat' around its mode. These features justified the use of quantile regression techniques in Chapter 2. The present study goes back to linear models that rely on well behaved residuals, and hence consumption outcomes enter the left hand-side of the regressions in logarithms.

The bottom panel of Table 3.1 reveals that there are more girls of age 5 to 18 who are currently enrolled in school (at time of survey) living in households ineligible to microfinance programs. Girls from eligible non-bororwing housheolds are the worst off with little over 40% of them who go to school, while daughters of microfinance clients have one in two probability of being schooled. The proportion of boys in school is greater than that of girls overall, but boys in eligible non-participating are even worse off than their female counterparts. By the late 1990s, over 60% of children are schooled, no matter the borrowing or eligiblity status of the household and no matter the sex of the child.

Altogether, children from ineligible households are still more likely to be enrolled in school than others, but the gap is much narrower than 7 years ago. In the meantime, microfinance initiatives have strived as witnessed by the amounts of loans to women

[6] Recall that household choice is determined by the presence of a microcredit program in the village and the eligibility status of the household, which is taken from census and survey data.

Table 3.1 **Summary statistics of household credit and expenditure variables**

	1991-92				**1998-99**			
	Borrowers	**Non-borrowers** *Eligible*	**Non-borrowers** *Non-eligible*	**Full sample**	**Borrowers**	**Non-borrowers** *Eligible*	**Non-borrowers** *Non-eligible*	**Full sample**
HH total microcredit cumulative borrowings	10,295.2			2,515.3	17,985.7			7,408.6
	(9,741.1)			(6,538.2)	(19,508.5)			(15,332.5)
Female microcredit cumulative borrowings	6,717.5			1,641.2	15,196.2			6,259.6
	(8,726.8)			(5,189.4)	(19,242.0)			(14,436.1)
Male microcredit cumulative borrowings	3,577.7			874.1	2,789.4			1,149.0
	(7,490.6)			(4,008.0)	(8,937.5)			(5,896.7)
HH per capita total expenditure, annual	4,028.0	3,864.0	5,664.2	4,513.7	5,039.9	4,971.0	6,989.2	5,373.0
	(1,645.7)	(1,683.6)	(3,553.9)	(2,605.2)	(3,324.2)	(3,599.6)	(5,387.5)	(3,963.2)
HH per capita food expenditure, annual	3,082.4	3,039.5	3,646.5	3,255.6	3,224.1	3,256.8	3,960.2	3,373.5
	(799.6)	(946.8)	(1,053.1)	(991.2)	(1,140.2)	(1,550.6)	(1,868.6)	(1,491.6)
HH per capita non-food expenditure, annual	945.6	824.4	2,017.7	1,258.1	1,815.8	1,714.2	3,029.1	1,999.4
	(1,194.1)	(984.5)	(2,980.4)	(2,011.9)	(2,727.8)	(2,541.6)	(4,693.2)	(3,156.7)
Observations	781	611	246	1,638	840	612	186	1,638
Girls age 5-18 currently enrolled in school (1=yes)	0.495	0.431	0.622	0.517	0.629	0.660	0.674	0.649
	(0.500)	(0.496)	(0.486)	(0.500)	(0.483)	(0.474)	(0.470)	(0.477)
Number of individuals	736	514	248	1,498	873	574	201	1,648
Boys age 5-18 currently enrolled in school (1=yes)	0.548	0.401	0.662	0.545	0.600	0.616	0.679	0.621
	(0.498)	(0.491)	(0.474)	(0.498)	(0.490)	(0.487)	(0.468)	(0.485)
Number of individuals	803	520	303	1,626	949	608	207	1,764

Note: Sample means and standard deviations (in parentheses) for household microcredit and expenditure variables, and for individual-level school enrollment for girls and boys age 5-18. Statistics account for sampling weights. All monetary amounts are deflated and given in 1992 Taka. The word household is abbreviated by 'HH'. The eligibility status of a household used to identify sub-samples is taken from census data for the first wave of data (1991-92) and from the questionnaire module on loans for the second wave (1998-99).

that more than doubled. In the interval between the two waves of surveys, eligible non-borrowers are clearly those for whom education of children has improved the most. We can imagine that eligible housheolds share characteristics that bring them closer together and make them more likely to exchange and interact, whether they borrow or not. The interesting fact is that children education also improved in ineligible households. Of course, this improvement can very well be explained in many ways, but it is tempting to envisage the potential for social interactions at the community level that could have seen the gains in interest for schooling of borrowing and eligible households spill over to the third category.

3.3 Results of direct program impact estimates

3.3.1 The consumption effects of microcredit

Estimates of the impact of microcredit on the consumption of borrowing households are reported in Table 3.2. Recall that both the dependent variable and explanatory variables of interest enter regressions in logarithm, so coefficient estimates can be interpreted as elasticities. Baseline results from simple OLS regressions with household-level fixed effects show that a 10% increase in the total amount of household borrowing boosts household total annual per capita expenditure by 0.37%, with the estimate statistically significant with 99% confidence.

They also suggest this effect is positive and significant whether credit is offered to women or men, with a larger elasticity estimate for the latter. Although household borrowings seem to have a small positive impact on household food consumption with an elasticity of 0.014% significant at the 5% level, the significance of this effect vanishes when we differentiate by gender of borrowers (coefficient estimates still take positive values).

Table 3.2 **Impact of microcredit borrowings on the log of household per capita annual expenditure**

	OLS FE			Time-varying village effects		
Measure of HH per capita expenditure	**Total**	**Food**	**Non-food**	**Total**	**Food**	**Non-food**
Log of household borrowings	0.037***	0.014**	0.106***	0.025**	0.008	0.078***
	(3.484)	(1.984)	(4.898)	(2.453)	(1.093)	(3.788)
F-statistic				3.819	5.566	3.030
p-value				0.000	0.000	0.000
Log of women's borrowings	0.033***	0.012	0.097***	0.025**	0.007	0.076***
	(2.951)	(1.627)	(4.311)	(2.389)	(1.011)	(3.592)
Log of men's borrowings	0.041**	0.017	0.096**	0.014	0.004	0.047
	(2.129)	(1.449)	(2.444)	(0.775)	(0.366)	(1.337)
F-statistic				3.847	5.571	3.055
p-value				0.000	0.000	0.000

Note: Elasticities estimates from fixed effects regressions with a panel of 1,638 households observed twice over time. Heteroskedasticy robust absolute t-statistics in parentheses. Significance levels: *** 1%; ** 5%; * 10%. The bottom two lines of each panel report the F-statistic and p-value associated to the test of joint significance of time-varying village effects.

It appears that the bulk of the positive increase in household consumption thanks to microcredit falls on non-food expenditure which increases by 1.06% when households borrow 10% more. The contributions of women's and men's borrowings are almost identical (elasticities of 0.97% and 0.96% for a 10% increase in credit, respectively) and statistically significant.

The three rightmost columns provide results from similar regressions that allow for the impact of time-invariant unobserved heterogeneity at the village level to change over time. We can see that time-varying coefficients on unobserved village fixed effects are jointly significant across the board. As a consequence, coefficient estimates are lower and a 10% increase in household total credit generates a 0.25% gain in total expenditure (against 0.36% earlier).

The estimated elasticity associated to women's borrowing is of the same value and statistically different from zero while the impact of microcredit issued to men, albeit being positive, is insignificant. In the new specification we find only very small effects of microcredit on food expenditure that are not statistically distinguishable from zero, irrespective of the gender of borrowers. Finally, we confirm the previous observation that the impact of microcredit is the strongest for non-food expenditure with an estimated gain of little more than 0.78% for a 10% increase in total household credit or in women's borrowings alike, both coefficients being strongly significant. If microcredit issued to men also generates small positive impacts, the latter are not statistically significant.

Overall, the positive impacts of microcredit on household per capita annual expenditure seem to be driven by the positive benefits accruing to those households with female borrowers, especially on non-food expenditure. We do not find strong evidence that microfinance loans help borrowers consume significantly more food than non-borrowers, and neither can we conclude that microcredit issued to men is beneficial.

We now turn to results obtained from two-stage least squares (2SLS) fixed effects regressions carried out in an attempt to deal with the potential endogeneity of a household's decision to participate to microfinance programs and household-level outcome behaviours. Indeed, the previous approach dealt with the issue only to the extent that unobserved characteristics that influence both the decision to borrow and the level of expenditure are fixed over time. Instrumentation is one option to account for potentially time-varying hidden factors.

The first three columns of 3.3 show the output of 2SLS fixed effects regressions when census eligibility is used in the construction of instruments[7]. We find no significant impact of microcredit on total expenditure, although the estimates are of the same sign and order of magnitude as those obtained previously, and results from Hansen's over-identification test suggest that our instrument set is valid. However this conclusion is reversed when the outcome is either food or non-food expenditure. Surprisingly, the

[7] Census eligibility refers to the eligibility status of a household reported in the census data use for sampling households in villages in 1991-92, and to the eligibility status reported directly by households in the 1998-99 survey.

Table 3.3 **Impact of microcredit borrowings on the log of household per capita annual expenditure, IV estimates**

	Census eligibility			Landholdings eligibility		
Measure of HH per capita expenditure	**Total**	**Food**	**Non-food**	**Total**	**Food**	**Non-food**
Log of household borrowings	0.024	0.017	0.130***	0.022	-0.010	0.071
	(0.919)	(0.942)	(2.577)	(0.822)	(0.596)	(1.319)
J-statistic	126.2	139.3	138.1	149.5	144.0	122.8
p-value	0.111	0.023	0.027	0.012	0.026	0.248
Log of women's borrowings	0.017	0.005	0.133**	0.013	-0.017	0.061
	(0.600)	(0.244)	(2.439)	(0.474)	(0.963)	(1.104)
Log of men's borrowings	0.051	0.057*	0.085	0.104**	0.075**	0.119
	(1.204)	(1.876)	(1.061)	(1.989)	(2.026)	(1.154)
J-statistic	126.2	138.0	137.2	140.9	131.0	123.5
p-value	0.099	0.024	0.026	0.034	0.106	0.216

Note: Elasticities estimates from two-stage least squares regressions with fixed effects on a sample of 1,638 households each observed twice over time. Instruments are generated by interacting the *credit choice* dummy with household and village covariates as well as the full set of village dummy variables. Household eligibility to microcredit, used to build the *choice* variable, is defined either from census and survey data or from the exogenous 'landholdings rule' (eligible if hold less than half an acre of land). The bottom two rows of each panel report the J-statistic and p-value from Hansen's overidentification test. The second stage includes all covariates and controls for time-varying unobserved village heterogeneity. Household-cluster-robust t-statistics in parentheses. Significance levels: *** 1%; ** 5%; * 10%.

only significant (10% level) impact we find on food consumption pertains to microcredit issued to men with an estimated elasticity of 0.057%, three times its value from baseline OLS regressions. Our findings in the case of non-food expenditure, however, are rather consistent with previous observations given our statistically significant estimates of 0.13% and 0.133% for the elasticity of the outcome to household total and female borrowings, respectively, while the impact of male microcredit is not distinguishable from zero.

The results displayed in the three rightmost columns of Table 3.3 are quite puzzling. Although estimates of the impact of total and female borrowings on total expenditure are qualitatively in line with the first estimates (close in value and insignificant), the output shows a significant positive and large effect of borrowing by men on household total expenditure. Furthermore, Hansen's tests yield conclusions opposite to the former case and suggest that the set of instruments is not valid. When we consider food consumption as the outcome of interest household total microcredit and women's borrowings still have no significant impact, however the coefficient estimates are negative while male microcredit appears to generate food consumption gains.

Conclusions regarding the quality if instrumentation are mixed, and we reject the null that over-identifying assumptions are respected when microcredit is not split by gender but do not reject it when estimating gender-based impacts. Finally, we conclude that the instruments set is valid when non-food expenditure is the outcome of interest, in regressions where estimated elasticities are positive but none are significant.

At any rate we apprehend the above IV estimates with great care. Indeed, as hinted previously when discussing our empirical methodology, we have some doubt about the premise on which our instrumentation strategy is founded. The validity of the quasi-experiment offered by the landholdings eligibility criterion as a source of identification for program impacts in our dataset has been a long-standing debate, starting with the critic of Pitt and Khandker (1998) by Morduch (1998), before a response by Pitt (1999) and the critical replication of Pitt and Khandker's study by Roodman and Morduch (2014), to which Pitt (2014) responded once more.

The main argument is to know whether the substantial amount of mis-targeting of ineligible households by microfinance programs invalidates the use the landholdings criterion of eligibility as an exogenous cut-off point to identify parameters of interest. Although the aforementioned debate considers the cross-sectional case, Roodman and Morduch (2009) also casted doubt on its relevance in their replication of Khandker (2005) using panel data. Our own results also suggest that this instrumentation strategy is not very reliable, given the ever changing conclusions from Hansen's tests.

When we use census eligibility to build the household *choice* variable that is then interacted with exogenous covariates to build instruments, we rely on an eligibility rule that is *ad hoc* and not identifiable in the data, i.e. census eligibility does not strictly match the landholdings criterion and hence the way microfinance officers deem households eligible to group-lending remains unknown. On the other hand, the landholdings eligibility rule is "more" exogenous but provokes a pattern of results at odds with that observed earlier, especially the negative impacts of household total and female microcredit on food expenditure (although insignificant) and the strong positive impact of male credit on this same outcome.

Altogether, we feel that OLS regressions with household fixed effects and time-varying village unobserved heterogeneity are probably the best way to go, acknowledging the somewhat costly assumption that in that case household unobserved characteristics are restricted to being time-invariant. Note that our results are quite different from those obtained by Khandker (2005) who finds a significant and positive impact of female credit on all three measures of consumption with estimated elasticities of 0.009% for total expenditure, 0.006% for food expenditure and 0.018% for non-food expenditure, whereas our estimates are about three to four times larger for total and non-food expenditure and insignificant for food consumption. The author also finds systematically negative and insignificant coefficients attached to male microcredit, which is not our case.

These discrepancies can arise for various reasons. Even though we use the same dataset, we constructed our own variables which might differ from those used in the previous study. Moreover, regression specifications are also quite different since we do not include the lagged values of microcredit, we include a different set of covariates (e.g. a household economic dependency ratio and dummy variables to control for the occurrence of borrowing from sources other than microfinance programs) and we account for time-varying impacts of village unobserved effects. Finally, it is likely that Khandker (2005) uses a different censoring value for the logarithm of zero in microcredit variables, as pointed out in Roodman and Morduch (2009) (see sub-section 3.2.2). In sum, we do not seek to replicate nor to criticise Khandker's findings, and carry on with our own analysis of the data.

3.3.2 The impacts of microcredit on educational outcomes

Table 3.4 **Impact of microcredit borrowings on children education for borrowing households, individual-level outcome (binary)**

	Girl age 5-18 in school (1=yes)			Boy age 5-18 in school (1=yes)		
	Pooled probit with CRE		**LPM**	**Pooled probit with CRE**		**LPM**
Household microcredit	0.033	0.035	0.017	0.012	0.021	-0.014
	(1.042)	(1.140)	(1.466)	(0.250)	(0.485)	(1.050)
Women's microcredit	0.043	0.039	0.018	0.028	0.039	-0.010
	(1.324)	(1.201)	(1.436)	(0.516)	(0.804)	(0.688)
Men's microcredit	-0.046	-0.010	0.011	-0.115	-0.112*	-0.038
	(0.460)	(0.102)	(0.345)	(1.638)	(1.793)	(1.473)
Village covariates	yes	no	yes	yes	no	yes
Village dummy variables	no	yes	yes	no	yes	yes
Number of observations	2,470	2,470	2,470	2,732	2,729	2,732

Note: Estimated effects of microcredit on school enrolment of children age 5-18. Outcome variable is binary. We use pooled probit regressions with household-level correlated random effects (CRE) and village covariates or dummy variables on a balanced sub-sample. 'LPM' stands for linear probability model estimated via OLS with household fixed effects. Actual coefficient estimates are very small in magnitude, so tables display coefficients multiplied by 10,000. Absolute robust t-statistics in parentheses clustered at the household level. Significance levels: *** 1%; ** 5%; * 10%.

Table 3.4 reports the estimated impacts of microcredit on the probability of being currently schooled for children from borrowing households. Note that the values found in the tables are the estimated coefficients multiplied by 10,000 because of the small magnitude of actual estimates . The first two columns show results from pooled probit regressions with household correlated random effects assuming strict exogeneity of the regressors. Girls of age 5 to 18 living in borrowing households are more likely to be in school thanks to microcredit issued to men and less likely so when loans are issued to men. This observation is consistent whether we include village covariates or dummy variables, however none of the estimated coefficients is significant. We observe the same pattern of results from pooled probit regressions when the response variable is the schooling of boys. Male children of age 5 to 18 are more likely to be enrolled in school if they live in households where women borrow, the converse being true for those in households with male borrowers. The negative estimated impact of male credit on the education of boys is statistically significant when village dummy variables are included. The corresponding linear probability models suggests that microcredit favors the schooling of girls irrespective of the gender of borrowers, but again without finding any significant impact. In the case of the male education, all coefficients estimated via OLS with fixed effects are negative and insignificant.

Results from switching to a household-level measure of education, i.e. the proportion of girls (or boys) of age 5 to 18 in the household currently schooled, are qualitatively similar in sign and magnitude with no significant estimated impacts, and hence they are not presented here (they can be found in Appendix M). At a glance, borrowing by women increases the proportion of girls enrolled in school while borrowing by men decreases it, but neither effect is statistically distinguishable from zero. The pattern of results is the same for the education of boys. Much like in the case of a binary outcome, estimates from OLS regressions with household fixed effects suggest that microcredit is always beneficial to the education of girls an always detrimental to male education, whether loans are issued to women or men. Statistical significance also eludes the latter set of results.

To the extent schooling decisions are made at the household-level, there is room for potential endogeneity bias in our estimates if household-level heterogeneity is not accounted for properly by correlated random effects or fixed effects. Results from the control function approach following Papke and Wooldridge (2008) to account for endogenous regressors in pooled probit models with correlated random effects are reported in Table 3.5 in the case of a binary educational outcome at the child level. The procedure is repeated 1,000 times on bootstrap samples generated by re-sampling households with replacement.

We use bootstrap percentile confidence intervals for inference, drawing directly on the empirical distribution of coefficient estimates to build the lower and upper bounds of the interval. Another option was to use basic bootstrap confidence intervals whose validity rely on the normality of the bootstrapped quantity of interest. As can be seen from Quantile-Normal plots in Appendix N, the distribution of most of our bootstrap estimates is likely to be normal. However, Shapiro-Wilk and Shapiro-Francia tests do reject the null hypothesis of bootstrap estimates being normally distributed in a few instances, so for the sake of clarity we prefer to use the same type of confidence intervals for all estimates (basic bootstrap confidence intervals yield qualitatively similar results).

Table 3.5 **Impact of microcredit borrowings on children education for borrowing households, IV estimates, individual-level outcome (binary)**

	Girl age 5-18 in school (1=yes)		Boy age 5-18 in school (1=yes)	
	2SLS for panel data	**Pooled probit CRE - Control Function**	**2SLS for panel data**	**Pooled probit CRE - Control Function**
Household microcredit	0.133***	0.080	0.018	0.088
	(0.041)	(0.066)	(0.027)	(0.078)
Household microcredit – first-stage residuals		-0.083		-0.084
		(0.071)		(0.056)
J-statistic	91.1		113.8	
p-value	0.505		0.061	
Women's microcredit	0.134***	0.029	0.026	0.073
	(0.041)	(0.070)	(0.029)	(0.085)
Women's microcredit – first-stage residuals		-0.016		-0.048
		(0.075)		(0.059)
Men's microcredit	0.063	0.463*	-0.094	0.131
	(0.232)	(0.196)	(0.098)	(0.173)
Men's microcredit – first-stage residuals		-0.493**		-0.267
		(0.176)		(0.153)
J-statistic	88.2		112.2	
p-value	0.564		0.066	

Note: IV estimates of the effects of microcredit on school enrolment of children age 5-18. Outcome variable is binary. We use pooled probit regressions with household-level correlated random effects (CRE) with a control function approach to deal with endogenous credit variables, for which we also report the coefficient on first-stage residuals that enter second stage regressions. Inference based on bootstrap percentile intervals with 1,000 replications. Bootstrap standard errors in parentheses. Other results are obtained with two-stage least squares (2SLS) for which the J-statistic and p-value of Hansen's overidentification test are reported. Standard errors clustered at the household-level in parentheses. Actual coefficient estimates are very small in magnitude, so coefficients and standard errors are multiplied by 10,000. Significance levels: *** 1%; ** 5%; * 10%.

The estimated impact of female microcredit on the probability of a girl to be enrolled in school is positive and similar in magnitude to regressions without instrumentation, and is insignificant. The impact of male credit on female education becomes positive and very large but remains insignificant. The strong significance of the coefficient attached to first-stage residuals, i.e. the device controlling for endogeneity, suggest that men's microcredit is not strictly exogenous, but we cannot say the same for women's microcredit or for household total microcredit. Male education is also improved by the occurrence of microcredit borrowing in the household, but not statistically significantly.

Impacts estimated via two-stage least squares with household fixed effects tell a different story. Total household microcredit significantly increases the probability of a girl form a borrowing household to be currently enrolled in school, an effect due to women's credit (the estimated impact from men's microcredit is close to nil and insignificant). On the other hand, the probability of boys being schooled is improved by a little thanks to female borrowings and is hurt by male borrowings although neither impact is significant. Interestingly, the null is never rejected by the over-identification restrictions tests in the case of schooling as a binary outcome.

Table 3.6 **Impact of microcredit borrowings on children education for borrowing households, IV estimates, household-level outcome (fractional)**

	Proportion of girls age 5-18 in HH currently enrolled		Proportion of boys age 5-18 in HH currently enrolled	
	2SLS for panel data	**Pooled fractional probit CRE - Control Function**	**2SLS for panel data**	**Pooled fractional probit CRE - Control Function**
Household microcredit	0.128***	0.091	0.029	0.064
	(0.045)	(0.066)	(0.033)	(0.070)
Household microcredit – first-stage residuals		-0.077		-0.028
		(0.066)		(0.055)
J-statistic	87.5		103.4	
p-value	0.612		0.196	
Women's microcredit	0.132***	0.039	0.043	0.053
	(0.045)	(0.070)	(0.036)	(0.076)
Women's microcredit – first-stage residuals		-0.005		0.005
		(0.071)		(0.058)
Men's microcredit	-0.002	0.477*	-0.135	0.105
	(0.225)	(0.189)	(0.101)	(0.163)
Men's microcredit – first-stage residuals		-0.535***		-0.207
		(0.169)		(0.143)
J-statistic	84.1		101.2	
p-value	0.682		0.217	

Note: IV estimates of the effects of microcredit on school enrolment of children age 5-18. Outcome variable is fractional. We use pooled fractional probit regressions with household-level correlated random effects (CRE) with a control function approach to deal with endogenous credit variables, for which we also report the coefficient on first-stage residuals that enter second stage regressions. Inference based on bootstrap percentile intervals with 1,000 replications. Bootstrap standard errors in parentheses. Other results are obtained with two-stage least squares (2SLS) for which the J-statistic and p-value of Hansen's overidentification test are reported. Standard errors clustered at the household-level in parentheses. Actual coefficient estimates are very small in magnitude, so coefficients and standard errors are multiplied by 10,000. Significance levels: *** 1%; ** 5%; * 10%.

The pattern of results is very similar when schooling outcomes are measured at the household level. Indeed, Table 3.6 shows a small positive insignificant impact of female microcredit on the proportion of girls in the household who are enrolled in school when estimated via a control function approach in a pooled fractional probit regression with correlated random effects. The same regression confirms the positive and significant impact of microfinance loans issued to men on the education of girls, while the schooling benefits accruing to male children of borrowing households are never significant. Implementing our 2SLS fixed effects approach with household-level measures of schooling confirms the findings of Table 3.5, i.e. that women's microcredit is significantly beneficial to the education of girls while credit issued to men does not affect it. The proportion of boys in the household currently in school is improved by female borrowings and decreases because of male borrowings, but these impacts are not significant.

Overall, it is hard to conclude on the potential benefits or detriments of microcredit on schooling outcomes. Without accounting for endogenous credit, results from pooled probit regressions suggest that microfinance loans issued to women yield better educational outcomes for boys and girls alike, and the reverse for male microcredit. Linear probability models hint that microcredit, whether issued to women or men, is beneficial for female education and detrimental for male education. However, statistical significance completely eludes these results.

Resorting to instrumentation does not particularly plays in favour of the consistency of the results. All estimated coefficients from pooled probit regressions with a control function approach become positive, and the sign reversal in the impact of male credit on female education measured at the household level is accompanied by the appearance of statistical significance. Two-stage least squares regressions yield an unequivocally positive and significant impact of female microcredit on the education of girls, and insignificant effects of microcredit on male education. However, provided the many reserves already mentioned about the validity of the instrumentation strategy on this data, we refrain from giving more credit to the appealing results from IV estimates. Our subsequent investigation on the potential existence of spillover effects might help shed some light on the inconsistency of the above results.

3.4 Spillover effects

3.4.1 Regression framework

Section 2 introduced the idea that average treatment effects (ATE) can be under or over-estimated in the presence of spillover effects. Consider a simple setting where the comparison of mean outcomes in the treated population to their counterpart in the untreated population yields a positive difference. If treatment is randomly assigned and compliance is perfect, under the typical Stable-Unit-Treatment-Value-Assumption – or SUTVA – and an independence assumption between treatment and potential treatment outcomes, this difference is an unbiased estimate of the ATE which is positive.

However, if the untreated population is somehow negatively affected by the treatment (in the form of an externality or otherwise), then the SUTVA is violated and the ATE is over-estimated, because the control group is supposed to provide a counterfactual to the observed outcomes for the treatment group, i.e. a picture of "what would have happened" had the treated population not received treatment. And conversely, if control units indirectly benefitted from the program then the ATE is under-estimated. In some contexts, one cannot rule out either the possibility that treated units themselves benefit from indirect treatment effects, hence further under-estimating the ATE.

The above example is obviously overly simplified, but we feel it spells out clearly the importance of spillover effects. In section 2 we also defend the idea that spillover effects can arise from microfinance programs and that their detection could substantially improve the current state of knowledge about the actual impacts of such initiatives.

Philipson (2000) argues that exact identification of external program effects is achieved when the unit of observation, or the group, completely internalises every external effects amongst its members. The choice of the relevant sphere within which spillover effects are expected to occurr is therefore crucial. For instance, Townsend (1994) recognises that the extended family is probably the most accurate unit of study for informal insurance networks in rural areas of developing countries, an idea corroborated by Angelucci, De Giorgi, Rangel, and Rasul (2010).

Okten and Osili (2004) and Wydick et al. (2011) show the importance of various networks for the diffusion of information about new sources of credit, especially microfinance. Unfortunately, absent precise data that could allow us to narrow down the circle of influence of households in our study, we have to assume that spillover effects are embedded at the village level. This is a rather conservative approach in that the village might be quite large a unit compared to the actual groups that internalise the social interactions at the source of spillovers.

The latter remark might make one worry about the so-called ecological fallacy , i.e. the potential bias inherent to using aggregate data to draw conclusions about behaviour at lower levels (see e.g. Clark and Avery (1976); or Wakefield (2008) for a review). In our context the analogous fallacy is to draw conclusions about actual social networks based on village-level detection of spillover effects. But our approach need not be invalidated by such concerns, to the extent that some spillover effects arise directly at the village level, in line with the expected impacts of microfinance programs that are also implemented with the alleged hope of triggering community-wide effects. Therefore we build on the rationale that village economies are relevant units of study in developing countries, a well established fact in development economics (see e.g. Townsend, 2016). For a preliminary detection of potential spillover effects as the focus of this sub-section, we do not need to worry about the ecological fallacy, however we need to assume there are no cross-villages externalities within our sample, a hypothesis our data does not allow us to check.

When envisaging spillover effects arising within the village, the interpretation of village unobserved effects changes. While the latter are usually used to control for non-random program placement, they are now partly a consequence of external program effects to the non-participating population and to program participants alike. That is, program placement causes village heterogeneity.

This is pointed out in Khandker (2005) who therefore augments his baseline household fixed effects regressions with an extra regressor capturing the intensity of treatment in the village, with which Equation 3.1 becomes:

$$Y_{ijt} = \phi_t + C_{ijt}\delta + X_{ijt}\gamma + \Pi_{jt}\pi + \alpha_{ij} + \eta_j + \epsilon_{ijt} \tag{3.4}$$

where Π_{jt} is a measure of treatment intensity at the village level that varies with time, which Khandker (2005) suggests could be village average microcredit borrowings. Then, parameter π captures the indirect impact of microfinance on non-borrowers as well as program impacts on borrowers above and beyond the direct effects from credit uptake. We can recover an estimate of indirect effects on every household in a village by elucidating part of the previously unexplained village heterogeneity in the fashion proposed by Khandker (2005) and expressed in Equation 3.4.

One drawback of this approach is that indirect treatment effects to participants and non-participants are constrained to be equal. To account for potential heterogenous spillover effects to households with different borrowings status our regressions include an additional interaction term between treatment intensity at the village level and a dummy variable equal to one if a household does not borrow. We consider in turn consumption and educational outcomes and use linear panel data estimation with household fixed effects and time-varying impacts of village unobserved effects throughout.

3.4.2 Evidence of indirect program effects

Spillover effects to the non-participating and participating population are captured by a village average level of borrowing. The top left quadrant of Table 3.7 suggests that the village-level stock of total household microcredit has an significant positive impact on per capita total and non-food expenditure, whereas direct consumption gains from microcredit for borrowers are not significant.

Interestingly, we find no direct impacts of microfinance on the education of children nor indirect impacts to the population as a whole but there is potentially an adverse spillover effect on the proportion of boys enrolled in school in the household for the non-participating population only. This is captured by the interaction term between village average borrowings and a dummy equal to one when the household does not borrow. The coefficient is negative and significant with 90% confidence. Although

Table 3.7 **Spillover effects of microcredit borrowings on consumption and educational outcomes**

	HH per capita expenditure			Female education		Male education	
Dependent variable	**Total**	**Food**	**Non-food**	**Girl currently in school (1=yes)**	**Proportion of girls in school**	**Boy currently in school (1=yes)**	**Proportion of boys in school**
Log of household borrowings	0.024	0.019	0.056	0.007	0.004	-0.038	-0.025
	(1.234)	(1.473)	(1.588)	(0.257)	(0.155)	(1.549)	(0.910)
Log of village average HH borrowings – (V)	0.082***	0.036	0.171***	-4.328	-4.482	0.145	0.604
	(2.959)	(1.573)	(3.476)	(1.457)	(1.412)	(0.060)	(0.279)
(V) x Non-participation dummy	-0.000	0.004	-0.008	-0.009	-0.007	-0.014*	-0.011
	(0.067)	(1.048)	(0.776)	(1.014)	(0.798)	(1.783)	(1.297)
F-statistic	3.747	5.566	2.993	5.002	3.664	2.521	2.057
p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Log of women's borrowings	0.052***	0.036***	0.078**	0.013	0.005	-0.048*	-0.034
	(2.840)	(2.730)	(2.114)	(0.447)	(0.175)	(1.760)	(1.124)
Log of village average women's borrowings – (VF)	0.045**	0.043***	0.074*	0.099	0.111	0.051	0.097
	(2.172)	(2.627)	(1.652)	(0.995)	(1.050)	(0.652)	(0.935)
(VF) x Non-participation dummy	0.010*	0.011**	0.000	-0.006	-0.006	-0.018**	-0.015
	(1.818)	(2.420)	(0.033)	(0.623)	(0.638)	(2.096)	(1.576)
Log of men's borrowings	-0.052	-0.016	-0.047	-0.004	0.007	-0.008	0.001
	(1.319)	(0.660)	(0.597)	(0.071)	(0.128)	(0.180)	(0.024)
Log of village average men's borrowings – (VM)	0.093***	0.037**	0.154***	0.284	0.285	-0.019	-0.036
	(3.288)	(2.110)	(2.587)	(1.513)	(1.428)	(0.131)	(0.270)
(VM) x Non-participation dummy	-0.022*	-0.007	-0.030	-0.009	-0.003	0.012	0.010
	(1.810)	(1.092)	(1.250)	(0.566)	(0.196)	(0.818)	(0.691)
F-statistic	3.844	5.567	3.088	4.873	3.597	2.554	2.129
p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Note: Elasticities estimates from fixed effects regressions with 1,638 households. Coefficients on log of village average borrowings – (V), (VF) and (VM) – capture indirect program effects on all households in the village. Interaction terms with a dummy variable indicating that a household does not participate to microfinance programs measure the extra spillover effects to non-borrowing households beyond and above the overall indirect effect. Heteroskedasticy robust absolute t-statistics in parentheses. Significance levels: *** 1%; ** 5%; * 10%. The bottom two lines of each panel report the F-statistic and p-value associated to the test of joint significance of time-varying village effects.

spillover effects to the whole community are not significant, this coefficient has to be interpreted as a specific effect to the non-partiticpating population *beyond* overall indirect effects. A 10% surge in the stock of microcredit in the village increases the proportion of boys in school by 1.45% for every household, but this increase is 0.14% less important for non-borrowers.

Much like our baseline regressions, the lower left panel of Table 3.7 shows that female microcredit generates positive gains on all measures of consumption, the impacts being typically significant with at least 95% confidence. There is also evidence suggesting positive and significant spillover effects from female microcredit at the village level on all three measures of household per capita expenditure, especially on non-food consumption. Additionally, extra indirect impacts emerge for the non-participating population. There are small positive food consumption gains for non-borrowers on top of overall spillover effects as a response to an increase in village average female credit.

There are no significant estimated direct impacts from microfinance loans issued to men, which are small in magnitude with negative signs. The stock of men's borrowings in the village, however, appears beneficial to the population as a whole. There are strong positive impacts associated to spillover effects from male credit across all three measures of consumption but they are the more salient for non-food consumption. However, the interaction term suggests an additional negative indirect impact on the total per capita expenditure of non-borrowers, significant with only 90% confidence, but this negative spillover is not found on measures of food and non-food expenditure separately.

The rest of the coefficients presented in Table 3.7 tend to show no direct or indirect effect of microfinance participation to the education of children, with the exception of the schooling of boys that appears to be negatively impacted by female microcredit, when the outcome is measured at the household level. Additionally, when considering this same outcome the results hint at an extra, small negative spillover effect stemming from the intensity of women's borrowing in the village that affects the schooling of boys in non-borrowing households. This is to be balanced with the estimated overall indirect effect to the whole community that is positive and larger in magnitude than the negative additional non-borrowers specific spillover, albeit statistically not significant. Recall that the interaction term captures additional spillovers to non-borrowers above and beyond the overall village-level indirect effects.

In sum, the estimates commented above offer at least partial evidence of the potential for spillover effects to stem from microfinance programs. There exist direct consumption gains from participating in microfinance for female borrowers, as well as positive overall spillover effects and even an extra positive indirect effect for the non-borrowing population. Indirect village-level effects from male borrowing are large and positive, with additional food consumption gains specific to the non-bororwing population.

One tentative explanation relates to theories of risk-sharing in village economies that imply that under efficient risk-pooling and re-distribution of resources then individual consumption is insured against idiosyncratic shocks and co-moves with aggregate consumption. This explanation is seducing because risk insurance has been shown to be a reliable device for rural households of developing countries. To the extent microfinance programs provide extra resources at the aggregate level, these can be re-distributed at the local economy level through daily trade transactions, income sharing or even gift-giving, which is suggested by Angelucci and De Giorgi (2009) to explain increases in the consumption of ineligible households living in PROGRESA areas.

One pitfall of this methodology is it cannot investigate precisely what type of spillover effects are at work. Absent network data or precise data on informal sources of credit and gifts, the only type of spillover effects possibly identifiable with the present dataset are social interactions, which is the topic of the next section.

4 Spillover effects as social interactions

Indirect effects from policy interventions can be broadly classified under four categories: externalities, general equilibrium effects, social interactions and contextual effects (Angelucci & Di Maro, 2016). The former define those spillover effects that go strictly from the treated population to the untreated population. One example is the study of deworming programs by Miguel and Kremer (2004) in Kenya where deworming drugs are provided to all children of randomly selected schools. The authors identify significant cross-school externalities resulting in improved health outcomes for children in untreated neighboring schools as a consequence of lower exposure to this type of infectious diseases.

We discussed some forms of general equilibrium effects from microfinance programs in Section 2. They have been analysed mainly through the lens of how new sources of widely available cheap credit affect pre-existing financial institutions, especially their impact on interest rates served by informal lenders (Demont, 2016). A few studies also attempt to capture economy-wide impacts of microfinance on wages and earnings by evaluating structural models of general equilibrium (Buera et al., 2012, is one example).

The distinction between the two latter types of spillover effects is somewhat more subtle. Indirect program effects can be conveyed through the various social spheres within which local treated units interact with local untreated units, for instance in the context of risk-sharing networks. Contextual effects, however, arise when a policy intervention have an influence on pre-existing social norms and when said modified norms are then followed by the local population as a whole.

Our first attempt at detecting spillover effects in section 3.4 potentially captures every type of indirect effects mentioned above. The idea that social interactions are particularly relevant for the study of microfinance programs is already grounded in the evaluation literature on such initiatives, and they have been studied mostly as information transmission channels that impact awareness of newly available microfinance sources and ultimately the uptake of loans. Section 2 defends the view that social interactions can also play a role in yielding broader welfare benefits from microfinance programs than just direct effects on their beneficiaries, potentially influencing meaningful outcomes such as consumption and education for the non-borrowing population as well. The remainder of this study attempts to find whether this can be verified empirically.

4.1 Econometric framework

4.1.1 The linear social interactions model

The behaviour of an economic agent, often modelled as an individual constrained utility maximisation problem, can also depend on the behaviours of other agents in her reference group. The idea that an individual makes welfare-inducing decisions partly based on the information provided by the decisions of others is not new, however the empirical investigation of such social interactions remains a challenge to this day. The seminal paper by Manski (1993) was paramount in determining the identification problems pertaining to the estimation of the popular linear model of social interactions. The latter can be presented as follows, omitting the constant:

$$Y_{ig} = \alpha X_{ig} + \beta \bar{X}_g + \gamma V_g + \theta \bar{Y}_g + \epsilon_{ig} \tag{3.5}$$

where outcome value Y_{ig} of individual i in group g is determined by individual exogenous characteristics X_{ig}, mean exogenous characteristics in the reference group $\bar{X}_g$, exogenous features of the group's environment V_g, and group realised mean outcome $\bar{Y}_g$; ϵ_{ig} is an idiosyncratic *i.i.d.* error term. The structural model exposed in Manski (1993) actually uses expected values of group mean characteristics and group mean outcome, but realised values are typically used in empirical applications under the assumption of self-consistency which states that subjective expectations formed by individuals about the average outcome in the group is equal to the mathematical expectation of said outcome in the group (Blume, Brock, Durlauf, & Jayaraman, 2015)[8].

Equation 3.5 provides a clear visualisation of how similarities in the behaviour of individuals belonging to the same group might arise (Bobonis & Finan, 2009). Following the nomenclature made popular by Manski (1993), parameters α and γ represent correlated effects, formalising the proposition that people tend to behave in a similar fashion because they have similar characteristics and/or face a similar environment. The influence of exogenous group characteristics on individual outcomes, coined peer exogenous or contextual effects, is captured in parameter β. Finally, parameter θ measures peer endogenous effects, the extent to which group behaviour (or mean outcome in an empirical application) affects individual-level decisions. In sum, social interactions matter for the determination of individual outcomes when β includes non-zero elements and/or when θ is non-zero (Blume & Durlauf, 2006).

The crucial contribution of Manski (1993) is to show that parameters in Equation 3.5 are not identified because it represents an equilibrium where individual outcomes are realised simultaneously (Moffitt, 2001). Setting $V_g = \bar{X}_g$ as in Manski (1993) (dropping

[8] Blume, Brock, Durlauf, and Ioannides (2011) provide microeconomic foundations to derive the linear empirical model presented here as well as the condition of self-consistency.

$\bar{X}_g$ from Equation 3.5), realised group outcome is expressed by:

$$\bar{Y}_g = \frac{\alpha + \gamma}{1 - \theta} V_g \tag{3.6}$$

This linear dependence of group mean outcome on a constant and group-level characteristics leads to a failure to identify structural parameters and disentangle the impacts of the two types of social interactions (Blume et al., 2011). This is what Manski coined the reflection problem: any correlation between individual behaviour and group average behaviour might actually stem from the effect of contextual variables (V_g) on individual decisions (Blume & Durlauf, 2006).

Going back to Equation 3.5, taking expectations on both sides conditional on X and Z, solving for average group outcome and plugging the latter back in (3.5) (see e.g. Bobonis & Finan, 2009), the reduced form equation for individual outcomes is:

$$Y_{ig} = \frac{\alpha}{1 - \theta} X_{ig} + \frac{\beta}{1 - \theta} \bar{X}_g + \frac{\gamma}{1 - \theta} V_g + \epsilon_{ig} \tag{3.7}$$

In a context where structural parameters in (3.5) are not identified, we see that composite parameters in (3.6) are. As such, a regression to the mean of individual outcome Y onto explanatory variables X, $\bar{X}$ and V would still be informative about the existence of social interactions, however without enabling the researcher to distinguish between peer exogenous and endogenous effects. This result shows how our preliminary detection of spillover effects through the inclusion of village average borrowings as an extra regressor could indeed yield partial evidence of social interactions.

4.1.2 Instrumental variables to break the reflection problem

In spite of the identification problems pertaining to the linear social interactions model, Blume et al. (2011) mention a useful result stating that identification of structural parameters in (3.5) is possible under the necessary condition that the group average of at least one element of X_i is excluded from V_g. To see why, notice that relaxation of Manski's restriction that $V_g = \bar{X}_g$ yields the following expression for expected group mean outcome in (3.5):

$$\bar{Y}_g = \frac{\alpha + \gamma}{1 - \theta} \bar{X}_g + \frac{\gamma}{1 - \theta} V_g \tag{3.8}$$

Compared with equation (3.6) that represents average group outcomes as perfectly collinear with a constant and contextual variables, the presence of $\bar{X}_g$ in Equation 3.8 implies that it is not the case if the right-hand side first term does not itself depend linearly on a constant and V_g, hence the exclusion restriction mentioned above as a necessary condition for identification (Blume et al., 2011). The associated sufficient conditions for identification are summarised in Durlauf and Tanaka (2008) and basically state the (i) existence of such a variable in X_i (ii) whose group average is independent of the component of V_g that does not include $\bar{X}_g$ and (iii) is orthogonal to the error term (this must hold for other exogenous variables too). Essentially, structural parameters of interest, and most notably endogenous effects, can be recovered from estimating Equation 3.5 while instrumenting for group average behaviour (Blume et al., 2011).

4.2 Empirical strategy

What follows draws mostly on the study of peer effects in schooling choices of children from ineligible household in the context of PROGRESA, by Bobonis and Finan (2009), and also to a lesser extent on a similar study by Lalive and Cattaneo (2009). Recall that PROGRESA was a nationwide program in Mexico whose implementation started in the late 1990's that targeted all rural villages and provided cash transfers to poor households (based on a poverty indicator constructed from income data) conditional on children attending school and good health practices. They consider a structural equation of the same form as Equation 3.5 for the sub-sample of children from ineligible households. The dependent variable is an indicator of schooling (change in schooling in Lalive and Cattaneo (2009)) for ineligible children i in reference group g, which is determined by a set of covariates similar to those found in (3.5) including peer group mean school enrolment behaviour from which endogenous peer effects are assumed to originate. Estimation of the latter hinges on the availability of a variable that can be used to instrument for group mean outcome while being excluded from second stage Equation 3.5.

In the context of PROGRESA, the program was gradually phased in starting with randomly selected villages. Therefore program availability makes for a good instrument in the spirit of a partial-population experiment as proposed in Moffitt (2001). Even if group assignment is not random, identification remains possible when there are policy variables that provide treatment to some individuals in the group without affecting directly the rest of its members. Effectively, Bobonis and Finan (2009) use the availability of PROGRESA in a village as an instrument for peer group average school enrolment, and Lalive and Cattaneo (2009) use the share of eligible group peers in treated villages. The resulting reduced-form first-stage equation is:

$$\bar{Y}_g = \alpha X_{ig} + \beta \bar{X}_g + \gamma V_g + \delta T_g m_g + \epsilon_{ig} \tag{3.9}$$

where T_g is a dummy for availability of threatment in group g and m_g is the share of individuals eligible to treatment in group g. Equations 3.5 and 3.9 provide the two stages of a linear instrumental variable estimation that identifies endogenous social interactions.

We follow a similar strategy to evaluate whether spillover effects from microfinance programs stem from endogenous peer effects, although the weaknesses of our data do not offer as clean a design a PROGRESA evaluation data. Our dataset contains data on 1,638 households successfully interviewed three times in 1991-92 and once again in 1998. We focus on the sub-sample of households ineligible to microfinance, and further exclude those who obtained microfinance loans nonetheless because of mis-targeting. We estimate our regressions on both waves of data separately, following the previous discussion which only considers the cross-sectional case.

Our instrument for village-level average behaviour is the share of eligible households in program villages (i.e. $T_g m_g$ in Equation 3.9). Therefore, we assume that the availability of microfinance programs affects ineligible non-participants only through its influence on the behaviour of the population that are able to take up treatment, i.e. eligible households. Strictly speaking, both the share of eligible households in the village and its interaction with the program placement dummy enter our first stage equation while being excluded variables from the second stage.

Note that at the time of the first round of interviews, 72 villages were selected based on the availability of a microfinance program for at least three years, and 15 villages had no program. By the second wave of interviews in 1998-99, however, all villages had access to microfinance borrowings, so there is only one excluded instrument in our regressions on the second wave of data. Regressions using the first wave of data where control villages exist include the program placement dummy as an extra regressor in the second stage in order to account for unobserved factors that potentially differ systematically between program and control villages. The eligibility status of a household is taken from census and survey data, from which we compute the proportion of eligible households in the village.

Our specification also includes contextual variables alongside village covariates to control for other sources of social interactions and isolate endogenous peer effects. With consumption outcomes as dependent variables, regressions include village-level averages of the following household covariates: education, gender and age of the household head; landholdings; the economic dependency ratio; and the three indicator dummy variables for the occurrence of borrowing from sources other than microcredit (banks, informal lenders and relatives). In the case of educational outcomes, the latter three dummy variables are not used to construct contextual variables, however the latter include village averages for the highest education level of any female in the household and its counterpart for males.

Finally, note that we assume neighbourhood peer effects at the village level which implies to exclude household i when computing group mean outcome (Bobonis & Finan, 2009; Moffitt, 2001). That is, the endogenous regressor used to capture peer effects in group g is constructed as follows:

$$\bar{Y}_{-ig} = n_g^{-1} \sum_{j=1, j \neq i}^{n_g} Y_{jg} \tag{3.10}$$

where n_g is the size of group g.

4.3 Results

4.3.1 Consumption outcomes

Table 3.8 **Endogenous peer effects from microfinance programs on the consumption of ineligible households**

	1991-92			1998-99		
Measure of HH per capita expenditure	**Total**	**Food**	**Non-food**	**Total**	**Food**	**Non-food**
Village-level mean outcome	0.796*	0.832	0.805**	1.820	0.996	3.861
	(1.747)	(0.921)	(2.513)	(1.484)	(0.564)	(0.664)
First stage:						
F-statistic	3.015	0.413	6.440	1.707	0.940	0.312
p-value	0.054	0.663	0.002	0.197	0.336	0.578
Sample size		246			186	

Note: Endogenous peer effects from microfinance programs on ineligible households. Village-level mean outcome is instrumented by the share of eligible households in program villages. The bottom two rows show the F-statistic and p-value from first stage regressions. Standard errors are clustered at the village level, absolute robust t-statistics in parentheses. Significance levels: *** 1%; ** 5%; * 10%.

Table 3.8 reports estimates of peer endogenous effects from microfinance programs on consumption outcomes. We find that in 1991-92 a 1% increase in total per capita expenditure of village peers leads to a 0.8% increase in household total per capita expenditure for ineligible non-borrowers, the estimate being statistically significant with 90% confidence. The first-stage F-statistic is quite low but there is evidence that our instruments have explanatory power. What our empirical model essentially captures is that the tendency of non-borrowers to follow the average consumption behaviour of households in the village is significantly related to the number of eligible households in their environment. In other words, the number of households eligible to microfinance in the village influences average peer consumption, which has a feedback effects on the consumption of ineligible households through social interactions.

The estimated peer endogenous effect is of similar magnitude in the case of food consumption but statistically insignificant, and statistics from the first stage stage suggest poor explanatory power of our set of instruments. The case of non-food per capita expenditure is where our instruments seem to perform best. The p-value for the first-stage F-test is below 1%, and our estimate suggests that a 1% increase in peer village average non-food consumption induces ineligible households to increase theirs by 0.805%, with 95% confidence that this impact is statistically meaningful.

Results from the second wave of data also show positive endogenous peer effects on all measures of household expenditure. For instance, a 1% increase in average peer non-food consumption is estimated to boost that of ineligible households by 3.8% through social interactions. However, but none of the estimated peer endogenous effects is statistically significant, and first-stage regression statistics actually suggest that our instruments do not fulfil their purpose as well as they do in the 1991-92 data, possibly because of the lack of control villages in 1998-99.

Overall, we find only mild evidence in support of the idea that consumption spillover effects from microfinance programs can stem from social interactions. Findings from the first wave of data indicate that if such effects exist, they influence mostly non-food expenditure. This is in line with our program impact estimates which unravelled that the observed substantial consumption gains from microfinance loans to women could be almost fully imputed to increases in non-food expenditure. One possible interpretation of this result is to consider the existence of conformist or status-seeking behaviour in the communities under scrutiny. Indeed, our measure of non-food expenditure includes several items that can be considered as conspicuous goods, such as purchases of shoes and clothes, and expenses incurred for social or religious ceremonies. In a context where individuals concern themselves with how they fare relative to others, increased spending on visible goods can improve social status and ultimately yield greater welfare.

If eligible households who take up microfinance loans divert part of their extra resources to status-conferring expenses, it might compel others in the community to do so. Of course, microcredit initiatives target the poorest of the poor, so the ineligible households in our estimation sample are likely already better off. Nevertheless, some rank-based theories of status suggest that if welfare improves with one's relative position in the distribution (her rank) it is also affected by the local shape of the distribution (Hopkins & Kornienko, 2004, 2009). In other words, status-seeking agents are better off the wider the gap with other agents in the relevant social space (Akerlof, 1997). Endorsing this view, it is possible that ineligible non-borrowers, no matter how better off, feel the need to increase their own conspicuous consumption to maintain their status which is "threatened" by a surge in spending on visible goods in the community after the inception of microfinance programs.

4.3.2 Schooling outcomes

Table 3.9 **Endogenous peer effects from microfinance programs on the education of children of ineligible households**

	Child currently enrolled in school (1=yes)				Proportion of children in the HH currently enrolled in school			
	1991-92		*1998-99*		*1991-92*		*1998-99*	
Dependent variable	**Girls**	**Boys**	**Girls**	**Boys**	**Girls**	**Boys**	**Girls**	**Boys**
Village-level mean outcome	-0.168	2.089***	-5.014	-2.729	-0.491	1.567***	-4.506	0.347
	(0.168)	(2.590)	(1.607)	(0.063)	(0.293)	(2.649)	(1.051)	(0.314)
First stage:								
F-statistic	2.143	3.484	2.964	0.016	0.721	3.869	0.908	2.905
p-value	0.124	0.036	0.092	0.901	0.490	0.025	0.346	0.095
Sample size	248	303	201	207	158	169	117	125

Note: Endogenous peer effects from microfinance programs on ineligible households. Village-level mean outcome is instrumented by the share of eligible households in program villages. The bottom two rows show the F-statistic and p-value from first stage regressions. Standard errors are clustered at the village level, absolute robust t-statistics in parentheses. Significance levels: *** 1%; ** 5%; * 10%.

We try and assess the extent to which microfinance programs can impact school attendance of children from ineligible non-borrowing households through social interactions. Results of this endeavour are presented in Table 3.9. In the 1991-92 data, we find that girls from ineligible households are less likely to be enrolled in school when the village average enrolment rate of girls increases, but this impact is insignificant.

On the other hand, we observe strong and significant positive endogenous peer effects on the probability of schooling for boys in ineligible households. Recall that in subsection 3.4 results from Table 3.7 shows a negative indirect impact specific to the non-borrowers that only mitigates an otherwise positive (and larger in magnitude) overall positive spillover effect on boy schooling. Table 3.9 suggests that this positive spillover effect to boys schooling in non-borrowing households does exist in 1991-92, and that it works through social interactions.

Endogenous peer effects on boy schooling are insignificant in 1998-99, and are of opposite sign depending on the chosen outcome. There is tentative evidence that our instruments are stronger when the endogenous regressor is the village average enrolment rate of boys age 5 to 18 in 1991-92. However, the relevance of the instruments is questionable in the 1998-99 estimates. The measured endogenous effects are large and negative but too imprecise to be distinguishable from zero.

The right panel of Table 3.9 shows similar results when educational outcomes are measured at the household level. Endogenous peer effects on girls schooling are consistently negative, and very large in 1998-99, albeit not statistically significant. Our instruments have low explanatory power when the endogenous average peer outcome under consideration is girls education. They do a better job in the case of male education and we find again a positive and significant increase in the probability that a boy age 5 to 18 from an ineligible household be enrolled in school as a result of higher peer average levels of schooling. The estimates for 1998-99 are still insignificant, although the endogenous peer effects on male education are positive while the estimated peer effects for girls remain negative.

With the exception of social interaction effects on the schooling of boys in 1991-92 which are positive and significant, it is hard to put much faith into the rest of the results. Endogenous peer effects on boys schooling are also the specifications for which our instruments work best, except for binary educational outcomes in 1998-99 where we find the only negative estimate of such effects (insignificant). Based on descriptive statistics from Table 3.1 in sub-section 3.2, we see that schooling of boys in borrowing and eligible non-borrowing households increase dramatically over time. The estimated spillover effects would suggest that the ineligible population followed this trend and decided to also send more boys to school, even though boys schooling was already higher in that population than in other sub-groups.

4.4 Limitations

Our attempt at measuring endogenous peer effects on ineligible households in villages with microfinance programs suffers several limitations. First, the sample size is very small due to our focus on this particular population because eligible households were over-sampled in the study to maximize statistical power. We have only little over 200 observations in 1991-92 and less than that in the second wave of data. One option to drastically increase sample size is to consider program non-participants instead of just ineligible, but we feel it would only raise more problems.

Indeed, non-participating households fall into two categories: ineligible and eligible to group lending. The introduction of the latter invalidates the proposed identification strategy that uses variations in the proportion of eligible households at the village-level to instrument for peer average outcomes. It is likely that the mere availability of a new credit source itself directly influences behaviours in the eligible population. Household decisions might start to evolve based on the knowledge that extra resources are potentially accessible in the near future.

Second, we do not control for unobserved village characteristics that potentially matter.

The introduction of group dummy variables in the cross-sectional linear social interactions model renders identification impossible (Blume and Durlauf (2005)). There is, of course, the possibility to use a fixed approach on panel data (Blume et al. (2010)), but the problem is that sample composition evolves between 1991-92 and 1998-99. Some households see their eligibility status change, while an ever greater proportion of ineligible households report having taken up microcredit loans in the second wave data. This implied having to restrict the estimation sample only to those ineligible non-participants whose status remains the same over time, resulting in hardly more than 100 observations.

Finally, the study design makes control villages available only in 1991-92. This can be an explanation why our identification strategy of endogenous peer effects is stronger in the first wave of data than in the second one.

5 Conclusions

The chapter discusses the importance of spillover effects in program evaluation and establishes their relevance in the context of microfinance programs. The sparse literature that undertakes to measure indirect program effects pertaining to microcredit largely focus on the competition shock on rural credit markets provoked by the entry of new players, namely microfinance institutions. More specifically, theoretical models and empirical evidence offer the result that informal lending rates increase in the locality when microfinance becomes available. Some researchers, such as Demont (2016), clearly recognise in this phenomenon a negative impact from microfinance programs that spills over to non-participating households who still need to rely on informal sources of finance and now do so at a higher price. Other approaches envisage the general equilibrium effects of microfinance from a macroeconomic perspective, and show an improvement in economic conditions regarding wages (Batbekh & Blackburn, 2008).

One channel of spillover that is particularly relevant to microfinance interventions is that of social interactions. Through the existence of peer endogenous or exogenous effects, programs can have much broader impacts than only the intended benefit if the treatment to the treated population, as has been shown to be the case for instance in PROGRESA conditional cash transfers program. The premise at the foundation of our expectation that such effects can arise is the well documented interconnectedness of rural communities in developing countries. The poor interact to share resources, to pool risk, to exchange information and transmit technological knowledge. Microfinance as a new means of insurance, income-smoothing device and welfare enhancing policy intervention is bound to have broader impacts.

The chapter first provides a more basic program evaluation studies in line with previous studies using the same data. We estimate that a 10% increase in microcredit boosts per capita total consumption by 0.37%, and by up to 1.06% in the case of non-food expenditure. Only the latter effect is robust to implementing an IV approach, and it appears that the observed positive returns from microcredit mostly pertain to loans issued to women. Our estimates are larger in size that those found by Khandker (2005) using the same data, possibly because the variables used in this thesis were built from the raw data and the econometric applications use different sets of covariates and slightly different specifications.

The current study adds to the literature by considering educational outcomes as binary or fractional dependent variables in panel data estimation, with an application of pooled probit regressions with instrumental variables on quasi-experimental data to study the non-monetary impacts of microfinance. We obtain consistent estimates that show women's credit to be beneficial for girls and boys schooling alike, while men's credit is detrimental to both. However, these results are almost never significant, and they are sensitive to the implementation of our IV approach which indicates positive education gains for both girls and boys stemming from male microcredit.

Then, the chapter unravels partial evidence of spillover effects, especially in terms of consumption. Village-level spillover effects on household expenditure thanks to microcredit are positive and large, with an extra positive spillover effect from women's borrowings to the non-borrowing population. Our final set of results confirms that the non-borrowing population benefits indirectly from microfinance in terms of consumption, most notably in terms of non-food expenditure, and that these benefits stem from social interactions with households who are eligible to microfinance, although our estimates are significant only for 1991-92.

Evidence on educational spillover effects is more scarce, but results suggest an overall positive spillover effects to boys schooling that is slightly smaller for the non-borrowing population. These spillovers too are found to come from social interactions in 1991-92, while results on 1998-99 data stay mute.

Overall, the chapter succeeds in establishing the existence of indirect effects from microfinance programs on household non-food consumption, as well as benefits to boys in non-borrowing households thanks to social interactions with households targeted by microfinance programs.

Chapter 4

Need for insurance and the impacts of microfinance on household vulnerability

1 Introduction

The aim of microfinance initiatives is to broaden the access to financial services for the poorest of the poor. Its original component, microcredit, is therefore thought to be a crucially useful device in unlocking investment opportunities. Ultimately, the hope of microfinance advocates is that credit-boosted income-generating activities will flourish, thereby increasing resources for the household in the long run. This effect is expected to ripple to outcomes usually related to welfare in the mind of economists such as consumption, education and social empowerment. As such, the success of microfinance is often judged on its capacity to tackle poverty.

The state of affairs is that, since the unequivocal findings of Pitt and Khandker (1998) advertising magnificent consumption returns to microcredit when issued to women, empirical evidence has accumulated to this day to provide a rather mixed set of evidence (Banerjee, 2013). One dimension that is often overlooked in evaluation studies of microfinance programs is that of household vulnerability to risk. Only a handful of studies have attempted to investigate the link between microcredit and household vulnerability (Morduch, 1998; Swain & Floro, 2012). Although the concept relates to poverty, it is different in nature in that it is the probability of falling into (or further into) poverty in the near future. The latter is intimately linked to the capacity of households in developing countries to cope with adverse shocks, especially in rural areas where traditional credit and insurance markets fail to provide reliable safety nets (Conning & Udry, 2007).

Informal risk-sharing contracts amongst rural households and their efficiency to help cope with idiosyncratic shocks have been extensively studied in the literature. Because of incomplete credit and insurance markets, the indigent in poor countries rely heavily on social networks and family ties as risk-mitigating devices. In spite of facing hardship and unkind environments, the poor usually manage quite well to achieve a satisfying degree of income and consumption smoothing. That being said, it seems natural to envisage that access to a new source of finance in that context can reshape the landscape for insurance and offer new opportunities for income diversification and income smoothing. Hence, microfinance programs can be expected to play an important role in the lives of the poor as to how well they insure against risks.

After defining vulnerability more precisely, section 2 of the chapter examines tentative evidence that households in the study sample are potentially vulnerable to seasonal income shocks and to idiosyncratic shocks in general, such as illnesses. Section 2 concludes by a brief review of the microfinance literature that undertook to study this topic. In section 3, a precise empirical methodology is described to measure vulnerability, and the choice of the method is discussed and defended. Finally, poverty and estimated vulnerability profiles are commented in section 4 before turning to the impacts of microfinance on household vulnerability. Section 5 concludes.

2 Definition of concepts and descriptive evidence

2.1 Definition of vulnerability

A general definition of vulnerability can be given as the likelihood that an individual will experience a level of welfare below some pre-determined threshold (Hoddinott & Quisumbing, 2010). Conceptually, there are no restrictions on the scope of welfare measures and time horizon in the definition of vulnerability. In the context of developing countries, it is often seen as the probability of falling into poverty, and so the welfare measure mentioned in the definition would be consumption, and the poverty line would be the benchmark. It is important to note the difference between two related concepts: vulnerability is an *ex ante* appreciation of some event yet to come, while poverty can be observed at any given point in time, i.e. it is an *ex post* measure. Chaudhuri, Jalan, and Suryahadi (2002), who propose the aforementioned distinction, offer to consider that "the observed poverty status of a household is the ex-post realization of a state, the ex-ante probability of which can be taken to be the household's level of vulnerability". This view implies that vulnerability is relevant only for sub-groups of the population that are considered non-poor, i.e. those for whom the risk of becoming worse off exists (Kamanou & Morduch, 2002).

Vulnerability as expected poverty is not the only approach in the literature. For instance, Ligon and Schechter (2003) argue that it is not a good representation of a household's aversion towards risk. They propose an alternative measure, coined vulnerability as low expected utility, by imposing a functional form on utility to define a certainty-equivalent level of consumption above which a household is not considered vulnerable, then comparing the expected utility of consumption to that benchmark to measure the degree of vulnerability (Hoddinott & Quisumbing, 2010). This approach further allows for a decomposition of vulnerability into expected poverty and two components of risks, namely idiosyncratic and aggregate risk.

Therefore, household vulnerability relates to the ability to insure against unexpected adverse shocks. This third approach, coined vulnerability as exposure to uninsured risk, is used in Tesliuc and Lindert (2002) as a backward looking assessment of vulnerability that considers the contributions of covariant and individual-specific shocks to the variability of individual consumption. This concept relates to full insurance models studied for instance in Cochrane (1991) and Townsend (1994), stating that under perfect resource pooling within the community individual consumption should co-move with aggregate consumption only and be insensitive to transitory income shocks.

2.2 Sources of vulnerability

2.2.1 Lack of income diversification

Individuals living in rural areas of poor countries typically face risky environments. Engagement in agricultural activities implies being vulnerable to severe variations in income (Morduch, 1995). The ability to smooth income and consumption is then dependent on conditions on the local insurance market which hinges on informal contracts (Udry, 1994), and community-level resource pooling in order to mitigate idiosyncratic shocks (Townsend, 1994). Moreover, because a large share of the local population is usually also involved in the agricultural sector, adverse shocks that affect all crops in the area (such as extreme weather events) cannot be insured properly. This is one reason why the extended family is a crucially efficient device for risk-sharing, as strong kinship ties make it a reliable insurance network, while strategic marriages can be used with the objective to "place" one family member in a different locality that faces different states of the world (Rosenzweig, 1988a, 1988b).

In rural Bangladesh where rice is the main cultivated crop, the demand for agricultural labour can fluctuate greatly with sometimes dramatic consequences, especially during two precise lean seasons following the plantation of the Boro and Aman crops, in March-April and September-November of each year, respectively. Khandker (2012) studies the seasonality of income in the Greater Rangpur region in North-Western Bangladesh (one of the poorest regions in the country), where the period following the plantation of the Aman crops is when famines are the most likely to occur and the most severe[1]. Khandker (2012) points out that these famines are actually caused by the lack of financial resources rather than the unavailability of food. As a consequence, strategies for income and consumption smoothing include diversifying crops and also engaging in non-farm activities (Morduch, 1995).

Table 4.1 **Repartition of household labour supply by type of work, sample means**

	1991-92				**1998-99**	
	Program villages		Non-program villages		All villages	
Household labour supply	**Hours per month**	**% of total**	**Hours per month**	**% of total**	**Hours per month**	**% of total**
Waged employment	149.0	41.0	169.3	47.7	120.8	38.8
Non-agricultural work	80.5	22.2	104.5	29.4	61.2	19.6
Agricultural work	68.5	18.8	64.8	18.2	59.6	19.1
Permanent	5.9	1.6	4.7	1.3	2.8	0.9
Seasonal	62.6	17.2	60.1	16.9	56.9	18.2
Self-employment	214.5	59.0	185.9	52.3	190.8	61.2
Agricultural activities	122.1	33.6	123.4	34.7	76.0	24.4
Non-agricultural activities	92.4	25.4	62.5	17.6	114.8	36.9
Total	363.5	100	355.2	100	311.6	100
Number of observations	1,364		274		1,638	

Note: Weighted sample means of household labour supply by type of work. We distinguish between program and control villages in 1991-92 only, because all villages in the sample have access to microfinance programs in 1998-99.

Table 4.1 shows average characteristics of household total labour supply in the study sample for both waves of data[2]. Statistics for the full sample are driven by program villages given their over-sampling in our dataset (see Pitt & Khandker, 1998), and hence statistics are also calculated separately for program and control villages in 1991-92 to examine any systematic difference between the two groups regarding income diversification as a source of vulnerability.

A first observation is that household total labour supply is a bit larger in program villages (363.5 hours per month) than in control villages (355.2 hours per month). Households in program villages allocate a larger share of their labour supply to self-employment activities than their counterparts in control villages (59% versus 52.3%). This is likely a symptom of the availability of microcredit in the former group of villages given that microfinance loans are issued mostly for productive purposes and to favour the creation of small businesses. Household members spend more than half that time on agricultural activities, however those in program villages allocate more

[1] This time of year is known as *Monga* in this part of Bangladesh.

[2] We use a balanced panel dataset. Please see Chapter 1 of the thesis for details.

working time to non-farm enterprises than in control villages. Again, this is tentative evidence that access to group loans can help rural households diversify their activities and maybe mitigate vulnerability to seasonal income.

In both sub-samples between 18% and 19% of household labour supply is allocated to wage agricultural work, almost exclusively in the form of seasonal or casual work. Households in control villages allocate 29.4% of their labour supply to wage non-agricultural work (against 22.2% in program villages), most likely because they lack access to financial resources to start their own businesses, unlike those living in program areas. By 1998-99 all villages had access to microfinance programs, and the share of total labour supply allocated to wage employment falls to 38.8% across all villages. The time allocation to seasonal wage agricultural work is quite stable over time (19.1% in 1998-99) but households work less time as employees in the non-agricultural sector. More than 60% of household labour supply are allocated to self-employment activities, and about 60% of that time is invested into working on non-farm enterprises. Discrepancies in labour supply allocation between program and control villages in 1991-92 and its observed evolution over time tend to suggest that microfinance potentially mitigates vulnerability arising from the lack of income source diversification. Nevertheless, almost one fifth of labour supply is allocated to seasonal agricultural employment, so households in the sample could still be subject to large income variations.

2.2.2 Idiosyncratic shocks

Incomplete credit and insurance markets are typical in rural areas of developing countries, making it hard to cope with unexpected shocks, for instance shocks relating to health and illness. Asfaw and von Braun (2004) find that food and non-food expenditure are significantly negatively affected by illness shocks, while Skoufias and Quisumbing (2005) suggest that food consumption is probably well insured against adverse health outcomes. Ultimately, resilience to bad health depends on a household's ability to quickly activate risk-coping mechanisms in the face of hardship, one widespread strategy being the sale of livestock (Rosenzweig & Wolpin, 1993). Therefore, the proneness of households to encounter such adverse conditions can still be seen as a source of vulnerability: not all households can get help from relatives and friends, nor do all of them necessarily have enough livestock to sell.

Table 4.2 summarises information collected in 1991-92 about household members who were ill in the 30 days prior to the survey. Almost every household in either program or control villages reported at least one such occurrence. In about 40% of cases the individual had to stop her usual activities, the average length of the spell during which she could not work as a result of illness being 9 days in program villages and 10 days in

Table 4.2 **Health shocks reported by households in 1991-92**

	Program villages	**Non-program villages**
Number of households who reported at least one member ill in last 30 days	1,299	260
% of cases where household member had to stop working	43.2	39.9
Average number of days without working	9	10
Number of households who reported at least one member ill for over a month	571	133
Number of observations	1,364	274

non-program villages[3]. In an environment where access to savings facilities and credit is scarce, being off work for over a week can have dire short-term welfare consequences for the family. Finally, about half the households reported at least one member had been ill for over a month. Continued illness can preclude one from being a reliable income source meanwhile being supported by other income earners in the household. Additionally, long-term illness can generate unanticipated substantial and frequent medical expenses, for instance in some cases of chronic illness.

Interviews carried out during the second wave of data collection did not include similar questions about the health status of household members. However, they included a module regarding situations of distress encountered over the previous three years. A wide variety of distresses are considered including the death of the main income earning member of the household, loss or destruction of crops, loss of money, eviction from land, or damages to the house because of natural events[4].

Table 4.3 **Distress faced in the last three years reported by households in 1998-99**

Number of households who reported at least one distress in the past 3 years	901
Number of households who reported crop losses	297
Number of households who reported large medical expenses	353
Median amount of damage because of distress (1992 Taka)	3,370
Median amount of expenses incurred because of distress (1992 Taka)	2,696
Number of households who reported being unable to face distress in at least one case	290
Number of households in sample	1,638

[3] This information was asked for by the interviewers when the household member who was ill was at least 10 years old.

[4] The full questionnaire of the 1998/99 collection data can be accessed on the World Banks Microdata website at http://microdata.worldbank.org/index.php/catalog/1318/download/24079.

Table 4.3 shows that more than half the households in the sample reported facing at least one distressful situation over the past three years. Almost 300 households reported at least one instance of crop losses or destruction of crops, and 353 reported having incurred large medical expenditures[5]. The crucial monetary consequences of facing distressful situations are well represented by the median amount of damage incurred from such unexpected adverse shocks is Tk 3,370, while the median amount of expenses required to deal with the situation is Tk 2,696 (e.g. funeral expenditures). In comparison, Table 1.5 in Chapter 1 showed that median household per capita annual food expenditure is Tk 3,068, so facing an extreme event can cost almost as much money as is typically spent to feed one person for a year. Indeed, 290 households out of the 901 who faced an extreme event reported not being able to handle the distress. Along with tentative evidence about income source diversification and health shocks, this last observation reinforces the idea that there exists a need for insurance against idiosyncratic shocks for households in our sample.

2.3 Vulnerability in the microfinance literature

Policy interventions seeking to expand access to financial services for the poorest of the poor have typically been gauged on their effect in helping the indigent exit poverty. Burgess and Pande (2005) show that the expansion of rural banks in India has a significant impact in reducing rural and aggregate poverty, and Khandker (2005) claims that microfinance helps significantly reduce the incidence of extreme poverty in rural Bangladesh. However, little attention has been paid to the potential consequences of microfinance programs in terms of risk insurance and, ultimately, on household vulnerability to adverse shocks.

Numerous claims have been made that microfinance institutions fail to reach their alleged targets (e.g. Morduch, 1999a; Simanowitz, 2002, among others), i.e. the poorest of the poor, part of the reason being that there exists a trade-off between the depth of outreach and the financial efficiency of micro-lenders (Hermes et al., 2011). The topic at hand raises yet another question, which is to know whether microfinance programs reach the most vulnerable fringes of the population. Amin et al. (2003) use data from two villages in Northern Bangladesh and build on Townsend (1994) to construct a measure of vulnerability as the extent of risk-sharing a household can achieve within the village. Their results suggest that microfinance does actually reach the poor but seems to leave out the *vulnerable* poor.

[5] The selection of these two types of distresses is made so as to echo the previous sub-section on the risk associated to agricultural activities, and the previous paragraph on health outcomes in 1991-92

An early investigation of the direct impact of microfinance on vulnerability is Morduch (1998)[6] who finds that microcredit helps borrowers achieve better consumption smoothing. In a study of Self-Help Groups in rural India, Swain and Floro (2012) show that members of such initiatives are significantly poorer than non-members but not more vulnerable, with larger security benefits for longer-term members. Finally, Islam and Maitra (2012) evaluate the efficiency of microcredit as a risk-coping device when households face health shocks. In spite of weak evidence that microcredit is an efficient consumption-smoothing device in the short run, the study shows that microcredit recipients are better able to cope with health shocks without resorting to selling livestock – or to the least they have a lesser need to do so thanks to microfinance.

3 Empirical methodology

3.1 Measuring vulnerability

3.1.1 Conceptual framework

Vulnerability to poverty is measured in this study as it is in Swain and Floro (2012) based on the methodology developed by Chaudhuri et al. (2002) (CJS afterwards)[7]. Essentially, the vulnerability level v_{it} of a household i at time t is defined as the probability that the household will become poor in the next time period:

$$v_{it} = \Pr(C_{it+1} \leq z) \tag{4.1}$$

where the chosen welfare measure is consumption C and benchmark z is an adequate consumption poverty line. As CJS point out, this conceptual approach implies that the current vulnerability status of a household can only be estimated, but never observed, unlike current poverty status that can be assessed at any given point in time. This unusual feature is at the heart of the literature on measuring vulnerability. Therefore, one needs to define a framework that adequately accounts for inter-temporal and cross-sectional characteristics that determine consumption patterns.

CJS build their reasoning on the following premise: household consumption at time t depends on several factors such as income, wealth or uncertainty, whose realisations can themselves be seen as stemming from observed and unobserved household characteristics and from features of the local environment, i.e. aggregate-level characteristics.

[6] That study uses the three rounds of survey used to construct the first wave of data exploited in this thesis.

[7] This section draws heavily on their article.

A possible reduced form for household consumption is then:

$$C_{it} = c(X_i, \beta_t, \alpha_i, \epsilon_{it}) \tag{4.2}$$

where X_i are household characteristics at time t; β_t are parameters measuring the impact on consumption of economic conditions at time t; α_i is a time-invariant unobserved household effect; and ϵ_{it} captures idiosyncratic shocks that explain differences in welfare of households with similar characteristics. Combining equations 4.1 and 4.2 gives:

$$v_{it} = \Pr(C_{it+1} = c(X_i, \beta_{t+1}, \alpha_i, \epsilon_{it+1}) \leq z | X_i, \beta_t, \alpha_i, \epsilon_{it}) \tag{4.3}$$

Equation 4.3 formalises the idea that household vulnerability depends on the stochastic nature of consumption streams over time which in turn derive from household characteristics and economic factors describing the environment of the household. The authors point out that the above expression allows for a wide variety of household socio-economic characteristics and characteristics of the local environment as determinants of vulnerability. They design an estimation procedure that is relevant to data limitations usually found in cross-sectional survey from developing countries.

3.1.2 Econometric procedure

Measuring vulnerability as expected poverty as suggested by Equation 4.3 entails to at least estimate expected consumption and the variance of consumption[8]. Some assumptions are needed to achieve that from a single cross-section of data, starting with a functional form for the stochastic generating process of consumption proposed as follows:

$$\ln(C_i) = X_i\beta + \epsilon_i \tag{4.4}$$

where the dependent variable is the logarithm of per capita food expenditure, X_i is a set of household characteristics and ϵ_i is a disturbance term with mean zero capturing household-level shocks that affect consumption and hence generate differences in consumption between households who are observationally equivalent. The assumption in (4.4) that idiosyncratic factors are *i.i.d.* over time for each household leads to the assumption that there are no unobservable characteristics of consumption that are persistent over time, as would be the case for instance with serial correlation or household-level unobserved effects. Note that parameters β are no longer indexed by time. This is the second main assumption of the CJS approach, namely that economic conditions are relatively stable over time, at least to the horizon of prediction relevant

[8] This sub-section draws on Chaudhuri et al. (2002), Swain and Floro (2012) and Hoddinott and Quisumbing (2010).

to the chosen assessment vulnerability. The approach rules out uncertainty about future consumption as stemming from uncertainty about the future state of the economy.

Nevertheless, aggregate shocks need not be identically distributed across households. Relaxation of the assumption that the variance of disturbance term ϵ_i is the same for every household is one of the main contributions of CJS. In order to alleviate the constraint, one can specify a simple functional form for the variance of idiosyncratic shocks:

$$\sigma^2_{\epsilon,i} = X_i\theta \tag{4.5}$$

Then, estimates of β and θ are used to predict expected log consumption and the variance of log consumption respectively as:

$$\hat{E}[\ln(C_i)|X_i] = X_i\hat{\beta} \tag{4.6}$$

$$\hat{V}[\ln(C_i)|X_i] = X_i\hat{\theta} \tag{4.7}$$

Assuming a log-normal distribution for consumption, the vulnerability level of household i with characteristics X_i can be estimated by the following probability:

$$\hat{v}_i = \phi\left(\frac{\ln(z) - X_i\hat{\beta}}{\sqrt{X_i\hat{\theta}}}\right) \tag{4.8}$$

Where $\phi(.)$ is the cumulative density of the standard normal distribution. CJS propose to estimate β and θ via three-step feasible generalised least squares (FGLS), a method designed by Amemiya (1977). The estimation procedure consists in the following steps:

1. Equation 4.4 is estimated by OLS to predict residuals $\hat{\epsilon}_i$. Their square is used as a raw estimate of the variance of the disturbance term and used as a dependent variable in the following linear regression:

$$\hat{\epsilon}^2_{OLS,i} = X_i\theta + \nu_i \tag{4.9}$$

2. The predictions from estimating Equation 4.9 via OLS are used to transform the equation as such:

$$\frac{\hat{\epsilon}^2_{OLS,i}}{X_i\hat{\theta}_{OLS}} = \left(\frac{X_i}{X_i\hat{\theta}_{OLS}}\right)\theta + \frac{\nu_i}{X_i\hat{\theta}_{OLS}} \tag{4.10}$$

   Estimation of (4.10) by ordinary least squares yields an asymptotically efficient FGLS estimate of θ – denoted $\hat{\theta}_{FGLS}$ – and a consistent estimate of the variance of idiosyncratic consumption shocks given by:

$$\hat{\sigma}^2_{\epsilon,i} = X_i \hat{\theta}_{FGLS} \tag{4.11}$$

3. Finally, the original Equation 4.4 is transformed by dividing both sides by the estimated standard error of idiosyncratic consumption shocks (i.e. the square root of the left-hand side quantity in (4.11)):

$$\frac{\ln(C_i)}{\hat{\sigma}_{\epsilon,i}} = \left(\frac{X_i}{\hat{\sigma}_{\epsilon,i}}\right)\beta + \frac{\epsilon_i}{\hat{\sigma}_{\epsilon,i}} \tag{4.12}$$

Regression Equation 4.12 is estimated via OLS and the resulting coefficient $\hat{\beta}_{FGLS}$ is consistent and asymptotically efficient. Inference can be made by dividing the coefficients estimated standard error by the regression standard error. Finally, the FGLS estimates are used to predict expected consumption and consumption variance which are ultimately used to compute household-level vulnerability from Equation 4.8. The final measure of vulnerability used in CJS and Swain and Floro (2012) is an indicator variable equal to one if the estimated probability in Equation 4.8 is greater than a given threshold.

3.2 Justification for the choice of vulnerability measure

There are at least three broadly accepted approaches to measuring vulnerability. CJS propose to take it as the likelihood of falling into poverty in the next time period, Ligon and Schechter (2003) follow a similar reasoning using a utilitarian framework, and Tesliuc and Lindert (2002) view household vulnerability as failure or limited ability to insure against risk.

The choice to follow CJS is based on its easiness of conceptual interpretation and on its applicability to cross-sectional data. However the latter feature of that method comes at the cost of rather strong assumptions. Given the availability of two waves of data, one might wonder why we do not seek to apply a measure of vulnerability that exploit the benefits of observing the same unit twice. This could allow to account for unobservable household-level features that persistently affect idiosyncratic consumption shocks over time.

For instance, Pritchett, Suryahadi, and Sumarto (2000) extend the time horizon of the measure of vulnerability as expected poverty proposed by CJS. The authors point out limitations of their proposed methodology when only two waves of data are available, but the main point to be made here is that the dataset used in this thesis embeds two waves of data collection that happened 7 years apart. Controlling for time-invariant household unobserved effects would aim at correcting estimates of household-specific components of the variance of consumption capturing variations over a 7-year period. Then, a measure of vulnerability as expected poverty including said estimated heteroskedastic variance of consumption would be relevant to predict the likelihood of falling into poverty *7 years later*. Notwithstanding the merits of evaluating the long-term impacts of microfinance, a time horizon of 7 years is probably too long from a policy-making perspective. Estimating vulnerability from the two cross-sections in the dataset provides an assessment of the probability of falling into poverty in next year, which makes for a more relevant horizon from the standpoint of policy makers.

Vulnerability as exposure to uninsured risk can also be estimated from panel data by capturing the extent of risk-sharing achieved by each household within the village, an approach followed by Amin et al. (2003). The upshot is that long time series are needed to estimate individual-level regressions[9]. An alternative empirical strategy to measuring a similar concept can be implemented on cross-section data provided the researcher has access to variables that adequately capture idiosyncratic and aggregate shocks (Tesliuc & Lindert, 2002). Covariant shocks can even be controlled for through community-level dummy variables that are differenced out from the equation when panel data are available, as in Islam and Maitra (2012). The latter article considers health shocks as realisations of idiosyncratic risks. While the approach is appealing, the dataset under scrutiny in this thesis does not contain consistent measures of health outcomes to proxy idiosyncratic shocks, and income measures are usually deemed too imprecise in survey data from developing countries (Deaton, 1997). Indeed, as previously mentioned a questionnaire about illnesses of members of the household was administered in 1991-92 only. While some questions about distressful situations from the 1998-99 interviews relate to health issues and large unexpected medical expenditure, they are less detailed and report any such event that happened over the previous three years, whereas in 1991-92 households reported illnesses over the month prior to the survey.

Finally, a comparative study of various empirical strategies to estimate vulnerability provided in Ligon and Schechter (2004) shows that when the environment is stationary and there is no measurement error in consumption expenditure variables, then the estimator proposed by CJS is the measure of vulnerability that performs best.

[9] Amin et al. (2003) use monthly income and consumption data collected over a whole year.

3.3 Choice of benchmark and estimation of impacts

3.3.1 Choice of poverty line

Recall from Equation 4.1 that vulnerability as expected poverty is computed as the probability that consumption in the next time period will fall below a previously specified benchmark. Choosing the official national poverty line is a natural way to go (Swain & Floro, 2012). Poverty lines are typically computed using information from Household Integrated Economic Surveys (HIES), with each new round used to update the previously set poverty line. There are several methods used to compute poverty lines, two of which are the Direct Caloric Intake and the Cost-of-Basic Needs (CBN) methodologies. In Bangladesh, individuals who cannot manage to take in 2,122 Kcal per day are classified as absolute poor, while those who consume less than 1,805 Kcal per day are considered hard core poor (Ahmed, 2004). The CBN approach values the average consumption basket required to achieve the defined caloric intake, producing poverty lines in monetary terms. The higher daily caloric intake benchmark defines the *upper poverty line* (UPL) and the lower number of calories the *lower poverty line* (LPL).

Table 4.4 **Poverty lines in rural regions of Bangladesh (amount per capita per month, in 1992 Taka) and national-level headcount ratios**

	1991-92		1995-96		2000		1998-99*	
Geographic area	***LPL***	***UPL***	***LPL***	***UPL***	***LPL***	***UPL***	***LPL***	***UPL***
Rural Barisal	413	467	403.2	455.5	360	406.2	381.6	430.85
Rural Chittagong	438	541	426.1	526.5	383.8	474.1	405	500.3
Rural Dhaka	425	512	401.6	484.1	361.3	434.5	381.5	459.3
Rural Khulna	420	497	407.3	483.2	347.5	411.4	377.4	447.3
Rural Rajshahi Bogra Rangpur	426	487	382	436.7	336.3	383.8	359.2	410.25
Rural Rajshahi Pabna	459	540	436.7	514.3	386.4	455	411.6	484.65
Rural Sylhet	432	558	407.3	525.7	377.2	486.6	392.3	506.15
National-level headcount ratios (% of total population)								
Rural	44	59	39.4	54.5	37.9	52.3	38.7	53.4
Urban	23.6	42.6	13.7	27.8	19.9	35.1	16.8	31.45
Total	41.3	56.8	35.1	13.7	34.3	48.9	34.7	31.3

Note: Lower (LPL) and Upper (UPL) poverty lines for rural areas of regional Bangladesh, and national-level headcount ratios. The asterisk signifies that 1998-99 data are estimates, namely averages of 1995-96 and 2000 poverty lines.

One HIES was undertaken in Bangladesh in 1991-92, the first time period in our dataset, and the next ones were carried out in 1995-96 and 2000-01. Table 4.4 presents the upper and lower poverty lines for rural Bangladesh by region for the various areas represented in our sample. All amounts are food consumption per capita per month valued in 1991-92 using World Bank consumer price index data. Absent poverty lines for 1998-99, the average between the 1995-96 and 2000 poverty lines are used to compute vulnerability in the second wave of data. Note that poverty lines computed with the CBN method also include an allowance for non-food expenditure (Ahmed, 2004), and hence the dependent variable used to compute vulnerability indicators is the logarithm of household per capita total annual expenditure[10].

In the analysis, both the UPL and LPL are considered in order to provide a more comprehensive picture of vulnerability in the data, hence generating two measures of vulnerability, to moderate poverty and to hard core poverty, respectively. What then remains to be decided is the threshold at which households are deemed vulnerable. A common choice in the literature is 0.5, which means that a vulnerable household is one that has an estimated probability of falling into poverty in the next time period (our measure of vulnerability) greater than 50%. The final measure of vulnerability becomes an indicator variable.

Nonetheless, a threshold of 0.5 is likely much too low for our data, as will be confirmed later. Table 4.4 shows that incidence of rural poverty at the national level in Bangladesh in 1991-92 was close to 60%. Swain and Floro (2012) propose to use the observed poverty rate in the population as a relative vulnerability threshold, arguing that it is a good approximation of the average level of vulnerability in a group that does not face aggregate shocks. In line with this view, two thresholds are used in the present study: regional and national poverty headcount ratios. The resulting dependent variables are four binary variables, i.e. measures of vulnerability to moderate and extreme poverty, relative to the regional or national poverty headcount ratios. Note that our sample consists of rural villages, hence poverty headcount ratios in the rural population should be used. While the national headcount ratios for rural areas are available, we could not find such data at the regional level in every time period under study, and therefore we estimate poverty headcount ratios to rural regional poverty lines from our sample.

3.3.2 Estimation of impacts

The aim is to assess the impact of microcredit on household vulnerability to poverty. The latter is measured following CJS for each wave of data separately and is then used as a dependent variable in a panel data household fixed effects regression. In practice, it is performed via OLS on first differenced variables to take out household time-invariant unobserved heterogeneity, and village-level unobserved fixed effects are removed by the same token.

To check the robustness of our results we also use a richer specification and control for time-varying impacts of village fixed effects by re-introducing village dummy variables

[10] Monthly poverty lines are transformed adequately.

after taking the first difference. Inference relies on robust standard errors clustered at the household-level.

The only difference with fixed effects linear regressions run in Chapter 3 is that treatment variables are cumulative borrowings *in level*. Indeed, vulnerability is ultimately a binary outcome, therefore there is no need to take the logarithm of credit, which has the added advantage to avoid having to deal with the logarithm of zero for non-borrowing households. Please refer to the methodology section of Chapter 3 of the thesis for further details. All specifications, for the estimation of vulnerability and microfinance impacts alike, include the full set of household and village covariates used previously in Chapter 2 and Chapter 3 of the thesis. The details of the variables included in regressions as well as summary statistics can be found in Chapter 1.

4 Results

4.1 Poverty and vulnerability in the sample

The natural first step after computing vulnerability measures is to explore the overall levels of vulnerability in the sample under consideration. Incidence of poverty and estimated vulnerability are presented in Table 4.5. Poverty is extremely salient in the sample. Based on regional poverty lines, 89% of households in the sample are moderately poor in 1991-92 while 80% classify as hard core poor. Extreme poverty (i.e. using the LPL) is more present in the sub-sample of borrowers than for non-participating households, with headcount ratios of 84% and 76%, respectively. In that context, one in two non-borrowing household are classified as vulnerable, and over 60% of borrowers face a severe risk of falling into poverty in the next time period. Given that the incidence of rural poverty at the national level in 1991-92 is much lower than in our sample (59%), vulnerability gauged against this measure is very high: nine in ten households in the sample are vulnerable, with this proportion reaching a striking 97% for borrowing households.

For similar reasons headcount ratios and vulnerability levels are very high when considering upper poverty lines. Based on regional UPL almost 90% of households in the sample classify as moderate poor (92.8% amongst borrowing households), and over 60% of households are vulnerable (70% for borrowers). One observation is clear however: there are more vulnerable households in the group of borrowers than amongst non-borrowers, and the incidence of poverty is higher in the former sub-sample too.

In comparison, the 1998-99 data provide teasing results. Poverty reduces everywhere in the sample. About 70% of households in the sample are moderate poor and 60% are hard core poor, with very similar proportions in the sub-groups of borrowers and

Table 4.5 **Poverty profile and estimated vulnerability of households in the sample**

	1991-92			1998-99		
	Borrowers	**Non-borrowers**	**Full sample**	**Borrowers**	**Non-borrowers**	**Full sample**
At Lower Poverty Line						
Headcount ratio at regional poverty line	84.3	76.4	80.2	58.5	60.7	59.5
Vulnerability – Regional poverty level	61.8	50.4	55.8	31.6	35.9	33.7
Vulnerability – National poverty level	97.0	85.7	91.0	76.4	71.5	74.0
At Upper Poverty Line						
Headcount ratio at regional poverty line	92.8	85.4	88.9	71.4	70.7	71.1
Vulnerability – Regional poverty level	71.2	56.8	63.6	36.2	40.1	38.1
Vulnerability – National poverty level	98.1	87.3	92.4	74.6	69.6	72.2

Note: Poverty profile and estimated vulnerability of households in the sample. Expected consumption and consumption variance are computed for each cross-section of data separately, and used to measure vulnerability using the regional poverty line. Household vulnerability levels are then compared to regional and national poverty headcount ratios to assess the severity of vulnerability (regional headcount ratios are computed from our sample using rural regional poverty lines).

non-borrowers, even though the headcount ratio at regional LPL is now slightly higher for the non-borrowing population in our sample. Regarding vulnerability based on regional poverty level, it would seem that the sample counts fewer vulnerable households borrowers (31.6% at LPL and 36.2% at UPL) than vulnerable non-borrowing households (35.9% at LPL and 38.1% at UPL), while the pattern observed in 1991-92 persists when using national poverty as the relative vulnerability threshold. Overall, the vulnerability headcount ratio is much lower in 1998-99 (a decrease by at least 20% when using regional poverty levels), and the dispersion of vulnerable households seems to get tighter as shown by the much narrower gaps in vulnerability measures against lower and upper poverty lines. The drop in poverty and vulnerability is particularly spectacular for households participating in microfinance programs. Whether this tentative evidence of correlation actually points to causal impacts of microfinance programs on vulnerability is the topic of the next sub-section.

4.2 Microcredit to mitigate vulnerability

The regression results shown in Table 4.6 are unequivocal[11]. In OLS regressions with household fixed effects, the coefficient estimates on household total cumulative borrowings are systematically significant at least at the 5% level. They are consistently negative and of the same magnitude across the board, and suggest that on average an additional Tk 1,000 in microcredit reduces the probability of being classified as vulnerable by 0.2% and 0.23% when using the regional and national LPL, respectively. This impact is a reduction of 0.35% and 0.22% when using the regional and national UPL, respectively.

Table 4.6 **Impact of microcredit borrowings on household vulnerability to poverty**

	OLS FE				Time-varying village effects			
Vulnerability to poverty at:	**Regional LPL**	**Regional UPL**	**National LPL**	**National UPL**	**Regional LPL**	**Regional UPL**	**National LPL**	**National UPL**
Household borrowings	-0.020**	-0.035***	-0.023***	-0.022***	-0.023**	-0.041***	-0.026***	-0.024***
	(2.158)	(3.295)	(2.817)	(2.699)	(2.401)	(3.647)	(3.355)	(3.006)
F-statistic					1.929	1.413	2.361	2.109
p-value					0.000	0.014	0.000	0.000
Women's borrowings	-0.022**	-0.035***	-0.023***	-0.019**	-0.025**	-0.043***	-0.027***	-0.020**
	(2.280)	(3.224)	(2.579)	(2.205)	(2.556)	(3.625)	(3.283)	(2.421)
Men's borrowings	-0.011	-0.033	-0.026	-0.044**	-0.006	-0.026	-0.019	-0.047**
	(0.350)	(1.045)	(1.521)	(2.417)	(0.221)	(0.950)	(0.988)	(2.439)
F-statistic					1.924	1.416	2.358	2.110
p-value					0.000	0.014	0.000	0.000

Note: Impact estimates from fixed effects regressions with 1,558 households. 'UPL' stands for upper poverty line and 'LPL' for lower poverty line. Standard errors are clustered at the household-level, absolute t-statistics in parentheses. Significance levels: *** 1%; ** 5%; * 10%. The bottom two lines of each panel report the F-statistic and p-value associated to the test of joint significance of time-varying village effects.

Differentiating the impacts of microcredit by gender shows that the aforementioned effect is mostly driven by loans issued to women. The coefficient estimates on women's credit are significant with at least 95% confidence and show a reduction in the probability of being vulnerable by 0.19 to 0.35% for an extra Tk 1,000 in microfinance borrowings. Loans issued to men also seem to help reduce household vulnerability, but the estimated impact is significant only when vulnerability is measured against national poverty at the UPL, i.e. for moderate poverty. Table 4.5 shows that more households are considered vulnerable when using UPL compared to LPL – the former

[11] Because microcredit variables enter in level, the scale differential with the dependent variable that is binary generates estimates that are small in magnitude and hence regression coefficients displayed in the table are multiplied by 10,000.

being higher – and when using national instead of regional headcount ratios – the former being lower. Therefore, this significant coefficient suggests that men's microcredit helps reducing vulnerability for those households classified as vulnerable with respect to a harsher criterion, i.e. households 'marginally' vulnerable to moderate poverty assessed on a national scale. In contrast, women's microcredit helps reduce household vulnerability to both moderate and extreme poverty, whether measured at the national or regional levels.

Accounting for potential time-varying impacts of the unobserved village-level time-invariant heterogeneity does not affect the results, but rather strengthens them. Results from the F-test show that village fixed effects have jointly significant impacts that change over time. Household total and female microcredit are still exhibiting a strong significant negative impact on the probability of being vulnerable. It is slightly larger in magnitude that in the previous set of estimates and systematically significant with at least 99% confidence. On average, an extra Tk 1,000 in microcredit reduces the probability of being vulnerable to moderate poverty by 0.23%, and to extreme poverty by 0.41%, with respect to regional headcount ratios (the impact is around -0.25% when considering national-level poverty). Gender-based estimated impacts are also consistent with the previous specification, although slightly larger in magnitude for women's credit, especially so for vulnerability to moderate poverty (regional). The results confirm the efficiency of male microcredit in helping to mitigate vulnerability to moderate poverty when measured at the national level.

Overall, microfinance loans are beneficial in lowering the probability of being vulnerable across the board. The impact is typically larger on vulnerability to moderate poverty than that on extreme poverty (even in the case of male microcredit when it is not significant), in line with descriptive evidence that showed a sharp decrease of vulnerability in the sample over time, especially for borrowing households with respect to regional moderate poverty. It seems that these benefits arise most often when microcredit is issued to women. One exception is when vulnerability is measured against nationwide incidence of moderate poverty, as in that case microcredit issued to both women and men helps significantly reducing the probability of being vulnerable. Furthermore, the reduction imputable to male credit is twice as large as that due to female credit, suggesting that the former matters for marginally vulnerable households.

5 Conclusions and discussion

Poverty and vulnerability are often mentioned in discussions about developing countries, and while they go hand in hand they are two quite fundamentally different concepts. Vulnerability relates to how well individuals can cope with risk, and it is often overlooked in program evaluation studies of microfinance initiatives. This chapter aims at filling this gap and adds to the scarce literature by estimating the impact of microfinance programs on household vulnerability.

In our dataset, the first wave of data reveal that households allocate most of their labour supply to seasonal agricultural employment and farming self-employment activities, exposing them to potentially severe income variations due to the seasonality of the demand for wage agricultural work, and making them sensitive to extreme weather events. While better diversification of the labour supply towards non-agricultural activities is achieved in the late 1990s, tentative evidence from the surveys suggest that households in the sample are prone to reporting health shocks and other forms of distresses such as crop losses. This sum of tentative evidence establishes the potential existence of a need for insurance against adverse shocks for households in our sample. Unfortunately, survey data does not readily offer a measure of idiosyncratic shocks consistent across both time periods.

The chapter then seeks to find a measure of vulnerability that is consistent across time periods, and can be used to assess the impact of microfinance on household exposure to risk. Of the three broadly accepted methods for the empirical measure of vulnerability, two consider the latter as the likelihood of falling into poverty in the next time period, and it can potentially be computed for any measure of welfare relative to any relevant arbitrarily chosen benchmark. We follow the method developed by Chaudhuri et al. (2002) and estimate household vulnerability as expected poverty, which is forward-looking in nature. Combined with regional and national data on the incidence of poverty, our study shows that a large share of households in the sample can be classified as hard core poor. Furthermore, estimates on data from the early 1990s show that participants in microfinance programs are classified as vulnerable more often than non-participants. The poverty and vulnerability profiles of the population under scrutiny improve over time, and the incidence of poverty and vulnerability becomes comparable in both groups. Moreover, in 1998-99 vulnerability to moderate and extreme poverty occurs less often in the sub-sample of borrowers than for non-borrowers when it is measured with respect to regional standards of poverty. This finding further motivates the investigation about the role microfinance might have played in this drastic change.

We use linear regressions with household fixed effects – and alternatively time-varying village effects – to estimate the impact of microfinance borrowings on the probability of being vulnerable to falling into poverty in the next time period, using our cross-sectional measure of vulnerability as the dependent variable in a panel data analysis. Our results show a strong significant negative influence of microcredit on the probability that households be vulnerable. On average, an extra Tk 1,000 in microfinance loans can yield a reduction in the probability of being vulnerable of 0.2% to 0.41%, depending on the benchmark used to define vulnerability. More specifically, we find this impact to be larger for our measures of vulnerability to moderate poverty, especially so when regional poverty is used as the threshold. Gender-based estimates suggest that this decrease is mostly due to female microcredit which has a significant negative impact on every measure of vulnerability. Additionally, male microcredit is found to substantially reduce the probability of being vulnerable only when considering nationwide incidence of moderate poverty as the threshold, in which case the estimated impact is larger than that associated to female credit. This indicates that microfinance loans issued to men help mitigating the risk of falling into poverty for marginally vulnerable households, i.e. those households classified as vulnerable against a harsher criterion.

There are some limitations to our study. The choice of using a cross-sectional measure of vulnerability has already been defended. It is a way to obtain an indicator that can itself be used as a dependent variable to assess the impact of microcredit on vulnerability. One main issue is whether microcredit borrowings should be included in the regression specification for three step FGLS that provides the expected consumption and consumption variance predictions. On one hand it seems pointless to use credit as a predictor of a variable that it then tries to explain, on the other one cannot ignore the potential of microcredit to serve as a risk mitigating device and excluding it from the measure would seem arbitrary.

In their study on Indian Self-Help Groups and vulnerability, Swain and Floro (2012) include a dummy variable equal to one for households who are members of a microfinance group in their regressions. In their setting it is a treatment indicator. The authors then use Propensity Score Matching (PSM) techniques to compare mean outcomes of observationally similar households in the treatment and control groups. There is no such device in the present study. The use of PSM on the dataset used here would be ill advised given that control villages are not available anymore. The premise being that households who participate in microfinance programs are potentially different from non-participants, the reasoning extends to the distinction between eligible households and ineligible. In the second wave of data, there are only ineligible and eligible households left in the sample, all in program villages, and hence performing PSM would result in matching treated units mostly to eligible untreated units and rarely to ineligible households.

The other issue pertaining to the chosen measure of vulnerability is the assumption that the economic environment is overall stable, which is linked to the incapacity of using community-level dummy variables to control for time-invariant hidden features or, with cross-sectional data, to merely control for all heterogeneity at the village level. Indeed, the problem is that said dummies would then serve as predictors, and it is famously known that although they efficiently control for unobserved fixed characteristics the coefficient estimates attached to them are biased, and hence unfit for predictions. CJS are able to mitigate the issue by running regressions on data from different localities separately. This option is not available here given the limited number of observations per village in the sample.

Albeit being subject to empirical limitations, this chapter shows that vulnerability to risks is a relevant dimension along which to gauge the effectiveness of microfinance to help the poor not only alleviate themselves out of poverty, but to improve their prospects in risky environments. Our results are encouraging in that they show microcredit to be efficient in mitigating vulnerability to both moderate and extreme poverty.

Conclusion

After forty years of existence, microfinance initiatives are still amongst the most popular poverty alleviation policies implemented in the developing world. Quite surprisingly, the large body of empirical evidence relating to such practices that has accumulated over the past 20 years has yet to provide a clear and unequivocal picture of the true effect of providing financial services to the poorest of the poor.

The main goal of the thesis was to contribute to the debate in an original fashion by investigating potential welfare consequences of microfinance programs other than traditional assessments of average impacts on the borrowing population. We make three important contributions by: unravelling the distributional impacts of microfinance on household expenditure; assessing whether spillover effects from microfinance programs can arise through social interactions; and gauging the efficiency of microcredit as a tool to reduce vulnerability to poverty.

The motivations for the use of quantile regression techniques in Chapter 2 are laid out in part in Chapter 1 as they are inherent to the nature of the outcome variables of interest. Indeed, household per capita total, food and non-food expenditure all exhibit leptokurtic and right-skewed distributions, therefore questioning the reliability of simple measures of central tendency to investigate the welfare implications of microcredit. Consumption variables are not distributed similarly for borrowers and non-borrowers, the latter typically achieving higher levels of consumption on average but not necessarily at every point of the distribution. Additionally, the distribution of consumption appears to evolve over time not only in terms of location but also with respect to shape, strengthening the idea that one could expect the welfare impacts of microcredit to be potentially different across borrowers.

A large part of Chapter 2 is devoted to discussing appropriate econometric techniques, and hence we will refrain from doing so here. Let us just recall that our most sensible specifications perform quantile regression at every decile on by pooling both waves of data together and defining household correlated random effects. The first important finding from the empirical analysis in Chapter 2 is that microcredit yields consumption gains at every point of the distribution. To that extent it is in line with similar studies using quasi-experimental data. For instance, our estimated returns to an extra

Tk 100 in loans issued to women in terms of total annual expenditure for a household at the median of the conditional distribution are between Tk 12.9 to Tk 18.5, similar in magnitude to the marginal average effects advertised by Khandker (2005), i.e. between Tk 15 and Tk 21.

However, our second important finding shows that returns to microcredit are *heterogeneous*. Specifically, we measure that an extra Tk 1,000 in credit would yield between Tk 11 to Tk 14 per capita per year in total expenditure for relatively low-consuming households (i.e. at the bottom decile of the conditional distribution), while these benefits go up to between Tk 42 and Tk 61 for relatively high-consuming households (i.e. those at the top decile). That is, some borrowers reclaim *four times as much* benefits as others. Understandably, one would like to know which borrowers exactly enjoy the largest welfare gains. The drawback of conditional quantile regression is that we cannot answer that problem. But we can nonetheless draw crucial welfare implications from the uncovered heterogeneity, especially in light of our last finding.

Heterogeneous returns to microcredit are driven by the impact of the latter on non-food expenditure. Indeed, formal statistical testing invites us to accept that all categories of borrowers probably enjoy similar food consumption benefits, i.e. between Tk 6 and Tk 15 per capita per year for an extra Tk 1,000 in credit. On the other hand, the highest consumers of non-food items (relatively to their socio-economic characteristics) can experience returns to microcredit between 6 to 10 times larger than their relatively low-consuming counterparts. In light of the stylised fact that even the poorest of the poor in developing countries tend to have an incompressible level of non-food consumption, we argue that non-food expenditure is an important part of how households in our sample can signal social status. Then, in the spirit of social distance models, the higher the realised level of non-food expenditure, the higher the associated social welfare utility.

Additionally, our estimates are conditional in nature, and hence the highest consumption returns benefit to those who already consume relatively much compared to similar peers. A group of similar households is the relevant sphere in which social status matters and is determined. In a status-seeking environment, what matters is not only to rank higher than others in the distribution but also to leave them as far behind as possible. We conjecture that microfinance offers limited opportunities of upward mobility within a reference group, given that households at bottom quantiles of the conditional distribution will find similar households who already consume more than them to benefit from credit to a greater extent than they do. In that sense, microcredit can have inequality-sharpening consequences in terms of welfare derived from one's social status, even though impact estimates advertise positive gains throughout the distribution. We feel that such findings are crucial in bettering our understanding of the ramifications of poverty alleviation programs.

It is in a similar spirit that we set out to investigate the potential spillover effects of microfinance, and more precisely their nature. Chapter 3 shows that village-level spillover effects on household expenditure thanks to microcredit are positive and large, with an extra positive spillover effect from women's borrowings to the non-borrowing population. Evidence on educational spillover effects is more scarce, but results suggest an overall positive spillover effects to boys schooling that is slightly smaller for the non-borrowing population than for others. The raw estimation of village-level spillover effects does not allow however to disentangle between the mechanisms at play.

We are interested in knowing whether such consumption – and to a minor extent children education – spillovers can stem from social interactions, given that existing studies have typically focussed on the impacts of microfinance on market conditions as sources of indirect program impacts. Our final set of results confirms that the non-borrowing population benefits indirectly from microfinance in terms of consumption, most notably in terms of non-food expenditure, and that these benefits stem from social interactions with households who are eligible to microfinance. Similarly, boys from non-borrowing households are more likely to be educated as a consequence of the village-wide increase in the education of boys driven by the population of borrowers and eligible non-borrowers.

Finally, we explore one last dimension against which to gauge the efficiency of microcredit: household vulnerability. Chapter 4 introduces the idea that although its main focus is poverty alleviation, microfinance ought to be judged on its overall ability to help households improve their welfare. For instance, it could have risk mitigation properties that would be welcome in the risky environments typically faced by rural households of developing countries. Tentative evidence from survey data establishes the potential existence of a need for insurance against adverse shocks for households in our sample. To assess it in a more formal way, we follow the method developed by Chaudhuri et al. (2002) and proceed to estimate household vulnerability as expected poverty, which is forward-looking in nature. Combined with regional and national data on the incidence of poverty, our study shows that a large share of households in the sample can be classified as hard core poor. Furthermore, estimates on data from the early 1990s show that participants in microfinance programs are classified as vulnerable more often than non-participants. The poverty and vulnerability profiles of the population under scrutiny improve over time, and the incidence of poverty and vulnerability becomes comparable in both groups. This finding further motivates the investigation about the role microfinance might have played in this drastic change.

We then use linear regressions on panel data to estimate the impact of microfinance borrowings on the probability of being vulnerable to falling into poverty in the next time period, using our cross-sectional measure of vulnerability as the dependent variable. Our results show a strong significant negative influence of microcredit on the probability that households be vulnerable. On average, an extra Tk 1,000 in microfinance loans can yield a reduction in the probability of being vulnerable of 0.2% to 0.41%, depending on the benchmark used to define vulnerability. Gender-based estimates suggest that this decrease is mostly due to female microcredit which has a significant negative impact on every measure of vulnerability. Additionally, male microcredit is found to substantially reduce the probability of being vulnerable only when considering nationwide incidence of moderate poverty as the threshold, in which case the estimated impact is larger than that associated to female credit. This indicates that microfinance loans issued to men help mitigating the risk of falling into poverty for marginally vulnerable households, i.e. those households classified as vulnerable against a harsher criterion.

Altogether, in spite of important data limitations, findings from the three empirical chapters provide a clearer picture of what can be expected from microfinance programs. The thesis also succeeds in motivating the idea that the efficiency of policy interventions has to be gauged on multiple dimensions, and not only on their predetermined announced goals. What appear like positive results on the face of it can hide more complex socio-economic phenomena with potentially adverse consequences, as was the case with the unravelled heterogeneity in consumption gains from microcredit. Many conceptual and statistical tools are readily available that can easily help in providing more comprehensive evaluations of microfinance programs, and of policy interventions in general, than mere impacts at the mean.

Appendices

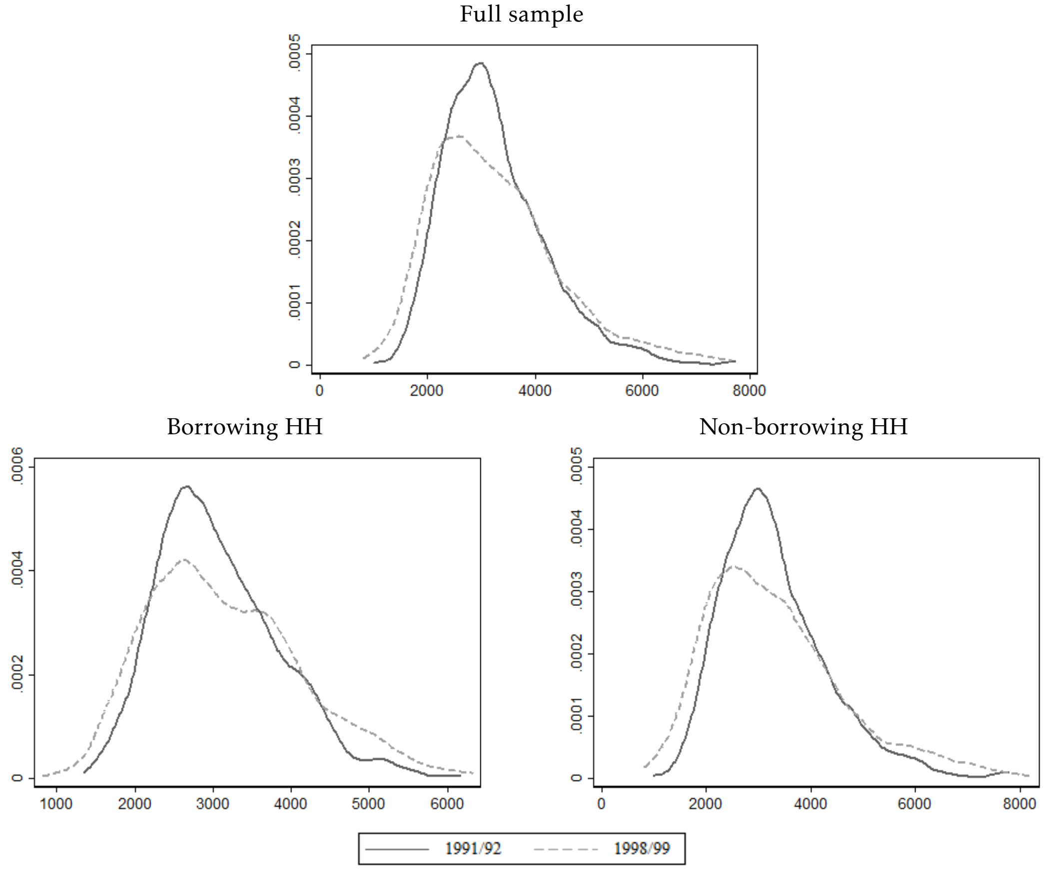
List and contents of appendices

Appendix A

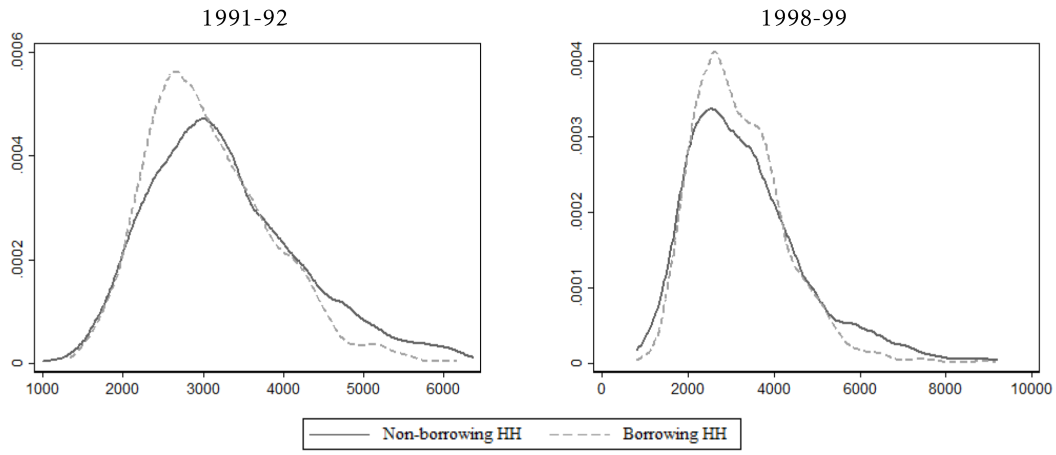
Kernel density estimates and quantile-quantile (Q-Q) plots for household per capita food and non-food expenditure

Figure A.1 **Kernel density estimates for household per capita food expenditure**

(a) Panel A: By borrowing status



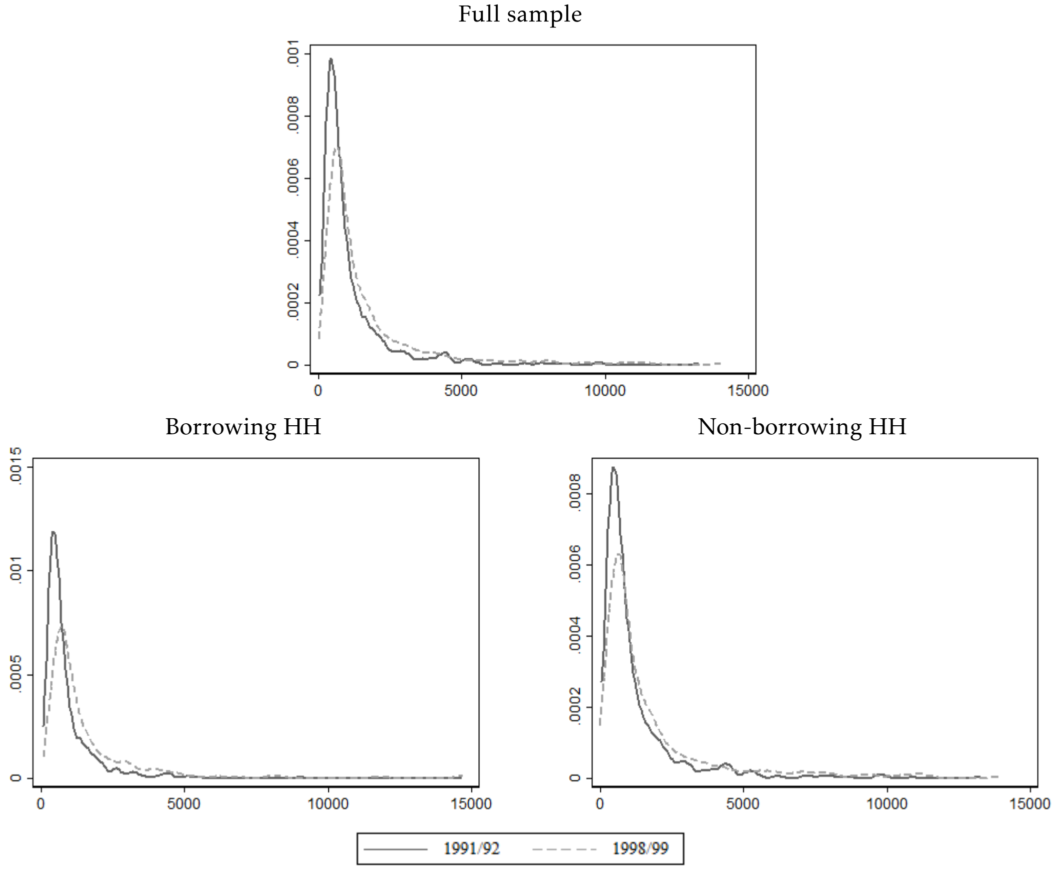
(b) Panel B: By time period



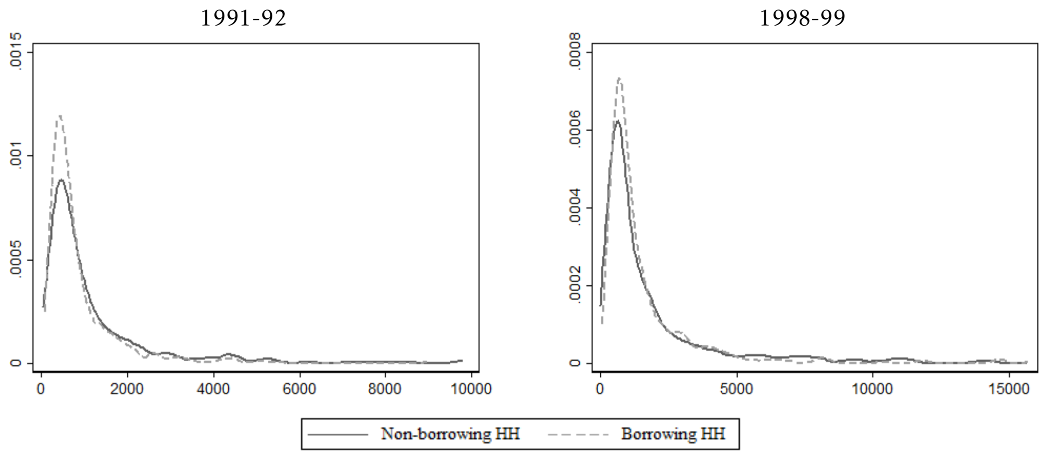
Note: Kernel density estimates using an Epanechnikov kernel function and sampling weights.Because of the long right tails, the top 1% observations of the grand distribution of the sub-sample considered in each graph are trimmed to improve visualisation.It does not hamper the overall shape of the density estimates.

Figure A.2 **Kernel density estimates for household per capita non-food expenditure**

(a) Panel A: By borrowing status



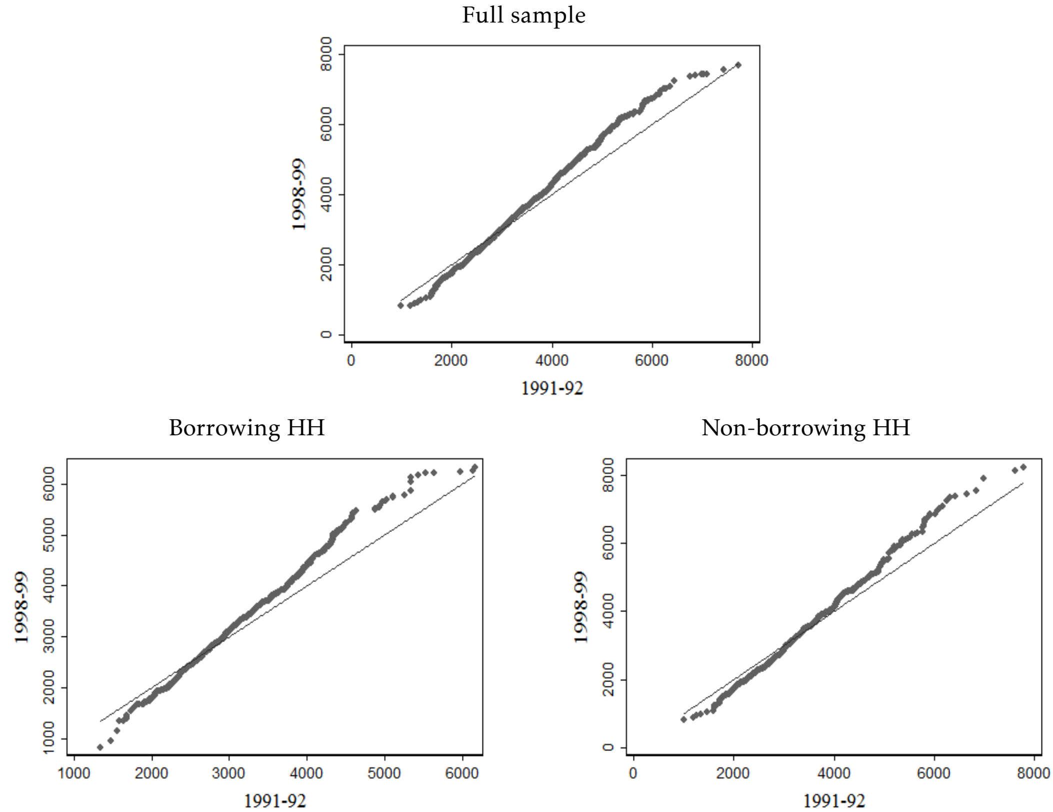
(b) Panel B: By time period



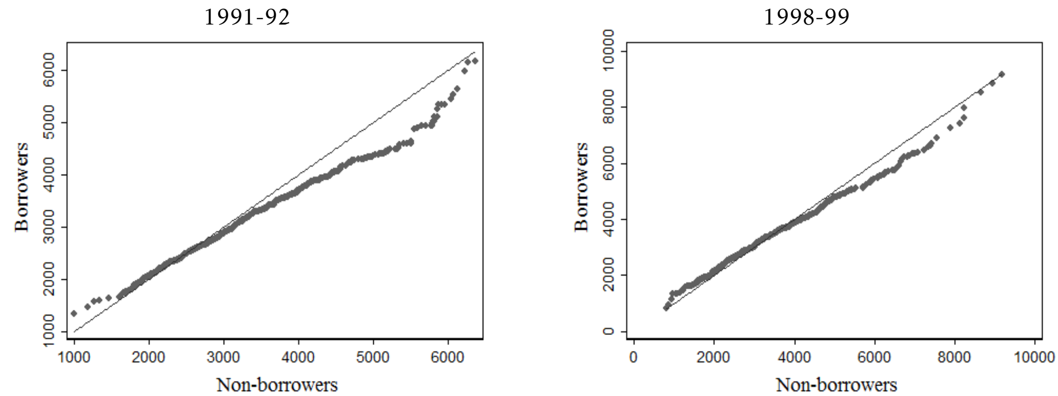
Note: Kernel density estimates using an Epanechnikov kernel function and sampling weights.Because of the long right tails, the top 1% observations of the grand distribution of the sub-sample considered in each graph are trimmed to improve visualisation.It does not hamper the overall shape of the density estimates.

Figure A.3 **Quantile-quantile plots for household per capita food expenditure**

(a) Panel A: Comparison across time, by borrowing status



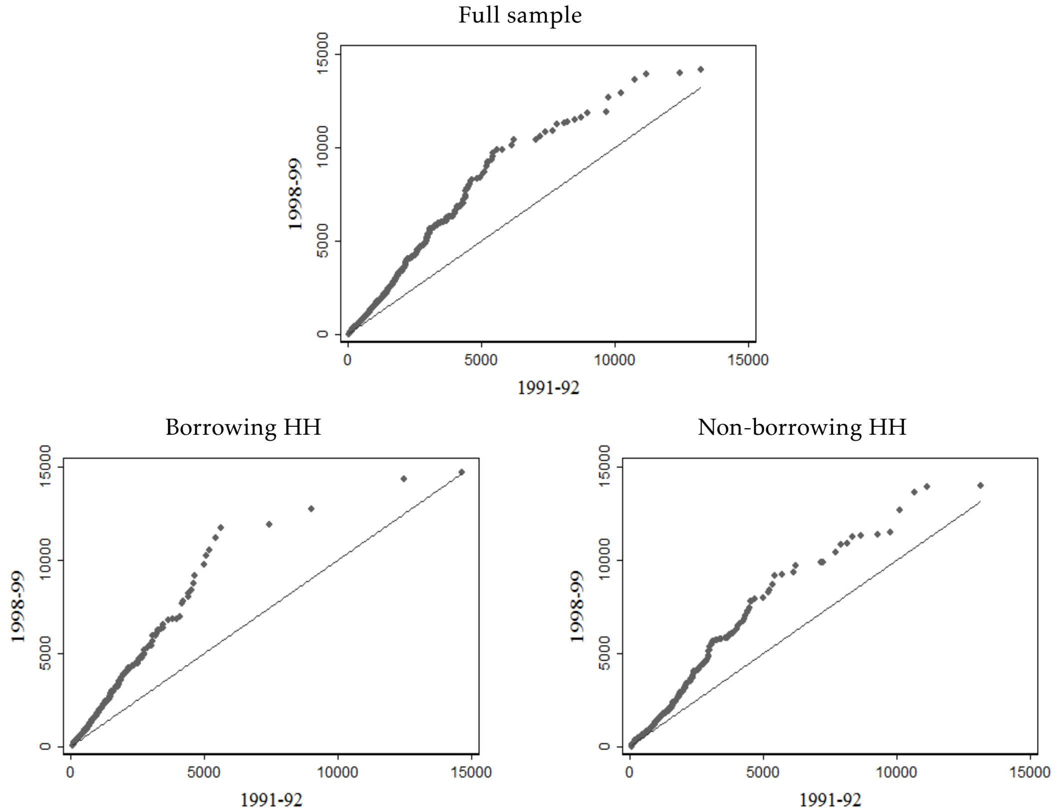
(b) Panel B: Comparison across borrowing status, by time period



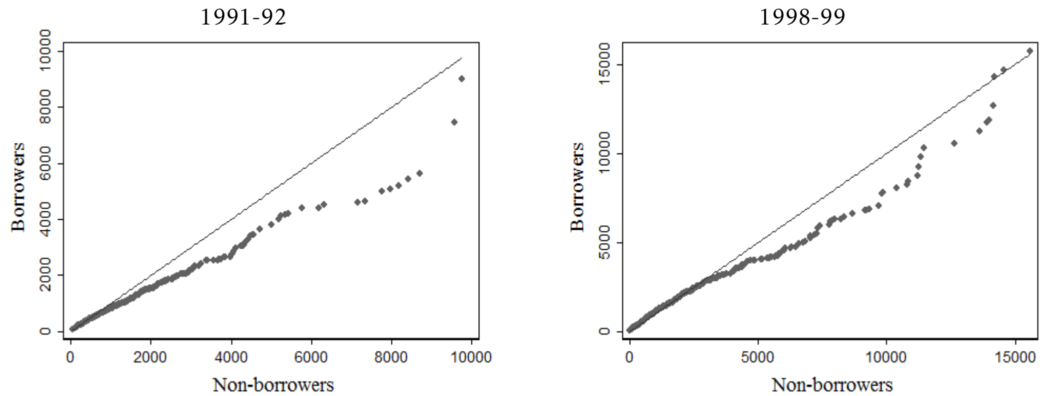
Note: Quantile-quantile plots. The straight line is the 'identity' line, i.e. the benchmark case in which both distributions are identical. Because of a few very extreme observations, the top 1% observations of the grand distribution of the sub-sample considered in each graph are trimmed to improve visualisation. It does not hamper the overall shape of the plots.

Figure A.4 **Quantile-quantile plots for household per capita non-food expenditure**

(a) Panel A: Comparison across time, by borrowing status



(b) Panel B: Comparison across borrowing status, by time period



Note: Quantile-quantile plots. The straight line is the ‘identity’ line, i.e. the benchmark case in which both distributions are identical. Because of a few very extreme observations, the top 1% observations of the grand distribution of the sub-sample considered in each graph are trimmed to improve visualisation. It does not hamper the overall shape of the plots.

Appendix B

Regression tables for cross-section quantile regression estimates

Table B.1 **Cross-section quantile regressions, household total expenditure**

	1991-92					1998-99				
Quantile	10%	30%	Median	70%	90%	10%	30%	Median	70%	90%
Household microcredit borrowings	0.006	0.005	0.007	0.006	0.025	0.010	0.014**	0.017**	0.028**	0.053**
	(0.005)	(0.005)	(0.006)	(0.008)	(0.016)	(0.005)	(0.004)	(0.007)	(0.011)	(0.016)
Household characteristics										
Education of HH head	-29.09	-21.21	25.56	139.8**	270.0**	29.79	49.79*	65.55*	107.1*	146.8
	(35.56)	(25.01)	(31.30)	(46.83)	(102.6)	(29.89)	(28.84)	(38.89)	(67.60)	(154.9)
Age of HH head	-9.139**	-11.50**	-9.184**	-9.398**	-6.937	1.644	-0.574	-7.823	-17.99	-20.43
	(3.606)	(3.950)	(4.468)	(5.118)	(8.471)	(4.911)	(4.412)	(5.892)	(7.985)	(14.12)
Gender of HH head	127.1	-88.74	-177.2	-42.80	40.86	112.9	-318.9	-164.5	-451.8	-2211.8
	(226.8)	(243.9)	(183.0)	(199.5)	(894.6)	(248.1)	(209.5)	(308.1)	(598.8)	(1147.0)
Highest education of men in HH	80.58*	82.38**	48.61*	-16.17	55.91	50.23*	70.12**	83.46**	121.7**	281.1**
	(33.12)	(22.48)	(25.86)	(36.29)	(76.61)	(26.48)	(19.53)	(24.98)	(41.64)	(104.9)
Highest education of women in HH	25.64	51.52**	74.27**	57.39*	47.86	51.01**	91.23**	137.8**	203.7**	342.7**
	(21.69)	(19.96)	(25.09)	(35.77)	(78.05)	(25.47)	(22.53)	(34.57)	(59.45)	(146.7)
Landholdings	1.152**	1.052**	1.051**	2.841**	3.719*	1.937*	1.887**	2.229**	2.685**	17.33
	(0.492)	(0.606)	(1.095)	(1.240)	(2.153)	(0.988)	(0.701)	(1.086)	(1.901)	(13.00)
HH economic dependency ratio	-104.7**	-123.7**	-180.5**	-240.9**	-225.8**	-95.98**	-166.8**	-220.4**	-295.1**	-293.4**
	(31.67)	(23.81)	(26.95)	(35.17)	(94.42)	(32.44)	(32.38)	(38.17)	(63.61)	(125.9)
# of HH head relatives owning land	25.03	15.36	21.62	10.94	28.19	21.78	36.06*	56.08*	66.01*	60.68
	(12.55)	(12.58)	(17.48)	(24.17)	(41.59)	(30.48)	(23.26)	(36.02)	(43.80)	(99.14)
# of HH head's spouse relatives owning land	-21.77	-4.451	-1.855	8.667	-14.00	49.64**	32.59**	9.876	29.24	-0.534
	(11.57)	(11.43)	(13.48)	(15.50)	(33.96)	(20.16)	(15.94)	(22.60)	(38.90)	(57.59)
# of HH head relatives living outside thana	0.0832	8.990	-23.19	-33.11	-48.76	-33.63	-17.55	-10.84	-40.89	-49.91
	(20.15)	(17.46)	(18.41)	(32.69)	(53.04)	(29.89)	(23.57)	(29.93)	(38.69)	(70.13)
# of HH head's spouse relatives living outside thana	0.867	-2.160	10.69	11.69	49.22	-3.116	-6.647	-5.804	-9.632	13.33
	(10.98)	(10.70)	(14.42)	(19.04)	(43.98)	(15.62)	(14.58)	(18.12)	(24.30)	(40.71)
Loans from traditional banks (1=yes)	428.0	324.3**	732.9**	870.5**	1578.3	363.1	452.9*	314.9	926.4	1952.8*
	(209.7)	(205.0)	(246.6)	(360.6)	(890.3)	(297.5)	(234.0)	(410.5)	(808.3)	(1928.3)
Loans from informal sources (1=yes)	-104.1	15.63	-42.25	396.1	440.3	523.0*	48.54	62.73	-316.6	-25.47
	(190.6)	(154.4)	(204.5)	(338.7)	(509.8)	(220.5)	(181.4)	(256.2)	(426.3)	(1938.3)
Loans from relatives (1=yes)	-30.75	117.8	78.23	83.31	338.3	-26.43	348.0*	567.3**	943.7**	1882.3**
	(200.1)	(174.6)	(162.3)	(299.6)	(744.7)	(220.8)	(191.0)	(261.6)	(398.0)	(787.5)
Eligibility of HH (1=yes)	-321.7**	-351.8**	-417.0	-269.0	-136.8	-318.4**	-717.3**	-816.2**	-1063.0**	-544.8
	(131.7)	(137.5)	(208.6)	(289.7)	(612.8)	(222.6)	(213.2)	(268.7)	(503.2)	(967.9)
Village covariates										
Average male wage	6.677	4.295	0.298	-1.356	47.44	-1.647	-4.236	-0.863	4.520	-20.42
	(9.684)	(9.101)	(9.943)	(11.90)	(42.98)	(7.769)	(6.638)	(9.849)	(12.79)	(23.92)
Average female wage	4.422	4.531	4.678	6.401	42.31	-0.471	-4.089	3.302	-8.997	18.25
	(6.352)	(5.679)	(7.977)	(10.21)	(32.20)	(7.854)	(8.672)	(11.95)	(16.90)	(30.50)
Primary school (1=yes)	-190.4	-121.1	-209.0	-178.1	62.29	135.9	112.8	-88.98	-528.0	-1557.6
	(120.2)	(135.9)	(163.7)	(178.4)	(379.3)	(317.7)	(304.6)	(533.2)	(644.6)	(1097.1)
Food program (1=yes)	-74.23	-141.7	-172.1	-333.9	-569.8	-225.1	-310.1*	-372.9	-373.8	-331.3
	(118.2)	(111.6)	(138.9)	(175.1)	(425.5)	(193.0)	(193.7)	(255.2)	(337.6)	(603.6)
Distance to nearest bank (km)	-12.98	-36.24	-40.35	-48.48*	-88.99	-17.37	-11.71	-19.50	13.71	-28.86
	(21.75)	(21.67)	(28.00)	(29.85)	(83.09)	(37.90)	(39.12)	(51.01)	(64.89)	(126.2)
Distance to nearest *pucca* road (km)	2.428	-10.14	-1.154	21.83	122.6	-61.38	-36.75	-92.41	-55.32	47.43
	(19.95)	(18.69)	(24.12)	(31.16)	(109.6)	(56.92)	(66.49)	(83.71)	(132.1)	(252.9)
Distance to nearest shop/market (km)	28.90	59.38	51.33	101.2**	80.78	41.62	0.382	28.72	87.19	-308.7
	(36.88)	(33.75)	(39.10)	(47.61)	(111.0)	(111.8)	(104.5)	(137.5)	(168.0)	(272.5)
Electricity in village (1=yes)	315.9	352.8**	405.8**	698.8**	1170.9**	482.3**	335.0*	424.9	270.6	973.8
	(122.1)	(126.9)	(158.7)	(184.8)	(561.4)	(202.8)	(211.9)	(291.6)	(382.9)	(710.1)
Price of rice	-73.93	78.61	35.44	-11.43	-41.59	70.66	108.4	44.14	107.2	213.3
	(83.03)	(83.74)	(104.3)	(130.8)	(268.2)	(67.93)	(71.47)	(103.5)	(128.3)	(248.4)
Price of wheat flour	57.74	34.10	72.59	162.4	153.4	-45.51	-180.4	-181.7	-224.0	-508.4*
	(74.42)	(79.17)	(98.34)	(115.1)	(283.7)	(114.9)	(109.4)	(143.2)	(181.9)	(351.3)
Price of mustard oil	8.626	1.640	3.753	-9.148	-84.25	0.0719	8.820	6.717	21.84	42.47
	(10.73)	(11.23)	(14.04)	(17.00)	(46.59)	(12.79)	(14.19)	(19.88)	(25.99)	(49.15)
Price of hen's eggs	-19.98	-15.17	-28.47	-76.59	-123.6	117.0	199.6	321.7	507.8	860.8*
	(142.9)	(162.4)	(272.4)	(370.5)	(579.2)	(210.3)	(205.3)	(285.2)	(389.2)	(728.5)
Price of milk	38.16	31.36	68.72**	99.20**	49.14	-1.120	4.216	12.13	-7.672	-75.02
	(31.35)	(30.79)	(38.23)	(42.03)	(101.6)	(31.80)	(29.14)	(38.24)	(52.46)	(86.48)
Price of potatoes	-27.22	41.71	-34.58	-26.62	-41.79	-6.677	11.32	-41.74	0.634	32.41
	(48.09)	(46.36)	(63.72)	(86.60)	(155.3)	(37.69)	(40.80)	(56.00)	(90.45)	(153.6)
Overall intercept	2246.3	1900.5	2556.6	2803.2	5789.0	1806.0	4040.4**	5603.1**	5985.0**	10847.9**
	(1144.0)	(1277.3)	(1423.9)	(1501.2)	(3704.1)	(1554.4)	(1468.0)	(1934.4)	(2783.7)	(5787.9)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table B.2 **Cross-section quantile regressions, household total expenditure, by gender**

	1991-92					1998-99				
Quantile	10%	30%	Median	70%	90%	10%	30%	Median	70%	90%
Female microcredit	0.008	0.008	0.008	0.004	0.015	0.011*	0.015**	0.018**	0.030**	0.054**
borrowings	(0.005)	(0.005)	(0.007)	(0.008)	(0.022)	(0.005)	(0.005)	(0.007)	(0.011)	(0.017)
Male microcredit	-0.001	0.001	0.003	0.015	0.026	-0.001	0.012	0.007	0.007	0.007
borrowings	(0.010)	(0.009)	(0.014)	(0.017)	(0.022)	(0.009)	(0.009)	(0.012)	(0.016)	(0.026)
Household characteristics										
Education of HH head	-31.51	-22.82	25.08	135.0**	262.5**	29.76	49.02*	67.50*	110.9*	150.6
	(35.78)	(24.86)	(31.42)	(47.08)	(101.3)	(30.01)	(28.98)	(38.52)	(67.09)	(155.3)
Age of HH head	-9.266**	-11.69**	-9.279**	-9.716*	-7.850	1.899	-0.787	-7.558	-16.05	-19.21
	(3.621)	(3.945)	(4.441)	(5.075)	(8.480)	(4.899)	(4.434)	(5.912)	(7.935)	(14.26)
Gender of HH head	196.5	-96.22	-197.6	-62.51	14.60	109.5	-313.3	-176.8	-396.3	-2045.3
	(228.9)	(243.1)	(183.1)	(199.0)	(892.2)	(250.2)	(207.4)	(309.8)	(605.5)	(1143.4)
Highest education of men in	86.28*	83.74**	49.59*	-16.26	56.81	50.37*	71.88**	84.97**	108.9**	288.4**
HH	(33.50)	(22.42)	(25.91)	(36.02)	(74.91)	(26.43)	(19.85)	(24.80)	(40.90)	(106.3)
Highest education of women	24.29	50.96**	73.37**	60.34*	52.23	54.01**	88.22**	136.1**	204.7**	343.5**
in HH	(21.76)	(20.06)	(25.04)	(36.14)	(78.27)	(25.48)	(22.77)	(34.68)	(59.55)	(146.4)
Landholdings	1.155**	1.053**	0.941**	2.857**	3.762*	2.004**	1.842**	2.188**	2.871**	16.70
	(0.495)	(0.602)	(1.098)	(1.238)	(2.156)	(0.979)	(0.712)	(1.150)	(1.894)	(12.98)
HH economic dependency	-103.2**	-122.5**	-180.6**	-236.6**	-231.5**	-99.77**	-172.1**	-224.8**	-297.3**	-289.4**
ratio	(32.14)	(23.70)	(26.37)	(35.28)	(94.75)	(32.69)	(32.40)	(38.32)	(64.49)	(124.8)
# of HH head relatives	24.31	16.36	21.61	9.871	29.79	24.75	36.45*	60.82*	64.27*	54.66
owning land	(12.54)	(12.48)	(17.61)	(24.10)	(41.41)	(31.00)	(23.64)	(36.49)	(44.55)	(100.2)
# of HH head's spouse	-22.76	-3.852	-1.349	8.324	-14.73	46.06**	35.77**	9.486	29.76	3.571
relatives owning land	(11.63)	(11.43)	(13.51)	(15.51)	(34.12)	(20.24)	(15.88)	(22.70)	(39.18)	(58.26)
# of HH head relatives living	0.926	8.108	-23.20	-31.64	-46.31	-36.70	-19.83	-11.18	-42.07	-35.92
outside thana	(19.85)	(17.31)	(18.45)	(32.60)	(53.35)	(30.14)	(23.67)	(29.52)	(38.77)	(69.52)
# of HH head's spouse	-0.00514	-2.414	10.59	11.69	49.34	-3.426	-5.693	-6.547	-10.98	12.39
relatives living outside thana	(10.78)	(10.67)	(14.26)	(18.83)	(43.91)	(15.70)	(14.68)	(18.07)	(24.24)	(41.63)
Loans from traditional	410.6	324.2**	708.9**	861.3**	1540.1	338.2	436.4*	322.9	864.6	1929.1*
banks (1=yes)	(211.0)	(207.1)	(246.0)	(360.5)	(890.0)	(296.4)	(235.9)	(414.8)	(800.6)	(1944.8)
Loans from informal sources	-116.5	15.79	-45.09	397.1	443.7	551.9**	67.72	91.83	-218.5	40.86
(1=yes)	(190.4)	(154.3)	(205.1)	(338.4)	(512.9)	(218.0)	(181.3)	(255.5)	(423.2)	(1936.7)
Loans from relatives (1=yes)	-37.50	124.6	80.16	87.99	329.8	-2.800	334.4*	570.7**	893.6**	1796.8*
	(204.4)	(175.1)	(163.1)	(296.6)	(741.1)	(219.3)	(193.4)	(261.5)	(395.0)	(779.0)
Eligibility of HH (1=yes)	-337.2**	-345.4**	-434.4	-287.0	-131.4	-349.6**	-709.8**	-824.7**	-1139.3**	-385.3
	(132.5)	(136.0)	(208.7)	(290.3)	(614.7)	(224.7)	(213.1)	(267.5)	(503.1)	(958.4)
Village covariates										
Average male wage	7.120	4.457	0.600	-0.893	47.50	-1.591	-4.335	-2.072	4.150	-18.40
	(9.655)	(8.924)	(10.01)	(12.08)	(42.87)	(7.768)	(6.698)	(9.869)	(12.75)	(23.87)
Average female wage	4.314	4.633	4.469	6.415	42.64	-0.675	-3.935	3.329	-8.545	16.22
	(6.290)	(5.649)	(7.968)	(10.13)	(32.17)	(7.935)	(8.674)	(11.89)	(16.73)	(30.10)
Primary school (1=yes)	-179.6	-119.8	-201.1	-213.5	63.33	108.0	119.7	-150.1	-469.0	-1705.4
	(119.6)	(136.0)	(162.1)	(178.4)	(378.3)	(321.7)	(310.0)	(534.2)	(656.4)	(1136.6)
Food program (1=yes)	-74.19	-144.8	-166.6	-333.7	-578.0	-246.1	-314.8*	-366.7	-346.2	-358.1
	(118.5)	(112.1)	(138.4)	(175.6)	(426.2)	(189.6)	(195.5)	(256.8)	(338.2)	(601.8)
Distance to nearest bank	-15.29	-35.93	-39.07	-50.61*	-94.65	-13.48	-9.440	-13.97	12.73	-45.67
(km)	(21.60)	(21.63)	(27.94)	(30.20)	(82.85)	(38.43)	(39.45)	(51.65)	(65.33)	(125.9)
Distance to nearest *pucca*	4.339	-10.34	-1.991	22.40	119.4	-66.94	-37.29	-89.57	-53.06	18.82
road (km)	(19.85)	(18.73)	(24.15)	(30.99)	(109.5)	(57.19)	(68.30)	(85.20)	(135.1)	(249.6)
Distance to nearest	29.15	60.04	48.83	98.42**	79.69	49.03	-6.647	24.68	78.10	-279.0
shop/market (km)	(36.63)	(33.65)	(39.21)	(48.42)	(110.8)	(112.0)	(106.0)	(137.0)	(169.8)	(272.9)
Electricity in village (1=yes)	308.2	354.6**	390.8**	674.8**	1132.9**	447.5**	329.7*	433.2	233.8	864.9
	(121.6)	(126.9)	(159.4)	(186.8)	(558.6)	(200.0)	(213.8)	(291.6)	(384.1)	(704.8)
Price of rice	-73.89	78.31	31.33	-21.52	-49.65	74.28	109.8	33.04	101.8	224.6
	(83.07)	(84.24)	(104.2)	(130.8)	(267.7)	(68.34)	(72.55)	(106.1)	(129.1)	(247.8)
Price of wheat flour	55.03	29.77	77.16	166.4	153.4	-40.95	-177.9	-178.8	-233.3	-528.6*
	(74.33)	(79.16)	(98.69)	(115.2)	(282.4)	(114.9)	(111.4)	(144.1)	(181.7)	(352.8)
Price of mustard oil	9.171	1.844	3.529	-9.264	-84.14	0.385	8.874	7.685	23.00	35.86
	(10.75)	(11.08)	(14.00)	(17.08)	(46.41)	(12.88)	(14.36)	(19.98)	(26.11)	(50.04)
Price of hen's eggs	-19.46	-14.11	-28.49	-72.62	-119.7	145.4	208.2	323.8	524.7	819.9*
	(142.8)	(161.3)	(271.7)	(368.3)	(578.6)	(212.2)	(206.2)	(289.8)	(391.3)	(728.3)
Price of milk	39.36	31.27	68.27**	100.3**	45.60	-1.792	3.474	14.65	-11.31	-61.44
	(30.97)	(30.45)	(38.70)	(41.94)	(101.6)	(31.82)	(29.55)	(39.06)	(52.47)	(86.21)
Price of potatoes	-27.69	43.71	-33.99	-23.49	-41.21	-9.921	10.39	-43.78	-9.326	11.48
	(47.98)	(46.23)	(64.10)	(87.43)	(156.3)	(37.83)	(41.14)	(56.51)	(90.84)	(154.4)
Overall intercept	2160.4	1916.2	2608.3	2918.0	6019.7	1702.7	3984.0**	5741.6**	6132.1**	11192.9**
	(1141.1)	(1277.2)	(1425.2)	(1505.4)	(3719.7)	(1558.2)	(1480.1)	(1925.6)	(2814.8)	(5792.2)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table B.3 **Cross-section quantile regressions, household food expenditure**

			1991-92					1998-99		
Quantile	10%	30%	Median	70%	90%	10%	30%	Median	70%	90%
Household microcredit	0.002	0.002	0.002	0.000	-0.002	0.006*	0.008**	0.005	0.008	0.011
borrowings	(0.004)	(0.003)	(0.005)	(0.005)	(0.007)	(0.003)	(0.002)	(0.003)	(0.005)	(0.005)
Household characteristics										
Education of HH head	-10.71	-2.968	34.33	50.60**	116.8**	46.76	45.37**	70.91**	79.83**	210.2**
	(18.55)	(18.56)	(19.45)	(21.96)	(38.99)	(21.34)	(19.21)	(21.42)	(20.88)	(55.92)
Age of HH head	-5.094**	-3.806*	-6.065*	-1.668	1.258	3.677	2.512	2.972	-0.859	0.996
	(2.483)	(3.097)	(3.026)	(2.891)	(4.244)	(3.291)	(3.211)	(3.235)	(3.841)	(5.465)
Gender of HH head	202.9	40.12	-15.17	33.19	79.40	40.34	-140.8	-163.1	-412.1**	-1374.3**
	(167.2)	(149.1)	(158.0)	(140.6)	(328.4)	(181.4)	(137.5)	(197.2)	(249.8)	(464.4)
Highest education of men in	45.63**	42.41**	20.14	8.842	-3.468	9.597	28.37*	22.34**	35.36**	33.16
HH	(18.71)	(16.05)	(16.09)	(17.69)	(29.18)	(18.39)	(14.38)	(13.76)	(14.86)	(22.80)
Highest education of women	-0.100	-10.56	5.697	11.65	-10.44	1.341	23.15	39.42**	47.45**	0.103
in HH	(14.97)	(17.05)	(17.23)	(16.34)	(28.45)	(16.95)	(15.52)	(16.97)	(20.51)	(40.76)
Landholdings	0.395**	0.836**	0.735**	0.634**	0.823**	0.468*	0.806	0.948	1.790	2.451
	(0.268)	(0.285)	(0.306)	(0.432)	(0.675)	(0.500)	(0.766)	(0.849)	(0.894)	(2.489)
HH economic dependency	-89.88**	-86.22**	-134.9**	-154.5**	-167.9**	-82.78**	-110.0**	-145.1**	-162.3**	-219.7**
ratio	(23.58)	(17.51)	(18.81)	(20.89)	(45.06)	(20.36)	(23.54)	(26.49)	(30.85)	(51.87)
# of HH head relatives	8.464	11.74	3.434	2.269	-3.301	13.48	42.56	27.89*	26.36**	-11.16
owning land	(10.06)	(10.64)	(12.18)	(13.05)	(19.34)	(19.76)	(18.57)	(15.92)	(16.81)	(34.04)
# of HH head's spouse	-13.77	-1.159	-7.916	-4.435	-0.167	33.23**	9.894	3.368	12.58	49.90
relatives owning land	(8.482)	(9.343)	(8.650)	(9.044)	(17.43)	(12.34)	(11.72)	(13.54)	(18.12)	(28.66)
# of HH head relatives living	1.376	2.198	2.506	0.360	6.388	-9.290	-1.676	1.853	-0.685	-30.98
outside thana	(15.42)	(14.74)	(14.17)	(19.60)	(38.05)	(19.05)	(17.31)	(18.17)	(19.36)	(30.82)
# of HH head's spouse	14.73	-0.937	6.483	6.770	22.84	-10.39	1.998	2.615	-5.644	5.688
relatives living outside thana	(8.057)	(7.790)	(9.845)	(12.46)	(18.02)	(10.22)	(10.10)	(10.75)	(12.35)	(19.87)
Loans from traditional	100.7	270.0*	410.5**	385.3**	253.0	-12.45	40.39	127.8	-39.60	841.3
banks (1=yes)	(152.5)	(135.3)	(139.3)	(149.9)	(197.6)	(196.1)	(163.9)	(146.4)	(232.2)	(439.3)
Loans from informal sources	-188.7	4.390	49.56	-120.5	-36.20	255.7	244.3	104.7	117.9	-376.6
(1=yes)	(120.1)	(125.8)	(110.2)	(106.7)	(190.0)	(217.0)	(131.7)	(137.6)	(177.4)	(251.6)
Loans from relatives (1=yes)	-77.09	43.17	103.5	118.8	47.70	13.22	59.57	241.3	121.5	129.3
	(132.9)	(118.0)	(122.0)	(146.0)	(215.2)	(131.2)	(122.6)	(126.7)	(145.4)	(256.3)
Eligibility of HH (1=yes)	-309.6**	-187.1**	-251.0*	-249.6	-82.48	-207.6*	-359.7**	-400.5**	-417.1**	-28.36
	(93.40)	(96.09)	(119.8)	(134.2)	(204.0)	(133.9)	(152.2)	(149.8)	(167.3)	(300.0)
Village covariates										
Average male wage	6.369	8.993	6.560	3.409	-1.700	-1.958	-3.825	-5.437	0.841	1.504
	(7.995)	(8.151)	(8.011)	(7.500)	(11.64)	(4.882)	(5.013)	(5.469)	(7.006)	(13.01)
Average female wage	3.198	0.598	-3.461	-2.356	-1.514	-1.548	1.195	5.318	0.239	-8.031
	(4.604)	(4.668)	(5.798)	(6.082)	(9.383)	(6.037)	(5.964)	(7.108)	(8.811)	(12.84)
Primary school (1=yes)	-174.9**	-117.9	-162.5	-156.9	-94.91	88.19	94.94	19.55	-64.36	115.3
	(85.46)	(114.2)	(132.9)	(130.0)	(178.4)	(192.9)	(209.9)	(230.5)	(271.0)	(402.6)
Food program (1=yes)	-116.4	-88.99	-82.14	-200.5	-183.7	-207.4*	-213.8	-238.1	-225.1	-404.5*
	(87.43)	(90.12)	(110.9)	(119.7)	(167.3)	(131.0)	(134.2)	(151.0)	(185.2)	(272.6)
Distance to nearest bank	-21.26*	-28.71	-31.96*	-48.90**	-66.65**	-15.07	-7.224	9.600	45.09	21.98
(km)	(15.13)	(17.63)	(19.78)	(20.94)	(30.02)	(25.70)	(27.88)	(33.28)	(39.31)	(58.68)
Distance to nearest *pucca*	5.986	-1.215	3.673	18.39	18.68	-65.44	-57.22	-45.13	-60.74	-98.87
road (km)	(12.93)	(14.30)	(17.63)	(18.33)	(24.36)	(41.47)	(41.67)	(50.82)	(58.87)	(96.78)
Distance to nearest	7.926	18.71	30.40	47.88	62.78	32.81	-38.31	-54.79	-33.72	-26.47
shop/market (km)	(25.66)	(28.34)	(30.95)	(33.23)	(48.29)	(73.55)	(72.51)	(80.42)	(98.74)	(131.0)
Electricity in village (1=yes)	241.4**	249.3**	286.8**	314.7**	416.5**	273.0*	289.9	211.3	302.7	460.3
	(90.43)	(92.00)	(113.4)	(118.2)	(189.3)	(138.2)	(150.8)	(166.6)	(220.2)	(350.4)
Price of rice	-30.91	38.90	33.20	-58.37	-82.01	91.73*	108.4*	61.56	99.79	205.0*
	(59.27)	(70.89)	(83.76)	(87.91)	(113.6)	(42.51)	(48.33)	(56.88)	(79.53)	(126.6)
Price of wheat flour	37.72	41.02	32.05	124.6	101.9	-61.51	-116.1	-99.37	-174.8*	-328.3**
	(56.68)	(57.35)	(72.25)	(82.25)	(122.0)	(84.72)	(75.60)	(76.93)	(100.7)	(146.7)
Price of mustard oil	11.74	7.306	4.017	-0.194	-9.868	-5.366	6.717	9.782	7.855	-2.804
	(8.134)	(8.877)	(10.77)	(11.47)	(15.53)	(8.978)	(10.02)	(11.01)	(14.98)	(26.77)
Price of hen's eggs	-13.10	-5.091	-15.29	-35.02	-41.71	12.82	149.9	292.9*	430.8**	362.0
	(131.9)	(153.5)	(210.7)	(288.1)	(400.1)	(143.8)	(156.5)	(167.1)	(221.7)	(397.3)
Price of milk	15.36	16.92	39.98	56.23*	88.49*	4.618	6.560	2.589	-3.414	-14.53
	(24.06)	(27.71)	(31.80)	(31.08)	(49.04)	(22.04)	(22.57)	(23.05)	(30.85)	(42.56)
Price of potatoes	1.929	-4.180	-2.107	-5.894	1.122	17.32	24.87	14.46	19.68	46.05
	(35.22)	(41.80)	(47.82)	(57.42)	(62.05)	(26.17)	(28.76)	(33.40)	(45.03)	(72.50)
Overall intercept	1559.0*	1162.6	2032.1	2638.3*	3565.3**	1830.5	1934.5*	2396.4**	3256.8**	6377.1*
	(805.9)	(968.8)	(1189.0)	(1196.8)	(1548.9)	(1164.9)	(1098.2)	(1183.6)	(1618.2)	(2598.3)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table B.4 **Cross-section quantile regressions, household food expenditure, by gender**

	1991-92					1998-99				
Quantile	10%	30%	Median	70%	90%	10%	30%	Median	70%	90%
Female microcredit	0.003	0.004	0.009	0.002	0.002	0.007*	0.007**	0.005*	0.011	0.012*
borrowings	(0.004)	(0.005)	(0.006)	(0.006)	(0.009)	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)
Male microcredit	-0.005	-0.001	-0.003	-0.000	-0.006	-0.000	0.008	0.001	-0.005	0.001
borrowings	(0.007)	(0.005)	(0.006)	(0.007)	(0.009)	(0.006)	(0.005)	(0.005)	(0.006)	(0.013)
Household characteristics										
Education of HH head	-13.59	-6.459	31.28	50.86**	112.1**	40.02	44.96**	72.00**	71.28**	212.5**
	(18.73)	(18.40)	(19.53)	(21.95)	(38.76)	(21.39)	(19.15)	(21.54)	(20.77)	(55.74)
Age of HH head	-5.534**	-4.262*	-6.450*	-1.691	0.279	4.398	2.266	2.841	-2.231	0.993
	(2.486)	(3.088)	(3.021)	(2.924)	(4.231)	(3.314)	(3.206)	(3.254)	(3.875)	(5.436)
Gender of HH head	245.2	34.90	-51.32	34.10	41.55	107.7	-138.6	-166.3	-349.4**	-1377.7**
	(166.4)	(150.8)	(157.0)	(140.5)	(328.0)	(180.5)	(137.7)	(196.2)	(247.0)	(464.4)
Highest education of men in	47.06**	44.50**	23.54	8.832	-1.948	15.59	29.59*	22.43**	37.88**	33.62
HH	(19.04)	(15.99)	(16.12)	(17.72)	(29.10)	(18.21)	(14.33)	(13.78)	(14.80)	(22.64)
Highest education of women	1.142	-8.974	5.695	11.68	-7.522	0.814	22.68	40.51**	48.25**	0.361
in HH	(14.78)	(17.04)	(17.07)	(16.45)	(28.50)	(16.83)	(15.67)	(16.91)	(20.70)	(40.77)
Landholdings	0.417**	0.836**	0.739**	0.633**	0.710**	0.435*	0.785	0.941	1.846	2.376
	(0.269)	(0.284)	(0.307)	(0.431)	(0.678)	(0.516)	(0.769)	(0.861)	(0.856)	(2.484)
HH economic dependency	-73.17**	-84.59**	-137.7**	-153.9**	-169.0**	-81.64**	-110.2**	-143.8**	-159.2**	-218.5**
ratio	(24.09)	(17.86)	(18.48)	(20.82)	(45.02)	(20.56)	(23.73)	(26.62)	(30.42)	(51.76)
# of HH head relatives	7.409	11.14	0.984	2.405	-0.631	18.11	42.46	27.51*	30.33**	-16.18
owning land	(9.956)	(10.65)	(12.14)	(13.09)	(19.17)	(19.71)	(18.78)	(15.96)	(17.01)	(34.89)
# of HH head's spouse	-14.12	-0.902	-4.461	-4.447	-0.349	29.88**	10.24	4.191	9.337	50.36
relatives owning land	(8.349)	(9.415)	(8.712)	(9.104)	(17.30)	(12.30)	(11.73)	(13.40)	(17.86)	(28.69)
# of HH head relatives living	0.946	5.172	1.253	0.354	6.820	-10.13	-1.097	2.341	-2.504	-33.40
outside thana	(15.28)	(14.67)	(14.17)	(19.97)	(38.39)	(19.02)	(17.34)	(18.13)	(19.25)	(30.83)
# of HH head's spouse	16.21	-0.175	7.214	6.747	23.61	-7.861	1.378	2.018	-2.982	9.124
relatives living outside thana	(8.030)	(7.783)	(9.731)	(12.46)	(17.97)	(10.20)	(10.09)	(10.67)	(12.25)	(20.07)
Loans from traditional	145.4	225.7**	395.6**	383.6**	293.3	-49.02	37.14	125.0	46.80	832.4
banks (1=yes)	(153.6)	(135.1)	(139.6)	(150.3)	(198.9)	(194.1)	(164.9)	(146.9)	(227.4)	(432.5)
Loans from informal sources	-199.1	-3.032	45.81	-121.4	-17.43	221.2	248.6	112.2	107.1	-393.9
(1=yes)	(119.2)	(125.9)	(111.4)	(107.0)	(189.0)	(217.6)	(132.3)	(140.3)	(174.7)	(252.7)
Loans from relatives (1=yes)	-46.18	17.91	108.1	118.5	53.71	-32.29	62.18	240.5	95.12	100.4
	(135.7)	(118.4)	(121.4)	(146.0)	(217.0)	(132.0)	(122.8)	(127.3)	(143.6)	(254.9)
Eligibility of HH (1=yes)	-298.9**	-195.7**	-247.4*	-249.5	-96.22	-166.0*	-363.0**	-398.3**	-438.5**	-27.68
	(95.09)	(96.70)	(120.1)	(134.2)	(204.2)	(135.7)	(152.6)	(150.1)	(167.7)	(298.9)
Village covariates										
Average male wage	5.469	9.461	7.951	3.367	-2.247	-2.302	-3.815	-5.511	1.089	2.614
	(7.838)	(8.097)	(7.962)	(7.497)	(11.76)	(4.845)	(5.038)	(5.476)	(6.890)	(13.03)
Average female wage	3.036	1.136	-3.712	-2.410	-0.555	-1.528	0.981	5.181	-0.539	-6.020
	(4.573)	(4.669)	(5.795)	(6.074)	(9.347)	(6.030)	(5.987)	(7.066)	(8.703)	(12.63)
Primary school (1=yes)	-172.6*	-110.6	-152.7	-156.2	-67.16	139.4	96.65	21.98	23.17	95.99
	(84.84)	(114.4)	(131.8)	(130.0)	(178.6)	(193.0)	(214.9)	(233.2)	(266.2)	(408.8)
Food program (1=yes)	-109.8	-86.19	-82.63	-200.2	-189.4	-214.8*	-217.6	-234.8	-244.1	-395.3*
	(85.96)	(90.44)	(109.5)	(119.9)	(168.4)	(130.1)	(134.3)	(153.1)	(183.3)	(273.2)
Distance to nearest bank	-23.00*	-26.71*	-31.11	-48.77**	-68.95**	-16.72	-6.007	9.085	48.44	21.39
(km)	(14.98)	(17.57)	(19.50)	(21.06)	(29.94)	(25.82)	(27.83)	(33.48)	(39.01)	(58.25)
Distance to nearest *pucca*	2.661	-1.068	3.895	18.43	17.90	-50.83	-56.68	-45.66	-63.66	-111.2
road (km)	(12.71)	(14.25)	(17.50)	(18.44)	(24.48)	(42.06)	(41.90)	(51.53)	(58.69)	(96.86)
Distance to nearest	12.00	12.35	29.43	48.09	66.61	40.60	-37.07	-56.20	-29.78	-27.40
shop/market (km)	(25.02)	(28.18)	(30.63)	(33.31)	(49.05)	(72.74)	(73.55)	(81.58)	(97.62)	(130.5)
Electricity in village (1=yes)	233.5**	237.8**	299.4**	316.5**	412.7**	311.1*	302.0	206.5	270.1	427.9
	(89.33)	(92.10)	(113.4)	(119.3)	(189.9)	(135.4)	(151.2)	(166.6)	(219.8)	(350.4)
Price of rice	-21.96	49.52	43.19	-58.76	-74.54	87.16*	107.7*	61.66	116.6	196.1*
	(58.82)	(70.95)	(83.50)	(88.11)	(113.2)	(42.50)	(48.76)	(57.43)	(79.27)	(125.9)
Price of wheat flour	29.69	41.97	35.22	124.6	88.99	-61.76	-118.5	-96.27	-183.7*	-317.2**
	(56.54)	(57.36)	(71.33)	(82.12)	(122.2)	(84.61)	(76.19)	(77.18)	(100.1)	(146.5)
Price of mustard oil	10.88	7.721	2.911	-0.263	-11.00	-3.534	6.361	9.479	8.760	-6.162
	(8.095)	(8.852)	(10.75)	(11.39)	(15.55)	(8.936)	(10.17)	(11.06)	(14.89)	(26.65)
Price of hen's eggs	-13.35	-9.978	-21.29	-35.08	-42.27	17.96	151.7	295.8**	490.6**	351.6
	(130.3)	(153.0)	(210.3)	(287.2)	(400.1)	(143.9)	(157.0)	(168.6)	(220.7)	(395.2)
Price of milk	16.05	12.74	34.36	56.42*	91.37*	3.995	6.930	1.872	-8.166	-14.09
	(23.57)	(27.85)	(31.68)	(31.39)	(49.41)	(21.84)	(22.70)	(23.12)	(30.56)	(42.35)
Price of potatoes	4.512	0.283	-2.090	-6.388	-0.646	16.77	25.80	13.75	23.51	33.82
	(34.99)	(41.99)	(47.69)	(57.47)	(61.44)	(26.12)	(29.07)	(33.55)	(45.50)	(73.54)
Overall intercept	1551.4*	1064.0	2000.7	2644.3*	3712.4**	1594.2	1983.3*	2392.3**	2944.1**	6566.5*
	(796.4)	(961.1)	(1186.4)	(1200.6)	(1543.4)	(1164.6)	(1101.3)	(1190.4)	(1614.4)	(2582.8)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table B.5 **Cross-section quantile regressions, household non-food expenditure**

	1991-92					**1998-99**				
Quantile	10%	30%	Median	70%	90%	10%	30%	Median	70%	90%
Household microcredit borrowings	0.003*	0.003*	0.003	0.008	0.021	0.003*	0.004**	0.007**	0.013**	0.031**
	(0.001)	(0.001)	(0.002)	(0.004)	(0.010)	(0.001)	(0.002)	(0.003)	(0.007)	(0.013)
Household characteristics										
Education of HH head	-0.391	-4.496	4.471	25.92	149.1**	6.191	16.62	28.20	-0.753	4.195
	(8.980)	(10.09)	(14.82)	(29.39)	(80.54)	(7.423)	(12.44)	(24.91)	(43.47)	(132.3)
Age of HH head	-1.511**	-3.923**	-4.812**	-4.243*	-7.550	-4.449**	-2.003*	-3.950**	-5.987**	-12.90**
	(0.916)	(1.134)	(1.557)	(2.455)	(5.757)	(1.328)	(1.623)	(2.236)	(3.908)	(10.08)
Gender of HH head	27.49	-36.87	-47.32	-28.52	-219.2	77.53	-18.24	125.2	-20.43	-731.6
	(52.04)	(49.85)	(59.06)	(109.1)	(536.2)	(64.28)	(69.25)	(119.1)	(269.1)	(994.1)
Highest education of men in HH	16.21**	28.70**	30.54**	14.83	24.55	19.62**	32.29**	41.96**	90.75**	204.8**
	(8.427)	(9.101)	(13.72)	(22.66)	(50.30)	(7.523)	(8.777)	(18.11)	(28.50)	(94.44)
Highest education of women in HH	22.56**	31.95**	50.44**	89.33**	51.47	29.60**	36.64**	72.23**	112.8**	323.4**
	(5.877)	(7.981)	(14.07)	(38.59)	(62.80)	(5.965)	(9.344)	(18.74)	(41.29)	(130.8)
Landholdings	0.225**	0.248**	0.596**	1.446*	2.408*	0.583**	0.794**	1.550**	1.827**	12.41
	(0.120)	(0.271)	(0.719)	(1.199)	(1.502)	(0.332)	(0.408)	(0.718)	(1.366)	(11.09)
HH economic dependency ratio	-19.51**	-24.52**	-29.99**	-43.18**	-92.21	-29.39**	-46.87**	-62.60**	-87.76**	-14.75
	(7.511)	(9.207)	(11.38)	(17.05)	(51.44)	(9.507)	(11.28)	(18.01)	(32.82)	(99.11)
# of HH head relatives owning land	4.022	5.935*	9.393	20.50	0.902	5.573	2.390	12.79	33.25	87.30
	(4.258)	(3.750)	(6.702)	(13.17)	(27.40)	(6.413)	(8.751)	(17.15)	(25.16)	(102.5)
# of HH head's spouse relatives owning land	0.190	0.493	-1.919	-2.804	15.13	8.243	12.90	5.917	16.99	3.693
	(2.617)	(3.851)	(4.091)	(7.106)	(21.41)	(5.815)	(6.258)	(9.686)	(21.22)	(51.79)
# of HH head relatives living outside thana	1.982	1.419	-3.881	-5.502	-34.73	-11.66*	-10.80	-15.70	-16.16	-37.19
	(6.437)	(5.469)	(7.770)	(14.09)	(31.14)	(6.682)	(7.755)	(9.767)	(17.56)	(52.17)
# of HH head's spouse relatives living outside thana	-2.491	-2.147	-0.171	-4.052	45.82	-1.648	2.643	1.923	-5.644	-14.81
	(2.996)	(3.590)	(5.746)	(10.66)	(28.55)	(4.075)	(5.031)	(7.552)	(13.28)	(31.82)
Loans from traditional banks (1=yes)	144.1**	163.0*	256.8*	448.1*	1346.9	37.67	11.03	296.6	637.4	1776.2
	(55.79)	(81.26)	(144.3)	(305.1)	(915.5)	(63.30)	(120.9)	(283.1)	(467.2)	(2446.1)
Loans from informal sources (1=yes)	27.16	28.60	69.33	407.1**	554.3	-85.07	47.23	14.74	-109.2	132.0
	(51.44)	(51.79)	(93.18)	(196.3)	(372.3)	(66.93)	(95.79)	(111.6)	(224.0)	(2445.1)
Loans from relatives (1=yes)	69.51	68.04	81.33	126.2	374.2	83.71	186.6*	247.5**	595.0**	2322.0**
	(49.52)	(67.43)	(97.94)	(176.6)	(661.9)	(54.42)	(83.40)	(133.9)	(239.7)	(774.7)
Eligibility of HH (1=yes)	-93.86**	-104.9	-59.81	-138.8	-220.6	-102.0*	-134.8**	-374.7**	-570.2**	-897.4
	(47.02)	(49.82)	(94.57)	(191.8)	(408.5)	(60.88)	(84.92)	(164.7)	(277.4)	(940.0)
Village covariates										
Average male wage	-1.627	-0.0354	-0.202	0.784	20.71	0.776	0.784	1.104	2.463	-11.79
	(1.902)	(2.428)	(3.463)	(6.542)	(33.43)	(2.082)	(2.573)	(3.787)	(5.922)	(17.34)
Average female wage	1.925	1.364	4.085*	4.020	15.83	0.961	-1.260	-1.137	-0.225	9.000
	(1.676)	(2.022)	(2.666)	(5.034)	(23.67)	(2.339)	(2.798)	(4.347)	(8.323)	(21.57)
Primary school (1=yes)	-0.288	-8.352	-40.98	-74.96	-44.71	7.339	-14.53	-120.4	-302.7	-804.0
	(32.95)	(41.88)	(59.51)	(105.1)	(272.5)	(85.20)	(93.77)	(192.0)	(331.5)	(971.0)
Food program (1=yes)	-3.563	-50.17	-64.30	-104.1	-76.36	-35.76	-33.21	-70.75	-122.0	-403.2
	(33.78)	(39.43)	(54.62)	(94.57)	(319.5)	(52.93)	(61.43)	(98.86)	(168.3)	(438.9)
Distance to nearest bank (km)	3.172	-2.694	-4.931	-9.132	-18.34	0.521	-1.897	-8.918	-11.23	-19.37
	(6.004)	(6.963)	(9.128)	(14.38)	(53.73)	(11.68)	(13.93)	(19.64)	(30.60)	(85.35)
Distance to nearest *pucca* road (km)	-3.568	-1.807	0.234	-0.353	28.42	-1.982	-4.148	-28.39	-14.43	-43.82
	(4.980)	(6.857)	(8.715)	(15.73)	(86.26)	(14.88)	(20.98)	(30.00)	(54.62)	(184.3)
Distance to nearest shop/market (km)	6.578	19.85	23.59	39.92	-1.465	-1.767	0.614	20.63	30.82	-132.9
	(9.871)	(12.23)	(15.22)	(25.85)	(81.01)	(32.61)	(38.53)	(52.70)	(82.14)	(211.4)
Electricity in village (1=yes)	5.922	63.58	61.83	120.4	327.0	106.8*	97.37*	128.7	88.32	268.8
	(33.67)	(42.16)	(60.36)	(108.9)	(398.3)	(59.54)	(66.62)	(104.1)	(177.6)	(454.5)
Price of rice	10.31	8.481	5.077	65.02	53.80	-22.62	-34.93	-18.80	-33.63	-34.11
	(20.73)	(24.87)	(34.75)	(69.67)	(174.5)	(21.12)	(26.97)	(41.09)	(71.68)	(173.8)
Price of wheat flour	17.86	30.24	36.01	29.45	84.26	-19.48	-30.77	-72.05	-91.22	-309.0
	(20.62)	(30.21)	(37.63)	(64.06)	(193.5)	(32.73)	(35.91)	(56.82)	(80.19)	(249.2)
Price of mustard oil	2.237	2.357	2.315	-7.563	-45.25*	5.145	6.263	3.899	9.770	20.72
	(2.804)	(3.438)	(4.747)	(8.273)	(33.62)	(4.155)	(5.514)	(8.559)	(15.55)	(38.23)
Price of hen's eggs	-1.085	-6.016	-6.184	-13.03	-1.870	44.55	86.07	168.6	196.5	428.9
	(31.10)	(48.83)	(63.71)	(100.1)	(252.4)	(65.48)	(79.33)	(120.3)	(191.9)	(524.8)
Price of milk	7.812	6.157	11.82	17.95	16.26	-3.844	-1.270	-10.86	-5.542	-14.72
	(6.939)	(8.778)	(12.58)	(27.49)	(67.20)	(9.366)	(9.639)	(13.85)	(24.45)	(62.84)
Price of potatoes	0.315	-7.757	-6.752	14.74	16.96	-10.23	-20.84	-28.30	-40.10	-72.46
	(12.10)	(14.63)	(23.58)	(38.66)	(99.08)	(11.32)	(14.61)	(23.68)	(38.19)	(98.69)
Overall intercept	-135.1	56.02	63.64	4.320	1455.6	717.3	1131.0**	2030.6**	2774.9**	7685.6**
	(270.2)	(326.1)	(459.5)	(803.4)	(2311.3)	(481.7)	(540.6)	(793.9)	(1324.2)	(4064.0)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table B.6 **Cross-section quantile regressions, household non-food expenditure, by gender**

	1991-92					1998-99				
Quantile	10%	30%	Median	70%	90%	10%	30%	Median	70%	90%
Female microcredit	0.003	0.002	0.002	0.008	0.016	0.003*	0.005**	0.007**	0.013**	0.032**
borrowings	(0.001)	(0.001)	(0.003)	(0.005)	(0.013)	(0.001)	(0.002)	(0.003)	(0.008)	(0.015)
Male microcredit	0.003	0.004	0.003	0.006	0.026	0.001	0.003	0.003	0.006	-0.003
borrowings	(0.002)	(0.003)	(0.004)	(0.010)	(0.015)	(0.003)	(0.003)	(0.005)	(0.009)	(0.018)
Household characteristics										
Education of HH head	-0.252	-4.398	4.625	26.42	150.2**	7.208	15.98	27.46	-0.435	9.754
	(9.051)	(10.03)	(14.87)	(29.29)	(80.74)	(7.393)	(12.40)	(25.02)	(43.82)	(133.4)
Age of HH head	-1.526**	-3.802**	-4.669**	-4.346*	-6.442	-4.249**	-2.072*	-3.494**	-5.596**	-14.33**
	(0.918)	(1.123)	(1.541)	(2.454)	(5.730)	(1.317)	(1.612)	(2.253)	(3.942)	(9.976)
Gender of HH head	25.18	-35.15	-46.31	-36.62	-192.8	81.04	-17.98	118.1	2.614	-805.9
	(52.32)	(49.95)	(59.04)	(109.0)	(534.1)	(63.30)	(68.97)	(120.1)	(269.8)	(990.1)
Highest education of men in	16.15**	28.80**	29.96**	14.26	21.21	18.65**	32.89**	43.15**	88.68**	202.7**
HH	(8.442)	(9.143)	(13.83)	(22.62)	(49.91)	(7.516)	(8.778)	(18.06)	(27.96)	(95.49)
Highest education of women	22.51**	32.13**	50.78**	88.57**	60.68	29.24**	36.99**	70.98**	108.8**	324.8**
in HH	(5.926)	(8.005)	(14.10)	(38.48)	(62.73)	(5.973)	(9.471)	(18.81)	(41.26)	(130.4)
Landholdings	0.224**	0.245**	0.588**	1.442*	2.281*	0.608**	0.785**	1.526**	1.871**	11.86
	(0.119)	(0.270)	(0.719)	(1.202)	(1.510)	(0.329)	(0.415)	(0.746)	(1.340)	(11.07)
HH economic dependency	-20.02**	-22.68**	-30.60**	-44.28**	-81.78	-28.97**	-46.77**	-61.63**	-89.50**	-16.17
ratio	(7.638)	(9.268)	(11.46)	(17.00)	(51.47)	(9.366)	(11.34)	(18.01)	(32.77)	(98.08)
# of HH head relatives	3.958	5.927*	9.394	20.21	1.382	4.909	3.022	14.97	33.81	94.18
owning land	(4.295)	(3.738)	(6.701)	(13.21)	(27.53)	(6.402)	(8.874)	(17.38)	(25.67)	(103.7)
# of HH head's spouse	0.219	0.605	-1.867	-2.467	16.07	7.827	13.86	6.511	18.71	8.101
relatives owning land	(2.626)	(3.865)	(4.115)	(7.143)	(21.49)	(5.794)	(6.364)	(9.687)	(20.83)	(52.41)
# of HH head relatives living	2.195	1.035	-3.286	-6.075	-28.21	-11.51*	-9.578	-15.22*	-17.96	-39.95
outside thana	(6.465)	(5.524)	(7.748)	(14.06)	(31.00)	(6.726)	(7.813)	(9.742)	(17.72)	(51.23)
# of HH head's spouse	-2.459	-1.839	0.0405	-4.124	48.55	-1.080	2.483	1.884	-6.273	-9.354
relatives living outside thana	(2.999)	(3.611)	(5.751)	(10.62)	(28.37)	(4.171)	(5.071)	(7.592)	(13.19)	(31.91)
Loans from traditional	144.1**	157.3*	261.8*	453.3*	1267.3	40.60	4.733	297.5	624.0	1776.2
banks (1=yes)	(55.87)	(81.39)	(144.4)	(306.3)	(918.5)	(62.83)	(121.5)	(285.1)	(466.4)	(2462.4)
Loans from informal sources	26.78	35.64	67.60	406.1**	559.9	-51.57	54.10	8.962	-112.9	173.1
(1=yes)	(51.58)	(51.40)	(93.94)	(195.5)	(370.4)	(66.29)	(95.97)	(109.9)	(226.4)	(2428.6)
Loans from relatives (1=yes)	64.39	66.26	84.27	125.8	362.1	81.51	176.9*	255.2**	600.3**	2253.6**
	(49.65)	(67.67)	(97.97)	(176.2)	(661.8)	(54.08)	(82.77)	(134.0)	(240.5)	(762.5)
Eligibility of HH (1=yes)	-94.39**	-101.0	-58.96	-144.3	-213.7	-100.8*	-137.3**	-366.1**	-605.2**	-881.0
	(47.10)	(49.62)	(94.52)	(192.5)	(410.0)	(61.24)	(85.15)	(163.8)	(275.9)	(947.2)
Village covariates										
Average male wage	-1.641	-0.124	-0.299	0.839	20.56	0.702	0.781	0.675	2.537	-8.175
	(1.945)	(2.466)	(3.510)	(6.642)	(33.56)	(2.113)	(2.584)	(3.806)	(5.893)	(17.22)
Average female wage	1.961	1.348	4.020*	3.892	17.38	0.779	-0.812	-0.594	-0.0578	7.618
	(1.693)	(2.029)	(2.675)	(5.051)	(23.86)	(2.354)	(2.829)	(4.396)	(8.272)	(21.36)
Primary school (1=yes)	-3.349	-8.380	-42.06	-71.50	-47.98	8.395	-14.91	-114.8	-277.0	-751.7
	(33.32)	(41.96)	(59.48)	(105.6)	(274.2)	(86.22)	(93.98)	(193.5)	(331.7)	(985.6)
Food program (1=yes)	-2.061	-46.29	-62.99	-107.6	-63.85	-30.47	-34.51	-62.52	-115.1	-413.7
	(33.97)	(39.52)	(55.13)	(95.85)	(320.9)	(52.64)	(61.30)	(99.67)	(169.0)	(444.9)
Distance to nearest bank	2.964	-1.840	-5.277	-7.890	-24.03	3.101	-0.971	-8.472	-7.564	-25.24
(km)	(6.062)	(6.973)	(9.114)	(14.46)	(53.73)	(11.71)	(13.96)	(19.68)	(31.19)	(85.82)
Distance to nearest *pucca*	-3.336	-1.953	0.428	-1.136	29.15	-3.387	-6.189	-25.19	-16.01	-40.61
road (km)	(5.036)	(6.884)	(8.745)	(15.76)	(86.10)	(14.79)	(21.45)	(31.30)	(55.30)	(179.0)
Distance to nearest	6.758	18.61	22.30	39.00	2.766	-3.226	4.711	18.21	28.06	-123.2
shop/market (km)	(9.937)	(12.28)	(15.34)	(26.04)	(81.23)	(32.57)	(38.83)	(53.13)	(81.04)	(207.9)
Electricity in village (1=yes)	6.298	63.08	64.16	119.9	302.6	99.58	94.79*	124.4	83.77	244.3
	(33.60)	(42.36)	(60.16)	(109.2)	(399.4)	(59.33)	(67.11)	(105.0)	(177.2)	(453.7)
Price of rice	9.490	7.666	3.847	65.22	39.42	-18.62	-30.71	-21.88	-32.17	-57.25
	(21.02)	(24.98)	(34.52)	(69.36)	(175.3)	(21.45)	(27.17)	(41.84)	(72.62)	(175.9)
Price of wheat flour	16.99	31.15	32.13	30.17	66.73	-15.15	-31.36	-69.83	-89.42	-293.6
	(20.68)	(30.24)	(37.80)	(64.38)	(192.5)	(32.80)	(36.19)	(57.58)	(80.52)	(249.2)
Price of mustard oil	2.314	2.552	2.407	-7.607	-42.94*	5.385	6.089	5.037	10.48	19.16
	(2.808)	(3.436)	(4.742)	(8.318)	(33.67)	(4.175)	(5.528)	(8.577)	(15.60)	(38.05)
Price of hen's eggs	-0.886	-5.861	-5.587	-17.80	-5.522	55.96	89.67	166.8	209.6	507.7
	(31.58)	(48.87)	(63.99)	(99.90)	(250.7)	(65.51)	(79.46)	(121.0)	(192.8)	(522.5)
Price of milk	7.968	5.521	13.24	18.15	22.27	-4.612	-1.259	-9.485	-8.235	-19.52
	(7.086)	(8.911)	(12.70)	(27.68)	(68.11)	(9.364)	(9.676)	(13.93)	(24.61)	(61.07)
Price of potatoes	1.147	-7.627	-4.860	14.77	3.102	-8.940	-19.95	-30.64	-39.69	-83.07
	(12.15)	(14.67)	(23.70)	(39.28)	(100.5)	(11.56)	(14.73)	(23.94)	(38.45)	(99.57)
Overall intercept	-122.4	39.94	82.16	26.21	1524.0	561.9	1057.5**	1942.0**	2660.5**	7711.2**
	(271.3)	(325.8)	(459.5)	(807.1)	(2331.3)	(485.9)	(538.5)	(803.4)	(1325.0)	(4068.9)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Appendix C

Regression tables for cross-section quantile regression estimates with village quantile effects

Table C.1 **Cross-section quantile regressions with village quantile effects, household total expenditure**

	1991-92					1998-99				
Quantile	10%	30%	Median	70%	90%	10%	30%	Median	70%	90%
Household microcredit borrowings	0.007	0.008	0.007	0.006	0.008	-0.000	0.006	0.004	0.006	0.026
	(0.005)	(0.005)	(0.006)	(0.009)	(0.012)	(0.004)	(0.004)	(0.006)	(0.008)	(0.014)
Household characteristics										
Education of HH head	4.113	-15.18	32.50	91.84**	251.7**	38.47	34.92	77.99**	133.4**	200.4**
	(36.04)	(27.79)	(33.03)	(44.32)	(66.51)	(31.32)	(26.83)	(32.87)	(49.17)	(107.2)
Age of HH head	-10.13**	-12.13**	-13.33**	-8.713**	-11.28*	5.109	-3.648	-2.342	-10.66	-11.71
	(3.832)	(3.800)	(4.171)	(4.613)	(7.452)	(4.052)	(4.215)	(5.340)	(7.003)	(11.31)
Gender of HH head	44.18	155.0	174.3	-126.5	-582.5	400.2	-90.14	-149.9	-292.2	-946.3
	(229.2)	(170.9)	(225.1)	(233.4)	(836.4)	(229.0)	(249.3)	(305.4)	(438.3)	(744.8)
Highest education of men in HH	20.68	75.42**	53.28*	34.08	-25.96	41.48**	73.36**	67.29**	85.98**	67.50*
	(34.42)	(26.27)	(25.99)	(34.08)	(56.20)	(24.04)	(17.95)	(21.75)	(29.12)	(62.81)
Highest education of women in HH	55.34**	60.83**	75.37**	68.55**	41.76	34.67	62.44**	89.81**	135.3**	241.0**
	(22.86)	(18.10)	(23.05)	(29.38)	(48.76)	(23.67)	(21.82)	(29.11)	(42.10)	(97.75)
Landholdings	1.005**	1.104**	1.201**	2.776**	3.289**	2.133**	1.990**	1.651**	2.195**	2.909
	(0.467)	(0.596)	(1.013)	(1.207)	(1.752)	(0.580)	(0.717)	(0.973)	(1.765)	(6.513)
HH economic dependency ratio	-126.1**	-172.8**	-210.5**	-242.5**	-346.3**	-87.62**	-159.9**	-208.5**	-253.9**	-269.9**
	(32.84)	(27.70)	(22.46)	(36.47)	(56.77)	(31.02)	(31.98)	(38.73)	(48.62)	(97.56)
# of HH head relatives owning land	29.50**	21.52*	9.353	-4.979	-2.245	59.15**	63.76**	90.78**	110.7**	109.0
	(11.72)	(11.24)	(15.80)	(22.38)	(35.58)	(28.09)	(25.93)	(32.49)	(33.48)	(77.40)
# of HH head's spouse relatives owning land	-3.090	1.199	-9.217	22.04	29.63	31.05**	27.39*	23.85	24.36	41.71
	(8.864)	(9.950)	(13.48)	(16.97)	(25.43)	(16.64)	(15.47)	(21.02)	(28.35)	(49.41)
# of HH head relatives living outside thana	6.321	-16.87	-6.564	-7.691	53.84	-14.34	-29.19	-32.78	-22.39	-60.56
	(18.58)	(16.04)	(17.65)	(33.54)	(58.23)	(24.28)	(26.42)	(26.76)	(37.06)	(57.51)
# of HH head's spouse relatives living outside thana	10.04	-1.823	12.66	10.19	-12.40	7.160	-4.059	1.688	-12.98	-30.03
	(9.333)	(12.20)	(15.58)	(19.97)	(23.55)	(12.32)	(13.47)	(17.18)	(20.32)	(34.65)
Loans from traditional banks (1=yes)	354.4	506.8**	546.4**	985.7**	1364.9*	448.9**	493.4*	544.7*	1048.5**	2864.6**
	(218.1)	(156.0)	(195.1)	(466.4)	(681.2)	(182.7)	(257.8)	(418.6)	(579.1)	(1706.9)
Loans from informal sources (1=yes)	-48.46	85.07	150.0	216.3	421.4	115.9	-13.24	-67.54	23.89	386.5
	(165.8)	(142.5)	(183.7)	(310.8)	(436.8)	(225.6)	(198.2)	(259.9)	(453.4)	(1347.3)
Loans from relatives (1=yes)	173.9	-63.00	-41.09	-4.762	-37.95	-3.367	62.34	25.04	398.0	543.7
	(148.1)	(150.7)	(164.4)	(261.1)	(384.5)	(174.7)	(149.8)	(247.0)	(369.7)	(619.0)
Eligibility of HH (1=yes)	-557.6**	-320.4**	-381.0	-138.1	81.83	-628.7**	-891.5**	-774.2**	-942.6**	-725.2
	(119.0)	(131.0)	(205.9)	(239.0)	(386.1)	(203.8)	(211.9)	(265.6)	(366.2)	(766.3)
Overall intercept	3410.6**	3762.6**	4444.2**	4319.6**	5302.0**	3331.4**	6430.1**	7567.1**	9073.1**	11055.0**
	(407.7)	(374.9)	(485.8)	(523.3)	(1146.2)	(1020.7)	(1120.0)	(1445.8)	(1268.6)	(1964.1)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table C.2 **Cross-section quantile regressions with village quantile effects, household total expenditure, by gender**

	1991-92					1998-99				
Quantile	10%	30%	Median	70%	90%	10%	30%	Median	70%	90%
Female microcredit borrowings	0.007	0.011	0.003	0.003	0.004	0.003	0.007	0.007	0.011	0.031*
	(0.006)	(0.006)	(0.006)	(0.009)	(0.016)	(0.005)	(0.004)	(0.007)	(0.008)	(0.014)
Male microcredit borrowings	0.006	0.007	0.010	0.018	0.016	-0.017	-0.010	-0.005	-0.030	-0.003
	(0.008)	(0.008)	(0.013)	(0.017)	(0.016)	(0.008)	(0.009)	(0.011)	(0.021)	(0.035)
Household characteristics										
Education of HH head	1.944	-14.76	32.56	90.14**	262.7**	31.11	37.46	79.93**	143.0**	221.7**
	(35.98)	(28.08)	(33.04)	(44.56)	(66.65)	(31.47)	(26.52)	(33.60)	(48.51)	(106.0)
Age of HH head	-10.07**	-12.00**	-12.45**	-8.907**	-11.25*	4.779	-3.587	-3.191	-10.33	-11.87
	(3.834)	(3.785)	(4.178)	(4.635)	(7.538)	(4.169)	(4.286)	(5.341)	(6.916)	(11.21)
Gender of HH head	41.25	182.1	157.7	-141.7	-595.8	413.9	-111.3	-119.9	-218.3	-980.7
	(229.1)	(171.4)	(225.7)	(229.4)	(836.5)	(224.0)	(256.3)	(317.4)	(441.0)	(757.0)
Highest education of men in HH	22.57	75.31**	54.44*	35.04	-37.47	46.75**	71.92**	66.03**	85.33**	77.99*
	(34.40)	(26.19)	(25.89)	(34.44)	(56.53)	(24.12)	(17.86)	(21.95)	(29.51)	(64.86)
Highest education of women in HH	56.64**	60.52**	76.87**	70.95**	47.76	31.55	62.08**	91.77**	128.8**	235.6**
	(22.91)	(17.92)	(23.14)	(29.42)	(49.07)	(23.31)	(22.38)	(29.05)	(42.32)	(98.43)
Landholdings	1.026**	1.096**	1.143**	2.766**	3.413**	2.074**	1.998**	1.768**	2.363**	3.407
	(0.466)	(0.599)	(1.013)	(1.199)	(1.755)	(0.595)	(0.794)	(1.106)	(1.737)	(6.536)
HH economic dependency ratio	-124.8**	-172.1**	-206.0**	-239.2**	-348.7**	-89.38**	-158.9**	-210.0**	-258.9**	-311.8**
	(33.24)	(27.86)	(22.19)	(37.04)	(58.19)	(31.26)	(32.32)	(39.07)	(49.42)	(93.89)
# of HH head relatives owning land	29.09**	21.06**	10.16	-6.957	-7.004	66.06**	67.90**	100.9**	105.2**	101.9
	(11.94)	(11.25)	(15.74)	(22.37)	(35.28)	(27.03)	(26.42)	(34.30)	(34.41)	(81.05)
# of HH head's spouse relatives owning land	-3.222	1.081	-8.090	21.25	29.07	33.85**	25.53	16.10	23.27	54.98
	(8.954)	(9.927)	(13.32)	(16.98)	(25.28)	(16.13)	(16.06)	(22.89)	(28.29)	(51.10)
# of HH head relatives living outside thana	4.584	-15.88	-8.367	-6.845	55.58	-18.79	-32.77	-39.73	-22.55	-22.29
	(18.64)	(16.14)	(17.73)	(33.07)	(57.94)	(23.14)	(26.79)	(27.02)	(36.97)	(59.49)
# of HH head's spouse relatives living outside thana	9.221	-1.504	11.15	11.40	-7.135	9.019	-5.467	1.645	-6.284	-27.08
	(9.595)	(12.23)	(15.51)	(19.91)	(23.24)	(12.11)	(13.97)	(17.27)	(20.40)	(35.08)
Loans from traditional banks (1=yes)	359.6	500.0**	544.1**	957.7**	1363.9**	509.3**	489.7*	551.2*	1063.7**	2641.2**
	(218.2)	(156.7)	(196.2)	(466.3)	(687.4)	(182.1)	(277.4)	(426.3)	(562.2)	(1698.2)
Loans from informal sources (1=yes)	-49.86	91.25	126.3	198.7	474.6	134.3	-24.81	-36.47	68.17	18.91
	(165.7)	(142.9)	(182.5)	(310.0)	(439.0)	(233.5)	(204.1)	(260.7)	(459.9)	(1334.9)
Loans from relatives (1=yes)	166.1	-65.64	-58.64	1.806	-18.70	-11.34	73.09	0.252	336.6	371.5
	(149.7)	(151.5)	(163.1)	(260.2)	(386.0)	(177.2)	(149.8)	(248.9)	(367.1)	(632.0)
Eligibility of HH (1=yes)	-554.7**	-322.1**	-356.5	-168.4	97.10	-693.8**	-889.3**	-787.5**	-902.1**	-671.6
	(118.4)	(131.8)	(204.5)	(240.5)	(391.2)	(204.6)	(214.8)	(266.2)	(368.8)	(760.5)
Overall intercept	3404.6**	3733.2**	4390.3**	4344.7**	5320.7**	3399.5**	6473.3**	7583.1**	8950.2**	11086.7**
	(407.4)	(375.9)	(485.4)	(522.3)	(1156.5)	(1025.1)	(1140.2)	(1457.6)	(1281.9)	(2005.9)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table C.3 **Cross-section quantile regressions with village quantile effects, household food expenditure**

	1991-92					1998-99				
Quantile	10%	30%	Median	70%	90%	10%	30%	Median	70%	90%
Household microcredit borrowings	0.003	0.008*	0.004	0.002	0.002	-0.001	0.001	-0.001	-0.002	0.005
	(0.004)	(0.003)	(0.003)	(0.003)	(0.005)	(0.002)	(0.003)	(0.003)	(0.003)	(0.005)
Household characteristics										
Education of HH head	5.754	15.74	18.94	29.79*	67.87**	24.76	38.88	34.33**	64.06**	100.1**
	(21.47)	(19.38)	(17.96)	(17.78)	(26.13)	(18.40)	(19.26)	(15.19)	(19.70)	(34.20)
Age of HH head	-7.128*	-5.374*	-7.301**	-4.126*	-6.783	4.757*	1.302	5.156	2.117	2.170
	(2.616)	(2.699)	(2.431)	(2.412)	(3.926)	(2.352)	(2.292)	(3.174)	(3.341)	(5.163)
Gender of HH head	309.5*	185.8*	86.11	58.23	-340.6	188.3	-181.3	-198.6**	-308.7	-530.4**
	(173.5)	(128.5)	(156.3)	(142.2)	(316.0)	(190.1)	(147.3)	(132.8)	(220.8)	(345.4)
Highest education of men in HH	10.21	13.40	30.08*	28.65	14.04	17.78	22.15**	21.91**	24.54*	18.15
	(20.69)	(18.59)	(16.17)	(15.37)	(21.54)	(16.65)	(11.84)	(11.40)	(12.63)	(19.65)
Highest education of women in HH	1.440	5.396	14.16	-0.512	-11.89	1.027	13.67	26.70**	25.48	38.13
	(16.43)	(14.88)	(13.18)	(15.31)	(20.43)	(15.21)	(14.61)	(13.04)	(16.88)	(28.24)
Landholdings	0.269**	0.800**	0.745**	0.654**	0.968**	0.422**	1.347**	1.396	1.434	1.366
	(0.302)	(0.261)	(0.274)	(0.415)	(0.517)	(0.689)	(0.703)	(0.716)	(0.771)	(0.880)
HH economic dependency ratio	-98.38**	-125.5**	-143.3**	-184.2**	-202.8**	-63.83**	-96.39**	-103.2**	-109.4**	-190.0**
	(25.23)	(20.76)	(15.12)	(19.10)	(29.32)	(18.57)	(21.85)	(24.36)	(30.98)	(42.07)
# of HH head relatives owning land	22.30**	19.77*	8.726	8.851	-4.406	31.57**	26.06*	36.28**	36.88**	52.97
	(10.24)	(9.729)	(9.740)	(11.19)	(17.42)	(19.01)	(17.11)	(13.43)	(15.19)	(27.64)
# of HH head's spouse relatives owning land	-0.829	6.527	5.113	3.873	16.13	24.90*	23.28**	20.68*	24.37*	46.23**
	(7.089)	(6.399)	(8.624)	(10.94)	(13.59)	(11.70)	(9.901)	(10.41)	(13.29)	(22.10)
# of HH head relatives living outside thana	-11.76	-26.24*	-11.76	-7.020	-2.557	-16.03	-5.331	6.403	-3.291	20.80
	(14.86)	(12.67)	(12.70)	(16.24)	(33.12)	(16.46)	(13.52)	(14.49)	(19.27)	(30.23)
# of HH head's spouse relatives living outside thana	9.664	3.864	1.338	4.255	-10.26	3.352	7.744	-2.098	-6.492	-22.88
	(6.292)	(7.574)	(9.859)	(9.782)	(13.01)	(8.637)	(8.702)	(8.234)	(10.85)	(13.67)
Loans from traditional banks (1=yes)	164.7	251.6*	297.9**	227.6**	214.2	153.7	229.9**	345.8**	237.3	106.7
	(141.3)	(134.0)	(116.3)	(120.8)	(186.4)	(141.8)	(123.4)	(145.4)	(140.7)	(387.7)
Loans from informal sources (1=yes)	-52.16	18.36	5.855	27.73	-89.65	112.1	43.18	-96.90	-176.4	99.88
	(130.1)	(113.6)	(103.2)	(100.1)	(153.2)	(179.6)	(141.2)	(115.4)	(157.5)	(255.7)
Loans from relatives (1=yes)	77.55	-26.81	-106.9	54.20	-3.277	-39.97	-20.85	-18.05	-29.68	113.1
	(109.8)	(113.5)	(122.3)	(168.8)	(156.2)	(123.2)	(88.98)	(91.13)	(135.4)	(178.8)
Eligibility of HH (1=yes)	-437.7**	-248.9**	-145.0	-123.6	31.77	-362.2**	-424.8**	-413.3**	-348.3**	-292.0
	(94.97)	(88.42)	(93.72)	(110.8)	(167.1)	(130.3)	(146.0)	(109.1)	(171.8)	(304.0)
Overall intercept	2784.2**	3027.6**	3303.9**	3567.7**	4620.2**	2375.2**	3841.4**	4546.6**	5440.0**	7405.1**
	(287.3)	(280.0)	(250.8)	(268.2)	(542.1)	(727.0)	(573.7)	(785.8)	(679.9)	(1102.9)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table C.4 **Cross-section quantile regressions with village quantile effects, household food expenditure, by gender**

			1991-92					**1998-99**		
Quantile	10%	30%	Median	70%	90%	10%	30%	Median	70%	90%
Female microcredit borrowings	0.004	0.005	0.001	-0.002	0.001	-0.000	0.002	0.001	-0.001	0.006
	(0.005)	(0.004)	(0.005)	(0.005)	(0.007)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)
Male microcredit borrowings	0.000	0.012**	0.007**	0.005	0.003	-0.004	-0.004	-0.008	-0.013	-0.003
	(0.006)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.008)	(0.011)
Household characteristics										
Education of HH head	7.647	16.81	18.41	30.69*	66.89**	25.05	39.53	38.85**	68.04**	108.7**
	(21.54)	(19.53)	(17.78)	(17.70)	(26.55)	(18.14)	(19.20)	(14.76)	(19.28)	(34.18)
Age of HH head	-6.995*	-5.216*	-6.699**	-3.926*	-6.658	5.141*	0.894	5.304	1.638	3.247
	(2.609)	(2.762)	(2.473)	(2.421)	(3.905)	(2.377)	(2.356)	(3.229)	(3.265)	(5.123)
Gender of HH head	259.2*	187.8*	97.21	54.04	-328.0	196.8	-172.3	-183.8**	-289.4	-540.9**
	(174.9)	(129.1)	(157.9)	(142.9)	(315.9)	(192.3)	(147.1)	(133.3)	(220.0)	(344.4)
Highest education of men in HH	8.524	13.35	28.56*	26.80	14.08	17.90	22.73**	20.48**	23.22**	18.48
	(20.59)	(18.70)	(16.24)	(15.34)	(21.43)	(16.26)	(11.75)	(10.93)	(12.37)	(19.54)
Highest education of women in HH	2.234	4.732	14.44	0.988	-8.472	-1.088	13.56	25.78**	25.42	35.58
	(16.37)	(14.83)	(13.26)	(15.14)	(20.73)	(15.32)	(14.90)	(12.90)	(17.10)	(28.24)
Landholdings	0.268**	0.801**	0.746**	0.656**	0.991**	0.403**	1.265**	1.693	1.448	1.115
	(0.304)	(0.260)	(0.274)	(0.412)	(0.519)	(0.698)	(0.707)	(0.728)	(0.756)	(0.857)
HH economic dependency ratio	-97.68**	-125.0**	-140.2**	-185.6**	-205.2**	-64.42**	-100.1**	-102.4**	-122.0**	-190.5**
	(25.90)	(20.66)	(15.01)	(19.09)	(30.34)	(19.11)	(21.91)	(24.75)	(30.80)	(41.78)
# of HH head relatives owning land	19.88**	19.26*	9.666	8.849	-4.965	35.53**	25.77*	38.53**	41.60**	48.96*
	(10.31)	(9.753)	(9.835)	(11.12)	(17.38)	(18.54)	(17.57)	(13.74)	(15.07)	(27.70)
# of HH head's spouse relatives owning land	-1.044	6.206	6.209	3.984	15.15	25.86*	23.31**	15.76*	22.02*	52.56**
	(7.087)	(6.435)	(8.710)	(10.99)	(13.62)	(11.56)	(9.577)	(10.64)	(12.88)	(22.28)
# of HH head relatives living outside thana	-14.66	-26.43*	-14.09	-6.987	-2.294	-19.08	-2.165	7.901	-9.899	20.16
	(14.93)	(12.64)	(12.81)	(16.32)	(33.13)	(16.19)	(13.75)	(14.24)	(19.20)	(30.33)
# of HH head's spouse relatives living outside thana	9.819	2.411	2.355	5.156	-9.135	4.265	6.297	-0.351	-6.204	-24.31
	(6.494)	(7.635)	(9.982)	(9.802)	(13.02)	(8.590)	(8.611)	(8.046)	(10.57)	(13.97)
Loans from traditional banks (1=yes)	163.5	246.9*	294.1**	237.9**	210.3	157.0	237.1**	337.4**	230.9	110.6
	(141.6)	(132.2)	(119.7)	(120.5)	(188.0)	(143.3)	(123.7)	(146.1)	(140.6)	(381.5)
Loans from informal sources (1=yes)	-53.21	16.97	-4.067	30.41	-77.69	96.82	54.51	-98.56	-166.2	116.4
	(130.1)	(113.9)	(103.4)	(100.4)	(153.8)	(182.7)	(141.8)	(117.9)	(157.1)	(254.1)
Loans from relatives (1=yes)	87.77	0.0780	-92.01	64.31	-7.240	-30.63	-13.35	-8.865	-19.89	137.2
	(110.6)	(113.2)	(122.4)	(167.8)	(156.6)	(122.1)	(89.02)	(91.52)	(135.6)	(179.6)
Eligibility of HH (1=yes)	-451.8**	-247.5**	-131.1	-123.1	30.34	-371.5**	-438.8**	-413.9**	-343.1**	-285.6
	(94.90)	(87.86)	(93.92)	(111.5)	(168.2)	(129.4)	(147.6)	(110.5)	(171.4)	(302.3)
Overall intercept	2863.4**	3025.2**	3228.8**	3599.0**	4609.5**	2357.5**	3861.0**	4511.6**	5458.7**	7378.4**
	(286.4)	(279.5)	(250.0)	(272.8)	(541.3)	(721.9)	(590.6)	(800.1)	(676.2)	(1108.2)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table C.5 **Cross-section quantile regressions with village quantile effects, household non-food expenditure**

	1991-92					1998-99				
Quantile	10%	30%	Median	70%	90%	10%	30%	Median	70%	90%
Household microcredit borrowings	-0.000	0.002	0.002	0.006	0.018**	0.001	0.003	0.004	0.004	0.022*
	(0.002)	(0.001)	(0.002)	(0.004)	(0.009)	(0.001)	(0.002)	(0.003)	(0.005)	(0.010)
Household characteristics										
Education of HH head	-3.268	2.916	11.63	31.11*	83.87*	-0.103	7.603	5.335	43.26	89.08
	(7.484)	(9.716)	(16.19)	(22.66)	(47.33)	(6.725)	(10.84)	(21.62)	(34.32)	(68.11)
Age of HH head	-1.649**	-3.787**	-3.681**	-5.245**	-4.141	-2.806*	-2.623*	-4.661**	-3.223	-12.40*
	(0.965)	(0.945)	(1.447)	(2.279)	(4.819)	(1.331)	(1.468)	(1.998)	(3.654)	(7.576)
Gender of HH head	11.54	3.132	-21.10	-70.62	-104.2	149.9*	2.002	3.798	65.46	-300.4
	(40.82)	(46.99)	(58.08)	(103.4)	(387.7)	(62.71)	(83.72)	(122.6)	(240.9)	(369.7)
Highest education of men in HH	20.00**	23.82**	23.70*	26.53	5.405	22.96**	33.73**	44.71**	61.58**	72.76**
	(8.002)	(8.035)	(13.79)	(18.63)	(36.95)	(5.467)	(8.484)	(13.09)	(25.38)	(40.55)
Highest education of women in HH	22.33**	30.31**	54.16**	61.12**	155.1**	22.15**	25.07**	53.31**	69.13**	187.9**
	(5.648)	(7.869)	(13.02)	(22.57)	(47.73)	(4.962)	(8.957)	(15.73)	(31.79)	(60.16)
Landholdings	0.232**	0.246**	0.473**	1.921**	2.516**	0.796**	0.778**	1.479*	1.252*	1.717
	(0.127)	(0.293)	(0.687)	(1.120)	(1.164)	(0.292)	(0.446)	(0.639)	(1.393)	(5.284)
HH economic dependency ratio	-34.17**	-40.80**	-41.32**	-47.45**	-127.6**	-33.13**	-47.56**	-63.17**	-64.98**	-125.8*
	(9.776)	(8.385)	(12.27)	(17.00)	(31.93)	(7.986)	(10.44)	(15.07)	(27.96)	(59.31)
# of HH head relatives owning land	2.799	6.215**	7.120	4.442	-2.458	16.79**	17.28**	33.31**	57.62**	65.85*
	(3.638)	(3.387)	(5.245)	(10.17)	(20.80)	(6.295)	(8.629)	(17.55)	(23.15)	(53.95)
# of HH head's spouse relatives owning land	1.971	-2.462	-4.884	-1.050	8.583	8.903	11.66	6.129	-2.939	-18.80
	(2.688)	(3.140)	(4.582)	(7.489)	(15.61)	(6.053)	(6.125)	(8.869)	(16.35)	(28.92)
# of HH head relatives living outside thana	1.007	-2.262	-0.716	-1.922	7.939	-8.265	-9.703	-10.38*	-28.26	-52.45
	(6.469)	(5.406)	(8.035)	(13.90)	(26.59)	(6.381)	(9.167)	(9.759)	(16.27)	(40.91)
# of HH head's spouse relatives living outside thana	-1.714	0.771	0.569	7.084	10.29	2.238	-1.188	-0.246	2.437	19.93
	(3.226)	(3.434)	(5.783)	(11.07)	(17.73)	(3.403)	(4.913)	(6.939)	(10.80)	(25.03)
Loans from traditional banks (1=yes)	138.8**	123.3**	254.2**	449.3**	912.6**	152.2**	77.10	214.1	689.2*	3343.8**
	(53.61)	(80.69)	(149.7)	(299.2)	(670.1)	(51.47)	(101.9)	(272.8)	(424.1)	(2033.3)
Loans from informal sources (1=yes)	48.81	88.53**	76.07*	200.1	451.8*	-54.72	-36.34	106.8	70.64	98.99
	(47.09)	(42.34)	(74.21)	(156.0)	(265.0)	(59.57)	(98.06)	(125.1)	(219.4)	(1408.5)
Loans from relatives (1=yes)	41.88	4.438	-2.721	-68.98	25.46	19.09	105.5	74.26	232.0	542.1
	(45.45)	(54.49)	(82.48)	(130.8)	(208.2)	(54.73)	(67.57)	(111.6)	(224.1)	(389.3)
Eligibility of HH (1=yes)	-102.3**	-102.6*	-104.6	54.04	177.9	-146.2**	-208.9**	-337.9**	-420.1**	-296.5
	(42.29)	(45.96)	(93.90)	(158.8)	(204.4)	(64.17)	(86.60)	(145.4)	(252.9)	(521.4)
Overall intercept	436.4**	609.8**	741.2**	517.4**	868.1**	1180.9**	1463.8**	2467.5**	3739.9**	3781.7**
	(109.4)	(106.6)	(178.7)	(252.7)	(563.9)	(255.7)	(240.2)	(463.6)	(1012.2)	(1614.8)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table C.6 **Cross-section quantile regressions with village quantile effects, household non-food expenditure, by gender**

	1991-92					1998-99				
Quantile	10%	30%	Median	70%	90%	10%	30%	Median	70%	90%
Female microcredit borrowings	-0.000	0.002	0.001	0.005	0.005	0.001	0.003*	0.005**	0.008	0.030**
	(0.002)	(0.002)	(0.002)	(0.004)	(0.013)	(0.001)	(0.002)	(0.003)	(0.005)	(0.011)
Male microcredit borrowings	0.001	0.001	0.002	0.010	0.021*	0.001	0.001	-0.007	-0.008	-0.024
	(0.002)	(0.003)	(0.005)	(0.012)	(0.012)	(0.003)	(0.003)	(0.004)	(0.011)	(0.023)
Household characteristics										
Education of HH head	-3.098	2.682	12.32	32.87*	92.33*	-0.201	5.870	4.416	50.26	107.2
	(7.427)	(9.747)	(16.43)	(22.69)	(47.35)	(6.783)	(11.12)	(21.72)	(33.75)	(67.47)
Age of HH head	-1.636**	-3.855**	-3.546**	-4.930**	-3.759	-2.761*	-2.700*	-4.573**	-3.246	-15.70*
	(0.955)	(0.945)	(1.429)	(2.295)	(4.836)	(1.324)	(1.506)	(2.033)	(3.624)	(7.623)
Gender of HH head	12.31	3.153	-26.39	-71.54	-220.7	150.9*	1.105	13.76	49.58	-375.8
	(40.63)	(46.78)	(57.97)	(103.2)	(382.6)	(64.46)	(86.72)	(127.7)	(240.0)	(377.9)
Highest education of men in HH	19.78**	24.30**	23.01	25.15	-2.135	22.60**	34.91**	42.85**	59.76**	73.98**
	(7.994)	(8.138)	(13.89)	(18.77)	(36.94)	(5.500)	(8.698)	(12.97)	(24.51)	(42.83)
Highest education of women in HH	22.44**	30.13**	52.71**	60.14**	153.5**	22.32**	25.46**	56.73**	72.56**	179.3**
	(5.675)	(7.897)	(12.99)	(22.66)	(47.60)	(4.974)	(9.295)	(15.43)	(31.49)	(60.49)
Landholdings	0.228**	0.246**	0.488**	1.915**	2.484**	0.792**	0.742**	1.536*	1.242*	1.972
	(0.127)	(0.294)	(0.688)	(1.117)	(1.166)	(0.293)	(0.466)	(0.629)	(1.396)	(5.306)
HH economic dependency ratio	-34.15**	-40.65**	-41.59**	-48.10**	-114.7**	-32.74**	-46.19**	-67.77**	-67.24**	-135.3*
	(10.18)	(8.438)	(12.38)	(17.01)	(31.82)	(8.003)	(10.50)	(14.83)	(28.13)	(59.13)
# of HH head relatives owning land	2.781	6.023**	7.332	5.052	-4.435	16.73**	15.82**	37.08**	52.87**	76.94*
	(3.624)	(3.373)	(5.235)	(10.13)	(20.47)	(6.329)	(8.786)	(18.42)	(22.98)	(55.20)
# of HH head's spouse relatives owning land	1.804	-2.091	-5.212	-0.946	9.561	9.861	12.20	1.046	1.659	-8.036
	(2.707)	(3.201)	(4.596)	(7.511)	(15.44)	(6.026)	(6.393)	(8.983)	(15.59)	(30.06)
# of HH head relatives living outside thana	0.960	-2.554	-0.798	-2.654	5.849	-8.538	-8.809	-12.86*	-25.34	-53.48
	(6.432)	(5.477)	(8.050)	(13.65)	(26.63)	(6.408)	(9.392)	(10.32)	(16.35)	(39.95)
# of HH head's spouse relatives living outside thana	-1.619	0.862	1.344	6.798	6.281	2.219	-1.405	3.181	2.945	28.16
	(3.242)	(3.447)	(5.797)	(10.94)	(17.37)	(3.461)	(5.081)	(7.043)	(10.33)	(24.90)
Loans from traditional banks (1=yes)	140.0**	124.8**	254.2**	444.6**	910.9**	150.9**	81.00	152.4	736.0	3123.8**
	(53.90)	(80.34)	(150.9)	(299.5)	(667.1)	(52.26)	(104.8)	(273.4)	(432.8)	(2027.7)
Loans from informal sources (1=yes)	49.60	87.17**	70.77*	201.8	415.9*	-57.81	-30.96	111.9	77.73	223.2
	(47.05)	(42.79)	(74.48)	(155.0)	(262.5)	(60.69)	(98.04)	(119.9)	(224.2)	(1408.4)
Loans from relatives (1=yes)	42.40	6.200	2.118	-76.28	48.61	16.15	103.6	78.71	224.4	421.5
	(45.54)	(54.78)	(82.26)	(131.8)	(208.6)	(55.40)	(68.37)	(108.2)	(228.8)	(390.6)
Eligibility of HH (1=yes)	-104.3**	-103.1*	-102.8	54.39	188.6	-145.6**	-205.7**	-353.0**	-423.7**	-251.8
	(42.58)	(45.98)	(93.80)	(160.1)	(209.1)	(63.94)	(87.69)	(141.7)	(252.9)	(506.5)
Overall intercept	434.1**	611.2**	741.3**	516.9**	976.0**	1175.7**	1472.3**	2461.2**	3628.3**	4090.0**
	(109.9)	(106.6)	(179.6)	(254.8)	(558.3)	(256.4)	(241.4)	(468.0)	(996.7)	(1642.7)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Appendix D

Regression tables for cross-section quantile regression estimates with penalised village effects

Table D.1 **Cross-section quantile regressions with village penalised fixed effects, household total expenditure**

	1991-92					**1998-99**				
Quantile	10%	30%	Median	70%	90%	10%	30%	Median	70%	90%
Household microcredit borrowings	0.009	0.008*	0.009	0.013**	0.006	0.009**	0.011**	0.017**	0.022**	0.052**
	(0.006)	(0.004)	(0.007)	(0.007)	(0.009)	(0.002)	(0.003)	(0.005)	(0.007)	(0.016)
Household characteristics										
Education of HH head	-34.84	-31.56*	-1.629	75.91	118.1	11.00	39.00*	32.89**	72.63*	85.56
	(25.87)	(17.54)	(23.58)	(40.29)	(72.15)	(16.22)	(22.56)	(22.51)	(46.04)	(104.7)
Age of HH head	-5.811**	-11.05**	-11.23**	-11.97**	-19.48**	0.784	0.880	-3.167	-7.593	-15.86
	(3.648)	(2.719)	(3.152)	(4.385)	(7.092)	(2.971)	(3.404)	(4.431)	(5.854)	(9.259)
Gender of HH head	243.4	70.26	-33.00	-123.6	-453.3	116.0	-59.09	-295.5	-551.1*	-1748.4*
	(145.9)	(185.6)	(153.9)	(213.5)	(751.4)	(143.1)	(186.3)	(198.6)	(391.5)	(1197.2)
Highest education of men in HH	78.93**	77.73**	62.01**	42.10*	75.41*	56.52**	60.80**	82.58**	91.39**	203.4**
	(23.99)	(15.62)	(19.04)	(34.07)	(61.22)	(14.40)	(18.80)	(16.90)	(34.16)	(70.84)
Highest education of women in HH	15.96	66.76**	73.55**	75.34**	134.7*	28.37**	85.23**	102.7**	154.0**	362.3**
	(19.21)	(15.96)	(18.55)	(34.85)	(59.39)	(18.27)	(19.75)	(20.71)	(39.32)	(100.0)
Landholdings	0.966**	1.026**	1.724**	2.876**	2.294**	2.220**	2.148**	2.337**	3.742**	9.477*
	(0.388)	(0.503)	(1.006)	(1.135)	(1.698)	(0.826)	(0.507)	(0.920)	(1.653)	(8.935)
HH economic dependency ratio	-98.18**	-124.9**	-153.8**	-205.5**	-296.9**	-103.7**	-164.9**	-176.5**	-218.8**	-323.7**
	(23.30)	(18.57)	(24.21)	(28.39)	(39.19)	(21.72)	(23.27)	(24.72)	(39.19)	(82.21)
# of HH head relatives owning land	7.600	6.924	3.988	-1.620	-19.27	12.10	17.30	18.28	66.55*	69.49
	(8.748)	(9.838)	(14.69)	(21.86)	(27.04)	(17.07)	(16.89)	(23.66)	(32.80)	(99.60)
# of HH head's spouse relatives owning land	-3.745	-9.025	-7.270	-5.913	18.06	24.21**	20.06	19.03	12.05	34.65
	(9.105)	(7.751)	(10.68)	(14.00)	(22.22)	(11.91)	(13.42)	(16.77)	(23.65)	(52.08)
# of HH head relatives living outside thana	6.588	19.65	0.269	0.467	40.41	15.91	3.027	18.71	-2.395	48.51
	(17.43)	(13.73)	(13.16)	(23.36)	(61.55)	(13.73)	(15.74)	(20.05)	(26.06)	(48.18)
# of HH head's spouse relatives living outside thana	-2.278	5.499	3.547	10.85	-1.336	-9.439	9.542	-5.079	5.044	21.34
	(9.354)	(6.589)	(9.955)	(14.66)	(29.97)	(12.19)	(9.542)	(12.42)	(15.89)	(36.59)
Loans from traditional banks (1=yes)	331.6**	445.1**	536.1**	818.5**	2038.4**	102.4	381.9	327.4	686.9	1270.7
	(158.1)	(170.6)	(196.3)	(357.8)	(918.0)	(235.6)	(218.5)	(197.8)	(552.9)	(763.4)
Loans from informal sources (1=yes)	123.9	286.2**	391.8*	541.8**	1340.9**	360.7**	216.7**	16.53	-73.81	-309.9
	(114.2)	(102.6)	(215.6)	(254.3)	(359.8)	(116.2)	(109.4)	(178.5)	(276.4)	(632.1)
Loans from relatives (1=yes)	301.6**	55.28	120.6	339.5	256.2	99.21	262.9**	568.4**	808.5**	1480.9**
	(96.56)	(122.7)	(164.6)	(217.3)	(366.9)	(126.0)	(121.6)	(178.1)	(273.2)	(639.2)
Eligibility of HH (1=yes)	-395.0**	-354.6**	-445.7**	-437.2*	-1259.3**	-285.0*	-581.4**	-817.2**	-1120.7**	-842.8*
	(146.1)	(133.0)	(189.0)	(307.4)	(770.2)	(194.1)	(205.9)	(248.0)	(446.4)	(986.7)
Overall intercept	2853.7**	3710.5**	4349.5**	4993.7**	7414.2**	2485.7**	3503.9**	4603.7**	5974.7**	7986.1**
	(294.0)	(263.9)	(305.9)	(437.8)	(1185.2)	(284.7)	(298.5)	(427.5)	(675.4)	(1514.9)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table D.2 **Cross-section quantile regressions with village penalised fixed effects, household total expenditure, by gender**

	1991-92					1998-99				
Quantile	10%	30%	Median	70%	90%	10%	30%	Median	70%	90%
Female microcredit borrowings	0.016**	0.009**	0.010	0.011*	0.006	0.010**	0.014**	0.020**	0.027**	0.056**
	(0.005)	(0.005)	(0.007)	(0.007)	(0.010)	(0.003)	(0.004)	(0.005)	(0.007)	(0.016)
Male microcredit borrowings	0.004	0.003	0.010	0.023	0.020	0.003	-0.002	0.001	-0.001	-0.003
	(0.010)	(0.008)	(0.012)	(0.014)	(0.015)	(0.006)	(0.006)	(0.006)	(0.011)	(0.020)
Household characteristics										
Education of HH head	-42.47	-33.61	-3.175	75.25	111.2	10.95	38.04*	30.89*	75.01*	53.56
	(24.53)	(17.72)	(23.44)	(39.37)	(71.21)	(16.29)	(22.21)	(21.79)	(47.22)	(101.9)
Age of HH head	-6.808**	-11.25**	-11.39**	-11.28**	-19.34**	0.535	0.767	-2.202	-7.885	-17.70
	(3.481)	(2.738)	(3.265)	(4.332)	(7.164)	(2.880)	(3.364)	(4.474)	(5.799)	(9.385)
Gender of HH head	202.9	69.75	-37.50	-126.9	-467.1	141.5	-34.75	-240.8	-577.2	-1618.3
	(147.0)	(189.9)	(154.9)	(217.3)	(742.6)	(144.8)	(179.2)	(199.2)	(395.5)	(1187.9)
Highest education of men in HH	81.31**	79.38**	61.97**	39.58*	75.04*	56.93**	60.13**	83.03**	91.99**	222.7**
	(22.66)	(15.76)	(19.10)	(33.62)	(60.75)	(14.31)	(18.58)	(16.41)	(34.19)	(67.08)
Highest education of women in HH	25.12	67.08**	73.02**	78.47**	141.4*	30.75**	85.12**	103.6**	148.1**	360.6**
	(18.96)	(15.87)	(18.81)	(34.42)	(59.23)	(17.90)	(19.00)	(21.45)	(39.02)	(97.38)
Landholdings	1.048**	0.999**	1.754**	2.796**	2.189**	2.226**	2.008**	2.229**	3.624**	10.43
	(0.386)	(0.508)	(1.003)	(1.134)	(1.722)	(0.766)	(0.476)	(0.908)	(1.585)	(8.829)
HH economic dependency ratio	-106.4**	-121.8**	-153.4**	-209.9**	-300.8**	-101.5**	-165.1**	-176.3**	-217.4**	-330.0**
	(21.99)	(18.05)	(24.26)	(28.50)	(38.73)	(21.98)	(24.31)	(24.60)	(39.37)	(80.32)
# of HH head relatives owning land	13.42	6.645	3.309	-2.155	-16.33	16.00	20.13	25.59	62.49*	77.47
	(8.731)	(9.873)	(14.56)	(21.83)	(26.84)	(16.82)	(16.72)	(23.78)	(33.02)	(102.1)
# of HH head's spouse relatives owning land	-3.859	-8.638	-6.812	-5.959	14.72	22.98**	24.12*	15.83	16.07	47.48
	(8.399)	(7.762)	(10.59)	(13.87)	(22.75)	(12.10)	(13.17)	(16.49)	(23.43)	(52.57)
# of HH head relatives living outside thana	6.612	16.49	1.248	-2.107	46.13	15.48	3.568	11.12	-2.016	53.83
	(16.94)	(13.54)	(13.16)	(22.64)	(59.63)	(13.64)	(15.55)	(19.69)	(26.82)	(47.35)
# of HH head's spouse relatives living outside thana	-2.730	4.555	3.257	10.67	-0.0682	-10.04	8.762	1.057	5.806	20.54
	(8.977)	(6.574)	(9.964)	(14.52)	(29.84)	(12.13)	(9.365)	(12.22)	(15.92)	(35.58)
Loans from traditional banks (1=yes)	337.8*	432.3**	537.3**	750.2**	2014.3**	75.08	375.1	319.9*	568.5*	1058.5
	(157.7)	(172.4)	(198.6)	(354.9)	(915.9)	(235.6)	(228.5)	(188.8)	(535.8)	(767.9)
Loans from informal sources (1=yes)	141.8	269.7**	375.7**	559.8**	1378.3**	366.1**	208.6**	66.31	-62.58	-213.7
	(112.7)	(101.5)	(214.7)	(252.4)	(354.4)	(119.8)	(113.9)	(166.2)	(280.4)	(674.0)
Loans from relatives (1=yes)	322.6**	73.21	135.5	316.4	284.3	98.45	275.8**	563.2**	869.9**	1349.0**
	(94.22)	(122.6)	(164.9)	(220.0)	(360.3)	(126.5)	(118.9)	(180.7)	(273.1)	(629.7)
Eligibility of HH (1=yes)	-370.7**	-364.5**	-440.2**	-474.1*	-1272.2**	-283.8*	-582.1**	-784.2**	-1167.9**	-818.1*
	(144.7)	(133.8)	(187.5)	(309.2)	(768.4)	(195.4)	(204.7)	(248.5)	(450.5)	(985.9)
Overall intercept	2890.1**	3727.6**	4355.4**	5016.7**	7418.8**	2460.6**	3476.9**	4471.0**	6040.1**	7937.9**
	(289.4)	(264.6)	(303.6)	(438.8)	(1179.6)	(284.4)	(293.4)	(432.5)	(678.9)	(1493.7)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table D.3 **Cross-section quantile regressions with village penalised fixed effects, household food expenditure**

	1991-92					**1998-99**				
Quantile	10%	30%	Median	70%	90%	10%	30%	Median	70%	90%
Household microcredit borrowings	0.002	0.004	0.002	0.004	-0.004	0.005**	0.008**	0.007**	0.011**	0.013**
	(0.003)	(0.003)	(0.004)	(0.005)	(0.006)	(0.002)	(0.002)	(0.002)	(0.004)	(0.005)
Household characteristics										
Education of HH head	-25.03	-13.79	-7.433	37.07*	68.07	23.31	27.53**	33.85**	48.78**	97.34**
	(14.94)	(10.73)	(14.45)	(16.40)	(30.12)	(17.27)	(12.69)	(11.14)	(17.78)	(42.05)
Age of HH head	-6.411**	-5.605**	-7.269**	-5.185**	-5.200	0.505	-0.830	-0.399	-0.643	-4.927
	(2.420)	(2.186)	(2.137)	(2.462)	(3.282)	(2.415)	(2.638)	(2.362)	(3.156)	(5.218)
Gender of HH head	282.1**	73.49	30.90	-21.05	-11.04	7.801	-43.77	-253.8	-402.2**	-1125.7**
	(96.76)	(152.1)	(111.4)	(128.0)	(165.3)	(105.2)	(103.5)	(145.6)	(223.0)	(443.6)
Highest education of men in HH	48.30**	37.61**	37.09**	21.54	12.33	19.50	30.20**	30.23**	28.89**	41.30**
	(14.66)	(9.786)	(12.11)	(15.31)	(28.27)	(15.53)	(11.08)	(9.030)	(13.74)	(21.52)
Highest education of women in HH	4.132	15.11	40.09**	20.09**	45.22	19.59	35.87**	44.08**	62.05**	71.33**
	(11.02)	(11.24)	(13.15)	(12.40)	(29.13)	(13.58)	(12.16)	(11.85)	(16.56)	(34.55)
Landholdings	0.390**	0.517**	0.687**	0.833**	0.628*	0.619	0.698*	1.468**	1.929	2.613*
	(0.242)	(0.263)	(0.299)	(0.411)	(0.495)	(0.494)	(0.610)	(0.738)	(0.805)	(1.623)
HH economic dependency ratio	-77.01**	-80.62**	-109.3**	-136.8**	-210.6**	-84.91**	-106.8**	-133.9**	-130.5**	-213.6**
	(16.61)	(13.32)	(14.57)	(18.41)	(28.07)	(17.76)	(17.93)	(15.75)	(20.32)	(33.36)
# of HH head relatives owning land	8.029	1.999	-2.717	4.522	-8.361	6.411	3.069	7.286	16.62	15.04
	(7.442)	(5.744)	(8.658)	(10.35)	(14.49)	(13.32)	(10.68)	(11.37)	(14.01)	(30.25)
# of HH head's spouse relatives owning land	-0.619	-1.543	-2.259	-5.988	-6.416	17.77*	11.91	6.583	7.763	15.24
	(6.126)	(4.559)	(6.245)	(8.259)	(11.84)	(9.460)	(8.560)	(8.450)	(10.78)	(25.44)
# of HH head relatives living outside thana	-5.261	-5.757	-0.531	0.897	1.884	1.531	8.299	3.019	-0.217	2.453
	(16.55)	(9.835)	(9.867)	(13.54)	(25.91)	(10.52)	(9.932)	(10.13)	(12.41)	(24.34)
# of HH head's spouse relatives living outside thana	8.445	0.535	1.885	5.856	27.07*	-8.448	0.812	0.684	-4.147	-7.201
	(6.532)	(4.663)	(7.386)	(8.808)	(11.96)	(8.587)	(7.603)	(7.004)	(9.000)	(15.44)
Loans from traditional banks (1=yes)	131.8*	258.2**	221.9**	215.1**	283.7	65.27	166.2	65.53	53.94	362.9
	(95.69)	(92.01)	(100.8)	(136.4)	(181.4)	(156.9)	(109.7)	(115.4)	(149.9)	(350.4)
Loans from informal sources (1=yes)	53.72	36.99	115.3	141.1	383.7*	291.9**	166.5**	156.0**	167.9	-18.78
	(81.01)	(85.90)	(102.6)	(143.5)	(187.7)	(89.49)	(103.0)	(78.67)	(127.8)	(238.1)
Loans from relatives (1=yes)	33.30	68.40	87.66	80.31	85.81	76.16	127.5*	200.6**	145.6	304.7
	(92.18)	(94.99)	(93.48)	(136.1)	(166.1)	(92.24)	(88.05)	(78.70)	(115.6)	(205.2)
Eligibility of HH (1=yes)	-196.4**	-325.0**	-330.6**	-265.5**	-468.3*	-165.3	-222.6**	-425.8**	-477.1**	-384.9**
	(99.14)	(83.05)	(95.23)	(124.2)	(215.9)	(118.1)	(143.0)	(121.6)	(159.2)	(294.5)
Overall intercept	2389.7**	3056.9**	3500.4**	3790.9**	4740.9**	2102.7**	2656.1**	3477.5**	4022.1**	5812.6**
	(175.5)	(222.8)	(180.2)	(236.4)	(361.8)	(216.7)	(198.0)	(239.4)	(341.8)	(541.8)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table D.4 **Cross-section quantile regressions with village penalised fixed effects, household food expenditure, by gender**

	1991-92					1998-99				
Quantile	10%	30%	Median	70%	90%	10%	30%	Median	70%	90%
Female microcredit borrowings	0.005	0.002	0.002	0.003	-0.001	0.007**	0.008**	0.010**	0.013**	0.015**
	(0.003)	(0.003)	(0.005)	(0.006)	(0.006)	(0.002)	(0.002)	(0.002)	(0.004)	(0.005)
Male microcredit borrowings	0.000	0.005	0.002	0.002	-0.006	0.000	0.007	0.003	-0.003	-0.001
	(0.006)	(0.005)	(0.005)	(0.008)	(0.010)	(0.005)	(0.004)	(0.003)	(0.004)	(0.010)
Household characteristics										
Education of HH head	-22.15	-16.09	-5.834	35.25*	68.37	22.38	27.31**	38.12**	49.83**	94.36**
	(14.89)	(10.72)	(14.73)	(16.22)	(29.79)	(16.21)	(12.88)	(11.24)	(16.96)	(41.31)
Age of HH head	-6.169**	-5.684**	-7.199**	-4.588**	-4.340	-0.395	-1.168	-0.0185	-0.706	-4.966
	(2.355)	(2.213)	(2.187)	(2.479)	(3.334)	(2.369)	(2.667)	(2.346)	(3.106)	(5.246)
Gender of HH head	263.7**	77.99	47.48	-24.50	18.15	13.62	-57.59	-256.4	-429.1**	-1077.0**
	(103.9)	(152.2)	(111.6)	(128.1)	(165.7)	(105.9)	(104.0)	(143.4)	(223.0)	(444.8)
Highest education of men in HH	48.26**	37.78**	35.95**	22.31*	12.19	22.05	30.92**	25.37**	27.88**	41.99**
	(14.78)	(9.809)	(12.21)	(15.76)	(27.83)	(14.62)	(11.08)	(9.286)	(13.16)	(21.18)
Highest education of women in HH	2.695	18.91	40.59**	20.89**	45.35	16.79	34.80**	44.23**	60.42**	75.72**
	(11.07)	(11.19)	(13.26)	(12.61)	(29.26)	(13.02)	(12.34)	(11.69)	(16.70)	(33.56)
Landholdings	0.430**	0.484**	0.683**	0.816**	0.703*	0.590	0.661*	1.332**	1.939	2.161*
	(0.245)	(0.258)	(0.299)	(0.405)	(0.497)	(0.507)	(0.614)	(0.742)	(0.777)	(1.618)
HH economic dependency ratio	-79.74**	-81.54**	-109.0**	-134.3**	-207.4**	-87.74**	-111.3**	-134.4**	-127.3**	-218.3**
	(16.57)	(13.23)	(14.50)	(18.20)	(27.36)	(17.83)	(18.08)	(16.42)	(19.47)	(33.09)
# of HH head relatives owning land	8.092	3.795	-2.932	5.037	-8.584	14.33	3.916	9.120	14.39	24.13
	(7.638)	(5.660)	(8.564)	(10.22)	(14.54)	(13.25)	(10.73)	(11.78)	(13.73)	(30.92)
# of HH head's spouse relatives owning land	2.842	-1.392	-1.976	-6.036	-7.009	17.45*	11.77	10.96	8.080	13.10
	(6.134)	(4.555)	(6.331)	(8.230)	(12.07)	(9.771)	(8.916)	(8.565)	(10.23)	(25.47)
# of HH head relatives living outside thana	-5.103	-7.941	-0.332	1.328	3.024	-1.028	8.237	2.096	0.580	1.263
	(16.09)	(9.705)	(9.807)	(13.57)	(25.58)	(10.72)	(10.20)	(10.76)	(11.66)	(24.78)
# of HH head's spouse relatives living outside thana	5.941	2.031	1.161	3.807	24.99*	-9.445	1.704	-1.306	-2.232	-5.942
	(6.485)	(4.396)	(7.420)	(8.705)	(11.92)	(9.026)	(7.766)	(6.927)	(8.552)	(15.10)
Loans from traditional banks (1=yes)	152.0*	265.7**	223.7**	214.5**	275.5	72.56	173.0	46.90	66.28	314.7
	(96.24)	(91.95)	(100.7)	(137.1)	(184.3)	(154.9)	(112.1)	(111.4)	(136.9)	(343.8)
Loans from informal sources (1=yes)	52.03	32.46	112.2	136.3	406.9*	315.4**	163.0**	133.6**	178.8	-24.54
	(83.14)	(86.67)	(103.5)	(141.3)	(185.2)	(89.07)	(103.5)	(84.54)	(116.6)	(234.1)
Loans from relatives (1=yes)	60.53	64.48	85.56	80.83	99.37	56.96	123.1	193.4**	157.8	337.6
	(101.0)	(95.11)	(94.31)	(135.1)	(165.0)	(90.61)	(89.80)	(76.58)	(108.5)	(206.4)
Eligibility of HH (1=yes)	-172.2**	-319.8**	-334.1**	-270.7**	-425.6*	-190.5	-201.5*	-411.6**	-471.4**	-374.8**
	(99.71)	(83.23)	(95.11)	(121.2)	(215.4)	(118.5)	(143.7)	(121.5)	(158.7)	(294.0)
Overall intercept	2356.6**	3053.3**	3485.1**	3774.8**	4627.4**	2167.8**	2678.7**	3451.6**	4030.2**	5757.9**
	(178.6)	(221.8)	(181.9)	(232.3)	(361.9)	(211.6)	(197.0)	(237.7)	(335.9)	(544.6)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table D.5 **Cross-section quantile regressions with village penalised fixed effects, household non-food expenditure**

	1991-92					1998-99				
Quantile	10%	30%	Median	70%	90%	10%	30%	Median	70%	90%
Household microcredit borrowings	0.003**	0.003**	0.002**	0.007**	0.018**	0.003**	0.004**	0.006**	0.014**	0.041**
	(0.001)	(0.001)	(0.002)	(0.003)	(0.006)	(0.001)	(0.001)	(0.002)	(0.006)	(0.013)
Household characteristics										
Education of HH head	-5.186	-10.46	-2.637	4.282	78.12	1.712	5.785	9.704	3.727	-12.88
	(5.907)	(5.777)	(9.564)	(18.67)	(66.01)	(7.020)	(8.250)	(15.92)	(31.10)	(95.29)
Age of HH head	-2.052**	-2.837**	-4.246**	-5.256**	-10.70**	-1.901*	-1.640	-1.766	-6.193*	-7.088
	(0.644)	(0.746)	(1.031)	(1.507)	(4.979)	(1.272)	(1.035)	(1.645)	(2.719)	(6.826)
Gender of HH head	2.955	-43.40	-62.38	-98.50	-108.9	77.15	52.58	4.027	-62.04	-360.3
	(25.74)	(44.35)	(49.59)	(95.75)	(856.0)	(43.07)	(55.57)	(74.20)	(179.8)	(906.4)
Highest education of men in HH	22.11**	28.96**	31.09**	26.50**	92.82	15.89**	23.98**	36.96**	67.39**	171.3**
	(5.493)	(5.345)	(8.827)	(15.66)	(59.11)	(5.522)	(5.551)	(11.59)	(23.69)	(67.67)
Highest education of women in HH	15.15**	30.86**	37.30**	52.32**	52.62**	16.80**	30.14**	45.92**	79.49**	219.4**
	(4.499)	(5.693)	(8.207)	(22.35)	(46.14)	(6.087)	(8.599)	(12.33)	(23.89)	(94.54)
Landholdings	0.252**	0.330**	0.886**	1.917**	2.873**	0.819**	0.847**	1.526**	2.559**	5.621
	(0.0910)	(0.223)	(0.472)	(0.978)	(1.212)	(0.300)	(0.369)	(0.578)	(1.262)	(7.228)
HH economic dependency ratio	-22.08**	-21.06**	-28.50**	-52.18**	-97.33**	-19.54**	-30.76**	-50.39**	-59.28**	-23.43
	(4.624)	(5.784)	(7.535)	(9.120)	(27.89)	(5.920)	(8.194)	(11.96)	(18.85)	(67.13)
# of HH head relatives owning land	3.843	6.267*	2.091	6.389	-11.42	8.259*	4.047	5.293	27.44	192.3*
	(2.876)	(2.804)	(3.482)	(8.929)	(20.06)	(4.583)	(5.204)	(11.05)	(20.36)	(99.42)
# of HH head's spouse relatives owning land	0.875	0.307	0.0773	-2.775	23.00	6.927	7.650	10.04	6.966	-4.300
	(1.612)	(2.028)	(2.931)	(4.845)	(16.15)	(4.174)	(4.839)	(6.021)	(9.689)	(39.93)
# of HH head relatives living outside thana	3.813	0.784	-0.0919	7.621	24.95	-5.324	-4.310	-1.517	4.719	17.47
	(5.538)	(3.462)	(5.899)	(10.52)	(40.25)	(3.912)	(5.159)	(6.631)	(14.14)	(41.97)
# of HH head's spouse relatives living outside thana	-2.296	0.955	-0.923	2.863	-2.705	1.577	4.702	4.236	3.413	0.762
	(2.118)	(1.951)	(3.250)	(5.393)	(23.54)	(2.985)	(3.267)	(4.046)	(7.198)	(26.92)
Loans from traditional banks (1=yes)	119.6**	99.68**	211.5**	601.0**	1668.4**	38.08	68.97	162.7	676.0**	661.1*
	(38.31)	(57.86)	(153.3)	(272.1)	(936.3)	(60.84)	(77.83)	(171.1)	(316.9)	(795.0)
Loans from informal sources (1=yes)	56.05*	119.0**	177.5**	531.9**	592.6**	42.75	7.516	-0.219	-94.93	-298.0
	(33.26)	(46.58)	(74.51)	(162.3)	(169.7)	(43.22)	(43.35)	(64.56)	(115.3)	(489.2)
Loans from relatives (1=yes)	61.49**	85.00**	147.6**	212.9	252.3	28.90	87.15**	165.2**	461.4**	1732.8**
	(29.69)	(42.09)	(60.46)	(131.2)	(349.5)	(40.57)	(52.48)	(85.41)	(197.1)	(661.7)
Eligibility of HH (1=yes)	-92.97**	-78.72	-59.26	-171.5	-480.9	-76.07	-148.0**	-362.9**	-481.2**	-1062.3*
	(36.74)	(46.65)	(70.92)	(171.8)	(606.6)	(60.59)	(71.26)	(201.8)	(259.2)	(787.7)
Overall intercept	397.8**	566.9**	740.4**	1081.9**	2039.8**	426.9**	652.1**	1054.0**	1571.4**	2620.6**
	(53.08)	(66.41)	(98.50)	(210.0)	(1107.8)	(88.52)	(101.0)	(226.3)	(355.4)	(1250.1)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table D.6 **Cross-section quantile regressions with village penalised fixed effects, household non-food expenditure, by gender**

	1991-92					**1998-99**				
Quantile	10%	30%	Median	70%	90%	10%	30%	Median	70%	90%
Female microcredit borrowings	0.003**	0.003**	0.003**	0.008**	0.013	0.003**	0.004**	0.007**	0.016**	0.047**
	(0.001)	(0.001)	(0.002)	(0.003)	(0.009)	(0.001)	(0.002)	(0.003)	(0.006)	(0.014)
Male microcredit borrowings	0.004**	0.003	0.002	0.005	0.021*	0.001	0.002	0.000	-0.001	-0.004
	(0.002)	(0.002)	(0.003)	(0.007)	(0.010)	(0.002)	(0.002)	(0.003)	(0.007)	(0.016)
Household characteristics										
Education of HH head	-5.142	-10.01	-2.669	5.415	83.68	4.640	5.561	10.04	9.332	27.23
	(5.902)	(5.652)	(9.481)	(18.51)	(65.09)	(7.296)	(7.962)	(15.72)	(30.53)	(93.15)
Age of HH head	-2.060**	-2.744**	-4.230**	-5.181**	-11.20**	-1.948**	-1.477	-2.001	-5.776*	-7.022
	(0.643)	(0.759)	(1.056)	(1.492)	(4.994)	(1.251)	(1.023)	(1.616)	(2.804)	(6.579)
Gender of HH head	4.822	-44.65	-53.83	-96.12	-137.7	77.34	43.77	8.493	-26.72	-370.9
	(26.27)	(43.76)	(50.85)	(94.34)	(848.8)	(43.59)	(55.83)	(77.14)	(174.0)	(877.0)
Highest education of men in HH	22.09**	28.10**	31.15**	26.51**	82.03	14.92**	25.18**	39.23**	63.98**	160.2**
	(5.503)	(5.266)	(8.985)	(15.76)	(58.97)	(5.543)	(5.729)	(11.76)	(22.20)	(64.28)
Highest education of women in HH	14.99**	29.99**	37.28**	52.31**	65.47**	15.72**	30.93**	44.92**	77.71**	170.9**
	(4.594)	(5.689)	(8.282)	(22.14)	(45.82)	(5.991)	(8.392)	(12.65)	(23.30)	(92.30)
Landholdings	0.253**	0.330**	0.887**	1.952**	2.650**	0.805**	0.826**	1.520**	2.505**	6.297
	(0.0908)	(0.223)	(0.475)	(0.979)	(1.242)	(0.281)	(0.376)	(0.540)	(1.241)	(7.077)
HH economic dependency ratio	-22.05**	-20.45**	-28.28**	-52.31**	-96.94**	-21.24**	-29.76**	-51.50**	-60.27**	-22.02
	(4.752)	(5.633)	(7.567)	(9.638)	(28.36)	(6.121)	(7.974)	(11.99)	(19.28)	(67.51)
# of HH head relatives owning land	3.893	6.519*	2.049	5.669	-11.71	9.201	5.556	6.217	30.92	188.9*
	(2.921)	(2.805)	(3.582)	(8.780)	(20.88)	(4.774)	(5.505)	(11.54)	(21.42)	(98.53)
# of HH head's spouse relatives owning land	0.646	0.452	0.0473	-2.181	23.37	5.905*	9.445*	9.727	6.835	7.914
	(1.605)	(2.034)	(2.859)	(4.886)	(16.53)	(4.092)	(4.724)	(5.889)	(10.22)	(41.84)
# of HH head relatives living outside thana	3.752	0.201	-0.198	8.096	21.26	-5.249	-3.494	-2.390	3.821	17.40
	(5.527)	(3.435)	(5.844)	(10.69)	(41.37)	(3.963)	(5.101)	(6.540)	(14.00)	(39.13)
# of HH head's spouse relatives living outside thana	-2.449	1.058	-0.877	1.063	3.935	1.589	4.400	4.364	3.600	-10.27
	(2.097)	(1.969)	(3.232)	(5.354)	(23.16)	(3.070)	(3.190)	(4.132)	(7.471)	(27.58)
Loans from traditional banks (1=yes)	118.6**	98.35**	220.1**	601.3**	1610.9**	17.23	58.22	180.1	675.0**	766.1
	(38.07)	(58.07)	(155.4)	(270.7)	(933.4)	(62.81)	(78.74)	(170.0)	(304.1)	(795.1)
Loans from informal sources (1=yes)	56.78*	122.2**	177.9**	526.3**	577.7**	47.15	20.20	9.834	-102.6	-138.8
	(33.33)	(46.59)	(73.96)	(162.2)	(173.1)	(43.23)	(45.79)	(60.00)	(107.9)	(478.0)
Loans from relatives (1=yes)	61.27**	82.86**	146.8**	220.2	216.7	26.53	74.45*	154.3**	488.3**	1719.0**
	(29.06)	(42.57)	(59.38)	(130.7)	(349.4)	(40.19)	(53.13)	(81.36)	(197.9)	(664.2)
Eligibility of HH (1=yes)	-94.13**	-78.25	-58.65	-169.0	-479.7	-91.70	-152.9**	-343.9**	-512.0**	-1039.8
	(36.81)	(46.63)	(71.17)	(171.0)	(614.6)	(61.20)	(72.70)	(205.5)	(257.0)	(777.6)
Overall intercept	398.5**	563.4**	730.8**	1074.9**	2095.2**	454.5**	644.1**	1044.5**	1559.3**	2635.5**
	(53.15)	(66.25)	(98.18)	(206.8)	(1103.3)	(87.77)	(101.7)	(226.3)	(353.7)	(1222.5)

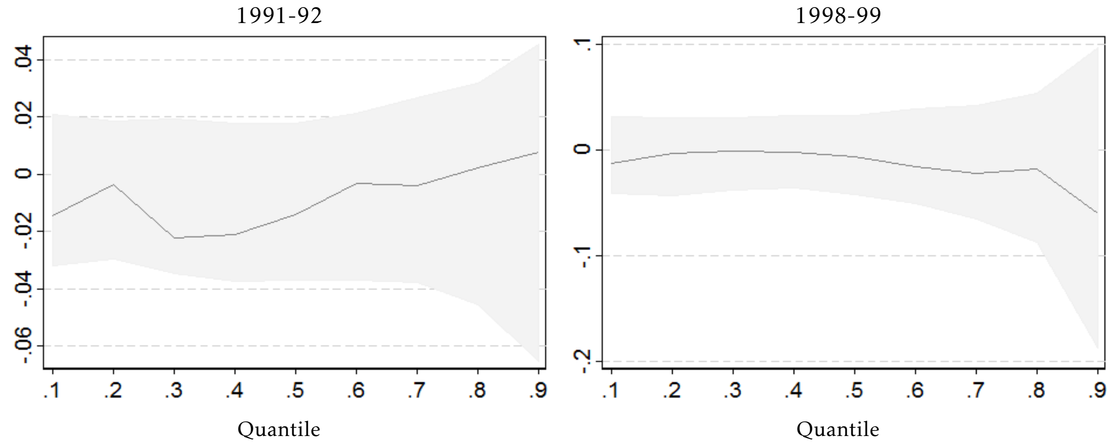
Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Appendix E

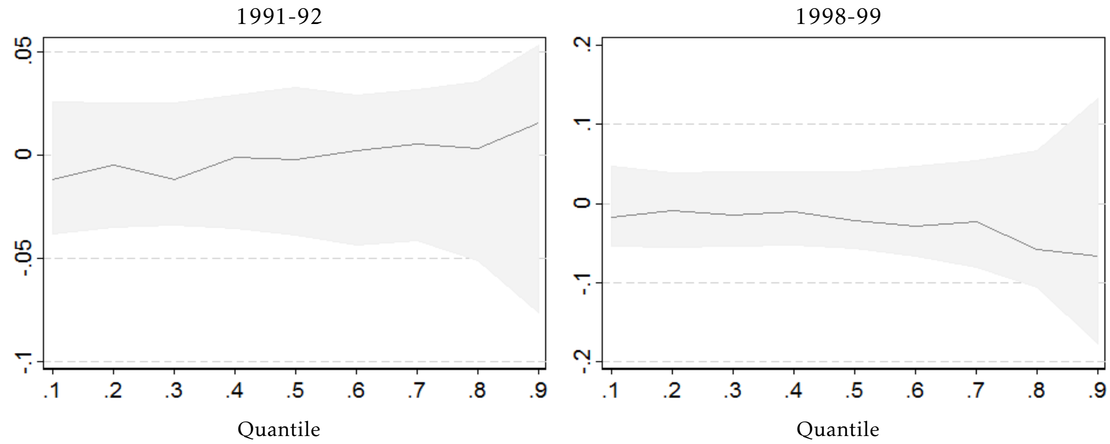
Plots of quantile processes for cross-section Two-Stage quantile regressions (2SQR), food and non-food expenditure

Figure E.1 **Distributional impacts of microcredit on household food consumption: cross-section two-stage quantile regressions with village covariates**

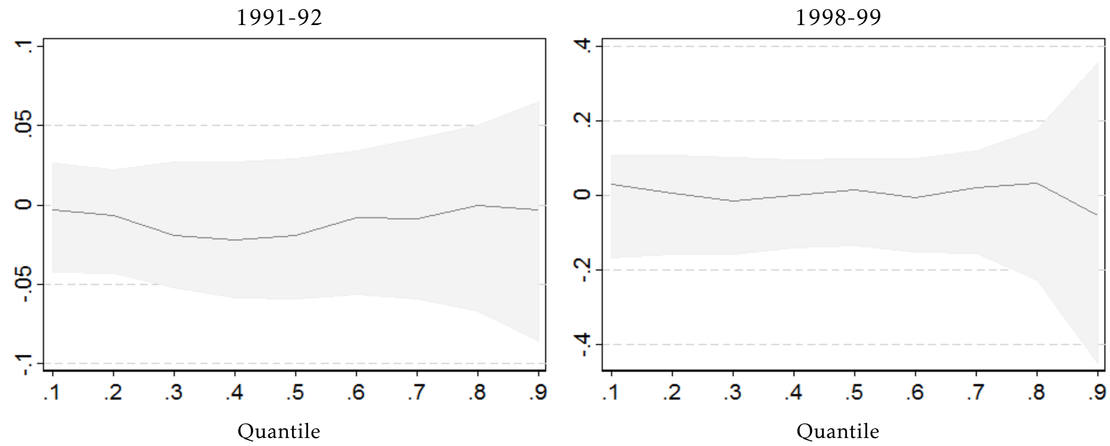
(a) Panel A: Household microcredit



(b) Panel B: Female microcredit



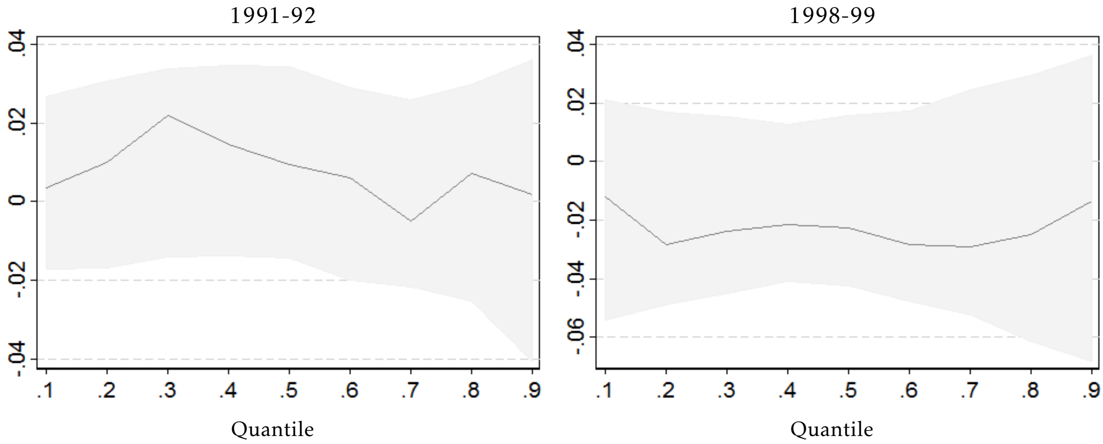
(c) Panel C: Male microcredit



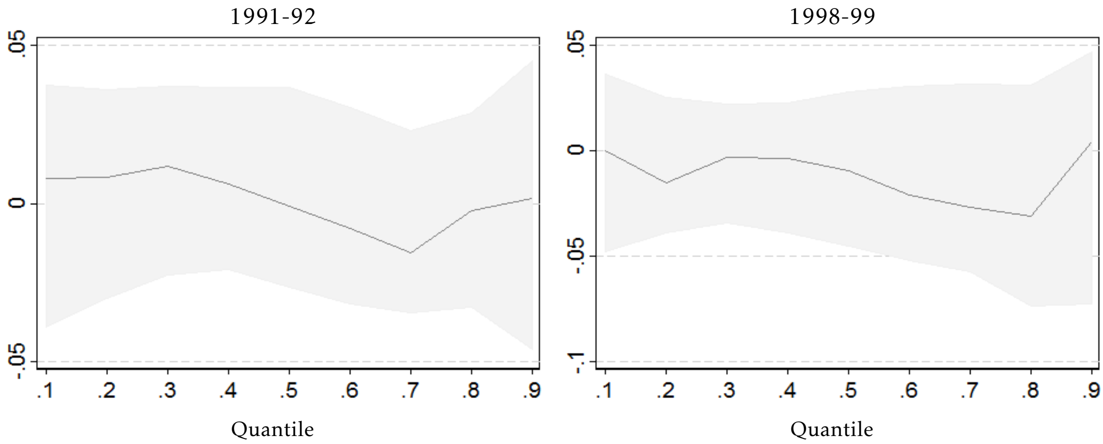
Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling villages with replacement to account for within-cluster dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. Microcredit is instrumented for following the approach in Pitt and Khandker (1998), using OLS in the first stage. Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Figure E.2 **Distributional impacts of microcredit on household food consumption: cross-section two-stage quantile regressions with village quantile effects**

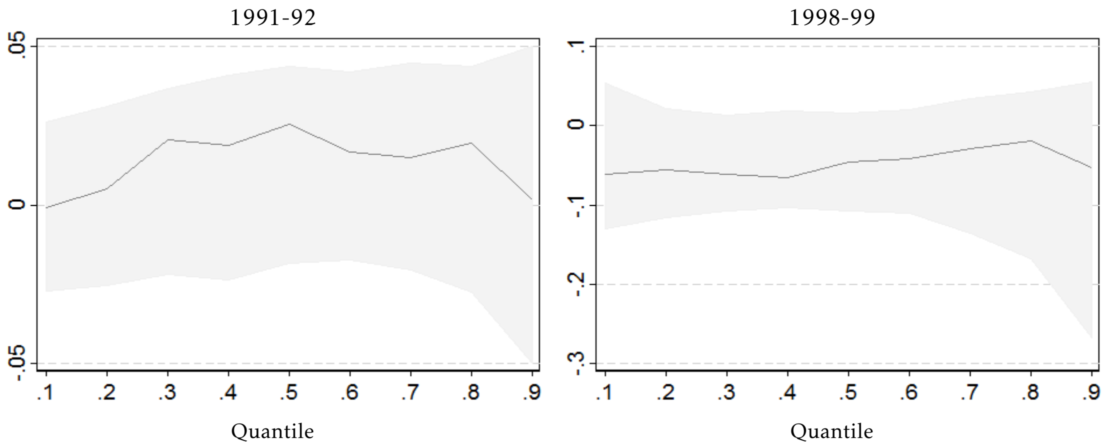
(a) Panel A: Household microcredit



(b) Panel B: Female microcredit

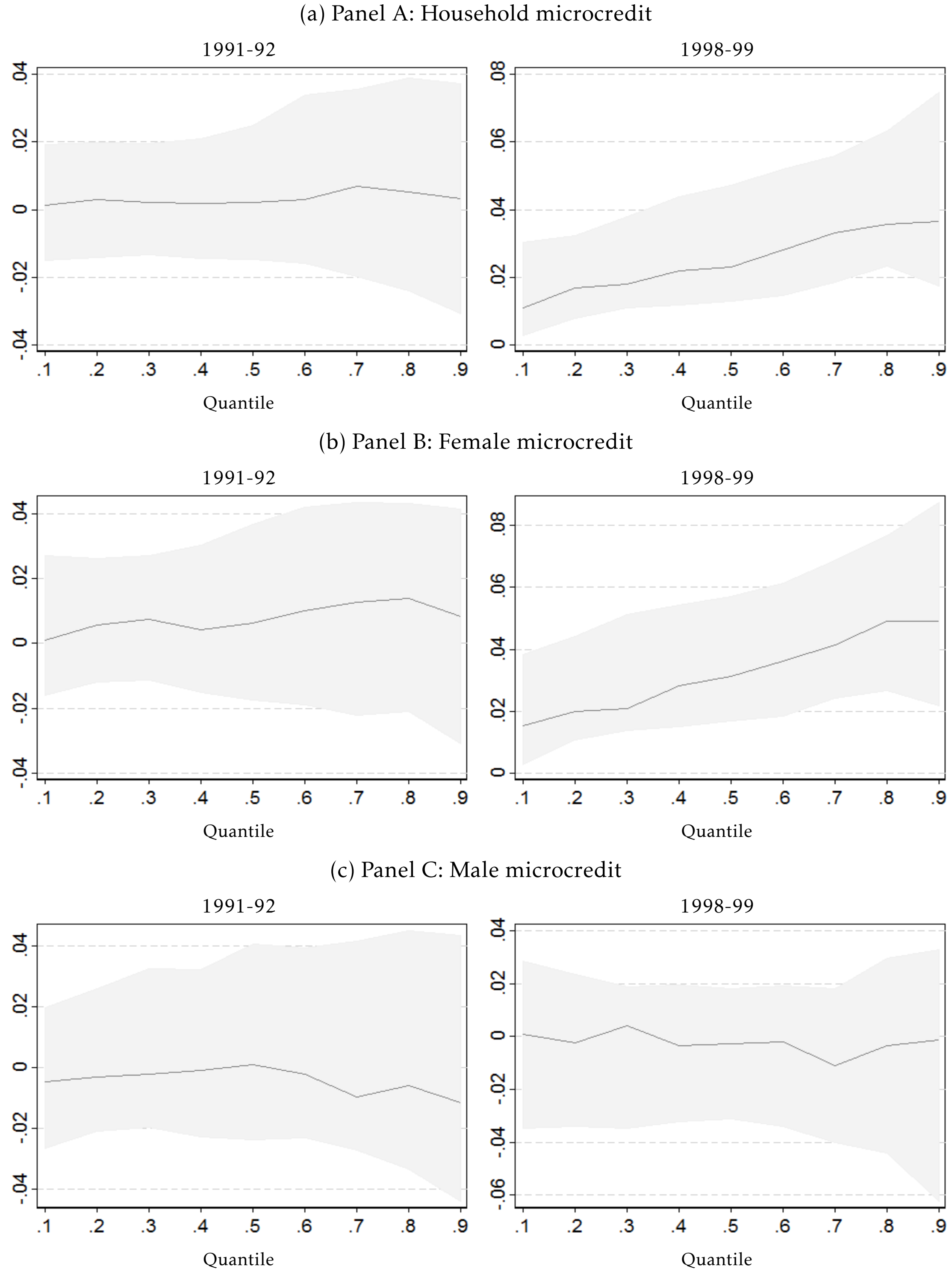


(c) Panel C: Male microcredit



Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling villages with replacement to account for within-cluster dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. Microcredit is instrumented for following the approach in Pitt and Khandker (1998), using OLS in the first stage. Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

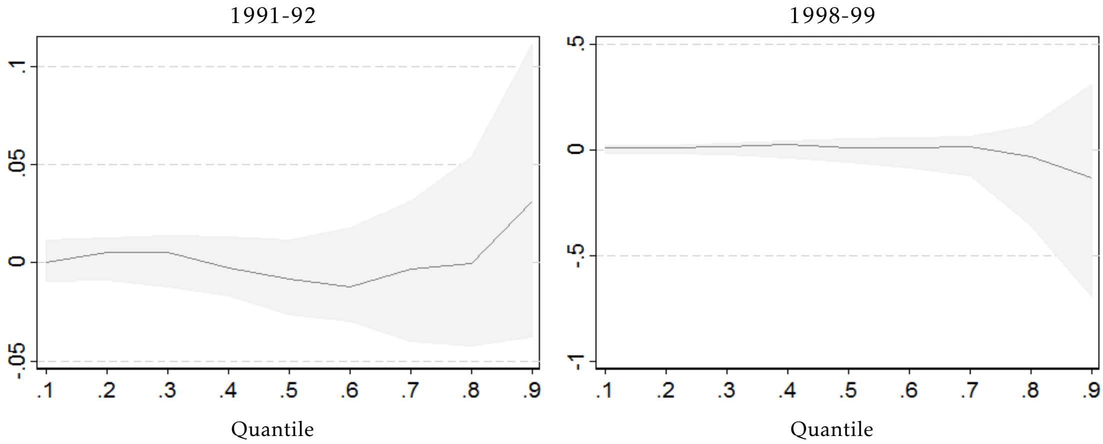
Figure E.3 **Distributional impacts of microcredit on household food consumption: cross-section two-stage quantile regressions with penalised village effects**



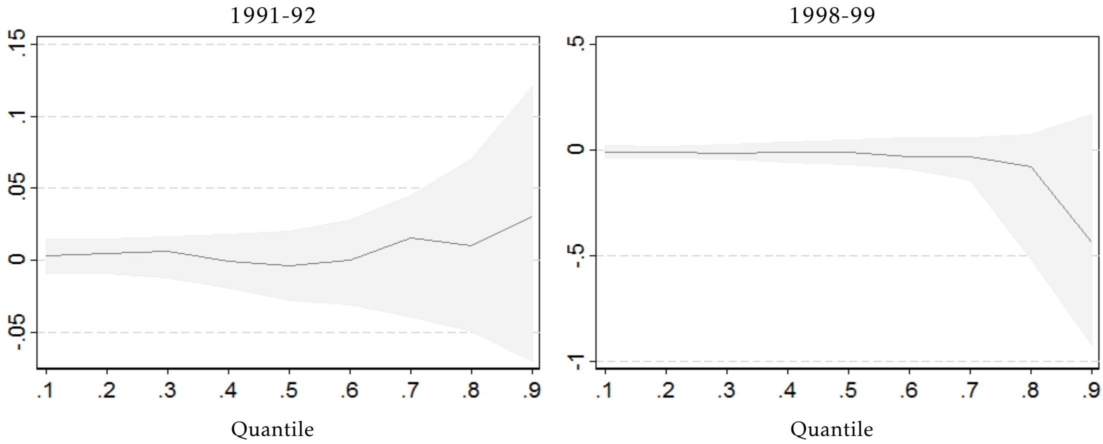
Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling villages with replacement to account for within-cluster dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. Microcredit is instrumented for following the approach in Pitt and Khandker (1998), using OLS in the first stage. Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Figure E.4 **Distributional impacts of microcredit on household non-food consumption: cross-section two-stage quantile regressions with village covariates**

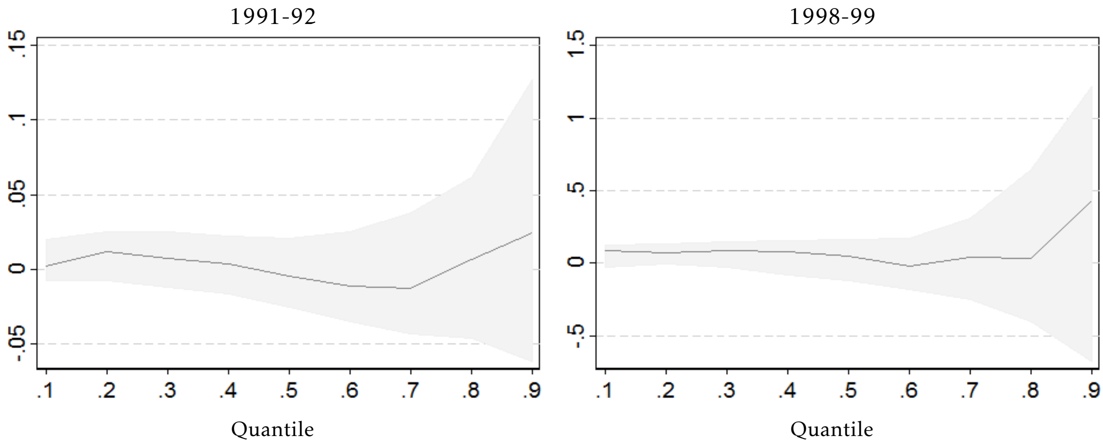
(a) Panel A: Household microcredit



(b) Panel B: Female microcredit



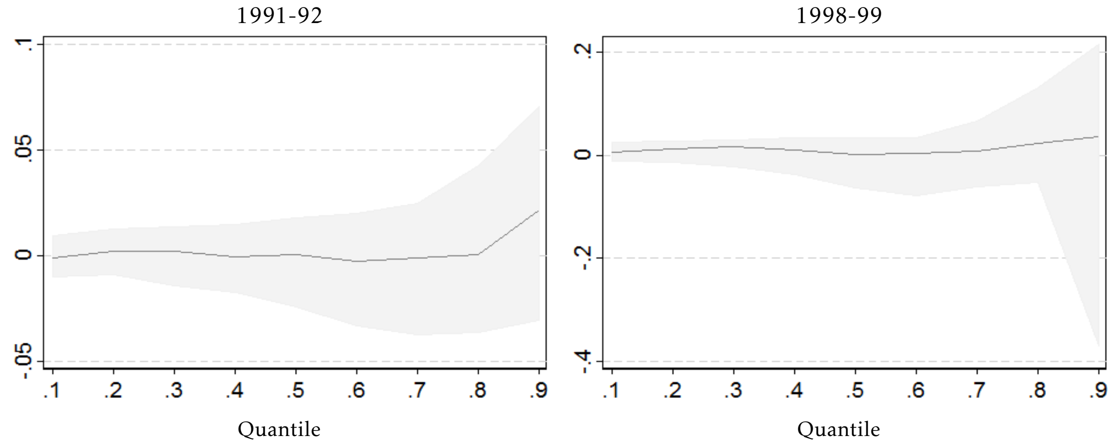
(c) Panel C: Male microcredit



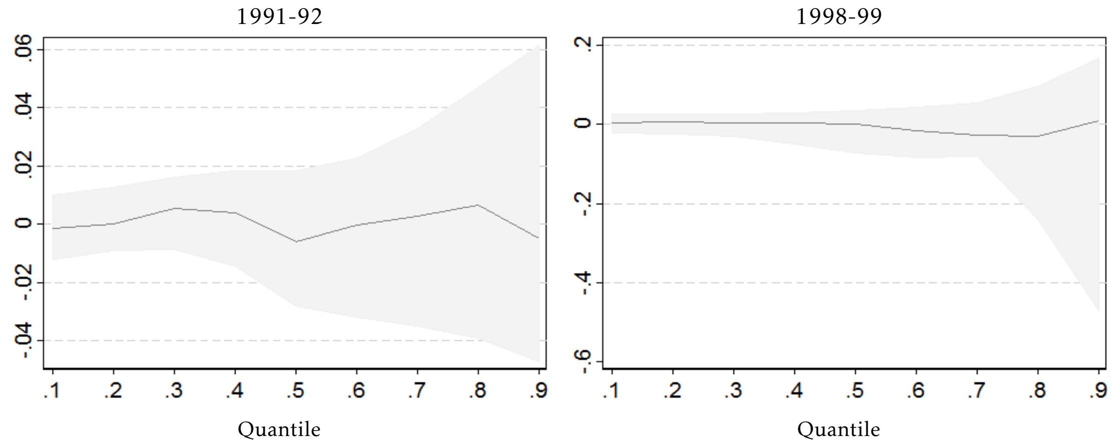
Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling villages with replacement to account for within-cluster dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. Microcredit is instrumented for following the approach in Pitt and Khandker (1998), using OLS in the first stage. Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Figure E.5 **Distributional impacts of microcredit on household non-food consumption: cross-section two-stage quantile regressions with village quantile effects**

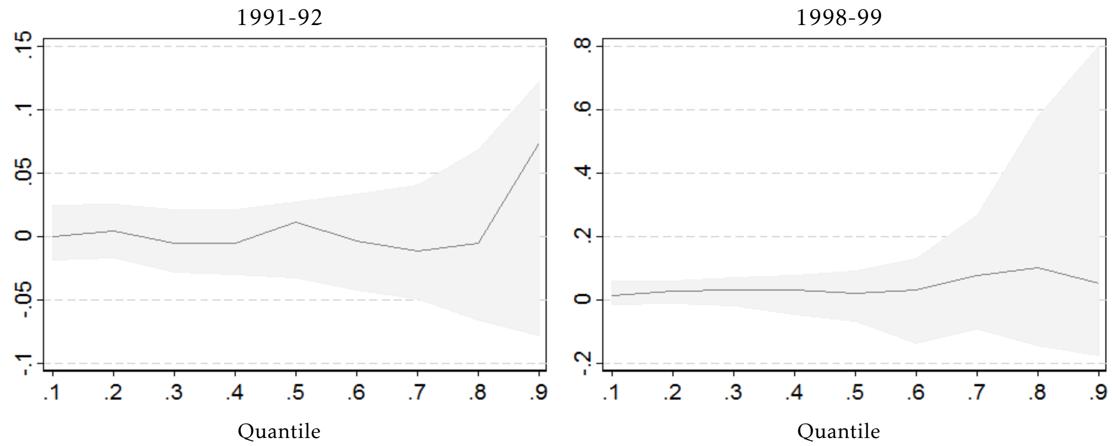
(a) Panel A: Household microcredit



(b) Panel B: Female microcredit



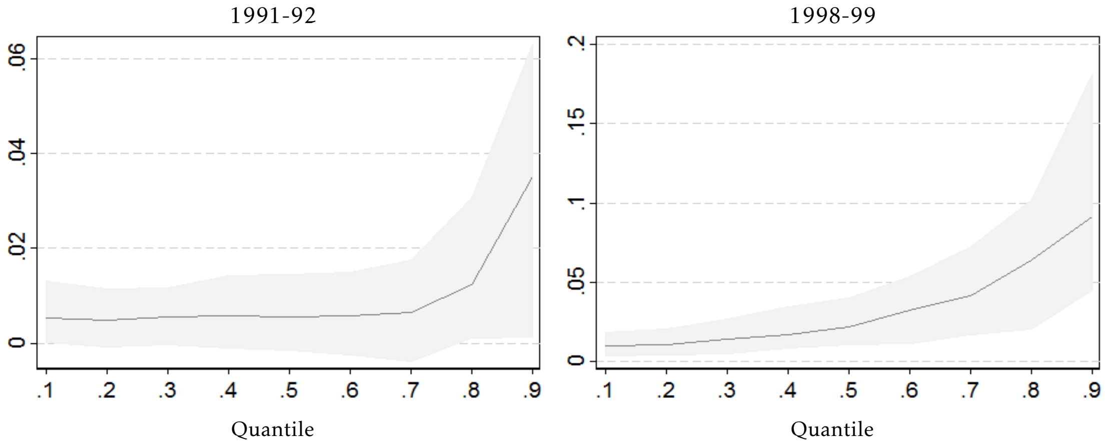
(c) Panel C: Male microcredit



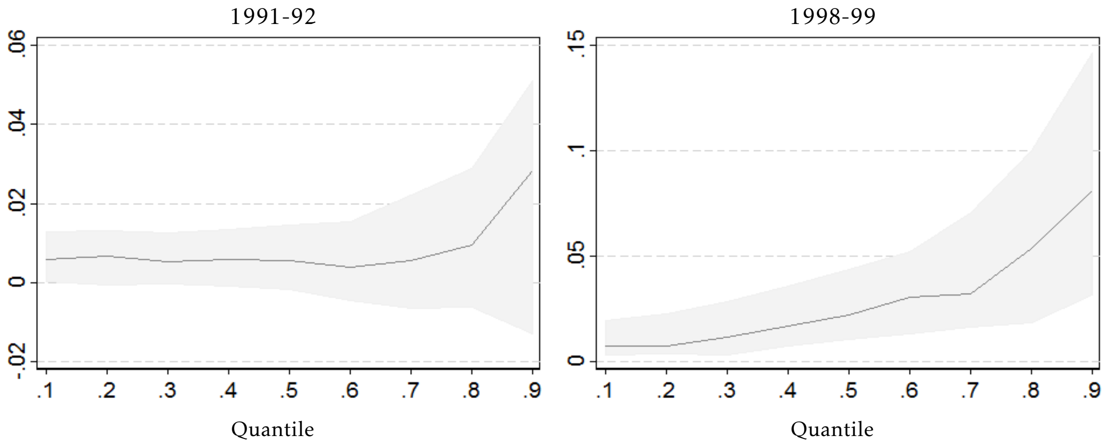
Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling villages with replacement to account for within-cluster dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. Microcredit is instrumented for following the approach in Pitt and Khandker (1998), using OLS in the first stage. Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Figure E.6 **Distributional impacts of microcredit on household non-food consumption: cross-section two-stage quantile regressions with penalised village effects**

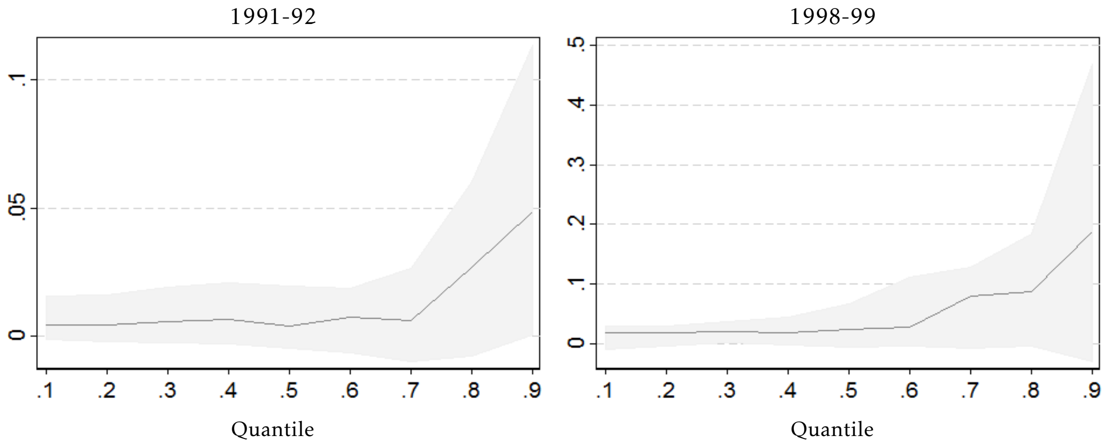
(a) Panel A: Household microcredit



(b) Panel B: Female microcredit



(c) Panel C: Male microcredit



Note: Solid lines show the credit coefficients estimates at each decile. The grey areas show 95% bootstrap percentile confidence intervals. We carry out 999 replications, re-sampling villages with replacement to account for within-cluster dependence of household-level observations through time. The estimation sample includes 1,638 households in each time period. Microcredit is measured as cumulative borrowings from microfinance programs over the previous six years, in 1992 Taka. Microcredit is instrumented for following the approach in Pitt and Khandker (1998), using OLS in the first stage. Specifications include either: village covariates; village quantile effects (i.e. village dummy variables); or penalised village effects (i.e. restricted to have a pure location-shift effect *à la* Koenker (2004)).

Appendix F

Coefficients tables for cross-section Two-Stage quantile regressions (2SQR), food and non-food expenditure

Table F.1 **Distributional impacts of microcredit on household food expenditure, cross-section two-stage covariates**

Quantile	1991-92 (1) Household credit	(2) Female credit	(3) Male credit	(4) Female = Male (p-value)	1998- (5) Household credit	(6) Female credit
10	-0.015	-0.012	-0.003	0.711	-0.013	-0.017
	(0.013)	(0.016)	(0.016)		(0.018)	(0.025)
20	-0.004	-0.005	-0.007	0.928	-0.003	-0.009
	(0.012)	(0.015)	(0.016)		(0.018)	(0.024)
30	-0.022	-0.012	-0.019	0.750	-0.001	-0.015
	(0.013)	(0.015)	(0.018)		(0.018)	(0.024)
40	-0.021	-0.001	-0.022	0.410	-0.002	-0.011
	(0.014)	(0.016)	(0.020)		(0.017)	(0.023)
50	-0.014	-0.002	-0.019	0.549	-0.006	-0.021
	(0.014)	(0.017)	(0.021)		(0.019)	(0.025)
60	-0.003	0.002	-0.008	0.727	-0.016	-0.028
	(0.015)	(0.018)	(0.022)		(0.022)	(0.027)
70	-0.004	0.005	-0.009	0.644	-0.022	-0.024
	(0.016)	(0.019)	(0.024)		(0.027)	(0.032)
80	0.002	0.003	-0.000	0.921	-0.018	-0.059
	(0.020)	(0.022)	(0.028)		(0.038)	(0.042)
90	0.008	0.016	-0.003	0.696	-0.060	-0.066
	(0.028)	(0.033)	(0.037)		(0.063)	(0.068)
Coefficients equal across quantiles (p-value)	0.433	0.937	0.971	0.996	0.976	0.943
Coefficients jointly zero (p-value)	0.447	0.951	0.980	0.996	0.981	0.932

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns (4 of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4) and (8) show joint hypotheses tests of the equ microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

Table F.2 **Distributional impacts of microcredit on household food expenditure, cross-section two-stage quantile effects**

	1991-92				1998-	
	(1)	(2)	(3)	(4)	(5)	(6)
Quantile	**Household credit**	**Female credit**	**Male credit**	**Female = Male (p-value)**	**Household credit**	**Female credit**
10	0.003	0.008	-0.001	0.705	-0.012	0.000
	(0.011)	(0.019)	(0.013)		(0.020)	(0.021)
20	0.010	0.008	0.005	0.892	-0.028	-0.015
	(0.012)	(0.018)	(0.014)		(0.017)	(0.015)
30	0.022	0.012	0.020	0.683	-0.024	-0.003
	(0.012)	(0.015)	(0.015)		(0.015)	(0.014)
40	0.015	0.006	0.019	0.531	-0.022	-0.003
	(0.012)	(0.015)	(0.015)		(0.014)	(0.015)
50	0.009	-0.001	0.025	0.219	-0.023	-0.010
	(0.012)	(0.016)	(0.015)		(0.015)	(0.019)
60	0.006	-0.008	0.017	0.259	-0.028	-0.021
	(0.013)	(0.015)	(0.015)		(0.017)	(0.022)
70	-0.005	-0.016	0.015	0.153	-0.029	-0.027
	(0.012)	(0.015)	(0.016)		(0.020)	(0.024)
80	0.007	-0.002	0.020	0.318	-0.025	-0.031
	(0.014)	(0.016)	(0.018)		(0.021)	(0.024)
90	0.002	0.002	0.002	1.000	-0.014	0.004
	(0.020)	(0.024)	(0.025)		(0.025)	(0.031)
Coefficients equal across quantiles (p-value)	0.394	0.860	0.709	0.878	0.878	0.595
Coefficients jointly zero (p-value)	0.473	0.912	0.701	0.898	0.754	0.670

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns (4 of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4) and (8) show joint hypotheses tests of the eq microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

Table F.3 **Distributional impacts of microcredit on household food expenditure, cross-section two-sta nalised village effects**

Quantile	1991-92 (1) Household credit	(2) Female credit	(3) Male credit	(4) Female = Male (p-value)	1998- (5) Household credit	(6) Female credit
10	0.001	0.001	-0.005	0.705	0.011**	0.015**
	(0.009)	(0.011)	(0.012)		(0.007)	(0.009)
20	0.003	0.006	-0.003	0.596	0.017**	0.020**
	(0.009)	(0.010)	(0.013)		(0.006)	(0.009)
30	0.002	0.007	-0.002	0.622	0.018**	0.021**
	(0.009)	(0.010)	(0.015)		(0.007)	(0.011)
40	0.002	0.004	-0.001	0.799	0.022**	0.028**
	(0.009)	(0.011)	(0.014)		(0.008)	(0.011)
50	0.002	0.006	0.001	0.819	0.023**	0.031**
	(0.010)	(0.014)	(0.015)		(0.009)	(0.012)
60	0.003	0.010	-0.002	0.607	0.028**	0.036**
	(0.013)	(0.016)	(0.016)		(0.009)	(0.012)
70	0.007	0.013	-0.010	0.358	0.033**	0.041**
	(0.014)	(0.016)	(0.018)		(0.010)	(0.012)
80	0.005	0.014	-0.006	0.436	0.036**	0.049**
	(0.016)	(0.017)	(0.020)		(0.011)	(0.014)
90	0.003	0.008	-0.012	0.512	0.036**	0.049**
	(0.017)	(0.018)	(0.023)		(0.016)	(0.019)
Coefficients equal across quantiles (p-value)	0.999	0.962	0.977	0.997	0.057	0.008
Coefficients jointly zero (p-value)	1.000	0.971	0.988	0.998	0.013	0.006

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns (4 of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4) and (8) show joint hypotheses tests of the equ microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

Table F.4 **Distributional impacts of microcredit on household non-food expenditure, cross-section two village covariates**

Quantile	1991-92 (1) Household credit	(2) Female credit	(3) Male credit	(4) Female = Male (p-value)	1998- (5) Household credit	(6) Female credit
10	0.000	0.003	0.002	0.960	0.009	-0.012
	(0.005)	(0.006)	(0.007)		(0.009)	(0.013)
20	0.005	0.004	0.012	0.471	0.011	-0.010
	(0.005)	(0.006)	(0.008)		(0.010)	(0.014)
30	0.005	0.006	0.007	0.942	0.017	-0.016
	(0.006)	(0.007)	(0.009)		(0.013)	(0.018)
40	-0.003	-0.001	0.004	0.740	0.027	-0.012
	(0.008)	(0.009)	(0.010)		(0.019)	(0.024)
50	-0.008	-0.004	-0.005	0.968	0.013	-0.008
	(0.009)	(0.012)	(0.012)		(0.027)	(0.029)
60	-0.012	-0.000	-0.011	0.614	0.009	-0.031
	(0.012)	(0.015)	(0.015)		(0.037)	(0.036)
70	-0.003	0.016	-0.012	0.322	0.014	-0.034
	(0.017)	(0.021)	(0.020)		(0.054)	(0.050)
80	-0.000	0.010	0.006	0.927	-0.029	-0.078
	(0.025)	(0.030)	(0.028)		(0.113)	(0.143)
90	0.032	0.030	0.024	0.931	-0.133	-0.438
	(0.037)	(0.046)	(0.046)		(0.263)	(0.290)
Coefficients equal across quantiles (p-value)	0.464	0.748	0.532	0.798	0.913	0.866
Coefficients jointly zero (p-value)	0.549	0.790	0.601	0.817	0.889	0.865

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns (4 of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4) and (8) show joint hypotheses tests of the equ microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

Table F.5 **Distributional impacts of microcredit on household non-food expenditure, cross-section two**
village quantile effects

	1991-92				1998-	
	(1)	(2)	(3)	(4)	(5)	(6)
Quantile	**Household credit**	**Female credit**	**Male credit**	**Female = Male (p-value)**	**Household credit**	**Female credit**
10	-0.001	-0.001	0.000	0.893	0.006	0.002
	(0.005)	(0.006)	(0.009)		(0.009)	(0.011)
20	0.002	0.000	0.005	0.693	0.012	0.007
	(0.005)	(0.006)	(0.010)		(0.010)	(0.012)
30	0.002	0.006	-0.006	0.401	0.017	0.004
	(0.007)	(0.006)	(0.012)		(0.013)	(0.014)
40	-0.001	0.004	-0.005	0.542	0.010	0.003
	(0.008)	(0.008)	(0.013)		(0.017)	(0.019)
50	0.000	-0.006	0.011	0.322	0.001	-0.000
	(0.011)	(0.011)	(0.016)		(0.022)	(0.026)
60	-0.002	-0.000	-0.003	0.899	0.004	-0.016
	(0.013)	(0.013)	(0.021)		(0.029)	(0.032)
70	-0.001	0.003	-0.011	0.637	0.007	-0.028
	(0.016)	(0.017)	(0.023)		(0.034)	(0.038)
80	0.000	0.006	-0.005	0.776	0.023	-0.029
	(0.020)	(0.022)	(0.034)		(0.063)	(0.083)
90	0.022	-0.005	0.074	0.156	0.037	0.008
	(0.024)	(0.026)	(0.047)		(0.117)	(0.165)
Coefficients equal across quantiles (p-value)	0.951	0.740	0.070	0.285	0.946	0.987
Coefficients jointly zero (p-value)	0.974	0.809	0.090	0.333	0.954	0.994

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence
rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns (4
of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4) and (8) show joint hypotheses tests of the equ
microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

Table F.6 **Distributional impacts of microcredit on household non-food expenditure, cross-section two penalised village effects**

Quantile	1991-92 (1) Household credit	(2) Female credit	(3) Male credit	(4) Female = Male (p-value)	1998- (5) Household credit	(6) Female credit
10	0.005**	0.006**	0.004	0.758	0.010**	0.007**
	(0.003)	(0.003)	(0.005)		(0.004)	(0.005)
20	0.005*	0.007*	0.004	0.658	0.010**	0.007**
	(0.003)	(0.003)	(0.005)		(0.005)	(0.006)
30	0.005*	0.005*	0.006	0.952	0.014**	0.011**
	(0.003)	(0.003)	(0.005)		(0.005)	(0.007)
40	0.006*	0.006	0.006	0.926	0.017**	0.017**
	(0.004)	(0.004)	(0.006)		(0.007)	(0.008)
50	0.006	0.006	0.004	0.821	0.022**	0.022**
	(0.004)	(0.004)	(0.006)		(0.008)	(0.009)
60	0.006	0.004	0.008	0.649	0.033**	0.031**
	(0.004)	(0.005)	(0.007)		(0.011)	(0.010)
70	0.006	0.005	0.006	0.945	0.041**	0.032**
	(0.005)	(0.007)	(0.009)		(0.015)	(0.015)
80	0.012**	0.009	0.027	0.398	0.064**	0.054**
	(0.008)	(0.009)	(0.018)		(0.020)	(0.022)
90	0.035**	0.028	0.049**	0.495	0.091**	0.081**
	(0.015)	(0.016)	(0.027)		(0.037)	(0.029)
Coefficients equal across quantiles (p-value)	0.794	0.826	0.755	0.918	0.160	0.079
Coefficients jointly zero (p-value)	0.378	0.487	0.758	0.714	0.180	0.106

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance is assessed based on bootstrap percentile confidence rows show Wald-type tests of the equality of coefficients for each credit variable across all quantiles, and whether they are jointly zero. Columns (4 of slope coefficients on female and male microcredit at each quantile. Bottom rows of columns (4) and (8) show joint hypotheses tests of the eq microcredit across all quantiles, and a similar tests for whether all these coefficients are simultaneously zero across all quantiles.

Appendix G

Regression tables for pooled quantile regression estimates

Table G.1 **Pooled quantile regressions, household total expenditure**

	Quantile				
	10%	30%	Median	70%	90%
Household	0.008**	0.009**	0.015**	0.022**	0.039**
microcredit borrowings	(0.003)	(0.003)	(0.005)	(0.007)	(0.014)
Household characteristics					
Education of HH head	-0.904	6.425	47.54*	114.1**	218.7
	(24.50)	(18.45)	(26.43)	(43.55)	(147.6)
Age of HH head	-4.656	-5.600**	-7.427**	-12.67**	-9.661
	(3.474)	(2.799)	(3.119)	(4.939)	(8.944)
Gender of HH head	249.0	-137.3	-388.5**	-256.1*	-1334.3**
	(169.0)	(192.7)	(157.5)	(290.5)	(836.0)
Highest education of	60.80**	77.52**	69.25**	81.68**	141.2**
men in HH	(22.40)	(15.10)	(18.87)	(33.93)	(97.44)
Highest education of	38.36**	82.34**	113.1**	143.6**	239.6**
women in HH	(16.81)	(16.85)	(20.86)	(37.60)	(106.4)
Landholdings	1.108**	1.026**	1.468**	1.812**	2.218
	(0.471)	(0.496)	(0.709)	(0.700)	(2.982)
HH economic	-83.72**	-148.3**	-185.4**	-227.6**	-207.6**
dependency ratio	(22.60)	(18.76)	(23.27)	(38.09)	(92.07)
# of HH head	13.17	7.658	12.01	24.96	15.01
relatives owning land	(12.36)	(12.89)	(16.69)	(23.81)	(42.31)
# of HH head's	-8.620	9.102	6.797	14.29	22.31
spouse relatives owning land	(11.12)	(9.892)	(12.28)	(19.55)	(32.25)
# of HH head	-0.132	5.041	-5.676	-14.50	-20.92
relatives living outside thana	(15.31)	(12.74)	(16.34)	(24.31)	(55.17)
# of HH head's	6.210	4.159	4.942	-5.197	22.90
spouse relatives living outside thana	(8.875)	(8.829)	(10.88)	(16.34)	(41.41)
Loans from	291.6*	478.2**	640.7**	740.1**	1438.3
traditional banks (1=yes)	(160.4)	(200.5)	(197.8)	(351.7)	(1171.6)
Loans from informal	95.38	2.535	0.454	145.4	524.2
sources (1=yes)	(141.4)	(111.5)	(153.8)	(295.2)	(542.6)
Loans from relatives	89.53	264.5**	335.0**	568.8**	1719.8**
(1=yes)	(161.8)	(107.5)	(129.1)	(271.9)	(692.7)
Eligibility of HH	-206.8**	-368.1**	-377.7**	-535.6**	-357.1
(1=yes)	(131.6)	(103.2)	(147.8)	(235.0)	(543.4)
Village covariates					
Average male wage	-2.006	-2.534	-1.903	-0.213	3.538
	(4.421)	(3.099)	(4.741)	(6.598)	(19.75)
Average female wage	2.044	4.589	3.379	-1.900	17.80
	(3.428)	(2.878)	(4.673)	(6.589)	(14.76)
Primary school	12.41	-110.1	-228.1**	-337.0**	-364.7
(1=yes)	(86.70)	(75.37)	(99.79)	(132.5)	(294.1)
Food program (1=yes)	-45.11	-157.0**	-169.1**	-269.1*	-424.3*
	(81.07)	(63.24)	(83.12)	(118.8)	(256.3)
Distance to nearest	-16.33	-35.56**	-45.74**	-57.22**	-108.9*
bank (km)	(17.01)	(12.75)	(15.81)	(23.42)	(50.87)
Distance to nearest	-16.70	-11.28	-6.802	9.147	39.71
pucca road (km)	(15.19)	(10.90)	(13.93)	(20.17)	(59.80)
Distance to nearest	34.13	37.47**	57.69**	97.96**	36.39
shop/market (km)	(27.99)	(21.91)	(27.28)	(38.99)	(67.10)
Electricity in	160.3**	279.2**	366.0**	489.9**	587.0**
village (1=yes)	(83.38)	(73.77)	(92.59)	(136.1)	(311.3)
Price of rice	4.714	41.33*	52.18	49.21	200.2
	(33.17)	(31.12)	(45.67)	(67.14)	(163.1)
Price of wheat flour	52.35	2.120	-9.662	-4.206	-42.91
	(46.22)	(37.78)	(53.24)	(66.99)	(164.0)
Price of mustard oil	10.28*	10.84*	1.052	-2.218	-34.70
	(6.943)	(5.994)	(7.927)	(10.38)	(22.43)
Price of hen's eggs	-20.01	-4.155	-10.70	-4.991	-51.93
	(15.83)	(14.24)	(18.02)	(47.33)	(251.1)
Price of milk	30.29*	30.52**	41.29**	65.58**	24.80
	(14.33)	(11.59)	(18.53)	(28.15)	(65.35)
Price of potatoes	-14.09	11.83	-29.50	-28.03	51.00
	(18.26)	(16.82)	(25.16)	(39.59)	(89.67)
Intercepts					
Second wave of data	-247.6	-17.23	464.4**	835.4**	725.2
(1=yes)	(216.5)	(167.0)	(233.8)	(381.0)	(787.3)
Overall intercept	1236.0**	2257.1**	3569.8**	4147.6**	6145.6**
	(611.5)	(492.6)	(671.0)	(1007.0)	(2376.7)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table G.2 **Pooled quantile regressions, household total expenditure, by gender**

	Quantile				
	10%	30%	Median	70%	90%
Female microcredit	0.009**	0.009**	0.016**	0.025**	0.049**
borrowings	(0.003)	(0.004)	(0.005)	(0.008)	(0.016)
Male microcredit	0.001	0.008*	0.009	0.019	-0.001
borrowings	(0.005)	(0.004)	(0.009)	(0.010)	(0.020)
Household characteristics					
Education of HH head	2.284	7.697	45.41*	118.9**	179.5
	(24.43)	(18.52)	(26.42)	(43.81)	(147.9)
Age of HH head	-4.832	-5.652**	-7.563**	-12.62**	-12.52
	(3.443)	(2.808)	(3.115)	(5.007)	(9.024)
Gender of HH head	242.8	-136.6	-383.1**	-261.9*	-1442.4**
	(171.3)	(194.2)	(156.5)	(287.2)	(835.5)
Highest education of	59.61**	76.47**	69.55**	76.82**	178.7**
men in HH	(22.40)	(15.08)	(18.80)	(34.00)	(98.06)
Highest education of	41.40**	82.04**	114.9**	142.0**	250.0**
women in HH	(16.96)	(16.98)	(20.98)	(37.38)	(106.4)
Landholdings	1.112**	1.031**	1.337**	1.820**	2.012
	(0.472)	(0.495)	(0.710)	(0.709)	(2.989)
HH economic	-90.40**	-148.9**	-186.6**	-227.9**	-211.2**
dependency ratio	(22.34)	(18.80)	(23.13)	(38.19)	(91.31)
# of HH head	13.03	7.121	13.86	23.82	22.48
relatives owning land	(12.17)	(12.92)	(16.74)	(24.08)	(42.00)
# of HH head’s	-6.791	9.293	7.160	17.58	25.32
spouse relatives owning land	(11.10)	(9.936)	(12.16)	(19.63)	(32.48)
# of HH head	0.832	4.992	-4.206	-17.77	-28.71
relatives living outside thana	(15.46)	(12.75)	(16.30)	(24.33)	(54.85)
# of HH head’s	6.116	3.886	4.992	-4.955	17.75
spouse relatives living outside thana	(9.036)	(8.788)	(10.82)	(16.45)	(41.95)
Loans from	212.9*	481.9**	669.7**	755.5**	1291.2
traditional banks (1=yes)	(160.8)	(200.8)	(199.5)	(352.4)	(1173.4)
Loans from informal	83.29	8.094	8.185	139.8	563.0
sources (1=yes)	(144.3)	(112.4)	(154.5)	(294.6)	(542.5)
Loans from relatives	93.67	262.0**	341.6**	597.9**	1797.6**
(1=yes)	(163.0)	(107.8)	(132.0)	(271.0)	(688.8)
Eligibility of HH	-211.3**	-373.8**	-385.3**	-542.0**	-263.0
(1=yes)	(131.8)	(103.3)	(148.2)	(235.7)	(543.1)
Village covariates					
Average male wage	-1.834	-2.625	-1.855	-0.246	-0.472
	(4.418)	(3.097)	(4.807)	(6.591)	(19.72)
Average female wage	2.311	4.726	3.935	-1.525	15.57
	(3.416)	(2.874)	(4.687)	(6.598)	(14.54)
Primary school	-1.088	-107.0	-214.9**	-301.2**	-306.7
(1=yes)	(87.33)	(75.99)	(99.48)	(133.3)	(293.9)
Food program (1=yes)	-63.91	-158.6**	-171.9**	-260.4*	-418.4*
	(80.55)	(63.73)	(82.69)	(119.4)	(253.8)
Distance to nearest	-17.77	-35.67**	-46.24**	-53.96**	-113.4*
bank (km)	(16.87)	(12.79)	(15.85)	(23.56)	(50.66)
Distance to nearest	-12.57	-11.61	-5.833	4.258	42.22
pucca road (km)	(14.90)	(10.94)	(13.90)	(19.98)	(59.10)
Distance to nearest	40.42	39.85*	62.85**	96.01**	46.88
shop/market (km)	(27.77)	(21.87)	(27.10)	(39.28)	(66.79)
Electricity in	158.2**	278.0**	363.5**	443.3**	551.2**
village (1=yes)	(82.16)	(74.23)	(93.04)	(137.6)	(315.7)
Price of rice	-3.336	44.17	56.09	77.24	179.5
	(33.16)	(31.27)	(45.85)	(69.24)	(161.6)
Price of wheat flour	66.15	2.813	-15.27	-12.04	-30.12
	(46.25)	(38.00)	(53.90)	(67.43)	(162.6)
Price of mustard oil	13.56*	10.94*	0.823	-1.844	-34.65
	(6.953)	(6.023)	(7.914)	(10.39)	(22.15)
Price of hen’s eggs	-19.82	-4.811	-11.78	-7.164	-57.56
	(15.94)	(14.37)	(18.04)	(47.30)	(250.5)
Price of milk	31.63*	29.98**	42.38**	63.79**	35.10
	(14.34)	(11.69)	(18.50)	(28.34)	(64.87)
Price of potatoes	-13.02	11.49	-33.38	-25.36	53.64
	(17.98)	(16.91)	(25.23)	(40.02)	(90.27)
Intercepts					
Second wave of data	-301.8	-21.07	482.7**	818.8**	702.3
(1=yes)	(215.3)	(168.7)	(232.8)	(381.3)	(782.7)
Overall intercept	1014.6**	2233.2**	3575.0**	3902.6**	6455.4**
	(609.6)	(494.8)	(671.4)	(1010.5)	(2338.0)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table G.3 **Pooled quantile regressions, household food expenditure**

	Quantile				
	10%	30%	Median	70%	90%
Household	0.005*	0.008**	0.006**	0.006**	0.007
microcredit borrowings	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)
Household characteristics					
Education of HH head	11.24	13.04	44.57**	70.53**	106.3**
	(13.56)	(12.34)	(13.28)	(16.63)	(39.85)
Age of HH head	-0.168	-1.064	-1.484	-0.741	-2.811
	(2.410)	(2.189)	(2.293)	(2.521)	(3.717)
Gender of HH head	215.5	-11.38	-145.4	-379.0**	-572.6**
	(138.8)	(118.5)	(126.1)	(164.8)	(328.9)
Highest education of	30.98**	37.69**	24.46**	28.01**	24.44
men in HH	(12.71)	(10.01)	(9.954)	(12.80)	(21.85)
Highest education of	4.627	18.00	37.10**	32.64**	32.54
women in HH	(11.52)	(12.24)	(12.03)	(12.99)	(33.35)
Landholdings	0.474**	0.704**	0.632**	0.644**	1.595**
	(0.220)	(0.226)	(0.203)	(0.354)	(0.831)
HH economic	-77.47**	-100.2**	-130.5**	-154.4**	-190.0**
dependency ratio	(15.25)	(13.73)	(14.49)	(19.88)	(37.01)
# of HH head	6.768	5.073	3.759	7.725	-6.007
relatives owning land	(9.607)	(8.256)	(10.77)	(10.31)	(16.81)
# of HH head's	-0.0993	4.463	1.975	-3.628	-5.414
spouse relatives owning land	(7.121)	(6.532)	(7.221)	(8.746)	(14.34)
# of HH head	-0.639	10.99	6.958	9.172	-9.896
relatives living outside thana	(11.20)	(8.866)	(11.13)	(13.53)	(26.12)
# of HH head's	6.919	4.120	7.699	7.891	18.14
spouse relatives living outside thana	(5.995)	(5.665)	(6.632)	(8.890)	(15.67)
Loans from	123.7	238.7*	249.6**	137.4*	333.4
traditional banks (1=yes)	(103.1)	(118.3)	(100.3)	(113.6)	(189.6)
Loans from informal	6.463	83.19	59.29	-60.94	-38.86
sources (1=yes)	(103.5)	(85.76)	(90.86)	(88.98)	(167.9)
Loans from relatives	-7.343	1.101	161.2*	100.9	55.53
(1=yes)	(102.6)	(75.66)	(76.89)	(94.11)	(194.4)
Eligibility of HH	-130.3**	-249.0**	-244.5**	-239.2**	-179.9
(1=yes)	(89.32)	(68.27)	(81.19)	(96.15)	(165.2)
Village covariates					
Average male wage	-0.749	-0.836	0.342	0.207	1.128
	(2.988)	(2.195)	(3.043)	(3.115)	(6.836)
Average female wage	0.471	1.272	0.872	1.546	-6.205
	(2.517)	(2.078)	(3.002)	(3.330)	(5.298)
Primary school	-25.30	-84.44	-155.3*	-143.0**	-40.63
(1=yes)	(64.59)	(55.66)	(68.00)	(69.37)	(117.7)
Food program (1=yes)	-119.1*	-106.2**	-93.45*	-193.5**	-219.7**
	(56.00)	(44.09)	(54.79)	(61.14)	(110.5)
Distance to nearest	-19.09**	-34.44**	-31.34**	-30.44**	-53.49**
bank (km)	(11.95)	(9.543)	(10.20)	(12.99)	(20.74)
Distance to nearest	-0.602	-3.984	-1.459	9.650	3.356
pucca road (km)	(9.825)	(7.958)	(10.31)	(11.80)	(16.58)
Distance to nearest	18.52	20.95	20.12	17.91	59.68
shop/market (km)	(16.89)	(16.07)	(17.09)	(20.71)	(29.62)
Electricity in	242.0**	263.0**	235.1**	304.0**	465.8**
village (1=yes)	(57.06)	(50.98)	(57.88)	(72.02)	(144.3)
Price of rice	52.93**	55.62**	41.60	43.65	67.65
	(21.27)	(21.36)	(31.82)	(35.10)	(58.50)
Price of wheat flour	-20.99	-27.89	-25.15	-19.42	-66.84
	(29.20)	(28.80)	(30.51)	(38.99)	(63.49)
Price of mustard oil	3.337	9.010**	3.010	-1.813	-11.71
	(5.305)	(4.000)	(4.658)	(5.409)	(9.906)
Price of hen's eggs	-15.61	-1.517	-8.301	-18.01	2.025
	(12.00)	(8.771)	(12.72)	(33.92)	(118.6)
Price of milk	22.55	23.77**	28.78**	40.39**	59.66**
	(10.60)	(9.120)	(10.66)	(15.33)	(28.11)
Price of potatoes	9.879	11.00	0.592	12.39	25.95
	(12.49)	(14.17)	(17.63)	(21.92)	(31.73)
Intercepts					
Second wave of data	-262.8*	-154.8	-5.556	7.079	630.6*
(1=yes)	(137.5)	(132.4)	(160.3)	(195.7)	(303.8)
Overall intercept	1254.3**	1686.0**	2644.4**	3251.4**	4511.6**
	(432.2)	(344.9)	(435.8)	(515.6)	(935.4)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table G.4 **Pooled quantile regressions, household food expenditure, by gender**

	Quantile				
	10%	30%	Median	70%	90%
Female microcredit	0.006**	0.008**	0.007**	0.008**	0.007*
borrowings	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)
Male microcredit	-0.002	0.007**	0.002	0.001	-0.013
borrowings	(0.004)	(0.003)	(0.004)	(0.004)	(0.007)
Household characteristics					
Education of HH head	10.43	12.68	47.55**	69.97**	106.7**
	(13.37)	(12.39)	(13.34)	(16.78)	(39.57)
Age of HH head	-0.229	-1.067	-1.274	-0.977	-3.245
	(2.402)	(2.190)	(2.290)	(2.553)	(3.755)
Gender of HH head	233.2	-12.95	-145.2	-376.6**	-564.5**
	(136.8)	(118.8)	(126.8)	(165.9)	(326.6)
Highest education of	31.80**	37.89**	23.31**	27.78*	25.12
men in HH	(12.68)	(10.08)	(10.04)	(12.97)	(21.73)
Highest education of	6.875	17.51	34.92**	33.67**	28.14
women in HH	(11.45)	(12.28)	(12.06)	(12.97)	(33.25)
Landholdings	0.501**	0.709**	0.630**	0.622**	1.605**
	(0.220)	(0.224)	(0.204)	(0.366)	(0.826)
HH economic	-82.86**	-100.8**	-128.3**	-152.8**	-184.7**
dependency ratio	(15.27)	(13.69)	(14.34)	(19.92)	(36.59)
# of HH head	3.641	4.825	3.055	8.860	1.270
relatives owning land	(9.393)	(8.259)	(10.71)	(10.28)	(16.74)
# of HH head's	2.236	5.047	2.889	-2.747	-4.581
spouse relatives owning land	(7.042)	(6.569)	(7.190)	(8.871)	(14.45)
# of HH head	-1.808	11.23	6.339	11.16	-11.30
relatives living outside thana	(11.19)	(8.855)	(11.24)	(13.55)	(25.74)
# of HH head's	6.659	3.980	8.387	6.910	16.41
spouse relatives living outside thana	(5.922)	(5.677)	(6.669)	(8.871)	(15.58)
Loans from	153.5	236.3*	254.0**	138.2*	375.4
traditional banks (1=yes)	(102.9)	(118.8)	(99.69)	(112.3)	(187.5)
Loans from informal	13.21	79.22	52.18	-62.80	-49.56
sources (1=yes)	(106.0)	(85.89)	(91.39)	(89.29)	(167.6)
Loans from relatives	-2.238	3.676	165.2*	100.4	33.98
(1=yes)	(103.1)	(75.79)	(75.97)	(93.01)	(192.0)
Eligibility of HH	-108.2**	-248.1**	-240.5**	-238.8**	-144.6
(1=yes)	(87.40)	(68.84)	(81.33)	(97.05)	(165.5)
Village covariates					
Average male wage	-0.837	-0.922	0.336	0.438	0.0841
	(2.921)	(2.194)	(3.059)	(3.102)	(6.887)
Average female wage	-0.321	1.444	0.574	0.853	-6.178
	(2.451)	(2.101)	(2.981)	(3.314)	(5.288)
Primary school	-24.27	-83.35	-165.2**	-166.8**	-45.36
(1=yes)	(65.30)	(56.38)	(67.01)	(70.31)	(119.6)
Food program (1=yes)	-130.0**	-102.8**	-96.41*	-186.2**	-219.2**
	(54.75)	(44.17)	(54.51)	(61.54)	(108.7)
Distance to nearest	-19.09**	-33.00**	-33.13**	-32.11**	-52.87**
bank (km)	(12.09)	(9.530)	(10.14)	(13.01)	(20.74)
Distance to nearest	-2.467	-4.189	-1.920	10.40	4.962
pucca road (km)	(9.854)	(7.958)	(10.26)	(11.76)	(16.72)
Distance to nearest	23.45	21.04	17.13	23.91	60.76
shop/market (km)	(17.32)	(16.04)	(17.03)	(20.72)	(30.18)
Electricity in	225.5**	259.4**	243.7**	314.5**	478.5**
village (1=yes)	(56.30)	(51.10)	(57.60)	(73.12)	(143.5)
Price of rice	52.26**	55.28**	35.39	42.48	61.53
	(21.07)	(21.52)	(31.76)	(36.00)	(59.09)
Price of wheat flour	-18.53	-25.68	-18.17	-28.05	-44.98
	(29.60)	(28.69)	(30.49)	(39.41)	(63.14)
Price of mustard oil	3.064	9.360**	2.263	-2.110	-11.59
	(5.214)	(4.001)	(4.669)	(5.494)	(9.999)
Price of hen's eggs	-15.92	-1.500	-9.221	-18.01	-2.722
	(11.91)	(8.753)	(12.79)	(34.30)	(119.9)
Price of milk	24.46	24.35**	28.73**	40.57**	59.37**
	(10.51)	(9.135)	(10.57)	(15.29)	(28.29)
Price of potatoes	6.102	10.87	-2.112	13.16	23.80
	(12.15)	(14.14)	(17.65)	(21.91)	(32.32)
Intercepts					
Second wave of data	-229.1*	-169.5	-6.740	49.10	635.5**
(1=yes)	(135.6)	(132.2)	(160.3)	(195.7)	(303.3)
Overall intercept	1237.4**	1641.4**	2709.1**	3362.7**	4381.1**
	(416.2)	(346.1)	(438.2)	(521.0)	(930.2)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table G.5 **Pooled quantile regressions, household non-food expenditure**

	Quantile				
	10%	30%	Median	70%	90%
Household microcredit borrowings	0.002**	0.004**	0.006**	0.010**	0.035**
	(0.001)	(0.001)	(0.002)	(0.004)	(0.011)
Household characteristics					
Education of HH head	2.087	-0.470	7.628	5.866	33.38
	(6.439)	(8.800)	(11.67)	(31.59)	(127.7)
Age of HH head	-2.435**	-2.986**	-4.501**	-6.304**	-6.816**
	(0.808)	(0.916)	(1.275)	(2.285)	(4.658)
Gender of HH head	48.39	-48.22	-32.85	-141.5	-739.1
	(54.43)	(41.61)	(62.64)	(108.1)	(705.7)
Highest education of men in HH	18.84**	29.04**	40.27**	67.31**	157.1**
	(5.901)	(6.443)	(8.816)	(22.94)	(88.72)
Highest education of women in HH	23.87**	43.26**	61.15**	109.8**	176.2**
	(4.173)	(6.852)	(11.41)	(28.79)	(102.8)
Landholdings	0.221**	0.260**	0.566**	1.256	1.370
	(0.119)	(0.232)	(0.478)	(0.742)	(1.574)
HH economic dependency ratio	-26.56**	-27.23**	-42.67**	-55.17**	11.55
	(5.467)	(7.088)	(10.06)	(17.67)	(56.93)
# of HH head relatives owning land	6.032	2.069	8.920	24.54	-3.190
	(2.993)	(4.062)	(6.839)	(14.39)	(28.99)
# of HH head's spouse relatives owning land	-0.154	4.924	0.640	3.463	36.85*
	(2.306)	(2.944)	(4.195)	(9.383)	(25.44)
# of HH head relatives living outside thana	-2.407	-4.046	-8.742	-6.934	-5.233
	(3.770)	(4.213)	(6.237)	(11.30)	(35.40)
# of HH head's spouse relatives living outside thana	1.380	2.583	4.088	-3.023	0.268
	(2.162)	(2.523)	(4.529)	(7.844)	(26.86)
Loans from traditional banks (1=yes)	84.85**	88.47*	285.9**	422.0*	1043.9
	(40.94)	(61.31)	(125.4)	(260.5)	(1476.6)
Loans from informal sources (1=yes)	-2.003	19.12	1.251	196.5	389.7
	(40.11)	(44.13)	(65.93)	(123.7)	(454.8)
Loans from relatives (1=yes)	68.65**	130.5**	190.9**	423.0**	2130.2**
	(33.12)	(44.20)	(70.47)	(181.5)	(807.8)
Eligibility of HH (1=yes)	-87.34**	-93.89*	-93.28	-184.2*	-577.4
	(29.70)	(38.31)	(70.67)	(146.2)	(465.3)
Village covariates					
Average male wage	-0.355	-0.371	-0.706	-0.825	6.289
	(0.905)	(1.319)	(1.731)	(2.993)	(11.79)
Average female wage	1.409	0.510	1.844	2.403	8.298
	(0.914)	(1.040)	(1.489)	(2.848)	(9.572)
Primary school (1=yes)	-23.37	-47.18*	-83.19**	-181.4**	-193.6
	(21.99)	(25.46)	(34.22)	(68.33)	(190.2)
Food program (1=yes)	2.188	-14.25	-60.88*	-59.96	-125.3
	(18.78)	(21.37)	(29.95)	(55.69)	(168.9)
Distance to nearest bank (km)	2.491	-4.308	-12.05**	-15.87**	-60.05
	(3.792)	(4.285)	(5.586)	(8.820)	(29.87)
Distance to nearest *pucca* road (km)	-2.102	-2.870	-3.621	-5.593	22.52
	(3.364)	(3.606)	(4.033)	(8.485)	(33.91)
Distance to nearest shop/market (km)	3.244	18.84*	22.89**	40.15**	34.37
	(7.311)	(7.493)	(9.924)	(16.68)	(46.07)
Electricity in village (1=yes)	47.99**	69.17**	62.78**	126.8*	251.7
	(19.48)	(23.64)	(33.44)	(63.19)	(184.2)
Price of rice	-12.34	-19.23	3.905	-9.273	74.85
	(10.03)	(11.90)	(15.80)	(28.36)	(98.77)
Price of wheat flour	9.954	10.41	-7.204	-0.607	-112.0
	(10.14)	(13.11)	(18.66)	(33.37)	(108.2)
Price of mustard oil	2.038**	2.961	2.369	-4.691	-30.45
	(1.473)	(2.069)	(2.820)	(4.966)	(14.20)
Price of hen's eggs	2.384	-2.410	-0.275	6.807	6.093
	(3.569)	(3.646)	(7.831)	(32.48)	(55.54)
Price of milk	4.226	7.854	9.338	16.10	18.88
	(3.591)	(4.257)	(6.190)	(12.49)	(43.76)
Price of potatoes	-0.978	-12.67**	-14.54	-32.85*	-31.86
	(4.900)	(5.674)	(9.440)	(17.05)	(55.61)
Intercepts					
Second wave of data (1=yes)	106.0**	214.5**	326.9**	599.4**	943.2**
	(46.04)	(63.40)	(89.43)	(173.2)	(551.6)
Overall intercept	208.9	481.8**	619.7**	1442.6**	3831.2**
	(153.1)	(188.8)	(243.9)	(444.8)	(1685.0)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table G.6 **Pooled quantile regressions, household non-food expenditure, by gender**

	Quantile				
	10%	30%	Median	70%	90%
Female microcredit	0.002**	0.004**	0.006**	0.011**	0.041**
borrowings	(0.001)	(0.001)	(0.002)	(0.005)	(0.014)
Male microcredit	0.001*	0.004**	0.004	0.009	0.012
borrowings	(0.001)	(0.002)	(0.003)	(0.007)	(0.016)
Household characteristics					
Education of HH head	2.459	-0.287	7.538	7.382	21.16
	(6.444)	(8.844)	(11.71)	(31.88)	(128.0)
Age of HH head	-2.505**	-2.958**	-4.305**	-6.239**	-5.960**
	(0.809)	(0.918)	(1.276)	(2.282)	(4.680)
Gender of HH head	48.52	-47.99	-37.42	-136.4	-773.4
	(54.58)	(41.60)	(62.97)	(107.1)	(703.5)
Highest education of	18.64**	28.81**	40.96**	63.93**	172.9**
men in HH	(5.935)	(6.452)	(8.831)	(23.13)	(89.56)
Highest education of	23.71**	43.33**	59.56**	108.6**	176.8**
women in HH	(4.151)	(6.842)	(11.52)	(29.04)	(102.7)
Landholdings	0.234**	0.258**	0.576**	1.356	1.419
	(0.118)	(0.232)	(0.480)	(0.751)	(1.568)
HH economic	-26.67**	-27.16**	-43.69**	-53.79**	5.536
dependency ratio	(5.467)	(7.093)	(10.25)	(17.53)	(56.27)
# of HH head	6.051	2.041	9.010	23.39	-3.804
relatives owning land	(3.010)	(4.091)	(6.905)	(14.28)	(29.44)
# of HH head's	-0.135	4.943	0.997	3.606	46.57*
spouse relatives owning land	(2.318)	(2.966)	(4.166)	(9.469)	(26.06)
# of HH head	-2.495	-4.166	-8.797	-7.025	-3.064
relatives living outside thana	(3.788)	(4.198)	(6.250)	(11.31)	(35.62)
# of HH head's	1.442	2.474	4.541	-2.203	-0.464
spouse relatives living outside thana	(2.173)	(2.529)	(4.524)	(7.756)	(27.16)
Loans from	88.30**	88.88*	294.7**	431.9*	949.5
traditional banks (1=yes)	(40.96)	(61.23)	(125.1)	(258.2)	(1477.4)
Loans from informal	-3.094	19.56	1.389	198.0	354.8
sources (1=yes)	(40.12)	(44.21)	(66.26)	(124.3)	(456.8)
Loans from relatives	67.10*	129.9**	192.9**	442.3**	1987.6**
(1=yes)	(33.30)	(44.11)	(70.20)	(180.4)	(805.9)
Eligibility of HH	-87.31**	-92.85*	-87.25	-188.0*	-427.2
(1=yes)	(29.67)	(38.44)	(70.47)	(146.4)	(462.9)
Village covariates					
Average male wage	-0.333	-0.379	-0.794	-1.056	5.593
	(0.929)	(1.314)	(1.725)	(3.009)	(11.67)
Average female wage	1.489	0.484	2.018	2.459	5.042
	(0.925)	(1.041)	(1.488)	(2.847)	(9.533)
Primary school	-21.18	-47.02*	-85.82**	-200.2**	-125.4
(1=yes)	(22.02)	(25.77)	(34.24)	(67.72)	(194.5)
Food program (1=yes)	3.134	-14.88	-63.62*	-61.75	-93.51
	(18.76)	(21.38)	(29.98)	(55.90)	(169.5)
Distance to nearest	2.501	-4.470	-12.27**	-16.22**	-68.27
bank (km)	(3.826)	(4.264)	(5.634)	(8.831)	(29.68)
Distance to nearest	-1.869	-2.965	-3.883	-5.941	17.69
pucca road (km)	(3.363)	(3.617)	(4.038)	(8.516)	(34.02)
Distance to nearest	3.828	18.93*	21.66**	39.85**	36.55
shop/market (km)	(7.288)	(7.496)	(10.00)	(16.57)	(46.18)
Electricity in	50.01**	68.86**	58.87**	118.2*	267.7
village (1=yes)	(19.58)	(23.66)	(33.54)	(63.77)	(184.3)
Price of rice	-11.35	-18.83	4.107	-8.943	83.26
	(10.10)	(11.95)	(15.86)	(28.68)	(99.04)
Price of wheat flour	10.42	10.79	-5.120	-0.647	-118.4
	(10.13)	(13.22)	(18.71)	(33.49)	(108.0)
Price of mustard oil	2.301**	2.896	2.820	-4.395	-34.10
	(1.474)	(2.059)	(2.834)	(4.954)	(14.06)
Price of hen's eggs	2.125	-2.527	-0.632	7.329	10.69
	(3.573)	(3.636)	(7.844)	(33.08)	(54.02)
Price of milk	3.944	7.732	8.777	14.91	17.49
	(3.670)	(4.255)	(6.148)	(12.57)	(43.33)
Price of potatoes	-0.976	-12.58**	-14.31	-32.57*	-43.68
	(4.952)	(5.759)	(9.536)	(16.98)	(55.63)
Intercepts					
Second wave of data	100.3**	215.2**	313.2**	594.6**	1081.2**
(1=yes)	(46.21)	(63.66)	(89.18)	(172.5)	(544.4)
Overall intercept	179.1	479.1**	580.8**	1463.9**	3930.1**
	(154.2)	(188.4)	(244.9)	(444.5)	(1651.2)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Appendix H

Regression tables for pooled quantile regression estimates with village quantile effects

Table H.1 **Pooled quantile regressions with village quantile effects, household total expenditure**

	Quantile				
	10%	30%	Median	70%	90%
Household	0.006	0.008*	0.011**	0.013**	0.023**
microcredit borrowings	(0.003)	(0.004)	(0.004)	(0.006)	(0.010)
Household characteristics					
Education of HH head	0.644	-4.659	29.59	96.92*	164.3**
	(19.79)	(19.29)	(27.89)	(38.03)	(69.13)
Age of HH head	-2.309	-8.349**	-3.800	-8.439*	-9.570*
	(2.841)	(2.828)	(3.551)	(4.483)	(6.556)
Gender of HH head	184.3	-38.17	-186.7	-336.3	-472.0
	(168.4)	(171.3)	(190.1)	(264.1)	(475.1)
Highest education of	53.33**	76.50**	80.83**	67.61**	77.53
men in HH	(17.76)	(16.53)	(19.71)	(28.33)	(50.83)
Highest education of	37.09**	67.03**	107.9**	122.5**	221.5**
women in HH	(16.25)	(16.17)	(20.86)	(31.25)	(66.39)
Landholdings	0.860**	0.962**	0.975**	1.326**	0.781
	(0.439)	(0.465)	(0.588)	(0.696)	(0.854)
HH economic	-115.9**	-148.5**	-182.0**	-202.3**	-246.5**
dependency ratio	(21.38)	(22.15)	(27.01)	(36.49)	(59.73)
# of HH head	35.23**	22.04**	45.06**	36.97*	27.43
relatives owning land	(11.32)	(13.20)	(18.18)	(23.61)	(32.28)
# of HH head's	6.919	11.75	1.609	22.47	56.20**
spouse relatives owning land	(8.268)	(10.50)	(13.69)	(17.05)	(24.46)
# of HH head	0.743	10.96	-1.864	6.899	-3.987
relatives living outside thana	(12.85)	(13.29)	(16.79)	(23.28)	(39.82)
# of HH head's	6.137	2.828	5.726	-4.125	-49.07
spouse relatives living outside thana	(8.195)	(9.713)	(11.66)	(15.34)	(24.47)
Loans from	481.6**	338.2**	653.5**	1063.9**	1759.2**
traditional banks (1=yes)	(152.7)	(176.2)	(231.4)	(393.0)	(626.4)
Loans from informal	-30.06	60.38	45.51	-27.58	306.7
sources (1=yes)	(129.0)	(128.1)	(162.9)	(242.4)	(430.6)
Loans from relatives	126.7	158.5	200.7	352.3	552.4
(1=yes)	(111.7)	(110.0)	(142.7)	(253.5)	(372.5)
Eligibility of HH	-326.3**	-390.0**	-417.1**	-604.1**	-684.1*
(1=yes)	(114.4)	(121.1)	(142.1)	(208.8)	(375.1)
Intercepts					
Second wave of data	78.87**	398.2**	565.4**	723.8**	1338.8**
(1=yes)	(75.26)	(88.56)	(107.3)	(146.8)	(262.5)
Overall intercept	3176.3**	4206.4**	4744.9**	6569.7**	8838.1**
	(398.2)	(341.4)	(463.4)	(845.2)	(2255.5)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table H.2 **Pooled quantile regressions with village quantile effects, household total expenditure, by gender**

	Quantile				
	10%	30%	Median	70%	90%
Female microcredit	0.007	0.009*	0.013**	0.014**	0.025**
borrowings	(0.004)	(0.005)	(0.005)	(0.007)	(0.013)
Male microcredit	0.004	0.003	0.005	0.008	-0.000
borrowings	(0.005)	(0.006)	(0.009)	(0.011)	(0.016)
Household characteristics					
Education of HH head	3.788	-2.990	26.71	97.53*	185.5**
	(19.96)	(19.18)	(27.97)	(38.04)	(69.15)
Age of HH head	-2.601	-8.233**	-3.834	-8.364*	-8.704*
	(2.823)	(2.816)	(3.566)	(4.509)	(6.479)
Gender of HH head	184.8	-41.94	-193.5	-330.8	-618.4
	(169.1)	(170.4)	(192.1)	(266.0)	(478.0)
Highest education of	50.59**	75.30**	83.94**	66.67**	76.15
men in HH	(17.93)	(16.34)	(19.73)	(28.54)	(50.98)
Highest education of	36.77**	67.75**	107.4**	121.0**	219.8**
women in HH	(16.27)	(16.17)	(20.69)	(31.22)	(67.25)
Landholdings	0.915**	0.962**	0.925**	1.342**	0.531
	(0.438)	(0.459)	(0.599)	(0.696)	(0.849)
HH economic	-115.9**	-150.3**	-182.9**	-202.6**	-250.7**
dependency ratio	(21.41)	(22.02)	(26.88)	(36.52)	(58.82)
# of HH head	35.49**	21.79**	45.14**	37.77*	17.08
relatives owning land	(11.29)	(13.24)	(18.17)	(23.89)	(31.76)
# of HH head's	6.399	11.31	1.378	20.00	64.84**
spouse relatives owning land	(8.355)	(10.53)	(13.72)	(17.05)	(24.88)
# of HH head	-0.475	10.86	-3.050	4.704	4.096
relatives living outside thana	(12.84)	(13.43)	(16.75)	(23.23)	(39.73)
# of HH head's	5.490	2.838	5.936	-4.152	-47.69
spouse relatives living outside thana	(8.307)	(9.750)	(11.53)	(15.44)	(24.42)
Loans from	490.4**	342.5**	613.3**	1082.4**	1830.0**
traditional banks (1=yes)	(155.3)	(177.8)	(230.8)	(395.6)	(631.2)
Loans from informal	-9.319	54.82	36.37	-6.357	398.8
sources (1=yes)	(130.2)	(129.1)	(162.4)	(242.2)	(427.2)
Loans from relatives	144.2	152.5	191.1	377.8	511.2
(1=yes)	(112.7)	(111.6)	(143.2)	(254.0)	(377.5)
Eligibility of HH	-320.8**	-384.9**	-422.3**	-602.3**	-645.3*
(1=yes)	(114.5)	(120.8)	(143.2)	(211.3)	(376.3)
Intercepts					
Second wave of data	85.39**	394.3**	554.4**	744.9**	1223.3**
(1=yes)	(75.61)	(88.11)	(107.7)	(148.1)	(266.4)
Overall intercept	3168.1**	4206.0**	4762.2**	6511.2**	8975.2**
	(401.2)	(340.0)	(464.9)	(849.2)	(2247.4)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table H.3 **Pooled quantile regressions with village quantile effects, household food expenditure**

	Quantile				
	10%	30%	Median	70%	90%
Household	0.003	0.006**	0.004*	0.003	0.001
microcredit borrowings	(0.003)	(0.002)	(0.002)	(0.003)	(0.004)
Household characteristics					
Education of HH head	22.06	16.34	17.95*	40.79**	108.9**
	(13.25)	(12.69)	(13.26)	(15.82)	(24.02)
Age of HH head	0.353	-2.063	-1.805	0.647	-3.786
	(1.917)	(1.920)	(2.040)	(2.420)	(3.041)
Gender of HH head	95.89	72.32	-160.8	-325.1*	-741.6**
	(110.8)	(109.3)	(132.1)	(139.2)	(234.2)
Highest education of	16.36*	29.43**	32.44**	26.66	15.80
men in HH	(11.96)	(10.67)	(10.43)	(12.21)	(18.22)
Highest education of	1.679	12.05	31.29**	36.30**	26.30*
women in HH	(9.186)	(10.44)	(12.91)	(12.93)	(19.06)
Landholdings	0.507**	0.450**	0.670**	0.467**	1.144*
	(0.238)	(0.229)	(0.224)	(0.265)	(0.501)
HH economic	-84.80**	-105.6**	-130.3**	-127.8**	-175.6**
dependency ratio	(14.71)	(14.18)	(16.02)	(19.31)	(27.67)
# of HH head	18.56**	16.12*	16.70*	19.91*	10.53
relatives owning land	(8.733)	(8.850)	(9.711)	(10.35)	(15.85)
# of HH head's	4.158	10.17	5.822	14.74	18.49
spouse relatives owning land	(5.282)	(6.511)	(7.692)	(8.565)	(13.06)
# of HH head	-2.834	12.01	4.292	4.961	27.18
relatives living outside thana	(9.420)	(8.608)	(10.21)	(12.38)	(24.03)
# of HH head's	6.103	2.005	7.094	7.905	-4.066
spouse relatives living outside thana	(5.647)	(5.580)	(6.980)	(7.650)	(10.80)
Loans from	159.3**	136.6*	212.0*	196.0*	100.7
traditional banks (1=yes)	(103.0)	(104.2)	(104.1)	(114.7)	(181.1)
Loans from informal	-55.59	81.72	26.02	-63.22	31.54
sources (1=yes)	(90.15)	(79.67)	(81.04)	(102.5)	(145.4)
Loans from relatives	37.04	13.21	119.4	46.39	-65.44
(1=yes)	(78.16)	(73.51)	(82.84)	(90.70)	(146.5)
Eligibility of HH	-205.2**	-265.2**	-229.8**	-262.3**	-178.1
(1=yes)	(80.27)	(79.32)	(79.59)	(95.57)	(148.9)
Intercepts					
Second wave of data	-100.4	61.98	147.3**	203.9**	498.2**
(1=yes)	(55.05)	(54.28)	(63.32)	(78.79)	(114.6)
Overall intercept	2413.9**	3140.1**	3704.6**	4393.0**	6162.0**
	(264.9)	(223.8)	(298.1)	(355.5)	(998.0)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table H.4 **Pooled quantile regressions with village quantile effects, household food expenditure, by gender**

	Quantile				
	10%	30%	Median	70%	90%
Female microcredit	0.003	0.005**	0.004*	0.003	0.003
borrowings	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Male microcredit	0.004	0.008*	0.004	-0.001	-0.006
borrowings	(0.004)	(0.003)	(0.004)	(0.005)	(0.007)
Household characteristics					
Education of HH head	21.69	18.13	18.16*	39.66**	108.0**
	(13.23)	(12.61)	(13.27)	(15.89)	(23.76)
Age of HH head	0.326	-1.684	-1.825	0.645	-3.931
	(1.923)	(1.923)	(2.043)	(2.416)	(3.004)
Gender of HH head	96.06	72.10	-164.4	-315.3*	-729.8**
	(111.5)	(109.3)	(131.9)	(139.4)	(234.5)
Highest education of	16.86*	27.43**	32.47**	27.39*	16.14
men in HH	(11.96)	(10.65)	(10.40)	(12.20)	(18.13)
Highest education of	1.360	12.99	31.42**	37.34**	26.20*
women in HH	(9.183)	(10.47)	(12.95)	(12.93)	(19.00)
Landholdings	0.502**	0.459**	0.671**	0.466**	1.173*
	(0.238)	(0.230)	(0.225)	(0.264)	(0.487)
HH economic	-85.12**	-104.5**	-130.9**	-129.5**	-177.7**
dependency ratio	(14.62)	(14.19)	(15.94)	(19.39)	(27.82)
# of HH head	18.50**	15.47*	16.59*	20.26*	10.77
relatives owning land	(8.726)	(8.844)	(9.729)	(10.37)	(15.79)
# of HH head's	4.236	10.49	5.930	13.63	19.63
spouse relatives owning land	(5.282)	(6.551)	(7.693)	(8.516)	(13.05)
# of HH head	-2.723	10.50	4.445	4.421	30.83
relatives living outside thana	(9.471)	(8.630)	(10.25)	(12.36)	(24.07)
# of HH head's	5.927	3.010	6.818	7.876	-1.707
spouse relatives living outside thana	(5.647)	(5.602)	(6.976)	(7.606)	(10.85)
Loans from	158.9**	134.3*	208.5*	194.7*	114.9
traditional banks (1=yes)	(102.4)	(103.9)	(104.3)	(114.4)	(180.1)
Loans from informal	-55.52	89.76	26.61	-61.10	12.42
sources (1=yes)	(91.08)	(79.62)	(81.02)	(102.6)	(144.5)
Loans from relatives	35.50	4.985	118.3	46.50	-59.72
(1=yes)	(77.37)	(73.21)	(81.96)	(90.26)	(144.6)
Eligibility of HH	-208.0**	-266.8**	-226.9**	-259.1**	-151.5
(1=yes)	(80.09)	(79.21)	(79.57)	(95.57)	(149.4)
Intercepts					
Second wave of data	-98.70	59.66	147.5**	199.1**	491.0**
(1=yes)	(55.03)	(54.18)	(63.42)	(79.31)	(114.4)
Overall intercept	2418.7**	3148.3**	3707.6**	4397.3**	6131.8**
	(265.5)	(225.9)	(297.8)	(355.6)	(997.9)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table H.5 **Pooled quantile regressions with village quantile effects, household non-food expenditure**

	Quantile				
	10%	30%	Median	70%	90%
Household	0.002**	0.003**	0.005**	0.007**	0.025**
microcredit borrowings	(0.001)	(0.001)	(0.002)	(0.004)	(0.011)
Household characteristics					
Education of HH head	-0.661	-4.441	7.989	39.02	67.61
	(4.839)	(8.146)	(11.98)	(24.88)	(49.86)
Age of HH head	-2.351**	-2.860**	-2.480**	-3.988**	-8.793**
	(0.759)	(0.968)	(1.249)	(2.124)	(4.019)
Gender of HH head	44.81	5.734	1.401	-93.00	-260.9
	(41.88)	(42.61)	(61.82)	(106.2)	(264.1)
Highest education of	23.72**	31.67**	36.66**	42.69**	77.02**
men in HH	(4.347)	(6.080)	(9.079)	(18.95)	(37.52)
Highest education of	21.44**	40.23**	51.47**	84.18**	186.2**
women in HH	(3.394)	(6.347)	(10.83)	(23.41)	(39.78)
Landholdings	0.173**	0.265**	0.360**	1.219	0.859
	(0.121)	(0.210)	(0.432)	(0.693)	(0.730)
HH economic	-25.86**	-31.78**	-50.01**	-52.79**	-107.2**
dependency ratio	(5.053)	(7.642)	(10.60)	(17.96)	(33.59)
# of HH head	7.915**	8.353*	8.882	15.21	2.271
relatives owning land	(2.891)	(4.007)	(6.686)	(13.47)	(19.08)
# of HH head's	3.433	3.926	1.153	3.837	31.51
spouse relatives owning land	(2.585)	(3.324)	(4.601)	(8.641)	(15.87)
# of HH head	-3.032	-2.633	-0.168	-8.738	-7.297
relatives living outside thana	(3.539)	(4.612)	(6.151)	(10.26)	(19.54)
# of HH head's	3.062	2.528	-3.177	2.339	-16.00
spouse relatives living outside thana	(2.176)	(2.791)	(3.838)	(7.363)	(13.41)
Loans from	132.7**	78.57**	228.5**	494.2**	1848.2**
traditional banks (1=yes)	(34.72)	(58.31)	(126.6)	(240.8)	(789.9)
Loans from informal	32.17	31.20	32.73	69.01	267.4
sources (1=yes)	(27.90)	(44.48)	(64.80)	(118.9)	(214.1)
Loans from relatives	26.64*	96.12**	127.6**	265.0**	547.8
(1=yes)	(31.12)	(43.94)	(70.92)	(143.5)	(298.8)
Eligibility of HH	-103.1**	-82.07*	-108.4	-198.2	-308.6
(1=yes)	(31.12)	(43.98)	(70.57)	(120.3)	(231.2)
Intercepts					
Second wave of data	177.4**	230.6**	285.9**	437.3**	810.3**
(1=yes)	(20.86)	(28.10)	(41.24)	(68.91)	(147.2)
Overall intercept	372.9**	684.4**	948.9**	1448.0**	2552.1**
	(106.4)	(143.7)	(187.3)	(450.2)	(2375.3)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table H.6 **Pooled quantile regressions with village quantile effects, household non-food expenditure, by gender**

	Quantile				
	10%	30%	Median	70%	90%
Female microcredit	0.002**	0.004**	0.006**	0.009**	0.027**
borrowings	(0.001)	(0.001)	(0.002)	(0.004)	(0.013)
Male microcredit	0.002	-0.001	0.001	-0.002	0.011
borrowings	(0.002)	(0.002)	(0.005)	(0.007)	(0.014)
Household characteristics					
Education of HH head	-0.654	-4.831	8.875	38.31	68.43
	(4.880)	(8.066)	(12.08)	(24.56)	(50.94)
Age of HH head	-2.353**	-2.890**	-2.348**	-3.739**	-8.807*
	(0.759)	(0.956)	(1.260)	(2.129)	(4.012)
Gender of HH head	46.59	1.842	-1.236	-75.20	-262.9
	(42.19)	(42.61)	(61.88)	(105.8)	(265.9)
Highest education of	23.54**	31.68**	36.78**	42.46**	77.59**
men in HH	(4.368)	(6.088)	(9.185)	(18.72)	(38.15)
Highest education of	21.48**	41.49**	49.84**	84.52**	186.1**
women in HH	(3.373)	(6.242)	(10.77)	(23.43)	(39.90)
Landholdings	0.173**	0.257**	0.353**	1.217	0.867
	(0.120)	(0.206)	(0.433)	(0.689)	(0.730)
HH economic	-25.70**	-32.43**	-48.53**	-50.72**	-108.6**
dependency ratio	(5.066)	(7.405)	(10.56)	(18.05)	(33.87)
# of HH head	8.006**	9.470**	8.028*	15.58	2.211
relatives owning land	(2.872)	(3.957)	(6.727)	(13.56)	(18.88)
# of HH head's	3.478	3.106	2.070	4.042	30.97
spouse relatives owning land	(2.607)	(3.277)	(4.552)	(8.669)	(16.03)
# of HH head	-3.044	-2.503	-1.370	-7.685	-8.304
relatives living outside thana	(3.557)	(4.470)	(6.196)	(10.35)	(19.44)
# of HH head's	3.091	2.303	-3.014	1.563	-16.53
spouse relatives living outside thana	(2.214)	(2.762)	(3.826)	(7.385)	(13.62)
Loans from	131.7**	67.65*	219.8**	506.0**	1857.5**
traditional banks (1=yes)	(34.67)	(57.48)	(127.1)	(238.8)	(788.9)
Loans from informal	31.25	38.76	29.99	56.73	266.5
sources (1=yes)	(27.73)	(43.72)	(64.41)	(117.8)	(216.2)
Loans from relatives	26.18*	93.19**	131.3**	273.5**	541.5
(1=yes)	(31.03)	(43.65)	(70.89)	(143.6)	(298.4)
Eligibility of HH	-103.2**	-89.20*	-113.3	-190.8	-319.4
(1=yes)	(31.19)	(43.73)	(69.32)	(119.7)	(232.9)
Intercepts					
Second wave of data	176.7**	221.9**	283.0**	429.7**	817.4**
(1=yes)	(20.83)	(27.72)	(41.89)	(68.88)	(148.7)
Overall intercept	371.3**	698.1**	940.7**	1401.3**	2568.5**
	(106.3)	(143.4)	(188.1)	(451.7)	(2375.4)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Appendix I

Regression tables for pooled quantile regression estimates with penalised village effects

Table I.1 **Pooled quantile regressions with penalised village effects, household total expenditure**

	Quantile				
	10%	30%	Median	70%	90%
Household	0.008**	0.010**	0.017**	0.022**	0.034**
microcredit borrowings	(0.002)	(0.003)	(0.003)	(0.004)	(0.011)
Household characteristics					
Education of HH head	-8.438	11.77	18.13	52.71*	128.6
	(15.55)	(15.35)	(17.85)	(31.12)	(77.40)
Age of HH head	-2.701	-5.055**	-5.694**	-8.957**	-18.27**
	(2.438)	(2.185)	(2.439)	(3.398)	(5.955)
Gender of HH head	173.8*	8.788	-212.5	-408.3**	-1196.8**
	(101.5)	(149.3)	(136.2)	(184.6)	(524.2)
Highest education of	49.08**	58.05**	68.25**	82.92**	144.5**
men in HH	(13.84)	(12.21)	(14.21)	(24.95)	(56.76)
Highest education of	40.85**	75.95**	94.83**	121.4**	233.8**
women in HH	(12.78)	(13.24)	(13.61)	(24.40)	(68.12)
Landholdings	0.947**	1.121**	1.756**	2.124**	2.012**
	(0.389)	(0.430)	(0.569)	(0.671)	(1.467)
HH economic	-112.8**	-139.6**	-187.4**	-209.2**	-236.5**
dependency ratio	(16.49)	(15.39)	(17.88)	(24.81)	(50.67)
# of HH head	11.61	9.996	2.992	19.63	23.34
relatives owning land	(8.282)	(9.400)	(11.57)	(16.67)	(28.35)
# of HH head's	9.711	5.157	9.117	7.120	22.35
spouse relatives owning land	(7.393)	(7.154)	(8.346)	(11.88)	(21.94)
# of HH head	17.35	11.07	1.668	16.37	92.69**
relatives living outside thana	(9.708)	(8.919)	(11.67)	(17.87)	(30.21)
# of HH head's	-1.546	7.900	7.274	11.25	10.05
spouse relatives living outside thana	(7.053)	(6.557)	(6.535)	(10.71)	(23.67)
Loans from	285.7**	328.0**	399.3**	698.3**	1409.9**
traditional banks (1=yes)	(122.5)	(111.4)	(144.8)	(334.5)	(673.5)
Loans from informal	169.7**	184.5**	181.5	340.3	867.5**
sources (1=yes)	(85.23)	(82.90)	(119.9)	(202.6)	(315.9)
Loans from relatives	190.5*	196.0**	350.8**	568.1**	1399.8**
(1=yes)	(88.46)	(87.37)	(106.7)	(163.3)	(385.2)
Eligibility of HH	-332.8**	-387.4**	-546.6**	-771.5**	-1274.9**
(1=yes)	(96.30)	(104.1)	(137.4)	(228.0)	(545.0)
Intercepts					
Second wave of data	65.20	195.1**	421.3**	524.2**	784.7**
(1=yes)	(54.10)	(55.05)	(70.81)	(96.22)	(199.7)
Overall intercept	2669.3**	3467.1**	4263.6**	5225.0**	7468.2**
	(188.8)	(203.5)	(218.0)	(334.5)	(809.4)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table I.2 **Pooled quantile regressions with penalised village effects, household total expenditure, by gender**

	Quantile				
	10%	30%	Median	70%	90%
Female microcredit	0.009**	0.013**	0.019**	0.023**	0.050**
borrowings	(0.003)	(0.004)	(0.003)	(0.005)	(0.014)
Male microcredit	0.005	0.005*	0.004	0.008	-0.000
borrowings	(0.004)	(0.003)	(0.006)	(0.008)	(0.014)
Household characteristics					
Education of HH head	-6.327	11.22	17.55	49.62*	126.4
	(15.58)	(15.27)	(17.81)	(31.66)	(76.65)
Age of HH head	-2.835	-5.281**	-6.669**	-9.312**	-18.96**
	(2.453)	(2.254)	(2.423)	(3.437)	(6.207)
Gender of HH head	187.9*	-9.372	-177.8	-370.8**	-1127.0**
	(102.8)	(149.2)	(137.5)	(186.0)	(530.6)
Highest education of	46.38**	57.81**	68.42**	81.58**	146.6**
men in HH	(14.01)	(12.49)	(14.08)	(25.33)	(55.48)
Highest education of	42.39**	77.58**	103.4**	131.8**	228.2**
women in HH	(12.91)	(13.38)	(13.83)	(24.21)	(67.11)
Landholdings	1.025**	1.097**	1.722**	2.100**	1.940**
	(0.396)	(0.422)	(0.581)	(0.665)	(1.507)
HH economic	-111.5**	-144.8**	-182.4**	-214.2**	-238.2**
dependency ratio	(16.23)	(15.54)	(17.73)	(25.44)	(51.10)
# of HH head	7.937*	8.876	3.740	17.58	14.80
relatives owning land	(8.329)	(9.256)	(11.59)	(16.66)	(28.55)
# of HH head's	12.28	6.943	9.213	9.707	38.77
spouse relatives owning land	(7.267)	(7.192)	(8.660)	(11.96)	(22.54)
# of HH head	18.25	12.00	3.949	16.36	73.47**
relatives living outside thana	(9.620)	(8.955)	(11.83)	(18.35)	(29.80)
# of HH head's	-1.099	9.153	6.683	11.32	6.508
spouse relatives living outside thana	(7.120)	(6.447)	(6.359)	(10.62)	(23.14)
Loans from	320.5**	354.9**	344.1**	712.9**	1325.4**
traditional banks (1=yes)	(122.9)	(109.1)	(146.7)	(325.5)	(658.2)
Loans from informal	177.8**	167.4**	172.0	330.0	772.5**
sources (1=yes)	(86.82)	(84.72)	(120.0)	(200.8)	(320.0)
Loans from relatives	191.1*	193.1**	336.1**	537.2**	1401.9**
(1=yes)	(88.21)	(87.54)	(108.1)	(163.9)	(380.9)
Eligibility of HH	-329.9**	-400.4**	-541.9**	-711.6**	-1263.2**
(1=yes)	(96.99)	(103.0)	(139.3)	(228.3)	(541.1)
Intercepts					
Second wave of data	59.73	176.4**	405.8**	519.1**	701.7**
(1=yes)	(54.95)	(55.66)	(71.54)	(97.18)	(204.0)
Overall intercept	2652.4**	3518.2**	4255.1**	5174.5**	7530.0**
	(190.9)	(204.3)	(220.4)	(337.2)	(811.8)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table I.3 **Pooled quantile regressions with penalised village effects, household food expenditure**

	Quantile				
	10%	30%	Median	70%	90%
Household	0.005**	0.008**	0.006**	0.007**	0.007**
microcredit borrowings	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Household characteristics					
Education of HH head	4.633	7.679	12.82*	33.92**	55.50**
	(12.42)	(9.563)	(8.968)	(13.16)	(24.51)
Age of HH head	-0.528	-2.617	-2.591**	-3.811	-6.063*
	(1.789)	(1.518)	(1.397)	(1.988)	(3.018)
Gender of HH head	126.5*	72.11	-130.1	-260.9**	-565.1**
	(86.75)	(103.6)	(97.50)	(96.64)	(254.2)
Highest education of	27.18**	29.27**	32.79**	32.10**	32.94*
men in HH	(11.32)	(7.822)	(7.052)	(10.77)	(20.13)
Highest education of	10.81	25.75**	48.49**	49.84**	69.94**
women in HH	(8.331)	(8.196)	(7.950)	(9.751)	(18.37)
Landholdings	0.356**	0.501**	0.635**	0.718**	0.905**
	(0.215)	(0.189)	(0.184)	(0.296)	(0.550)
HH economic	-84.82**	-96.45**	-129.2**	-147.2**	-172.7**
dependency ratio	(12.57)	(9.667)	(9.554)	(13.68)	(24.55)
# of HH head	6.986	5.086	-1.066	0.887	-5.754
relatives owning land	(6.168)	(5.861)	(5.768)	(7.773)	(13.35)
# of HH head's	7.301	3.561	2.802	0.392	-0.669
spouse relatives owning land	(5.643)	(4.838)	(4.568)	(6.188)	(8.954)
# of HH head	6.983	11.24	4.542	4.915	3.543
relatives living outside thana	(7.797)	(6.136)	(6.232)	(10.10)	(22.27)
# of HH head's	3.848	1.695	4.149	6.800	0.111
spouse relatives living outside thana	(4.792)	(4.049)	(4.362)	(5.756)	(10.70)
Loans from	151.0**	201.8**	196.0**	89.29	260.7
traditional banks (1=yes)	(80.80)	(72.36)	(69.18)	(94.56)	(171.1)
Loans from informal	63.02	142.7**	105.2*	51.48	228.3
sources (1=yes)	(68.24)	(68.36)	(57.96)	(91.14)	(175.7)
Loans from relatives	92.23	124.3*	136.4**	104.0*	177.6
(1=yes)	(67.01)	(62.61)	(50.60)	(76.92)	(137.5)
Eligibility of HH	-212.1**	-273.6**	-317.5**	-383.8**	-502.5**
(1=yes)	(68.16)	(66.35)	(59.50)	(92.91)	(164.7)
Intercepts					
Second wave of data	-138.8**	-51.90	59.87	143.4**	443.2**
(1=yes)	(45.78)	(41.04)	(40.74)	(62.36)	(101.4)
Overall intercept	2237.8**	2790.6**	3375.4**	3977.9**	5146.5**
	(138.8)	(131.8)	(121.8)	(149.3)	(314.8)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table I.4 **Pooled quantile regressions with penalised village effects, household food expenditure, by gender**

	Quantile				
	10%	30%	Median	70%	90%
Female microcredit	0.005**	0.008**	0.007**	0.009**	0.010**
borrowings	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)
Male microcredit	-0.000	0.007**	0.003**	0.001	-0.001
borrowings	(0.003)	(0.002)	(0.002)	(0.004)	(0.007)
Household characteristics					
Education of HH head	3.869	7.587	11.51*	36.30**	59.63**
	(12.00)	(9.579)	(8.661)	(13.36)	(23.76)
Age of HH head	-0.992	-2.766	-2.767**	-3.745	-5.507*
	(1.755)	(1.521)	(1.395)	(1.984)	(3.051)
Gender of HH head	126.9*	71.62	-131.3	-244.1**	-515.8**
	(87.06)	(102.7)	(96.99)	(97.76)	(255.2)
Highest education of	26.82**	29.34**	34.21**	32.67**	27.63*
men in HH	(11.02)	(7.851)	(6.939)	(10.78)	(19.80)
Highest education of	10.13	25.95**	47.79**	47.56**	69.62**
women in HH	(8.324)	(8.138)	(7.724)	(9.785)	(17.71)
Landholdings	0.373**	0.494**	0.636**	0.671**	0.918**
	(0.216)	(0.188)	(0.182)	(0.286)	(0.546)
HH economic	-84.58**	-97.51**	-129.9**	-147.9**	-176.7**
dependency ratio	(12.31)	(9.671)	(9.593)	(13.91)	(24.78)
# of HH head	7.376	5.211	-0.561	3.382	-7.301
relatives owning land	(6.174)	(5.819)	(5.946)	(7.637)	(13.38)
# of HH head’s	6.888	4.029	3.048	1.388	0.627
spouse relatives owning land	(5.566)	(4.805)	(4.585)	(5.996)	(8.937)
# of HH head	4.985	11.12	4.796	5.200	10.83
relatives living outside thana	(7.681)	(6.049)	(6.326)	(9.715)	(22.36)
# of HH head’s	4.567	1.350	3.842	5.592	2.202
spouse relatives living outside thana	(4.802)	(4.063)	(4.310)	(5.538)	(10.81)
Loans from	159.5**	197.8**	198.2**	106.1	254.5
traditional banks (1=yes)	(81.94)	(72.01)	(69.18)	(90.26)	(166.2)
Loans from informal	57.53	138.6**	107.8*	54.28	224.8
sources (1=yes)	(67.79)	(68.63)	(56.33)	(88.63)	(169.8)
Loans from relatives	81.34	123.1	140.3**	122.5*	197.0
(1=yes)	(66.80)	(62.65)	(49.91)	(75.76)	(137.9)
Eligibility of HH	-203.9**	-273.2**	-312.3**	-395.0**	-509.5**
(1=yes)	(68.62)	(65.58)	(58.73)	(93.08)	(163.4)
Intercepts					
Second wave of data	-139.1**	-50.10	58.02	124.0**	415.8**
(1=yes)	(45.89)	(40.82)	(40.92)	(61.37)	(102.2)
Overall intercept	2263.2**	2802.5**	3381.1**	3968.3**	5093.4**
	(137.8)	(131.5)	(121.3)	(150.7)	(317.2)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table I.5 **Pooled quantile regressions with penalised village effects, household non-food expenditure**

	Quantile				
	10%	30%	Median	70%	90%
Household	0.003**	0.004**	0.005**	0.013**	0.029**
microcredit borrowings	(0.001)	(0.001)	(0.001)	(0.003)	(0.009)
Household characteristics					
Education of HH head	2.314	-2.993	-0.438	9.061	50.72
	(5.062)	(5.935)	(8.652)	(19.86)	(61.95)
Age of HH head	-1.640**	-2.073**	-3.011**	-4.519**	-9.279**
	(0.713)	(0.657)	(0.891)	(1.504)	(3.679)
Gender of HH head	46.05	-8.759	-22.10	-138.1	-461.7
	(34.05)	(35.49)	(47.09)	(81.04)	(530.7)
Highest education of	14.38**	23.60**	34.57**	47.13**	96.37**
men in HH	(4.566)	(4.494)	(7.005)	(16.10)	(50.90)
Highest education of	21.40**	35.35**	46.75**	85.27**	179.0**
women in HH	(3.668)	(4.846)	(6.169)	(16.82)	(55.47)
Landholdings	0.278**	0.442**	0.813**	1.485**	1.430**
	(0.105)	(0.202)	(0.331)	(0.566)	(1.116)
HH economic	-22.52**	-28.08**	-45.75**	-58.11**	-81.66*
dependency ratio	(4.499)	(4.853)	(6.472)	(9.814)	(34.93)
# of HH head	6.520**	5.416**	2.212	4.542	6.523
relatives owning land	(2.464)	(2.604)	(4.036)	(6.789)	(23.14)
# of HH head's	2.831	3.779**	6.356*	0.277	32.20
spouse relatives owning land	(2.226)	(1.996)	(2.654)	(4.434)	(18.91)
# of HH head	-1.990	-0.986	-1.157	12.36	56.07**
relatives living outside thana	(2.679)	(2.844)	(4.196)	(8.535)	(27.42)
# of HH head's	0.905	3.002	2.184	3.783	-2.422
spouse relatives living outside thana	(1.930)	(1.834)	(2.609)	(4.307)	(15.91)
Loans from	57.04*	90.22**	232.2**	502.5**	1439.1**
traditional banks (1=yes)	(37.33)	(44.97)	(96.34)	(180.7)	(666.9)
Loans from informal	26.54	57.63**	88.83*	235.2**	520.0**
sources (1=yes)	(34.27)	(29.99)	(47.73)	(84.31)	(215.5)
Loans from relatives	58.44*	109.3**	153.6**	299.6**	1219.5**
(1=yes)	(25.77)	(31.39)	(46.48)	(121.9)	(473.9)
Eligibility of HH	-64.93**	-90.86**	-100.5**	-341.6**	-833.0**
(1=yes)	(31.18)	(36.17)	(54.00)	(121.7)	(460.9)
Intercepts					
Second wave of data	156.0**	185.9**	253.2**	331.9**	687.1**
(1=yes)	(17.35)	(16.94)	(27.18)	(41.93)	(165.5)
Overall intercept	292.7**	494.6**	678.4**	1193.5**	2421.1**
	(54.12)	(58.54)	(80.34)	(166.3)	(743.6)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table I.6 **Pooled quantile regressions with penalised village effects, household non-food expenditure, by gender**

	Quantile				
	10%	30%	Median	70%	90%
Female microcredit	0.003**	0.004**	0.007**	0.015**	0.042**
borrowings	(0.001)	(0.001)	(0.001)	(0.003)	(0.012)
Male microcredit	0.002*	0.003	0.001	0.003	0.021
borrowings	(0.001)	(0.001)	(0.002)	(0.005)	(0.014)
Household characteristics					
Education of HH head	2.919	-5.156	0.977	11.26	47.16
	(5.258)	(5.942)	(8.495)	(19.33)	(62.72)
Age of HH head	-1.571**	-2.281**	-3.090**	-4.457**	-9.415**
	(0.714)	(0.665)	(0.885)	(1.537)	(3.710)
Gender of HH head	43.29*	-4.728	-7.298	-111.2	-542.8
	(33.84)	(35.23)	(47.64)	(80.13)	(525.3)
Highest education of	14.08**	26.36**	33.34**	45.90**	114.9**
men in HH	(4.716)	(4.557)	(6.940)	(15.72)	(53.07)
Highest education of	20.73**	36.78**	46.75**	81.89**	167.4**
women in HH	(3.668)	(4.857)	(6.270)	(16.52)	(55.53)
Landholdings	0.275**	0.401**	0.722**	1.467**	1.824**
	(0.104)	(0.199)	(0.334)	(0.558)	(1.171)
HH economic	-22.44**	-27.81**	-46.67**	-57.42**	-71.31*
dependency ratio	(4.594)	(4.822)	(6.632)	(10.16)	(34.16)
# of HH head	6.524**	6.093**	4.509	5.792	13.35
relatives owning land	(2.459)	(2.661)	(4.089)	(6.955)	(24.17)
# of HH head's	3.695	4.493**	6.145*	1.976	26.51
spouse relatives owning land	(2.212)	(2.030)	(2.637)	(4.572)	(19.34)
# of HH head	-1.908	-1.106	0.0813	11.76	52.79**
relatives living outside thana	(2.689)	(2.909)	(4.232)	(8.804)	(27.73)
# of HH head's	0.889	3.118	2.296	4.569	-8.883
spouse relatives living outside thana	(1.948)	(1.821)	(2.621)	(4.555)	(16.29)
Loans from	51.14*	85.80**	240.7**	506.6**	1403.3**
traditional banks (1=yes)	(37.41)	(46.17)	(97.55)	(184.9)	(688.4)
Loans from informal	30.82	60.92**	94.36**	218.3**	492.6**
sources (1=yes)	(33.85)	(30.04)	(46.73)	(81.99)	(219.3)
Loans from relatives	60.01*	109.6**	161.1**	302.8**	1154.7**
(1=yes)	(26.37)	(31.86)	(45.85)	(124.0)	(481.1)
Eligibility of HH	-71.37**	-87.58**	-109.3**	-333.4**	-810.6**
(1=yes)	(31.33)	(36.26)	(52.87)	(121.9)	(465.9)
Intercepts					
Second wave of data	158.7**	188.2**	245.7**	325.9**	641.0**
(1=yes)	(17.58)	(17.09)	(26.89)	(43.85)	(165.7)
Overall intercept	294.9**	488.9**	677.0**	1163.0**	2450.0**
	(53.64)	(58.81)	(79.35)	(166.5)	(751.6)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Appendix J

Regression tables for panel data quantile regressions with correlated random effects (CRE)

Table J.1 **Panel quantile regressions with household correlated random effects, household total expenditure**

	Quantile				
	10%	30%	Median	70%	90%
Household	0.011**	0.016**	0.024**	0.036**	0.061**
microcredit borrowings	(0.005)	(0.005)	(0.006)	(0.009)	(0.017)
Household characteristics					
Education of HH head	43.10	-17.11	-43.09	-45.84	-120.6
	(41.56)	(34.28)	(44.01)	(66.65)	(175.3)
Age of HH head	10.00	-7.205	-5.239	-2.219	-4.459
	(8.883)	(6.841)	(7.372)	(11.60)	(20.43)
Gender of HH head	-329.0	-95.13	-117.9	-677.2	-2190.4**
	(330.2)	(264.2)	(308.6)	(391.8)	(913.8)
Highest education of	44.55	68.31**	75.93**	80.87	174.3
men in HH	(35.24)	(28.76)	(32.73)	(46.22)	(106.4)
Highest education of	-12.52	64.05**	89.08**	115.7**	197.9**
women in HH	(24.68)	(27.63)	(26.86)	(38.77)	(103.6)
Landholdings	-0.649	-0.0398	-0.633	-1.672	-3.404
	(0.573)	(0.522)	(0.909)	(1.314)	(3.253)
HH economic	-48.60**	-102.6**	-130.6**	-166.6**	-115.9
dependency ratio	(30.36)	(25.45)	(30.94)	(51.83)	(113.0)
# of HH head	48.27**	24.71	32.39	25.20	46.94
relatives owning land	(20.31)	(20.19)	(25.53)	(27.72)	(64.85)
# of HH head's	-15.20	-13.11	-10.71	3.181	40.55
spouse relatives owning land	(15.55)	(14.31)	(17.72)	(27.86)	(44.42)
# of HH head	15.35	27.44	5.503	-20.01	7.070
relatives living outside thana	(20.74)	(17.16)	(20.27)	(25.53)	(59.11)
# of HH head's	3.953	7.008	11.55	12.32	9.934
spouse relatives living outside thana	(16.51)	(14.73)	(14.89)	(22.72)	(52.31)
Loans from	67.11	258.7	337.8	179.4	-763.7
traditional banks (1=yes)	(223.6)	(239.3)	(291.3)	(473.1)	(1555.0)
Loans from informal	125.3	30.66	-125.7	124.8	548.0
sources (1=yes)	(169.0)	(181.3)	(191.5)	(323.3)	(711.7)
Loans from relatives	374.6	343.6**	393.9**	922.7**	1413.0*
(1=yes)	(178.0)	(141.1)	(170.8)	(265.5)	(614.6)
Eligibility of HH	-226.1	-168.7	-207.3	-327.2	-475.6
(1=yes)	(170.0)	(149.4)	(188.0)	(301.2)	(717.0)
Village covariates					
Average male wage	0.0376	-1.523	0.732	1.648	-7.617
	(4.123)	(3.534)	(4.373)	(6.722)	(17.32)
Average female wage	2.771	2.859	2.102	0.506	17.91
	(3.400)	(2.996)	(4.713)	(6.012)	(14.40)
Primary school	-54.43	-126.4	-223.0**	-309.8**	-202.7
(1=yes)	(90.62)	(83.80)	(111.4)	(137.6)	(276.3)
Food program (1=yes)	-138.2	-177.0**	-206.7*	-138.1	-358.7*
	(76.81)	(65.45)	(88.12)	(119.5)	(247.6)
Distance to nearest	-15.99	-39.47**	-49.55**	-62.14**	-92.79*
bank (km)	(17.57)	(12.90)	(16.92)	(23.31)	(52.14)
Distance to nearest	-15.34	-23.67*	-4.699	-11.71	25.82
pucca road (km)	(14.43)	(11.99)	(14.89)	(18.93)	(54.11)
Distance to nearest	47.73*	45.63**	64.38**	83.59**	19.54
shop/market (km)	(26.11)	(23.81)	(28.90)	(38.70)	(69.28)
Electricity in	258.0**	314.0**	415.6**	539.6**	586.3**
village (1=yes)	(78.76)	(76.09)	(94.73)	(131.6)	(287.3)
Price of rice	6.212	44.62	48.06	1.194	136.8
	(34.17)	(31.52)	(45.60)	(65.62)	(144.5)
Price of wheat flour	25.86	-63.55	-60.77	26.71	86.08
	(45.91)	(40.44)	(49.35)	(68.24)	(161.4)
Price of mustard oil	12.78	6.427	5.271	-2.789	-22.91
	(7.104)	(5.896)	(8.063)	(10.97)	(21.31)
Price of hen's eggs	-13.70	-7.139	-1.161	-41.91	-66.25
	(16.17)	(16.43)	(17.89)	(47.88)	(198.4)
Price of milk	28.54*	29.20**	50.74**	62.28**	53.51
	(13.49)	(13.28)	(17.58)	(27.57)	(57.84)
Price of potatoes	-8.769	-0.167	-19.73	-28.52	45.25
	(18.62)	(18.34)	(26.08)	(40.00)	(71.87)
Intercepts					
Second wave of data	-398.2	20.92	250.6	216.4	-70.75
(1=yes)	(216.4)	(208.2)	(248.6)	(369.5)	(688.3)
Overall intercept	1537.5**	3373.4**	3976.8**	4741.5**	4696.3**
	(629.5)	(561.9)	(728.3)	(1053.6)	(2372.9)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table J.2 **Panel quantile regressions with household correlated random effects, household total expenditure, by gender**

	Quantile				
	10%	30%	Median	70%	90%
Female microcredit	0.013**	0.016**	0.023**	0.040**	0.068**
borrowings	(0.005)	(0.006)	(0.007)	(0.010)	(0.020)
Male microcredit	0.003	0.014	0.023**	0.049**	0.032
borrowings	(0.011)	(0.009)	(0.012)	(0.014)	(0.038)
Household characteristics					
Education of HH head	50.40	-16.25	-31.09	-47.82	-93.43
	(41.09)	(34.61)	(44.65)	(65.23)	(174.0)
Age of HH head	10.29	-7.303	-5.109	-5.906	-7.636
	(8.826)	(6.885)	(7.462)	(11.74)	(20.36)
Gender of HH head	-332.8	-81.72	-172.7	-661.1	-2183.4**
	(331.0)	(263.1)	(306.3)	(388.4)	(910.2)
Highest education of	45.31	64.67**	74.94**	75.68*	180.7
men in HH	(35.37)	(28.69)	(32.64)	(46.80)	(106.7)
Highest education of	-7.145	65.04**	93.04**	117.8**	185.5*
women in HH	(24.78)	(27.83)	(26.89)	(38.74)	(104.0)
Landholdings	-0.652	-0.0526	-0.742	-1.520	-3.297
	(0.575)	(0.524)	(0.916)	(1.302)	(3.295)
HH economic	-59.01**	-101.0**	-130.1**	-164.6**	-104.5
dependency ratio	(30.85)	(25.60)	(30.63)	(52.11)	(113.3)
# of HH head	42.40**	25.12	32.90	27.26	44.09
relatives owning land	(20.35)	(20.30)	(25.41)	(27.84)	(64.09)
# of HH head's	-18.19	-13.99	-11.13	5.537	34.59
spouse relatives owning land	(15.52)	(14.31)	(17.79)	(27.56)	(44.54)
# of HH head	15.82	25.16	6.787	-13.53	13.46
relatives living outside thana	(20.61)	(17.15)	(20.19)	(25.95)	(58.62)
# of HH head's	2.703	4.617	14.07	11.66	11.98
spouse relatives living outside thana	(16.27)	(14.68)	(14.69)	(22.57)	(52.48)
Loans from	28.46	274.4	274.9	158.9	-600.5
traditional banks (1=yes)	(224.9)	(241.2)	(290.2)	(466.6)	(1541.0)
Loans from informal	148.2	25.66	-117.7	86.37	670.4
sources (1=yes)	(171.7)	(181.9)	(193.6)	(321.9)	(716.3)
Loans from relatives	324.6	352.5**	375.7**	889.1**	1460.0*
(1=yes)	(178.3)	(141.9)	(171.9)	(265.7)	(609.4)
Eligibility of HH	-244.8	-166.4	-201.5	-266.2	-600.7
(1=yes)	(169.4)	(149.3)	(188.5)	(301.2)	(713.2)
Village covariates					
Average male wage	-0.498	-1.724	0.701	1.304	-4.847
	(4.081)	(3.532)	(4.335)	(6.728)	(17.14)
Average female wage	2.800	3.159	2.563	2.037	12.56
	(3.404)	(2.972)	(4.739)	(5.986)	(14.41)
Primary school	-53.40	-117.0	-217.1**	-312.5**	-211.6
(1=yes)	(91.99)	(83.67)	(111.2)	(139.1)	(278.3)
Food program (1=yes)	-141.1	-185.7**	-215.5*	-122.3	-317.6
	(76.54)	(66.64)	(88.02)	(119.4)	(250.7)
Distance to nearest	-20.97	-39.91**	-51.87**	-66.11**	-99.58*
bank (km)	(17.45)	(12.84)	(16.96)	(23.68)	(52.22)
Distance to nearest	-9.721	-25.47*	-6.517	-14.42	33.42
pucca road (km)	(14.44)	(11.99)	(14.89)	(19.19)	(54.32)
Distance to nearest	51.20*	50.52**	66.67**	86.75**	33.90
shop/market (km)	(25.85)	(23.82)	(28.76)	(38.71)	(69.25)
Electricity in	262.8**	312.7**	418.7**	509.9**	609.9**
village (1=yes)	(78.80)	(77.16)	(95.20)	(131.7)	(292.3)
Price of rice	9.049	44.60	50.69	-1.533	152.3
	(34.74)	(31.93)	(46.08)	(65.86)	(141.3)
Price of wheat flour	20.66	-60.58	-68.15	34.26	112.2
	(45.87)	(40.58)	(49.25)	(68.49)	(161.1)
Price of mustard oil	12.78	6.396	4.685	-2.607	-23.71
	(7.115)	(5.895)	(8.074)	(11.04)	(21.18)
Price of hen's eggs	-15.95	-9.270	-5.087	-42.22	-77.89
	(16.27)	(16.34)	(17.91)	(47.81)	(199.0)
Price of milk	30.09*	28.02**	50.03**	58.04**	47.62
	(13.31)	(13.16)	(17.42)	(27.46)	(57.57)
Price of potatoes	-13.46	-0.758	-20.88	-25.33	39.48
	(18.63)	(18.42)	(26.15)	(40.12)	(72.13)
Intercepts					
Second wave of data	-372.6	20.48	253.1	158.8	-140.0
(1=yes)	(214.1)	(210.7)	(248.3)	(368.9)	(674.9)
Overall intercept	1575.0**	3373.5**	4018.4**	4729.9**	4465.3**
	(626.7)	(567.0)	(737.1)	(1054.6)	(2306.3)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table J.3 **Panel quantile regressions with household correlated random effects, household food expenditure**

	Quantile				
	10%	30%	Median	70%	90%
Household microcredit borrowings	0.006**	0.010**	0.010**	0.013**	0.011*
	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)
Household characteristics					
Education of HH head	22.70	-0.878	5.917	-13.03	37.97
	(24.55)	(19.51)	(20.74)	(27.67)	(55.61)
Age of HH head	1.791	-6.748	-6.998	0.370	17.68
	(6.997)	(6.303)	(4.087)	(5.270)	(11.52)
Gender of HH head	-93.18	-162.6	15.65	-278.9	-870.7*
	(221.1)	(202.0)	(246.4)	(235.9)	(416.6)
Highest education of men in HH	30.57	31.61**	25.65	8.796	23.27
	(22.84)	(17.10)	(18.27)	(24.81)	(35.77)
Highest education of women in HH	-28.35	-7.971	14.64	27.04	28.52
	(16.72)	(17.56)	(18.19)	(19.62)	(32.47)
Landholdings	-0.0357	0.142	0.265	-1.015	-1.166
	(0.372)	(0.428)	(0.653)	(0.846)	(0.911)
HH economic dependency ratio	-26.62	-51.29**	-72.57**	-105.7**	-118.5**
	(21.13)	(17.20)	(19.49)	(27.03)	(44.19)
# of HH head relatives owning land	25.19*	9.242	15.73	21.58	44.70*
	(14.37)	(12.43)	(15.11)	(16.71)	(25.96)
# of HH head's spouse relatives owning land	-4.081	-11.85	-5.567	-4.377	-2.128
	(10.87)	(8.640)	(10.28)	(11.51)	(21.66)
# of HH head relatives living outside thana	16.74	18.06	1.233	3.468	-4.316
	(14.71)	(12.05)	(12.99)	(16.70)	(29.05)
# of HH head's spouse relatives living outside thana	7.365	20.92*	18.00**	24.12**	12.34
	(10.80)	(10.15)	(9.509)	(11.03)	(20.02)
Loans from traditional banks (1=yes)	-160.8	-61.17	96.17	-16.45	-29.77
	(168.1)	(135.1)	(139.7)	(187.4)	(319.2)
Loans from informal sources (1=yes)	-8.919	101.8	-64.92	-120.2	-185.4
	(119.7)	(99.41)	(134.9)	(140.2)	(241.3)
Loans from relatives (1=yes)	97.06	131.1*	208.0*	156.2	73.95
	(101.8)	(91.54)	(103.7)	(124.4)	(209.3)
Eligibility of HH (1=yes)	-141.6	-173.0	-204.8	-56.89	91.62
	(105.6)	(110.3)	(117.8)	(147.3)	(225.9)
Village covariates					
Average male wage	-1.486	0.549	0.855	1.698	-1.282
	(2.863)	(2.306)	(2.920)	(3.383)	(7.098)
Average female wage	-0.334	1.642	0.650	1.219	-3.468
	(2.288)	(2.195)	(2.880)	(3.146)	(5.088)
Primary school (1=yes)	-81.23	-133.9*	-130.2*	-130.7*	-72.73
	(64.09)	(60.36)	(66.35)	(76.28)	(108.6)
Food program (1=yes)	-111.0*	-103.4**	-122.3**	-169.1**	-183.3*
	(56.49)	(45.31)	(50.51)	(62.74)	(110.2)
Distance to nearest bank (km)	-20.95	-28.47**	-27.51**	-36.12**	-55.90**
	(12.01)	(9.470)	(10.50)	(12.99)	(20.74)
Distance to nearest *pucca* road (km)	4.690	-9.694	-6.685	-0.325	6.808
	(10.37)	(8.514)	(10.29)	(11.73)	(17.27)
Distance to nearest shop/market (km)	20.50	20.75*	19.88	32.67	70.56
	(17.50)	(15.76)	(16.72)	(21.92)	(29.81)
Electricity in village (1=yes)	252.1**	288.6**	242.9**	356.1**	538.4**
	(53.61)	(50.69)	(55.36)	(73.12)	(145.3)
Price of rice	51.45**	50.79**	50.15*	57.25	30.83
	(23.49)	(21.74)	(28.54)	(36.28)	(54.92)
Price of wheat flour	-12.62	-26.38	-29.04	-27.33	-5.464
	(30.44)	(29.52)	(28.44)	(39.49)	(69.15)
Price of mustard oil	2.199	6.465	6.482	-1.090	-2.235
	(4.733)	(4.192)	(4.420)	(5.766)	(10.45)
Price of hen's eggs	-12.46	-8.086	-3.815	-13.74	-22.57
	(11.05)	(8.645)	(13.35)	(27.28)	(88.02)
Price of milk	24.69*	20.17**	32.05**	32.10**	61.48**
	(9.698)	(9.084)	(10.49)	(15.07)	(29.03)
Price of potatoes	-1.179	14.40	2.548	14.49	28.06
	(12.99)	(13.21)	(17.02)	(23.40)	(31.58)
Intercepts					
Second wave of data (1=yes)	-227.7**	-287.9*	-134.3	-194.6	83.12
	(151.5)	(155.2)	(173.1)	(203.2)	(306.5)
Overall intercept	1358.3**	1964.2**	2508.4**	3274.2**	4137.9**
	(435.2)	(403.8)	(446.5)	(528.3)	(915.3)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table J.4 **Panel quantile regressions with household correlated random effects, household food expenditure, by gender**

	Quantile				
	10%	30%	Median	70%	90%
Female microcredit	0.007**	0.011**	0.010**	0.013**	0.010*
borrowings	(0.003)	(0.003)	(0.004)	(0.004)	(0.006)
Male microcredit	-0.004	0.010**	0.011*	0.011**	0.012
borrowings	(0.008)	(0.004)	(0.006)	(0.006)	(0.015)
Household characteristics					
Education of HH head	19.13	-0.894	4.053	-6.867	39.90
	(24.42)	(19.67)	(20.88)	(26.91)	(55.39)
Age of HH head	1.886	-6.840	-6.708	0.156	17.38
	(6.972)	(6.290)	(4.106)	(5.383)	(11.74)
Gender of HH head	-64.80	-167.5	39.60	-240.4	-865.4*
	(221.0)	(202.3)	(246.5)	(238.1)	(421.6)
Highest education of	30.55	31.40**	27.71	12.67	27.80
men in HH	(22.83)	(17.15)	(17.96)	(24.63)	(35.12)
Highest education of	-20.46	-7.627	16.89	21.00	36.74
women in HH	(16.70)	(17.62)	(18.21)	(19.33)	(31.83)
Landholdings	-0.0498	0.141	0.226	-1.242	-1.261
	(0.374)	(0.431)	(0.656)	(0.842)	(0.899)
HH economic	-27.02	-51.27**	-70.21**	-107.5**	-114.9**
dependency ratio	(21.16)	(17.32)	(19.45)	(27.47)	(44.01)
# of HH head	26.17	8.994	17.00	24.61	40.07*
relatives owning land	(14.37)	(12.56)	(15.05)	(16.53)	(25.47)
# of HH head's	-3.204	-11.56	-6.453	-6.812	-3.332
spouse relatives owning land	(10.88)	(8.694)	(10.25)	(11.27)	(21.68)
# of HH head	16.22	18.03	-0.351	-0.752	-3.198
relatives living outside thana	(14.63)	(12.05)	(12.99)	(16.27)	(28.72)
# of HH head's	8.634	21.02*	16.81**	25.85**	19.07
spouse relatives living outside thana	(10.88)	(10.28)	(9.506)	(11.05)	(19.39)
Loans from	-162.3	-59.78	74.32	-52.08	-101.1
traditional banks (1=yes)	(169.8)	(135.2)	(140.9)	(189.4)	(317.9)
Loans from informal	-13.53	103.6	-38.84	-95.51	-217.7
sources (1=yes)	(122.5)	(100.4)	(135.7)	(139.4)	(237.8)
Loans from relatives	78.60	131.6*	190.7*	152.5	106.6
(1=yes)	(102.6)	(92.24)	(104.4)	(123.4)	(207.4)
Eligibility of HH	-131.3	-172.3	-210.2	-95.22	77.08
(1=yes)	(105.2)	(111.0)	(117.9)	(148.9)	(227.3)
Village covariates					
Average male wage	-1.507	0.566	0.738	1.596	-1.993
	(2.819)	(2.306)	(2.914)	(3.412)	(7.063)
Average female wage	-0.496	1.664	0.374	1.297	-2.757
	(2.229)	(2.199)	(2.901)	(3.113)	(5.052)
Primary school	-67.28	-134.5*	-127.9*	-164.4*	-39.43
(1=yes)	(64.22)	(61.28)	(66.25)	(76.36)	(108.4)
Food program (1=yes)	-117.0**	-103.2**	-128.0**	-169.9**	-218.5*
	(55.86)	(45.48)	(50.58)	(62.43)	(107.3)
Distance to nearest	-21.05	-28.48**	-26.88**	-36.38**	-55.80**
bank (km)	(11.95)	(9.478)	(10.54)	(13.15)	(20.71)
Distance to nearest	4.793	-9.472	-4.182	-1.995	5.969
pucca road (km)	(10.40)	(8.570)	(10.38)	(11.58)	(17.39)
Distance to nearest	23.20*	20.61*	20.49	29.10	74.51*
shop/market (km)	(17.49)	(15.80)	(16.83)	(22.29)	(29.46)
Electricity in	252.7**	287.2**	240.3**	341.3**	505.9**
village (1=yes)	(52.51)	(51.19)	(55.79)	(73.71)	(144.3)
Price of rice	54.69**	50.66**	55.53*	52.58	46.63
	(23.17)	(21.91)	(28.57)	(35.85)	(54.78)
Price of wheat flour	-16.62	-26.29	-25.31	-22.59	-19.60
	(30.39)	(29.38)	(28.30)	(39.32)	(68.77)
Price of mustard oil	1.562	6.627	6.876	-1.393	-1.088
	(4.693)	(4.223)	(4.462)	(5.852)	(10.62)
Price of hen's eggs	-12.16	-7.887	-3.954	-14.88	-26.47
	(11.04)	(8.811)	(13.61)	(26.86)	(88.57)
Price of milk	24.17**	20.14**	32.36**	33.60**	73.43**
	(9.578)	(9.170)	(10.42)	(14.81)	(28.74)
Price of potatoes	-0.0204	14.55	4.243	16.09	27.04
	(12.77)	(13.40)	(17.03)	(23.16)	(31.84)
Intercepts					
Second wave of data	-219.9**	-290.1*	-151.0	-216.4	-0.145
(1=yes)	(148.7)	(155.3)	(174.5)	(201.2)	(302.6)
Overall intercept	1376.1**	1965.4**	2385.4**	3259.7**	3913.8**
	(432.2)	(404.9)	(446.7)	(520.7)	(909.9)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table J.5 **Panel quantile regressions with household correlated random effects, household non-food expenditure**

	Quantile				
	10%	30%	Median	70%	90%
Household	0.004**	0.007**	0.012**	0.017**	0.040**
microcredit borrowings	(0.002)	(0.002)	(0.003)	(0.006)	(0.015)
Household characteristics					
Education of HH head	-13.17	-6.534	-19.98	-48.51	-214.2
	(10.29)	(12.83)	(18.25)	(35.75)	(149.3)
Age of HH head	-1.055	-0.610	-0.183	-4.350	-4.276
	(2.375)	(2.467)	(3.152)	(5.570)	(13.34)
Gender of HH head	59.93	99.21	41.46	-294.7	-1001.6**
	(105.0)	(65.94)	(114.3)	(203.0)	(799.9)
Highest education of	10.85	24.56**	42.31**	64.46**	179.8*
men in HH	(8.613)	(9.002)	(14.21)	(27.32)	(93.35)
Highest education of	19.28**	34.35**	59.83**	103.3**	95.49*
women in HH	(7.808)	(9.769)	(14.88)	(28.70)	(94.04)
Landholdings	-0.441*	-0.441*	-0.308	0.0705	-0.762
	(0.166)	(0.235)	(0.407)	(0.739)	(2.810)
HH economic	-16.75**	-34.74**	-38.90**	-58.39**	0.345
dependency ratio	(7.797)	(8.550)	(12.74)	(26.31)	(80.33)
# of HH head	22.46**	7.593*	7.807	16.78	2.138
relatives owning land	(5.769)	(6.991)	(10.52)	(18.10)	(48.95)
# of HH head's	-1.339	4.380	-0.391	7.932	65.78
spouse relatives owning land	(3.663)	(4.483)	(7.874)	(12.82)	(35.64)
# of HH head	-3.474	-4.927	1.243	1.763	35.38
relatives living outside thana	(4.514)	(5.224)	(7.353)	(12.47)	(37.17)
# of HH head's	1.672	2.241	0.342	-5.721	-39.51
spouse relatives living outside thana	(3.832)	(4.021)	(5.427)	(10.30)	(35.60)
Loans from	124.1	16.45	172.3	316.1	-181.3
traditional banks (1=yes)	(51.68)	(83.34)	(179.4)	(372.6)	(1863.0)
Loans from informal	-45.93	36.77	-9.273	226.2	327.9
sources (1=yes)	(50.73)	(54.10)	(88.88)	(154.7)	(599.3)
Loans from relatives	99.86**	141.5**	244.0**	454.8**	1908.2**
(1=yes)	(46.55)	(47.81)	(73.33)	(167.6)	(658.5)
Eligibility of HH	-32.01	-45.23	-39.47	-14.38	-376.4
(1=yes)	(44.41)	(55.37)	(76.09)	(159.9)	(582.9)
Village covariates					
Average male wage	0.460	-0.0989	-0.745	0.0267	1.919
	(1.064)	(1.211)	(1.650)	(2.904)	(11.16)
Average female wage	1.299	0.804	2.846	1.559	1.913
	(0.950)	(0.970)	(1.565)	(2.798)	(9.839)
Primary school	-16.39	-36.35*	-91.02**	-210.7**	-195.2*
(1=yes)	(23.12)	(25.81)	(39.74)	(68.73)	(202.4)
Food program (1=yes)	-11.33	-11.66	-47.39	-49.36	-106.8
	(20.17)	(21.30)	(33.07)	(57.89)	(181.6)
Distance to nearest	1.587	-4.248	-15.04**	-22.74**	-54.08
bank (km)	(4.035)	(4.427)	(6.447)	(9.656)	(31.33)
Distance to nearest	-1.511	-5.002	-3.757	-6.144	14.53
pucca road (km)	(3.259)	(3.832)	(4.756)	(8.600)	(35.60)
Distance to nearest	5.433	11.84*	23.23**	44.87*	30.31
shop/market (km)	(6.851)	(7.440)	(11.16)	(17.93)	(49.32)
Electricity in	26.25*	69.52**	60.65**	116.2*	202.5
village (1=yes)	(20.94)	(21.97)	(33.97)	(63.56)	(196.6)
Price of rice	-17.89	-17.49	0.930	1.314	28.80
	(9.586)	(11.60)	(16.52)	(29.44)	(99.88)
Price of wheat flour	8.587	11.93	-11.89	-11.70	-64.43
	(11.27)	(11.91)	(18.39)	(33.92)	(109.6)
Price of mustard oil	2.743	2.822	2.254	-3.803	-33.57
	(1.488)	(2.056)	(3.071)	(5.271)	(14.55)
Price of hen's eggs	1.228	-1.631	-1.655	-0.569	0.578
	(4.471)	(4.164)	(8.868)	(31.30)	(65.29)
Price of milk	3.667	8.595*	10.82**	17.94*	20.07
	(3.960)	(4.020)	(6.819)	(11.59)	(40.68)
Price of potatoes	-3.821	-13.43**	-7.079	-23.04	-64.43
	(5.208)	(6.043)	(10.40)	(18.38)	(50.56)
Intercepts					
Second wave of data	67.76	141.4**	159.0**	436.0**	1243.1*
(1=yes)	(54.89)	(63.35)	(95.25)	(190.9)	(537.7)
Overall intercept	235.3	543.1**	823.6**	1541.1**	4183.6**
	(168.4)	(183.8)	(282.3)	(489.6)	(1699.9)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table J.6 **Panel quantile regressions with household correlated random effects, household non-food expenditure, by gender**

	Quantile				
	10%	30%	Median	70%	90%
Female microcredit	0.004**	0.006**	0.012**	0.018**	0.047**
borrowings	(0.001)	(0.002)	(0.003)	(0.007)	(0.019)
Male microcredit	0.010	0.011**	0.013**	0.019**	0.027
borrowings	(0.004)	(0.006)	(0.008)	(0.009)	(0.025)
Household characteristics					
Education of HH head	-15.85	-6.406	-21.20	-54.22*	-239.7
	(10.30)	(12.89)	(18.28)	(35.77)	(149.8)
Age of HH head	-0.400	-0.876	0.585	-4.261	-3.072
	(2.357)	(2.478)	(3.177)	(5.562)	(13.30)
Gender of HH head	64.25	96.58	53.30	-288.8	-991.6**
	(105.0)	(66.84)	(114.5)	(199.6)	(805.7)
Highest education of	10.18	24.11**	42.01**	65.95**	188.0*
men in HH	(8.505)	(9.071)	(14.21)	(27.44)	(94.15)
Highest education of	16.73**	33.59**	60.05**	102.7**	107.6*
women in HH	(7.742)	(9.756)	(14.96)	(28.75)	(94.69)
Landholdings	-0.454*	-0.448*	-0.320	0.0328	-0.689
	(0.158)	(0.234)	(0.406)	(0.743)	(2.853)
HH economic	-13.37**	-34.55**	-40.41**	-58.00**	-12.03
dependency ratio	(7.834)	(8.645)	(12.93)	(26.00)	(81.48)
# of HH head	25.14**	8.212*	7.869	17.72	4.824
relatives owning land	(5.831)	(7.019)	(10.58)	(17.89)	(48.85)
# of HH head's	-2.212	3.810	0.689	7.414	64.73
spouse relatives owning land	(3.645)	(4.496)	(7.906)	(12.73)	(35.84)
# of HH head	-4.739	-5.467	1.335	1.071	31.61
relatives living outside thana	(4.542)	(5.209)	(7.333)	(12.55)	(36.32)
# of HH head's	1.982	1.962	-0.344	-5.774	-38.65
spouse relatives living outside thana	(3.771)	(4.000)	(5.385)	(10.14)	(35.93)
Loans from	106.8	19.10	179.5	286.2	-199.5
traditional banks (1=yes)	(51.92)	(82.99)	(180.1)	(372.1)	(1876.0)
Loans from informal	-40.36	34.55	-9.299	232.6	319.5
sources (1=yes)	(50.59)	(53.96)	(89.82)	(155.3)	(600.0)
Loans from relatives	91.69**	142.1**	246.0**	453.6**	1954.2**
(1=yes)	(46.72)	(48.42)	(74.27)	(167.1)	(657.1)
Eligibility of HH	-51.48	-42.89	-38.62	-16.81	-314.2
(1=yes)	(44.29)	(54.76)	(76.05)	(158.5)	(583.2)
Village covariates					
Average male wage	0.404	-0.0606	-0.625	0.152	2.600
	(1.062)	(1.202)	(1.647)	(2.902)	(11.07)
Average female wage	1.149	0.680	2.941	1.396	2.257
	(0.966)	(0.978)	(1.567)	(2.772)	(9.838)
Primary school	-14.83	-34.42*	-93.66**	-205.0**	-192.3
(1=yes)	(22.56)	(25.79)	(40.40)	(68.46)	(202.2)
Food program (1=yes)	-10.08	-11.33	-50.11	-52.65	-95.09
	(19.75)	(21.30)	(33.23)	(57.03)	(182.4)
Distance to nearest	1.968	-4.100	-14.99**	-22.08**	-55.30
bank (km)	(3.955)	(4.413)	(6.476)	(9.720)	(31.30)
Distance to nearest	-1.140	-6.007	-4.410	-6.145	11.70
pucca road (km)	(3.258)	(3.803)	(4.801)	(8.610)	(35.70)
Distance to nearest	6.135	12.37**	22.55**	44.66**	18.31
shop/market (km)	(6.754)	(7.460)	(11.13)	(17.98)	(49.53)
Electricity in	29.98*	69.41**	56.90**	108.8*	226.1
village (1=yes)	(20.71)	(21.90)	(34.24)	(63.05)	(196.2)
Price of rice	-18.83	-16.63	-0.322	-0.276	35.09
	(9.677)	(11.50)	(16.66)	(28.99)	(99.87)
Price of wheat flour	12.47	9.478	-12.39	-9.900	-66.30
	(11.32)	(11.95)	(18.52)	(33.96)	(109.5)
Price of mustard oil	2.827	2.829	2.534	-3.078	-36.50
	(1.492)	(2.034)	(3.132)	(5.272)	(14.52)
Price of hen's eggs	0.988	-1.426	-1.042	-0.549	4.033
	(4.521)	(4.142)	(8.779)	(31.00)	(65.29)
Price of milk	4.255	8.987*	10.97**	18.50	12.19
	(3.961)	(3.983)	(6.843)	(11.57)	(40.37)
Price of potatoes	-3.895	-13.29**	-7.355	-23.71	-60.49
	(5.290)	(6.092)	(10.36)	(18.19)	(50.96)
Intercepts					
Second wave of data	61.96	153.7**	150.7**	433.6**	1196.4*
(1=yes)	(54.30)	(63.27)	(96.02)	(187.8)	(532.1)
Overall intercept	162.9	547.1**	829.8**	1485.6**	4341.1**
	(169.2)	(185.0)	(284.3)	(488.8)	(1671.4)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Appendix K

Regression tables for panel data quantile regressions with correlated random effects (CRE) and village quantile effects

Table K.1 **Panel quantile regressions with household correlated random effects with village quantile effects, household total expenditure**

	Quantile				
	10%	30%	Median	70%	90%
Household	0.013**	0.022**	0.031**	0.038**	0.042**
microcredit borrowings	(0.005)	(0.006)	(0.007)	(0.009)	(0.014)
Household characteristics					
Education of HH head	23.26	-20.21	-43.96	-33.73	-39.17
	(37.24)	(35.72)	(44.82)	(63.66)	(121.9)
Age of HH head	11.71	-6.419	-5.411	4.652	-7.711
	(9.640)	(7.570)	(8.425)	(10.96)	(17.21)
Gender of HH head	-463.5	63.18	-429.6	-598.4	-928.8*
	(347.0)	(284.6)	(329.9)	(452.7)	(717.0)
Highest education of	17.55	71.32**	89.60**	81.61*	38.18
men in HH	(32.01)	(28.65)	(32.81)	(44.67)	(81.16)
Highest education of	-6.665	53.06**	83.51**	95.34**	264.3**
women in HH	(24.53)	(24.94)	(30.20)	(44.53)	(81.08)
Landholdings	-0.625	-0.253	-0.324	-0.866	-1.462
	(0.560)	(0.473)	(0.824)	(1.271)	(1.249)
HH economic	-47.39**	-117.8**	-139.2**	-104.8**	-212.3**
dependency ratio	(28.32)	(29.52)	(36.96)	(53.03)	(75.91)
# of HH head	36.97**	34.74*	35.18	24.80	61.07
relatives owning land	(17.94)	(19.38)	(26.85)	(31.86)	(49.22)
# of HH head's	3.367	-18.91	-28.75	12.45	44.91
spouse relatives owning land	(14.24)	(14.78)	(20.47)	(27.94)	(32.13)
# of HH head	23.52	26.00	23.59	14.75	-1.426
relatives living outside thana	(19.50)	(16.92)	(21.10)	(26.05)	(41.86)
# of HH head's	16.30	8.601	10.10	-21.40	-42.00
spouse relatives living outside thana	(13.74)	(15.95)	(18.51)	(23.68)	(37.00)
Loans from	132.8	328.5	548.6	167.2	545.0
traditional banks (1=yes)	(225.7)	(234.3)	(350.0)	(538.9)	(885.3)
Loans from informal	-91.91	153.2	37.93	-351.6	819.1
sources (1=yes)	(181.6)	(185.2)	(231.7)	(295.1)	(502.3)
Loans from relatives	211.9	359.4**	433.1**	866.4**	892.2**
(1=yes)	(146.2)	(145.1)	(192.8)	(293.3)	(459.3)
Eligibility of HH	-267.5*	-227.4	-172.5	-302.1	-373.7
(1=yes)	(174.1)	(158.6)	(210.6)	(301.2)	(501.7)
Intercepts					
Second wave of data	-172.3	114.4	115.4	223.0*	991.9**
(1=yes)	(117.4)	(106.5)	(133.8)	(177.2)	(311.1)
Overall intercept	3155.2**	4311.5**	5279.5**	7065.0**	7826.9**
	(480.7)	(391.1)	(511.8)	(870.1)	(2197.0)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table K.2 **Panel quantile regressions with household correlated random effects with village quantile effects, household total expenditure, by gender**

	Quantile				
	10%	30%	Median	70%	90%
Female microcredit	0.015**	0.022**	0.033**	0.037**	0.048**
borrowings	(0.006)	(0.007)	(0.008)	(0.010)	(0.016)
Male microcredit	0.011	0.022*	0.029**	0.041**	0.037
borrowings	(0.008)	(0.010)	(0.011)	(0.017)	(0.027)
Household characteristics					
Education of HH head	20.13	-17.43	-47.29	-30.17	-23.43
	(37.23)	(35.30)	(44.72)	(64.51)	(122.3)
Age of HH head	12.04	-6.670	-5.776	3.648	-4.809
	(9.739)	(7.598)	(8.561)	(11.00)	(17.32)
Gender of HH head	-453.2	32.35	-376.2	-661.0	-979.4*
	(346.5)	(282.0)	(331.6)	(452.1)	(714.3)
Highest education of	14.15	67.62**	92.31**	86.77*	41.06
men in HH	(32.51)	(28.74)	(32.63)	(44.84)	(81.97)
Highest education of	-2.805	53.40**	89.67**	104.8**	263.9**
women in HH	(24.43)	(25.14)	(30.14)	(44.82)	(82.06)
Landholdings	-0.695	-0.243	-0.461	-0.926	-1.565
	(0.561)	(0.476)	(0.826)	(1.275)	(1.257)
HH economic	-52.97**	-118.1**	-131.0**	-102.7**	-218.2**
dependency ratio	(28.60)	(29.45)	(36.76)	(53.26)	(76.19)
# of HH head	40.20**	34.25*	37.24	22.74	57.97
relatives owning land	(17.90)	(19.52)	(26.80)	(32.01)	(48.91)
# of HH head's	0.866	-19.04	-28.38	13.54	47.04
spouse relatives owning land	(14.40)	(14.75)	(20.41)	(27.96)	(32.09)
# of HH head	23.61	23.89	23.77	17.57	-4.878
relatives living outside thana	(19.59)	(17.21)	(20.97)	(26.11)	(42.10)
# of HH head's	14.71	9.334	8.125	-20.74	-44.41
spouse relatives living outside thana	(13.76)	(16.00)	(18.30)	(23.45)	(37.88)
Loans from	156.9	333.7	482.6	143.1	522.9
traditional banks (1=yes)	(226.9)	(235.7)	(349.6)	(542.1)	(885.0)
Loans from informal	-59.41	169.9	39.88	-375.3	765.4
sources (1=yes)	(183.2)	(186.8)	(230.9)	(295.4)	(504.8)
Loans from relatives	220.3	368.6**	442.3**	891.4**	930.8**
(1=yes)	(144.4)	(145.8)	(193.3)	(294.5)	(460.9)
Eligibility of HH	-304.8*	-225.4	-173.4	-312.4	-397.4
(1=yes)	(174.4)	(160.2)	(215.6)	(303.7)	(504.1)
Intercepts					
Second wave of data	-176.9	115.8	109.0	203.0*	963.7*
(1=yes)	(117.2)	(106.4)	(134.4)	(178.2)	(316.7)
Overall intercept	3241.4**	4294.6**	5241.8**	6914.3**	7861.3**
	(482.2)	(391.5)	(510.9)	(873.7)	(2192.9)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table K.3 **Panel quantile regressions with household correlated random effects with village quantile effects, household food expenditure**

	Quantile				
	10%	30%	Median	70%	90%
Household	0.008**	0.012**	0.012**	0.013**	0.010**
microcredit borrowings	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Household characteristics					
Education of HH head	30.39	-12.78	-2.793	20.74	11.01
	(20.84)	(23.32)	(22.24)	(24.81)	(37.98)
Age of HH head	9.444	1.600	-1.569	-3.361	16.55
	(6.894)	(5.492)	(4.808)	(6.002)	(9.725)
Gender of HH head	-268.4	-66.67	-140.8	-104.8	-803.4**
	(203.6)	(204.8)	(260.7)	(256.7)	(326.0)
Highest education of	7.447	28.31*	21.47	15.52	32.65
men in HH	(20.32)	(18.42)	(19.81)	(22.30)	(28.55)
Highest education of	-25.71	-3.069	16.65	22.07	30.98
women in HH	(14.82)	(18.85)	(19.03)	(19.99)	(25.45)
Landholdings	-0.152	0.0363	0.281	-0.489	-1.803*
	(0.365)	(0.392)	(0.570)	(0.806)	(0.680)
HH economic	-32.62**	-50.11**	-69.08**	-76.72**	-113.5**
dependency ratio	(19.79)	(18.65)	(21.96)	(27.06)	(32.31)
# of HH head	41.22**	7.232	13.69	20.97	25.10
relatives owning land	(13.58)	(14.26)	(16.11)	(17.52)	(23.69)
# of HH head's	0.277	-8.682	-13.77	-11.52	-7.199
spouse relatives owning land	(9.243)	(10.58)	(11.29)	(12.99)	(17.22)
# of HH head	9.756	21.83	9.864	12.04	40.91*
relatives living outside thana	(13.22)	(12.19)	(13.00)	(15.83)	(22.88)
# of HH head's	17.81	10.88	17.94*	25.60*	11.15
spouse relatives living outside thana	(9.084)	(11.00)	(11.19)	(11.64)	(15.00)
Loans from	87.09	47.63	-27.91	11.63	128.2
traditional banks (1=yes)	(140.1)	(150.5)	(156.3)	(196.0)	(270.4)
Loans from informal	-37.25	109.1	87.31	-105.9	-135.0
sources (1=yes)	(119.6)	(117.9)	(126.3)	(139.8)	(206.8)
Loans from relatives	56.74	108.1	229.9*	192.0	108.6
(1=yes)	(99.00)	(101.2)	(103.2)	(134.5)	(193.7)
Eligibility of HH	-246.4*	-219.0*	-142.5	-116.2	-88.18
(1=yes)	(111.9)	(116.0)	(119.1)	(145.5)	(179.4)
Intercepts					
Second wave of data	-246.5**	-97.07	-52.42	-4.679	-71.14
(1=yes)	(82.83)	(73.46)	(76.83)	(101.9)	(141.2)
Overall intercept	2563.2**	3231.4**	3826.1**	4403.8**	6623.0**
	(299.7)	(264.2)	(331.1)	(376.4)	(826.5)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table K.4 **Panel quantile regressions with household correlated random effects with village quantile effects, household food expenditure, by gender**

	Quantile				
	10%	30%	Median	70%	90%
Female microcredit	0.008**	0.013**	0.013**	0.013**	0.009**
borrowings	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Male microcredit	0.009	0.008	0.004	0.010	0.007
borrowings	(0.006)	(0.005)	(0.007)	(0.007)	(0.015)
Household characteristics					
Education of HH head	30.28	-11.84	-5.180	20.95	9.830
	(21.06)	(23.19)	(22.06)	(24.89)	(38.64)
Age of HH head	9.185	2.587	-1.733	-3.217	15.83
	(6.900)	(5.506)	(4.829)	(6.024)	(9.749)
Gender of HH head	-267.6	-89.12	-120.7	-117.0	-709.3*
	(204.3)	(205.0)	(260.9)	(258.0)	(330.7)
Highest education of	7.520	31.42*	20.82	14.85	37.85
men in HH	(20.46)	(18.33)	(19.71)	(22.15)	(28.57)
Highest education of	-26.90	-6.854	13.75	20.03	27.82
women in HH	(14.80)	(18.74)	(19.11)	(20.10)	(25.54)
Landholdings	-0.146	0.0258	0.281	-0.501	-1.812*
	(0.365)	(0.387)	(0.572)	(0.810)	(0.687)
HH economic	-35.36**	-50.62**	-72.98**	-76.12**	-111.3**
dependency ratio	(19.82)	(18.61)	(22.04)	(26.97)	(32.36)
# of HH head	42.58**	5.865	14.15	21.11	21.59
relatives owning land	(13.70)	(14.25)	(16.22)	(17.41)	(23.65)
# of HH head's	-0.591	-7.676	-14.61	-12.03	-6.027
spouse relatives owning land	(9.229)	(10.48)	(11.34)	(13.01)	(17.16)
# of HH head	9.140	23.94	7.281	15.14	43.36*
relatives living outside thana	(13.22)	(12.24)	(13.15)	(15.87)	(23.07)
# of HH head's	17.73	11.42	18.94*	24.15*	10.31
spouse relatives living outside thana	(9.126)	(11.01)	(11.22)	(11.67)	(15.14)
Loans from	100.2	47.57	-33.85	28.83	121.7
traditional banks (1=yes)	(140.1)	(148.8)	(155.9)	(196.5)	(272.8)
Loans from informal	-28.18	113.6	82.79	-130.7	-124.6
sources (1=yes)	(118.7)	(118.7)	(124.7)	(139.5)	(205.6)
Loans from relatives	55.36	100.2	217.8*	181.5	139.3
(1=yes)	(99.23)	(101.3)	(103.6)	(135.3)	(194.1)
Eligibility of HH	-238.5*	-212.1	-132.2	-119.5	-105.0
(1=yes)	(112.7)	(114.0)	(120.1)	(146.2)	(179.4)
Intercepts					
Second wave of data	-243.0**	-112.6	-40.86	-2.988	-88.75
(1=yes)	(82.92)	(73.06)	(77.26)	(102.8)	(142.3)
Overall intercept	2571.3**	3261.9**	3803.5**	4410.5**	6568.5**
	(299.9)	(264.6)	(330.5)	(379.7)	(826.8)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table K.5 **Panel quantile regressions with household correlated random effects with village quantile effects, household non-food expenditure**

	Quantile				
	10%	30%	Median	70%	90%
Household	0.007**	0.009**	0.014**	0.019**	0.040**
microcredit borrowings	(0.002)	(0.002)	(0.004)	(0.006)	(0.013)
Household characteristics					
Education of HH head	-14.15	-0.155	-15.12	-45.22	-24.96
	(11.39)	(14.07)	(18.48)	(39.62)	(87.58)
Age of HH head	-0.465	-1.036	-0.691	-3.679	-17.18
	(2.252)	(2.696)	(3.106)	(5.800)	(10.83)
Gender of HH head	127.8	61.59	5.256	-144.4	-760.5**
	(89.35)	(91.20)	(109.7)	(240.6)	(432.5)
Highest education of	14.83**	20.89**	30.53**	58.80**	30.76
men in HH	(7.957)	(9.405)	(14.02)	(27.32)	(56.75)
Highest education of	16.79**	40.79**	46.18**	82.05**	153.9**
women in HH	(6.811)	(11.54)	(14.59)	(29.91)	(55.86)
Landholdings	-0.541**	-0.457*	-0.134	-0.0491	0.253
	(0.159)	(0.214)	(0.361)	(0.656)	(0.853)
HH economic	-21.89**	-31.49**	-40.45**	-23.48	-122.8
dependency ratio	(7.058)	(9.802)	(13.51)	(26.91)	(46.77)
# of HH head	20.84**	10.90**	13.88	3.097	14.75
relatives owning land	(5.677)	(6.943)	(11.08)	(18.78)	(32.80)
# of HH head's	0.700	0.443	2.522	12.88	30.56
spouse relatives owning land	(3.680)	(4.878)	(7.496)	(12.80)	(20.90)
# of HH head	-2.619	0.648	0.891	13.40	2.255
relatives living outside thana	(4.548)	(5.709)	(6.970)	(13.56)	(24.46)
# of HH head's	6.930	-1.173	-2.343	-17.31	-42.14
spouse relatives living outside thana	(3.975)	(4.573)	(5.628)	(10.90)	(26.50)
Loans from	78.98	77.77	172.1	293.6	1128.2
traditional banks (1=yes)	(52.68)	(84.41)	(153.7)	(331.4)	(951.2)
Loans from informal	4.847	62.65	46.02	11.69	340.7
sources (1=yes)	(44.50)	(66.13)	(86.94)	(147.0)	(287.2)
Loans from relatives	116.0**	185.1**	246.2**	396.0**	599.2**
(1=yes)	(41.04)	(54.05)	(82.88)	(158.7)	(321.4)
Eligibility of HH	-88.17	-28.39	-1.417	-141.7	-251.9
(1=yes)	(43.63)	(56.88)	(86.45)	(168.2)	(348.8)
Intercepts					
Second wave of data	103.4**	130.2**	188.8**	263.3**	798.4**
(1=yes)	(27.28)	(35.62)	(50.08)	(87.98)	(193.1)
Overall intercept	545.5**	811.2**	1137.5**	1641.7**	1995.1**
	(124.0)	(160.2)	(203.8)	(454.2)	(2484.8)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table K.6 **Panel quantile regressions with household correlated random effects with village quantile effects, household non-food expenditure, by gender**

	Quantile				
	10%	30%	Median	70%	90%
Female microcredit	0.006**	0.010**	0.013**	0.020**	0.044**
borrowings	(0.002)	(0.002)	(0.004)	(0.007)	(0.014)
Male microcredit	0.012**	0.009**	0.026**	0.021*	0.014
borrowings	(0.004)	(0.006)	(0.008)	(0.010)	(0.021)
Household characteristics					
Education of HH head	-14.01	-1.809	-17.43	-50.96	-23.86
	(11.48)	(14.21)	(18.29)	(39.38)	(87.18)
Age of HH head	-0.0962	-1.078	-0.814	-2.287	-15.33
	(2.243)	(2.695)	(3.130)	(5.820)	(10.70)
Gender of HH head	109.5	67.67	20.12	-134.5	-639.8**
	(89.90)	(91.96)	(110.9)	(238.4)	(437.3)
Highest education of	14.51**	20.27**	32.98**	57.10**	32.42
men in HH	(7.958)	(9.280)	(14.05)	(27.22)	(57.26)
Highest education of	15.61**	41.30**	44.43**	81.40**	162.4**
women in HH	(6.933)	(11.50)	(14.59)	(30.04)	(55.62)
Landholdings	-0.547**	-0.457*	-0.121	-0.0753	0.126
	(0.156)	(0.214)	(0.361)	(0.658)	(0.850)
HH economic	-21.80**	-31.49**	-39.65**	-21.13	-100.3
dependency ratio	(7.065)	(9.696)	(13.58)	(27.04)	(46.98)
# of HH head	19.17**	10.93**	13.45	5.565	19.90
relatives owning land	(5.698)	(6.897)	(10.87)	(18.65)	(32.57)
# of HH head's	1.643	0.747	2.561	11.09	22.03
spouse relatives owning land	(3.722)	(4.949)	(7.498)	(12.69)	(20.94)
# of HH head	-3.172	0.853	0.687	12.80	13.08
relatives living outside thana	(4.557)	(5.750)	(6.975)	(13.43)	(24.75)
# of HH head's	6.744	-1.652	-1.924	-13.88	-27.38
spouse relatives living outside thana	(3.966)	(4.602)	(5.569)	(10.97)	(26.35)
Loans from	85.42	70.49	169.9	277.2	1077.5
traditional banks (1=yes)	(52.53)	(86.00)	(155.3)	(331.4)	(947.5)
Loans from informal	15.38	70.26	46.33	30.34	401.0
sources (1=yes)	(43.98)	(66.29)	(85.35)	(145.1)	(280.4)
Loans from relatives	118.1**	182.4**	236.4**	417.5**	582.8**
(1=yes)	(41.50)	(54.13)	(82.60)	(159.1)	(315.5)
Eligibility of HH	-80.61	-27.99	8.874	-157.3	-284.0
(1=yes)	(43.23)	(56.56)	(85.79)	(168.4)	(346.1)
Intercepts					
Second wave of data	106.7**	130.3**	197.4**	240.8**	687.4**
(1=yes)	(27.16)	(35.69)	(50.14)	(89.20)	(190.5)
Overall intercept	528.7**	790.2**	1141.2**	1571.2**	1962.4**
	(125.7)	(161.4)	(204.6)	(456.2)	(2485.9)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Appendix L

Regression tables for panel data quantile regressions with correlated random effects (CRE) and penalised village effects

Table L.1 **Panel quantile regressions with household correlated random effects with penalised village effects, household total expenditure**

	Quantile				
	10%	30%	Median	70%	90%
Household	0.014**	0.020**	0.026**	0.034**	0.058**
microcredit borrowings	(0.004)	(0.004)	(0.005)	(0.006)	(0.014)
Household characteristics					
Education of HH head	-2.894	-24.31	-33.69	-32.15	-13.19
	(31.21)	(27.05)	(28.93)	(47.34)	(114.9)
Age of HH head	1.233	-4.059	-0.0566	-1.764	-3.900
	(6.557)	(6.600)	(6.276)	(9.523)	(14.04)
Gender of HH head	-148.5	225.0	-249.2	-910.7**	-1974.6**
	(285.0)	(279.8)	(251.5)	(327.8)	(667.3)
Highest education of	57.20**	70.83**	85.73**	91.34**	192.2**
men in HH	(23.10)	(20.40)	(21.87)	(32.94)	(72.31)
Highest education of	34.26*	53.84**	60.86**	66.22**	154.1**
women in HH	(21.05)	(18.74)	(20.46)	(32.67)	(77.75)
Landholdings	-0.557	-0.342	-0.513	-0.529	-3.443*
	(0.516)	(0.451)	(0.730)	(1.400)	(2.704)
HH economic	-52.93**	-109.2**	-127.8**	-140.7**	-183.4**
dependency ratio	(23.52)	(19.59)	(24.82)	(35.43)	(71.67)
# of HH head	18.55*	25.98*	13.74	18.53	47.09
relatives owning land	(14.14)	(13.92)	(16.79)	(23.39)	(45.02)
# of HH head's	-3.406	-9.845	1.771	13.72	23.91
spouse relatives owning land	(10.49)	(10.19)	(12.47)	(18.10)	(34.61)
# of HH head	24.97*	13.14	17.75	35.49*	99.67**
relatives living outside thana	(12.65)	(12.65)	(15.41)	(21.18)	(34.98)
# of HH head's	9.947	19.31*	18.37	28.41	-9.115
spouse relatives living outside thana	(10.51)	(10.42)	(11.80)	(18.36)	(38.99)
Loans from	290.7	213.8	267.1	413.9	401.9
traditional banks (1=yes)	(178.3)	(161.6)	(207.8)	(322.9)	(875.0)
Loans from informal	231.4*	240.3*	139.7	167.0	1004.4**
sources (1=yes)	(106.7)	(132.2)	(171.0)	(233.1)	(423.7)
Loans from relatives	348.7**	184.9**	413.7**	593.0**	1447.9**
(1=yes)	(103.8)	(110.7)	(144.0)	(187.7)	(427.7)
Eligibility of HH	-368.8**	-233.2	-213.9	-50.56	-757.9
(1=yes)	(149.9)	(142.3)	(178.9)	(273.5)	(639.2)
Intercepts					
Second wave of data	-125.8	15.67	139.2	221.8*	437.0**
(1=yes)	(88.52)	(80.84)	(94.56)	(126.8)	(250.8)
Overall intercept	2929.0**	3798.3**	4469.3**	5836.1**	7577.0**
	(249.1)	(236.2)	(260.4)	(384.8)	(844.9)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table L.2 **Panel quantile regressions with household correlated random effects with penalised village effects, household total expenditure, by gender**

	Quantile				
	10%	30%	Median	70%	90%
Female microcredit	0.016**	0.021**	0.028**	0.034**	0.065**
borrowings	(0.005)	(0.005)	(0.005)	(0.007)	(0.016)
Male microcredit	0.014	0.018**	0.016**	0.033**	0.020
borrowings	(0.009)	(0.007)	(0.009)	(0.013)	(0.028)
Household characteristics					
Education of HH head	-8.962	-26.19	-28.83	-31.75	-1.625
	(31.06)	(26.90)	(28.22)	(47.98)	(115.6)
Age of HH head	2.035	-4.601	-1.188	-3.561	-7.387
	(6.594)	(6.691)	(6.438)	(9.757)	(13.99)
Gender of HH head	-154.6	197.5	-354.9	-858.0**	-1883.7**
	(282.6)	(279.2)	(245.5)	(327.4)	(673.7)
Highest education of	59.83**	70.84**	90.62**	95.06**	169.2*
men in HH	(22.92)	(20.21)	(21.39)	(33.30)	(71.16)
Highest education of	32.38*	54.73**	58.54**	62.40**	169.0**
women in HH	(20.96)	(19.29)	(20.51)	(32.68)	(77.35)
Landholdings	-0.568	-0.343	-0.601	-0.384	-2.788
	(0.522)	(0.439)	(0.715)	(1.392)	(2.824)
HH economic	-44.84**	-112.8**	-124.5**	-134.1**	-205.2**
dependency ratio	(23.73)	(20.13)	(24.46)	(36.23)	(74.84)
# of HH head	19.73*	27.17	12.55	17.66	60.42
relatives owning land	(14.11)	(13.97)	(17.19)	(23.03)	(46.73)
# of HH head's	-6.009	-8.447	2.290	12.19	20.80
spouse relatives owning land	(10.49)	(10.22)	(12.54)	(18.28)	(35.34)
# of HH head	27.56*	14.94	22.48	37.14*	95.85**
relatives living outside thana	(12.45)	(12.87)	(15.68)	(21.83)	(35.80)
# of HH head's	11.13	19.57*	20.88	27.03	-8.160
spouse relatives living outside thana	(10.52)	(10.34)	(11.88)	(18.32)	(39.55)
Loans from	295.8	219.0	234.5	372.0	504.4
traditional banks (1=yes)	(180.9)	(162.6)	(213.0)	(312.5)	(861.0)
Loans from informal	211.6*	228.5	124.5	195.6	990.8**
sources (1=yes)	(105.4)	(133.1)	(166.9)	(235.2)	(439.7)
Loans from relatives	328.8**	183.4**	385.2**	593.4**	1476.4**
(1=yes)	(104.2)	(113.7)	(144.3)	(184.6)	(429.6)
Eligibility of HH	-363.3**	-251.4	-145.3	-24.55	-805.7
(1=yes)	(151.3)	(143.3)	(180.7)	(270.7)	(644.1)
Intercepts					
Second wave of data	-162.9	25.37	108.9	224.2*	428.5*
(1=yes)	(88.12)	(81.12)	(94.80)	(130.5)	(256.8)
Overall intercept	2965.3**	3792.7**	4480.1**	5714.3**	7427.7**
	(251.0)	(234.7)	(262.5)	(384.0)	(846.0)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table L.3 **Panel quantile regressions with household correlated random effects with penalised village effects, household food expenditure**

	Quantile				
	10%	30%	Median	70%	90%
Household	0.008**	0.011**	0.012**	0.014**	0.014**
microcredit borrowings	(0.003)	(0.002)	(0.003)	(0.003)	(0.004)
Household characteristics					
Education of HH head	30.52	9.798	-7.772	6.064	-25.24
	(21.04)	(17.59)	(15.33)	(19.69)	(45.43)
Age of HH head	0.431	-3.102	-3.103	-1.549	6.619
	(5.553)	(5.081)	(3.551)	(5.406)	(9.788)
Gender of HH head	-190.5	-57.71	-171.1	-118.4	-807.8**
	(206.2)	(190.6)	(198.6)	(179.6)	(375.0)
Highest education of	12.75	32.90**	40.58**	34.11**	67.63**
men in HH	(18.59)	(13.32)	(13.03)	(18.25)	(30.58)
Highest education of	11.89	10.11	23.32**	14.65	30.49
women in HH	(15.37)	(12.14)	(12.31)	(15.40)	(29.90)
Landholdings	-0.135	-0.0461	0.146	-0.493	-2.021*
	(0.347)	(0.315)	(0.428)	(0.594)	(0.851)
HH economic	-38.46**	-58.86**	-73.23**	-97.57**	-125.0**
dependency ratio	(16.51)	(13.73)	(14.84)	(19.22)	(33.65)
# of HH head	20.25	14.20	0.0243	7.783	24.91
relatives owning land	(11.70)	(9.369)	(9.870)	(12.62)	(24.90)
# of HH head's	-1.821	-8.764	-0.533	-6.185	16.58
spouse relatives owning land	(7.640)	(7.248)	(7.058)	(8.935)	(18.51)
# of HH head	17.30	13.64	-2.900	4.584	19.39
relatives living outside thana	(10.15)	(8.635)	(9.610)	(12.81)	(25.28)
# of HH head's	8.332	13.67**	21.35**	25.51**	21.30
spouse relatives living outside thana	(7.405)	(7.134)	(7.085)	(8.908)	(18.19)
Loans from	125.2	30.61	8.764	47.62	364.0
traditional banks (1=yes)	(130.1)	(116.5)	(102.2)	(137.3)	(283.7)
Loans from informal	146.1	141.3	168.6	-8.556	212.7
sources (1=yes)	(86.96)	(95.16)	(94.81)	(113.6)	(225.7)
Loans from relatives	207.1**	101.4	133.5**	165.9	304.3
(1=yes)	(81.21)	(85.19)	(69.00)	(96.11)	(172.7)
Eligibility of HH	-257.7*	-201.7**	-195.6*	-68.38	5.121
(1=yes)	(125.8)	(94.22)	(98.06)	(127.0)	(199.2)
Intercepts					
Second wave of data	-252.4**	-141.6**	-47.26	-42.06	25.09
(1=yes)	(71.38)	(61.54)	(55.80)	(74.37)	(135.6)
Overall intercept	2282.4**	2914.2**	3606.5**	4197.8**	5590.0**
	(193.8)	(173.0)	(156.3)	(202.9)	(370.1)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table L.4 **Panel quantile regressions with household correlated random effects with penalised village effects, household food expenditure, by gender**

	Quantile				
	10%	30%	Median	70%	90%
Female microcredit	0.009**	0.012**	0.013**	0.013**	0.015**
borrowings	(0.003)	(0.002)	(0.003)	(0.004)	(0.004)
Male microcredit	0.001	0.007**	0.008**	0.006	0.016
borrowings	(0.007)	(0.004)	(0.005)	(0.006)	(0.011)
Household characteristics					
Education of HH head	25.77	13.62	-9.910	4.670	-25.56
	(20.56)	(17.49)	(15.20)	(20.05)	(44.20)
Age of HH head	-0.0602	-4.486	-3.066	-1.395	7.173
	(5.570)	(5.037)	(3.557)	(5.283)	(9.941)
Gender of HH head	-142.0	-62.68	-163.5	-88.98	-783.7**
	(209.5)	(188.0)	(197.2)	(176.5)	(387.4)
Highest education of	16.01	35.63**	41.53**	38.80**	66.23**
men in HH	(18.62)	(13.22)	(12.91)	(17.98)	(30.41)
Highest education of	12.82	7.423	25.79**	12.98	26.87
women in HH	(15.20)	(12.07)	(12.22)	(15.19)	(29.57)
Landholdings	-0.112	-0.0333	0.156	-0.480	-2.037*
	(0.352)	(0.311)	(0.429)	(0.585)	(0.854)
HH economic	-37.78**	-59.34**	-72.04**	-98.49**	-120.2**
dependency ratio	(16.81)	(13.72)	(14.71)	(19.06)	(34.06)
# of HH head	15.53	13.81	0.589	13.05	31.90
relatives owning land	(11.57)	(9.328)	(10.15)	(12.52)	(24.31)
# of HH head’s	0.0111	-7.492	0.164	-6.101	11.84
spouse relatives owning land	(7.655)	(7.199)	(7.130)	(8.758)	(18.12)
# of HH head	19.55	14.45	-3.339	3.827	17.53
relatives living outside thana	(10.05)	(8.640)	(9.549)	(12.50)	(25.20)
# of HH head’s	8.311	15.78**	18.53**	26.03**	18.57
spouse relatives living outside thana	(7.606)	(7.136)	(7.056)	(8.839)	(17.63)
Loans from	105.3	42.36	5.926	15.83	322.2
traditional banks (1=yes)	(128.7)	(116.9)	(103.0)	(133.1)	(281.2)
Loans from informal	106.8	136.4	163.5	10.74	265.9
sources (1=yes)	(87.76)	(95.46)	(95.66)	(111.2)	(218.0)
Loans from relatives	226.2**	86.29	122.3**	144.5	284.3
(1=yes)	(82.58)	(84.96)	(68.83)	(96.00)	(171.0)
Eligibility of HH	-273.5*	-200.9**	-186.0*	-76.03	-8.652
(1=yes)	(126.0)	(93.38)	(97.72)	(126.7)	(198.7)
Intercepts					
Second wave of data	-261.7**	-130.4**	-39.98	-42.99	4.552
(1=yes)	(70.58)	(61.88)	(56.06)	(72.63)	(137.4)
Overall intercept	2277.8**	2912.1**	3603.1**	4195.1**	5554.1**
	(195.7)	(173.4)	(156.7)	(204.2)	(370.9)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table L.5 **Panel quantile regressions with household correlated random effects with penalised village effects, household non-food expenditure**

	Quantile				
	10%	30%	Median	70%	90%
Household	0.006**	0.006**	0.009**	0.018**	0.046**
microcredit borrowings	(0.002)	(0.001)	(0.002)	(0.004)	(0.011)
Household characteristics					
Education of HH head	-11.70	-13.86	-9.848	-31.15	55.39
	(8.729)	(9.892)	(12.55)	(28.14)	(95.95)
Age of HH head	1.743	0.114	1.576	0.646	-0.838
	(2.180)	(1.944)	(2.333)	(3.933)	(10.21)
Gender of HH head	29.66	29.21	-62.37	-218.4	-1036.2**
	(83.28)	(70.95)	(88.74)	(150.8)	(553.5)
Highest education of	17.34**	27.86**	31.62**	44.52**	85.27
men in HH	(6.786)	(6.916)	(10.29)	(21.21)	(58.74)
Highest education of	12.96**	31.33**	33.13**	67.07**	107.9**
women in HH	(5.468)	(7.548)	(8.631)	(22.01)	(64.27)
Landholdings	-0.401*	-0.322	-0.182	0.361	-0.152
	(0.146)	(0.221)	(0.367)	(0.714)	(2.389)
HH economic	-16.57**	-24.52**	-33.07**	-44.02**	-104.8
dependency ratio	(6.789)	(6.476)	(8.870)	(17.05)	(50.31)
# of HH head	15.00**	11.58**	5.354	12.54	23.04
relatives owning land	(4.106)	(4.672)	(6.171)	(10.65)	(34.84)
# of HH head's	2.837	5.151	8.110	0.828	22.50
spouse relatives owning land	(2.748)	(3.500)	(4.866)	(8.701)	(25.94)
# of HH head	0.454	-1.263	5.615	17.08*	84.40**
relatives living outside thana	(3.372)	(4.336)	(5.079)	(9.934)	(29.76)
# of HH head's	6.735*	3.272	3.546	0.0264	-42.07
spouse relatives living outside thana	(2.969)	(3.160)	(4.031)	(7.686)	(28.00)
Loans from	38.41	62.02	168.7	396.7	600.4
traditional banks (1=yes)	(51.11)	(65.15)	(109.9)	(216.7)	(805.6)
Loans from informal	19.21	60.90	79.44	213.6*	634.3*
sources (1=yes)	(38.60)	(44.42)	(61.51)	(110.7)	(293.8)
Loans from relatives	73.58**	151.3**	209.5**	325.2**	1331.1**
(1=yes)	(35.31)	(38.75)	(53.42)	(126.7)	(455.4)
Eligibility of HH	-49.77	-10.65	16.54	-59.32	-619.2
(1=yes)	(45.60)	(45.15)	(66.82)	(162.6)	(536.4)
Intercepts					
Second wave of data	78.77**	134.8**	151.3**	252.2**	530.5**
(1=yes)	(26.77)	(28.09)	(36.32)	(56.19)	(190.8)
Overall intercept	357.6**	595.6**	868.7**	1259.6**	2203.1**
	(68.39)	(78.74)	(105.9)	(199.4)	(765.1)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Table L.6 **Panel quantile regressions with household correlated random effects with penalised village effects, household non-food expenditure, by gender**

	Quantile				
	10%	30%	Median	70%	90%
Female microcredit	0.007**	0.007**	0.010**	0.020**	0.050**
borrowings	(0.002)	(0.002)	(0.002)	(0.004)	(0.014)
Male microcredit	0.006*	0.005**	0.009**	0.013**	0.018
borrowings	(0.003)	(0.003)	(0.005)	(0.007)	(0.021)
Household characteristics					
Education of HH head	-11.96	-9.498	-8.998	-33.93	23.37
	(8.883)	(9.881)	(12.73)	(27.53)	(95.83)
Age of HH head	1.673	-0.0338	0.843	0.354	0.590
	(2.157)	(1.962)	(2.346)	(3.924)	(10.57)
Gender of HH head	19.16	24.93	-54.37	-170.2	-957.8**
	(82.84)	(71.47)	(89.71)	(149.2)	(557.2)
Highest education of	18.12**	25.77**	30.80**	48.40**	103.2
men in HH	(6.778)	(6.749)	(10.34)	(20.84)	(59.90)
Highest education of	12.04**	31.94**	31.27**	57.96**	114.9**
women in HH	(5.572)	(7.626)	(8.812)	(21.57)	(64.00)
Landholdings	-0.410*	-0.367	-0.294	0.121	-0.296
	(0.143)	(0.214)	(0.372)	(0.708)	(2.546)
HH economic	-16.83**	-23.84**	-35.50**	-47.96**	-109.4*
dependency ratio	(6.803)	(6.510)	(8.956)	(17.15)	(50.54)
# of HH head	14.52**	11.01**	8.688	11.06	17.95
relatives owning land	(4.159)	(4.714)	(6.367)	(11.04)	(35.73)
# of HH head's	3.743	4.490	8.315	-0.380	16.91
spouse relatives owning land	(2.777)	(3.475)	(4.934)	(8.814)	(26.17)
# of HH head	-0.305	-0.178	4.459	14.58*	71.53**
relatives living outside thana	(3.483)	(4.356)	(5.098)	(10.24)	(29.00)
# of HH head's	6.826*	2.779	2.743	-1.490	-29.37
spouse relatives living outside thana	(2.994)	(3.167)	(4.055)	(7.446)	(27.81)
Loans from	47.65	61.94	193.2	285.9	713.6
traditional banks (1=yes)	(51.89)	(64.98)	(111.6)	(215.2)	(818.7)
Loans from informal	33.08	56.97	88.35	209.7*	703.4*
sources (1=yes)	(38.41)	(44.66)	(62.34)	(110.9)	(311.2)
Loans from relatives	71.72**	141.1**	213.6**	327.1**	1315.1**
(1=yes)	(34.72)	(39.20)	(53.93)	(128.7)	(461.6)
Eligibility of HH	-47.63	-7.197	31.09	-88.64	-626.6
(1=yes)	(45.39)	(44.81)	(66.82)	(160.8)	(548.8)
Intercepts					
Second wave of data	81.56**	134.1**	167.4**	257.4**	512.1**
(1=yes)	(26.48)	(28.06)	(36.68)	(58.01)	(193.1)
Overall intercept	371.1**	596.8**	851.5**	1182.2**	2109.9**
	(67.88)	(78.64)	(106.4)	(198.6)	(783.6)

Note: Standard errors in parentheses are obtained via 999 bootstrap replications. Significance assessed based on bootstrap percentile confidence intervals: ** 5% level; * 10% level.

Appendix M

Impact estimates of microcredit borrowings on fractional educational outcomes

Table M.1 **Impact of microcredit borrowings on children education for borrowing households, household-level outcome (fractional)**

	Proportion of girls age 5-18 in HH currently enrolled		Proportion of boys age 5-18 in HH currently enrolled	
	Pooled fractional probit with CRE	**OLS FE**	**Pooled fractional probit with CRE**	**OLS FE**
Household microcredit	0.025	0.011	0.042	-0.008
	(0.700)	(0.817)	(0.844)	(0.579)
Women's microcredit	0.034	0.011	0.058	-0.004
	(0.913)	(0.784)	(1.078)	(0.230)
Men's microcredit	-0.038	0.008	-0.084	-0.036
	(0.370)	(0.254)	(1.084)	(1.395)
Village covariates	yes	yes	yes	yes
Village dummy variables	no	yes	no	yes
Number of observations	1,500	1,500	1,566	1,566

Note: Estimated effects of microcredit on school enrolment of children age 5-18. Outcome variable is fractional. We use pooled fractional probit regressions with household-level correlated random effects (CRE) and village covariates on a balanced sub-sample. 'OLS FE' stands for OLS with household fixed effects. Actual coefficient estimates are very small in magnitude, so tables display coefficients multiplied by 10,000. Absolute robust t-statistics in parentheses clustered at the household level. Significance levels: *** 1%; ** 5%; * 10%.

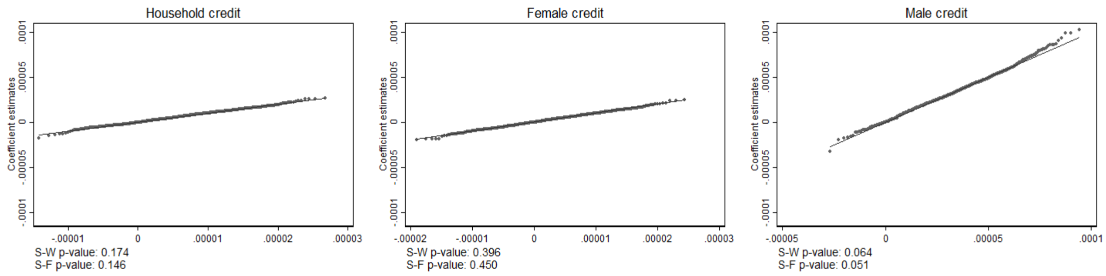
Appendix N

Quantile-Normal plots and Normality tests for bootstrap estimates from pooled probit regressions

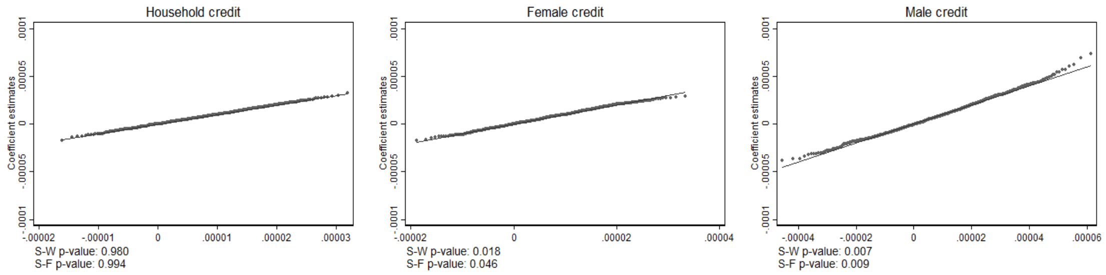
Figure N.1 **Normality of bootstrap coefficient estimates on credit variables from pooled probit regressions with endogenous regressors**

(a) Panel A: Binary outcome

Dependent variable: girl in school (1=yes)

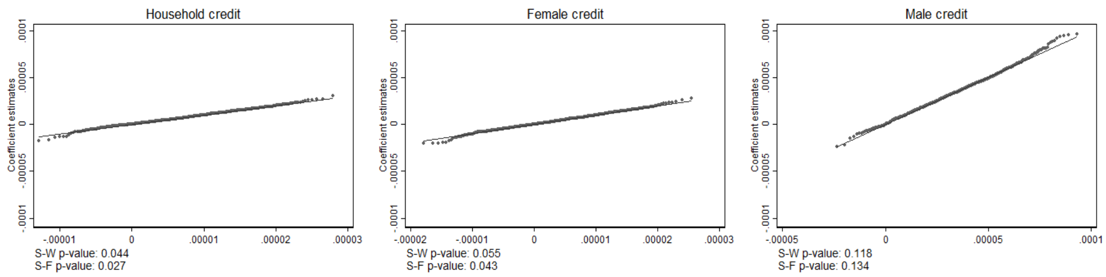


Dependent variable: boy in school (1=yes)

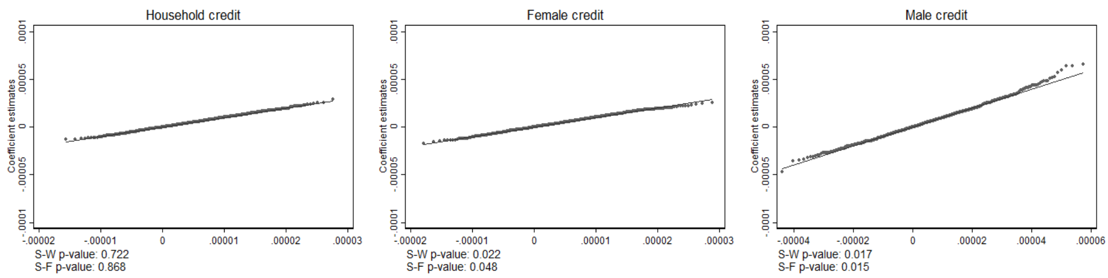


(b) Panel B: Fractional outcome

Dependent variable: proportion of girls in school



Dependent variable: proportion of boys in school



Note: Quantile-Normal plots of coefficient estimates on credit variables from pooled probit regressions with endogenous regressors, following a control function approach with 1,000 bootstrap replications. Quantiles of bootstrap coefficients (y-axis) are plotted against quantiles of the standard Normal distribution (x-axis). Below each plot are reported p-values from Shapiro-Wilk (S-W) and Shapiro-Francia (S-F) normality tests. A p-value below conventional significance levels suggests a rejection of the null hypothesis of normality.

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