Econometric Analyses of Microfinance Credit Group Formation, Contractual Risks and Welfare Impacts in Northern Ethiopia

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Thesis

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To my parents

&

Atsede Tesfay

(1951– 1998)

ABSTRACT

Lack of access to credit is a key obstacle for economic development in poor countries. The underlying problem is related to information assymetry combined with the poor's lack of collateral to pledge. New mechanisms in microfinance offer ways to deal with this problem without resorting to collateral requirements. The objective of this thesis is to examine the mechanisms of providing credit through microfinance and assess the long-run borrowing effects on household welfare in Ethiopia. The Ethiopian environment provides a suitable setting to examine these issues. To meet this objective, two uniqiue data sets - a five-wave panel data on 400 and a cross-sectional data on 201 households - from northern Ethiopia are used.

Borrowing decision is first conceptualized using a dynamic stochastic theoretical framework. Two types of risks involved in joint liability lending are incorporated, i.e., risk of partner failure and risk of losing future access to credit. Empirical analysis using recent dynamic panel data probit techniques show that these contractual risks indeed impede participation in borrowing. The impediment is higher for the new than repeat participants. Second, group formation is analyzed within the framework of alternative microeconomic theories of joint liability where the commonly held hypothesis that groups formed are homogeneous in risk profiles is tested. Empirical results reject this hypothesis indicating that the formation of heterogeneous risk profiles is an inherent feature in group formation and repayment. In fact, there is evidence that borrowers take advantage of established informal credit and saving, and other social networks, which also suggests that group formation outcomes vary depending on underlying socioeconomic contexts.

Third, the impact of long-term borrowing on household welfare is assessed from the dimension of *intensity* and *timing* of participation in borrowing. Panel data covering relatively long period enabled to account for duration and timing concerns in program evaluation. Recent parametric and semi-parametric panel data techniques are innovatively employed to mitigate participation selection biases. Results from both approaches indicate that borrowing has increased household welfare significantly: the earlier and more frequent the participation the higher the impact partly due to lasting effects of credit. This also suggests that impact studies that are based on a single-shot observation of outcomes and that do not account for the timing and duration of participation may underestimate microfinance credit impacts.

Key words

Microfinance, joint liability, contractual risk, group formation, risk-matching, impact evaluation, Panel data econometrics, dynamic panel probit, trend models, fixed-effects, composite counterfactuals, propensity score matching, farm households, Ethiopia.

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Guush Berhane Tesfay,
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CHAPTER 1

INTRODUCTION

1.1 Background

The role of finance in economic development has got considerable attention in the last four decades. Significant progress is made both in understanding and designing sound financial policies in the context of developing economies' financial markets following the seminal contributions by McKinnon (1973) and Shaw (1973); and parallel developments in theories of incentives (Stiglitz, 1974) and information asymmetry (Akerlof, 1970, Stiglitz and Weiss, 1981). An important stride was the inclusion of (informal) rural financial markets, which were otherwise considered as *fragmented* and *unbankable* (Hoff et al., 1993), into the domain of formal financial intermediation (Dichter, 2007: 1-6).

With the emphasis given to poverty alleviation in early 1970s, the overriding policy interest was to fill a presumed 'gap' between the demand for and supply of savings of the poor. Efforts were made to fill this gap by supplying subsidized credit to the 'needy poor', particularly small farmers, through specialized state-owned banks, which were later deemed as 'disasters' in the literature (e.g., Adams, Graham, and von Pischke, 1984; Morduch, 1999). The failure of these state-owned banks coupled with the implementation of the structural adjustment programs in many developing countries dramatically reduced state intervention in rural financial markets in the 1980s (Conning and Udry, 2005). These experiences and other parallel innovations, most notably the beginning of the Grameen Bank in Bangladesh, have led to a paradigm shift in the organization of providing finance to the poor, eventually giving birth to microfinance institutions (MFIs), reminiscent of their predecessors of mid-19th century European credit cooperatives (Guinnane, 1994; Ghatak and Guinnane, 1999). The new paradigm focused on problems of high transaction costs and risks of administering small and fragmented loans for collateral poor borrowers living in environments that are characterized by information asymmetry and weak enforcement mechanisms (Stiglitz and Weiss, 1981). It also recognized the need to build institutions founded on three, sometimes conflicting, policy pillars of what is now termed the microfinance revolution, viz. financial sustainability, outreach and impact on poverty (Zeller and Meyer, 2002: 3).

What is special in MFIs is that they use unconventional methods to deal with the problems of transaction costs, risk and loan enforcements. Particularly, by providing loans to groups in which all members are jointly liable, MFIs exploit social relationships and trust among local people to enforce repayments. Besides, the joint liability element generates individual incentives to screen (mitigating adverse selection) (e.g., Varian, 1990), monitor each other (mitigating moral hazard) (e.g., Stiglitz, 1990), and enforce repayments. The 'stick' of joint liability is often combined with a 'carrot' of repeat-loans, often termed dynamic incentives. MFIs therefore rely heavily on the promise of repeat-loans for those repaying and denying future access to those that do not (Besley, 1995: 2187; Morduch, 1999). The entire group, sometimes the entire village, is banned from future loans if one or more of group members fail to repay, although there is often a distinction between strategic (*free-riding*) and non-strategic repayment failures. The use of social relationships and trust to mitigate information and cost problems without having to depend on collateral is considered a 'win-win' solution to banking problems with welfare maximizing advantages over standard individual lending methods (e.g., Morduch, 1999; Ghatak, 2000)

The microfinance revolution has thus come with a lot of enthusiasm among advocates from around the world. Optimists see innovations in microfinance as powerful tools to help eradicate poverty, without much dependence on subsidy. Considerable amounts of donors', investors', as well as governments' resources are thus devoted to microfinance aiming at the broader objective of encouraging asset accumulation and reducing vulnerability of poor entrepreneurs, and eventually extricate them out of poverty. Skeptics are however wary of disincentive effects of risk and cost shifts onto poor clients; and see a trade-off between 'sustaining MFIs on business terms' and 'meeting the social objective of reducing poverty without subsidy'. Indeed, there are evidences of "mission drift" in microfinance due to the trade-off between profitability and serving the poorest population segments (Cull, Demirgúç-Kunt, and Morduch, 2007). A recent theoretical paper by Armendáriz de Aghion and Szafarz (2009) argues that the increased focus on commercialization in microfinance tends to deviate it from the original poverty reduction and women-focused missions.

MFIs have also attracted substantial academic interest on a wide range of issues. Most research focuses on theoretical explanations as to how microfinance innovations brought notable successes. There is indeed some evidence of success to celebrate (e.g. see Hermes and Lensink, 2007). However, more than three decades after MFIs came to being, there are still unresolved issues, mostly empirical. In general, while the debate on how to resolve conflicts among the pillar objectives remains unsettled, much effort is made on the theoretical fine-tuning of the joint liability theory, with little or no attention to the contexts in which poor borrowers operate. Most theoretical propositions rely on anecdotes of particular MFIs and have not been established as empirical regularities (Morduch, 1999). Thus, whether or not microfinance programs meet the needs of its clients effectively is a central question yet addressed inadequately. Some MFIs have now run for decades. According to the latest microfinance summit report (Daley-Harris, 2009: 1), the number of 'poorest' families served by MFIs globally has reached one hundred million in 2007. If true, this is a commendable achievement with respect to global targets set in previous summits. Unfortunately, these figures tell little about *real* achievements of the presence of MFIs. MFIs therefore merit investigation given the amount of resources and enthusiasm devoted to them.

Of particular interest in this thesis is the long-term impact of microfinance credit on poverty reduction. Microfinance impact evaluations, like many other social programs, are subject to estimation biases due to inherent characteristics of borrowers' self-selection and MFI program placement. Studies used different methods to overcome these problems and identify impacts attributable to credit (e.g., Pitt and Khandker, 1998; Coleman, 1999; Copestake et al., 2005; Tedeschi, 2008). However, most studies focus on either short-duration exposure to programs and lack sufficient time to capture long-term effects, or even if they cover long periods, they are less focused to the dynamics of borrowing during exposure by simply dealing with before and after comparisons. As a result, as in many other social programs, there is a renewed emphasis for, particularly long-term, program impact evaluations (Savedoff et al., 2006; Karlan and Goldberg, 2007; King and Behrman, 2009).

1.2 Problem statement

MFIs in developing countries, particularly in rural areas, operate in risky environments where livelihood is subject to the vagaries of nature. A natural question is therefore whether or not innovations in microfinance can help expand access to finance in those environments without hampering the sustainability of these institutions and at the same time without deterring the participation of their target groups. A simplistic assumption implicit in microfinance is that borrowers demand credit at any cost. One crucial aspect of group lending is the transfer of borrowing risks and costs from lenders to borrowers. It involves the risk of having to repay for a failed partner depending on own success. Moreover, there is the risk of being banned from future borrowings after group repayment failure. This banning threat gives MFIs some leverage to enforce repayments. However, it does so at the cost of magnifying borrowing risks. On the other hand, while it is commonly held in theory that non-strategic failures are bailed out (*limited liability*), MFIs tend to punish non-repayments indiscriminately since sorting out strategic and non-strategic failure is costly in practice. Under these circumstances, joint liability fully transfers lending costs and risks to borrowers. It follows that depending on individual risk aversion, poor households in risky environments (e.g., rain dependent poor producers in Ethiopia) would think twice before they assume the risk and costs of participation in MFI borrowing. This may partly explain the low participation rate of households in those areas despite the enormous amount of finance channeled through MFIs. Morduch (2008) puts it "*we see that even after decades of access to microfinance, loans are still only at 50 per cent in most villages - not anywhere close to the 100 per cent we would think they would be. There is still a lot to learn about why customers take or stay away from our products.*"

Given individual borrowers' decision to participate in group loans, crucial issue in group lending is the group formation process itself and the type of groups that arise. By considering differences in effective borrowing costs faced by risky and safe borrowers, a number of theoretical papers have shown groups formed are risk homogeneous (e.g., Ghatak, 2000). However, others (e.g., Guttman, 2008) have shown homogeneity is not always the case. This is an interesting, often context specific, question for MFIs. It is crucial because the types of groups that arise determine the risks of the borrower pool that emerge and subsequent intra-group relationships. These relationships, which vary with local specificities in turn, determine the overall success of microfinance. Nevertheless, despite the specific complexities in which they operate, most MFIs often choose to replicate the methods experimented by the 'first-wave' MFIs rather than innovating and adapting to their own conditions (Hulme and Mosley, 1996:135). A typical example is the introduction of *Grameen Bank style* innovations, originally implemented in Bangladesh's largely semi-urban petty sector, into rural settings in East Africa (e.g., the Ethiopian MFI where this thesis focuses).

Although adapting MFI services to local needs is a first step towards realizing the intended goals (Zeller and Meyer, 2002:1-7), the bottom line is if access helps improve livelihoods of participants. Evaluating effects of access to microfinance, particularly long-term, is however challenging. First, despite substantial similarities of the mechanisms used in most MFIs, outcomes vary with the diversity of contexts they operate in as well as due to specific features of program implementation. Second, empirical evaluations, particularly long-term impact assessments, require rigorous follow-up of the programs and their clients over time, which is costly and burdens the day-to-day operations of the MFI. Third, even when follow-up data is available, disentangling the effects of microfinance, or specific mechanism from other simultaneous effects is complicated by selection and temporal heterogeneities, which require constructing credible counterfactuals: what would have happened without the program. As a result, despite efforts to quantify effects of programs and their specific mechanisms, empirical evidences are still far from conclusive and lagging behind theoretical understandings. Thus, today, several years after the introduction of the 'new' paradigm, there are still many questions regarding what microfinance does and what it does not.

1.3 Objective of the thesis

The general objective of this thesis is to examine the mechanisms of providing credit through microfinance, viz. group formation, dynamic incentives and effects on borrowing decisions; and assess the long-term borrowing effects on household welfare. The focus is on understanding and empirically investigating the behavioral responses of borrowers to some of the building blocks of the innovative methods in microfinance as well as assessing observed household welfare effects of accessing microfinance loans over a relatively longer period. Four specific objectives deduced from the general objective are to:

i. Examine effects of microfinance contractual risks, viz. risk of partner failure and the threat of being denied access to future loans on households' borrowing decisions in the face of exogenous negative economic shocks that characterize the study area.
ii. Analyze and empirically test the risk matching behavior of borrowers in microfinance group formation and investigate reasons behind this group behavior in the case of Ethiopia.
iii. Estimate the impact of *intensity* (or number of times) of participation in microfinance credit on household welfare.
iv. Evaluate the impact of *timing* of (first-time) participation in microfinance credit on household consumption measured at several intervals after the onset of participation (participation), regardless of number of times of participation.

1.4 Methodological approaches and data

Different theoretical and empirical methodologies are used to meet the specific objectives in this thesis. A panel data set that comes from rural households in Tigray, northern Ethiopia is used to meet the first, third and fourth objectives. Meeting the second objective requires more detailed data on groups and group formation processes which was not available in the panel dataset. A cross-sectional survey uniquely conducted for this purpose on households from the same study area is used. The methods used to meet each objective are described in this section.

The first objective is about households' demand for and decision to participate in borrowing. The household borrowing decision is conceptualized using a microeconomic stochastic dynamic framework where borrowing is one element of households' intertemporal production and consumption decisions. This model takes two types of risks in joint liability borrowing into account: the risk of having to repay for a failing partner and the risk of being banned from future borrowings conditional on group failure. The five-wave panel data is used in the empirical analysis. Risk is first estimated from a hypothetical question included in the survey. A dynamic panel probit model is estimated using the Heckman (1981a) approach to account for initial individual heterogeneity and state-dependence. This method uses recent simulation techniques (i.e., Maximum Simulated Likelihood) to overcome previous computational difficulties of implementing the Heckman approach using standard ML estimation.

To meet the second objective, group formation is analyzed within the framework of alternative microeconomic theories of joint liability, which under static and dynamic household interactions predict different risk matching results. A structural risk model is specified incorporating the simultaneity between choices of own and partners' risks, as well as problems of obtaining preferred partners. The unique data set from the specialized survey conducted in 2003 on a cross-section of 200 borrowing households is used. Risk is estimated from observed indicators for each household, which in turn is used to estimate group risk heterogeneity for each group. The reduced form of the structural model is estimated using Tobit, ordinary least squares and two-stage least squares to account for endogeneity between own risk level and choice of group risk heterogeneity level. Explanations for the resulting group risk matching and implications for repayment are further investigated.

The third and fourth objectives focus on measuring the long-run benefits of participation in MFI credit from two important dimensions. Conceptually, in both objectives, repeated borrowings for production inputs and asset accumulation are assumed to eventually trickle down to improve household welfare over time. Impact is measured on two important household welfare indicators of rural households in Tigray, i.e. yearly consumption (in both objectives) and housing improvements (only in the third objective). In both objectives, recent developments in econometric techniques are employed to identify impact. In the third objective, impact is evaluated from variations in the *level of participation* over time. The four-wave panel data spanning ten years is used to meet this objective[1]. Repeated observations in the long spanning panel enable us to identify impact from the *intensity* or degree of participation as opposed to common methods of comparing participants and non-participants. The standard fixed-effects model, which by default controls for time-invariant unobservables is innovatively modeled to account for time-varying, seasonal and individual trend unobservables. It is also modeled further to allow borrowing to depend on individual trends, and again more flexibly to account for the number of times a borrower participated over time. An advantage of these models is that they enable to identify impact from participation levels over time while controlling for both time-invariant and time-varying individual specific unobservables.

The fourth objective is to evaluate impact of timing of (first-time) participation on consumption measured at several intervals after the onset of participation, regardless of number of times of participation. The aim is to evaluate differential impacts on batches of participants due to the timing of participation. Of particular interest is the effect of MFI credit on early versus late participants, particularly in the face of economic shock years such as droughts in 2000 and 2003 and the implications to reduce vulnerability after the shocks. Impact is thus assessed relative to onset of first-time participation, regardless of repeat-participation thereafter. Identifying the causal impact in this setup requires a non-standard conceptualization of counterfactuals because the outcome (i.e., consumption) is measured in several periods subsequent to a single treatment (i.e., participation). To analyze this problem a recent semi-parametric approach is used that establishes counterfactuals by accounting for initial differences as well as ‘potential future paths of participants had they not participated at a particular time’. The propensity score matching method is used to construct a composite of these future counterfactuals for each participant batch from a set of nonparticipants up to the

[1] Note that one of the available five-waves is not used in the last two objectives because it did not include welfare indicators needed in both studies.

onset of participation. This method is also compared against a simple pair wise effect of participation to get insight into the bias reduction due to the new method.

1.5 Outline of the thesis

Together with this introductory chapter, this thesis contains seven chapters. Chapters 3 to 6 were originally prepared as individual articles and are (to be) submitted to scientific journals. Chapter 4 is already published. As a result, some data description overlaps are possible. This section provides a brief outline of what is in each of the six chapters that follow.

Chapter two provides the background for the rest of the chapters. It frames the setting of the study environment. It provides the reader background information regarding when, where and how the study is undertaken. The biophysical and socioeconomic environment of the study sites, the operational specifics of the microfinance studied, and the sampling and data collection procedures are briefly described. Chapter three extends the discussion on the theory of joint liability lending, mimicking the practice in Ethiopia, and examining it as one-decision component of a rural household unit rather than a single decision of a 'profit-maximizing agent'. It thus provides first evidence as to what extent these new contractual methods limit borrowing.

Chapter four is about the group formation processes in group lending. It explores what kind of group is expected out of a pool of people who voluntarily form borrowing groups and become jointly liable, and what comes out in practice and why. It also discusses the implications of the outcome for the success of group lending itself. Chapter five and six deal with the rather intriguing question of long-term impact evaluation in microfinance. Not many studies are done yet to evaluate long-term effects of MFI borrowings. The fifth chapter deals with this issue. Besides, classical impact evaluation methods have little to offer when the treatment is long lasting and non-reversible as in the case of credit. Under such conditions, selection bias problems are complicated with temporal effects and dynamics of borrowing behavior. Chapter six combines parametric and nonparametric methods to overcome these problems. Finally, chapter seven presents the key conclusions of this thesis and gives implications for the operation of microfinance in Ethiopia and other similar environments. Suggestions for future research are also outlined in this chapter.

CHAPTER 2

THE STUDY AREA, MICROFINANCE IN ETHIOPIA AND DATA USED

2.1 Introduction

To give the reader an overview of the underlying socioeconomic and biophysical contexts in which microfinance operates, this chapter provides a brief description of the prevailing biophysical and socioeconomic conditions specific to the study region. It also describes the national policy environment, particularly, related to rural financial markets. Moreover, to put this study in the perspective of the emerging but delicate Ethiopian microfinance industry, a brief discussion on the evolution and present state of MFIs in Ethiopia is provided. With this background, the institutional structure and operational specifics of the microfinance on which this study focuses is discussed. Finally, in addition to the specific data set descriptions given in each chapter, a general description of the study villages, survey design, and data sets used is presented. This chapter proceeds as follows. Section 2.2 discusses the socioeconomic and policy background; section 2.3 outlines the evolution and state of MFIs in Ethiopia, and the institutional and operational experience of DECSI. A brief description of the survey designs, study villages, and data sets is given in section 2.4.

2.2 Socioeconomic and policy background

This study focuses on the Tigray regional state of Ethiopia. Tigray is the most northern region of Ethiopia, bordered in the north with Eritrea, in the west with the Sudan and in the south and east, respectively, with the *Amhara* and *Afar* regional states of Ethiopian (Figure 2.1). Topographically, the region stretches from the flat lowlands in the northwest to the ragged and mountain plateaus of the northern highlands of Ethiopia (Woldehanna, 2000:14). It covers a total area of 80 thousand square kilometers with 4.314 million inhabitants, 80.5 per cent of them residing in rural areas (Central Statistical Agency of Ethiopia, 2007:19).

Subsistence agriculture is the main stay of the rural population. It includes mainly crop, livestock and mixed farming. Farming systems are characterized by traditional ways of doing things. Labor and animal power is the main inputs in production. Irrigation is limited and production depends on short-season annual rainfall. With the exception of the southern plateau that enjoys additional short rainy season, the *Belg* (March-May), the principal rainy season in this region is the *Kiremt* (June-September) season. This season typically belongs to the monsoon rainy season of the semi-arid, *Sudano-Sahelian* dryland belt of Africa that extends from the west (Atlantic Ocean) to east (Ethiopia and Eritrea), which is characterized by erratic rainfall and recurrent droughts (Segele and Lamb, 2005). Erratic and recurrent droughts mean that subsistence is subject to the variability of nature, which in turn defines the way of life and adaptation to such variability.

Historically, this is one of hardest hit regions in Ethiopia by recurrent droughts. Of the 39 major recorded droughts in the last 200 years in the country that are characterized by food shortages, famines and excess mortality, more than half of them occurred in parts of the country that include this region (Webb, et al., 1992). This was exacerbated by the non-responsive and often discriminatory policies pursued in the country for several decades of the last century (Inquai, 2007: 15-24). Moreover, many of Ethiopia's historical cross-boundary wars (e.g., the 1896 and 1935 Italian invasion), recent civil wars (e.g., the protracted civil war that ended in 1991) and border conflicts (e.g., the 1998-2000 with Eritrea) took place in this region. Coupled with decades of poor governance all of this resulted in environmental and ecological imbalances in the region, which are manifested in degraded lands, poor resource bases, and population pressure, which led to further land fragmentations and mismanagement and hence to an even poorer performance of agriculture, also relative to the national average (Woldehanna, 2000:17-19). Studies indicate that close to 50 per cent of households in the region produced less than their annual food requirements in 1997 and 2000 (Hagos, 2003). In 2005, around 48 per cent of the population of Tigray was unable to meet the basic requirement of consumption (MoFED, 2006)[1].

These problems were further worsened by the unfavorable policy environment, particularly the command economic policy, the country followed in recent history before it shifted to the present market-led macroeconomic policy in 1991. The national policy before 1991 completely neglected subsistence agriculture in favor of large scale urban industrialization. Efforts were hardly made to improve rural infrastructures and complementary inputs, and incentives to promote local innovations were rarely provided. In fact, on the contrary, the command economy followed conscious policy of serving the urban bias at the expense of the rural sector. E.g., farmers were forced to sell their produce at lower than market price levels to government marketing boards, which were responsible for supplying cheap food grains to urban residents (Woldehanna, 2000: 20). There was no room for the private sector to play a role. Critical inputs such as finance were provided by the public sector and were either focused on large scale irrigation projects or simply missing at all (more on this in section 2.3).

For the vast majority of poor households, the main source of finance has been informal finance (Aredo, 1993:15), which includes (i) family and friends, (ii) moneylenders (iii) interest bearing informal micro credit and saving services often based on neighborhood and

[1] MoFED stands for Ministry of Finance and Economic Development of Ethiopia.

market relationships, (iv) non-interest bearing credit and saving associations, mainly *Equb*, a type of Rotating Saving and Credit Association (ROSCA) found in Ethiopia; and *Iddir*, a form of indigenous social insurance where a group of households in a neighborhood or people of similar social or ethnic background contribute money or other resources with the aim of providing assistance at times of difficulty, mainly, during funerals and mourning (Alula, 2000; Dercon et al, 2006).

The shift in economic strategy following the change in government in 1991 brought about fundamental policy changes in the rural economy of the region. The most fundamental is the priority given to agriculture as an initiating engine for growth and development. A national strategy of Agriculture Development Led Industrialization (ADLI) has been designed with broad objectives of increasing agricultural productivity, ensuring food security, promoting commercialization of agriculture and linkages to agro-industry (MEDaC, 1999; FDRE, 2000). Although ADLI has been adopted since 1994, a more comprehensive five-year national plan for Sustainable Development and Poverty Reduction Program (SDPRP) was implemented in the period 2002/03-2004/05 (MoFED, 2006: 1-2)[2]. The SDPRP was designed in consultation with donors' process of Poverty Reduction Strategy Programs (PRSP) and took poverty reduction as core national development agenda. It was built on previous commitments and took ADLI as one of its policy pillars. Other pillars of the SDPRP were the justice system and civil service reforms, decentralization and empowerment, and capacity building in public and private sectors (MoFED, 2002). As in ADLI, SDPRP recognized the need to provide financial services mainly to support the extension program that has already been in progress. The second five-year plan (2005-2010) broadly focuses on Accelerated and Sustained Development to End Poverty (PASDEP), again with ADLI remaining one of the main pillars (MoFED, 2006). The PASDEP is the most pragmatic and detailed policy exercise the country has ever seen. It included a wide range of issues with clear milestones to achieve before the next five-year plan sets in (MoFED, 2006). In this second plan, microfinance has been identified as a key instrument to reduce poverty in both rural and urban areas, in response of which the MFI industry has grown fast in recent years (more on this in the next section).

There has not been a comprehensive study that assesses the overall effects of the policy shift in recent years. There are some evidences from some specialized studies that indicate recent efforts have been able to reverse, or at best deter, the ever deteriorating environmental situation in the region (e.g., Nyssen et al, 2007). The evidence on poverty and growth effects has however been largely mixed, particularly within the period 1991-2003 (e.g., World Bank, 2005: 21-32; MoFED, 2006: 3). It is only in the last five years that the country has seen a steady GDP growth rate of 11.8 per cent per year (FAO/WFP, 2008). One important outcome of the liberalization and shift to agriculture-led macroeconomic policy is the restructuring of

[2] Discussions about SDPRP started since 2000 but was implemented in the last three years of its planned years.

the financial sector, particularly of the rural financial markets, which created a conducive environment to the emergence of microfinance institutions (MFIs) in the country. Many MFIs have been established as a result, each of them operating in specific regions and targeting specific populations. According to a recent FAO/WFO report, one result of farmers' increased access to MFI loans is that it created capacity to retain and stock their grains that would otherwise be dumped at lower post-harvest prices (FAO/WFP, 2008). Obviously, this has changed the landscape of rural financial markets significantly, which merits a discussion here. The following section discusses the state of MFIs in Ethiopia.

2.3 DECSI and microfinance in Ethiopia

Although lending to poor people through NGOs is not new, microfinance in its present form, i.e., providing financial services with business orientation, is a recent phenomenon in Ethiopia. The Dedebit Credit and Saving Institution (DECSI) is one of the pioneer MFIs in Ethiopia. Due to its contribution to the development of other MFIs in the country, the evolution and development of microfinancing in Ethiopia is closely linked to the development of DECSI in Tigray [3]. Understanding the evolution of DECSI therefore helps to understand the characteristics of MFIs in Ethiopia, which in turn is useful to put the present study in a broader national perspective. The following section briefly explains the evolution and state of MFIs in Ethiopia, with special focus to DECSI.

2.3.1 The evolution of microfinance industry in Ethiopia

The earliest microfinance activity in Ethiopia is the pilot Rural Credit Scheme of Tigray (RCST) that started in 1994 by a local NGO, the Relief Society of Tigray (REST)[4]. REST carried out a socio-economic study in 1993 in Tigray that indicated lack of access to credit was one of key obstacles to rehabilitate and develop the war torn region and this led to the establishment of RCST (Borchgrevink et al, 2005:1). Initially, interest was primarily focused on providing credit services to poor 'credit constrained' farmers. Soon, the need to provide other financial services (e.g., saving) was also recognized. Thus, following the legal framework provided by the national proclamation in 1996 (proclamation 40/96), RCST was transformed into a quasi-private 'business oriented' microfinance institution in 1997 and subsequently renamed Dedebit Credit and Saving Institution (DECSI). Nevertheless, REST

[3] For example, DECSI is the first to introduce group based loans to Ethiopia.
[4] Originally, REST is affiliated to the Tigray Peoples'Liberation Front (TPLF) and has been engaged in relief and rehabilitation activities during and after the civil war that led to a regime change in the country in 1991.

remained its major stakeholder, with the rest of stakes held by the regional government, regional youth's, women's and farmer's associations (SOS Faim, 2003). Like many MFIs, DECSI has benefited from international donor funding, particularly at early stages. Among its notable donors are NOVIB (the Netherlands), Norwegian People's Aid (Norway), and SOS Faim (Belgium and Luxembourg). From the outset, DECSI's operational role model was the Grameen Bank in Bangladesh. No surprise, DECSI molded its contractual arrangements and day-to-day operations in Grameen style (more on this in next section). It is important to note from the outset that despite its general orientation towards providing financial services on business grounds, given its historical affiliations as well as vested interests of the stakeholders, DECSI remains with substantial 'social' orientation of reaching the poorest.

In a similar fashion, a number of other microfinance programs which, among others, include the *Sidama* Microfinance Institution, the *Oromia* Credit and Saving share company, the *Amhara* Credit and Saving Institution (ACSI) were soon established in other regions of the country most of which followed DECSI's footsteps, replicating ownership structure, orientation and operational principles. For example, the group lending method is commonly instituted in most MFIs. Ever since, households that were previously 'unbankable' to conventional banks have become within the reach of banking. This is a historical leap, at least, in terms of creating a conducive situation to consider the provision of financial services to poor households within a national policy framework.

As of 2005, about 1.2 million households, of which 38 per cent were women headed, participated in the 26 microfinance institutions operating in the country receiving about 1.5 billion ETB credit. In the same period, they mobilized a total savings of half a billion ETB, a third of the loan amount extended (Wolday, 2006:18). In just a brief period (2001-2005), the industry grew by 263 per cent in terms of number of clients, 479 per cent in volume of loan portfolio and 206 per cent in savings (Wolday, 2006:18). Moreover, with the special focus recently given to the informal micro and small enterprise sector, MFIs mobilize a considerable amount of finance not just in rural but also in the urban and semi-urban areas of the country. Thus, the fast growing pace has been both in terms of number of MFIs and outreach (borrowers and savers). By the end of 2008, the number of registered MFIs has reached 28 and they mobilize a total outstanding loan of ETB 4.7 billion and savings of 1.7 billion ETB. Of these, 12 of them operate in the capital and the rest are engaged in the regional states (National Bank of Ethiopia, 2008:33), five of the latter accounting for 90.5 per cent of total credits extended and 92.6 per cent of total savings mobilized.

Thus, in terms of number of borrowers, despite its late entry, Ethiopia is home to two of largest MFIs in Africa, namely, DECSI and ACSI (Lafourcade et al, 2005). Yet, a vast majority of Ethiopian rural and semi-urban households remain unreached. In fact, according to a recent statistical map of microfinance users, existing rural microfinance institutions are much concentrated in the central highlands of the country (Central Statistical Agency and International Food Policy Research Institute, 2006:29). E.g., according to a national survey in

2005, only 26 per cent of farmers accessed credit nationally (MoFED, 2006: 36). There are hardly any comprehensive and rigorous studies to date on whether and to what extent those that accessed microfinance credit have benefited from the enormous amount of loans extended. The following section describes the operations of one of these MFI on which the present study focuses.

2.3.2 The operations of DECSI in Tigray

As discussed in the historical background, DECSI operates specifically in Tigray. For more than a decade, DECSI has been one of major development partners in the region. Building on its pilot experience and after its official launch in 1997, DECSI quickly expanded its network throughout the region. As an institution, DECSI has been able to build a strong staff and forge robust working relationships with partners at all levels, down to the lowest administration unit, the *Tabia* (Van Esbroeck, 2000:55). Its network of nine branches and 96 sub-branches, headquartered at Mekelle, the capital of the regional state, covers 91 per cent of the *Tabias* in the region (see Figure 2A).

DECSI's broader objectives include providing (i) credit services to enhance the productivity of small producers, start up capital for entrepreneurs and raise the standard of living of clients and their families; (ii) saving facilities and raise awareness on financial disciplining in the region; and (iii) employment opportunities by expanding DECSI's networks throughout the region, thereby attain its financial as well as institutional sustainability as a microfinance. Despite these broader objectives, at early stages, DECSI was preoccupied with credit provision in rural areas (SOS Faim, 2000). Thus, for example, other financial services such as mobilizing savings were given less priority and limited to compulsory deposits linked to credit. Moreover, DECSI's services were focused to rural areas and rural towns. Eligible to DECSI's credit are poor households with 'potential to use credit for production purposes'. There are no consumption loans and in principle loan diversion is not allowed. Since the aim is to help poor households to boost production and secure food, and step by step strengthen the marketability of production in the region, poor but credit constrained "productive" households are considered as main targets of the program. However, there is no mechanism by which these households are targeted; there is no clearly defined selection criterion (Borchgrevink et al, 2003). Women are however treated separately and given positive discrimination by allowing them to form women groups. In 2001, for example, 39 per cent of loans were extended to women headed families (SOS Faim, 2003)

Considering the difficulty for potential borrowers to secure collateral given the inherent poverty in the region and taking lessons from other institutions, mainly from the Grameen bank, DECSI designed group based loans. Potential borrowers are required to form groups of

3-7 members from which the screening committee selects[5]. Studies indicate that this committee appears very unlikely to reject a client and most screening takes place during group formation (e.g., Borchgrevink et al, 2003). Approved loans are given to individual members for which the group remains responsible. Failure to repay one or more of the group loans is punishable by denying future loans, which some times include the village. Partial repayment is not allowed and at times group members that can repay their individual share but unable to help their partners are forced to incur additional costs of keeping the principal. In fact, a special case in DECSI is that not only willingness but even inability to repay is harshly punishable. DECSI takes advantage of its historical affiliations and synergy with local administrators to implement a strict enforcement mechanism. This partly explains the exceptionally high repayment rates in the institution (Borchgrevink et al, 2003). Interest rate ranges between 12 and 15 per cent per year of outstanding loans. This is low compared to the ten times higher interest rate moneylenders ask in the area (Woldehanna and Oskam, 2002). Repayment periods vary between six to twelve months, depending on the type of activity for which a loan is extended. The maximum loan size for DECSI's standard loan types reaches up to ETB 5000 and Average loan size ranges between ETB 500 to 1000, during the study periods (Woldehanna, 2005: 240).

From the beginning, two types of loan products are provided, namely, the regular loans and agricultural input loans. Regular loans are targeted at income generating farm and off-farm activities (e.g., agriculture, trade, handcrafts, and services). These are basically group based loans and accounted for the lion's share of DECSI's loan portfolio. Agricultural input loans are given synchronized with the extension program, mainly for fertilizer, pesticides, improved and selected seeds. Since they are packaged with inputs provided by the extension program, agricultural input loans are administered in coordination with the bureau of agriculture. Agricultural input loans often accounted for smaller share of DECSI's loan portfolio (e.g., only 7 per cent in 2001) and declined over the years both in relative as well as absolute terms (Woldehanna, 2005:241).

DECSI has introduced two other loan products, namely, Micro and Small Enterprise (MSE) and 'household package' loans in 2003. MSE loans target at small entrepreneurs in urban areas and are given individually with some collateral arrangements. Household package loans are also given individually but with some collateral arrangements from the government. Moreover, the selection and enforcement mechanism are hardly within the premises of DECSI (Borchgrevink et al, 2005:87) and are therefore totally different from standard individual loans[6]. Note that borrowers of the old programs are also allowed to shift to the package program but only after settling down all debts from old loans.

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[5] In later years the group size was lowered to 3-5. The screening committee consists of branch credit officers and a *tabia* or community leader.

[6] Package loans are tied to specific activities (mainly, dairy farming, poultry, horticulture, and beekeeping) aimed at achieving household food security in the region. This is an integrated recent program run by the

In general, DECSI has excelled in terms of reaching a big number of households in its short period of existence. For example, in just two years after its official launch, over 210,000 households (6.6 per cent of the total population) accessed DECSI's credit with 1.4 million credit transactions amounting to ETB 447 million. The volume of loan increased each year until it slowed down in 1999 due to the border conflict with Eritrea (SOS Faim, 2000). However, the momentum quickly resumed afterwards, with the number of households that accessed credit reaching 423,830 in 2006 (See also table 3A.1 in chapter three). This trend however tells little about the overall borrower profiles and consistency of borrowing over the years. A preliminary evaluation of DECSI in 2000, for example, indicated that there was high borrower turnover every year. Although many households saw opportunities that could be exploited using credit and continued to borrow, still about 12 per cent dropped out each year and the study called for a more rigorous study of this issue (SOS Faim, 2000). A similar study in 2002 estimated the total number of clients that abandoned DECSI up to that year at 100,000-150,000 (SOS Faim, 2003). A major reason identified by the study is that most dropouts belong to those that "stepped back because they become aware of the risk; because of the difficulties using credit for productive purposes; or the disintegration of their group". However, this study, though qualitatively, finds improvement in the lives of participant families and attributes it to number of years they have been clients.

Apart from credit provision, DECSI has also recently introduced other financial products, mainly money transfer, pension payment, and deposit mobilization (savings). Money transfer and pension payments are two important services DECSI provides at each branch. Next to credit, deposit mobilization is the second most important financial product of DECSI. There are two types of deposits in DECSI: the compulsory deposits which credit group members have to save monthly and voluntary deposits which both regular loan clients and the public at large save. DECSI provides a deposit rate of 3 per cent. Its deposit portfolio has grown rapidly. This is one of DECSI's remarkable successes in recent years (Woldehanna, 2005:241). By 1999, it mobilized about ETB 74 million deposits of which 47 per cent of came from its credit clients, which included their compulsory savings (Van Esbroeck, 2000). By the end of 2001, the amount of deposits exceeded the total amount of loans disbursed in the same year, of which voluntary savings accounted for 75 per cent (DECSI, 2002b). The number of active deposit clients reached more than 160,000, which is close to half of overall active regular credit clients in 2006 (Mix Market, 2007).

In sum, although hardly quantitatively rigorous, existing studies (e.g., Meehan, 2000:109-111, SOS Faim, 2000; 2003; Borchgrevink et al, 2005, Woldehanna, 2005:236, Hagos, 2002:11) are positive about DECSI's contributions to the regional as well as overall economy. The improvements in the lives of its credit clients, e.g., as narrated by many success stories (Borchgrevink et al, 2005: 20-21), its ability to utilize local institutions in program

regional government offices in which DECSI provides credit that is subsidized and guaranteed by the regional government.

implementation, its ability to build reputation within society and mobilize local savings, and its ability to achieve a high level of efficiency and financial self-sufficiency (Van Esbroeck, 2000), and its overall rapid expansion in the region are some of the success indicators often mentioned.

2.4 The study villages and description of the data

This study uses two separate household level data sets, i.e., a panel data and a cross-sectional data surveyed at different times from different villages in Tigray. The cross-sectional data set is specifically used in chapter four. The rest of the chapters used the panel data set. This section briefly describes the design of each survey and the data sets.

2.4.1 The panel data set

The panel data set comes from a sub-sample of a bigger household survey that initially covered 100 villages[7] in Tigray. Four of the five administrative zones - Southern, Eastern, Central, and Northwestern- that cover most of the highlands of Tigray are included in this study[8]. This comprises eleven *Woredas* (districts) (see table 2.1 and Figure 2.1) where a DECSI branch is located to serve the villages in its premises. Sixteen villages are sampled from each zone. The survey was conducted in five rounds (1997, 2000, 2003, 2005, and 2006). Efforts were made to keep the seasonal comparability among rounds. To achieve better representation, sampling was done at two stages. First, stratified by altitude (mainly highlands), agricultural potential, population density, and access to infrastructure (mainly market, credit, and irrigation), four *Tabias* were selected from each zone. A *tabia* contains a group of villages. One village is selected from each sample *Tabia.*

Second, a total of 400 borrower and non-borrower households, 25 from each village were randomly selected from the village list. Table 2.1 presents a list of sample villages and their key characteristics by zone. A standardized household questionnaire that assesses household on-farm and off-farm income, consumption expenditure, housing and assets, credit and saving information and access to infrastructure was administered.

[7]The bigger survey was designed by a collaborative research project between Mekelle university, Ethiopia, the International Food Policy Research Institute (IFPRI), the International Livestock Research Institute (ILRI), funded by Norwegian Research Council, Norway. Building on this, follow-up surveys were run by author of this thesis as well as other researchers from Mekelle university and Norwegian University of life sciences.

[8]During the survey design, the present Northwestern and Western zones comprised one zone, and were represented by the four villages currently in Northwestern.

Table 2.1 Sample zones, villages and their key characteristics

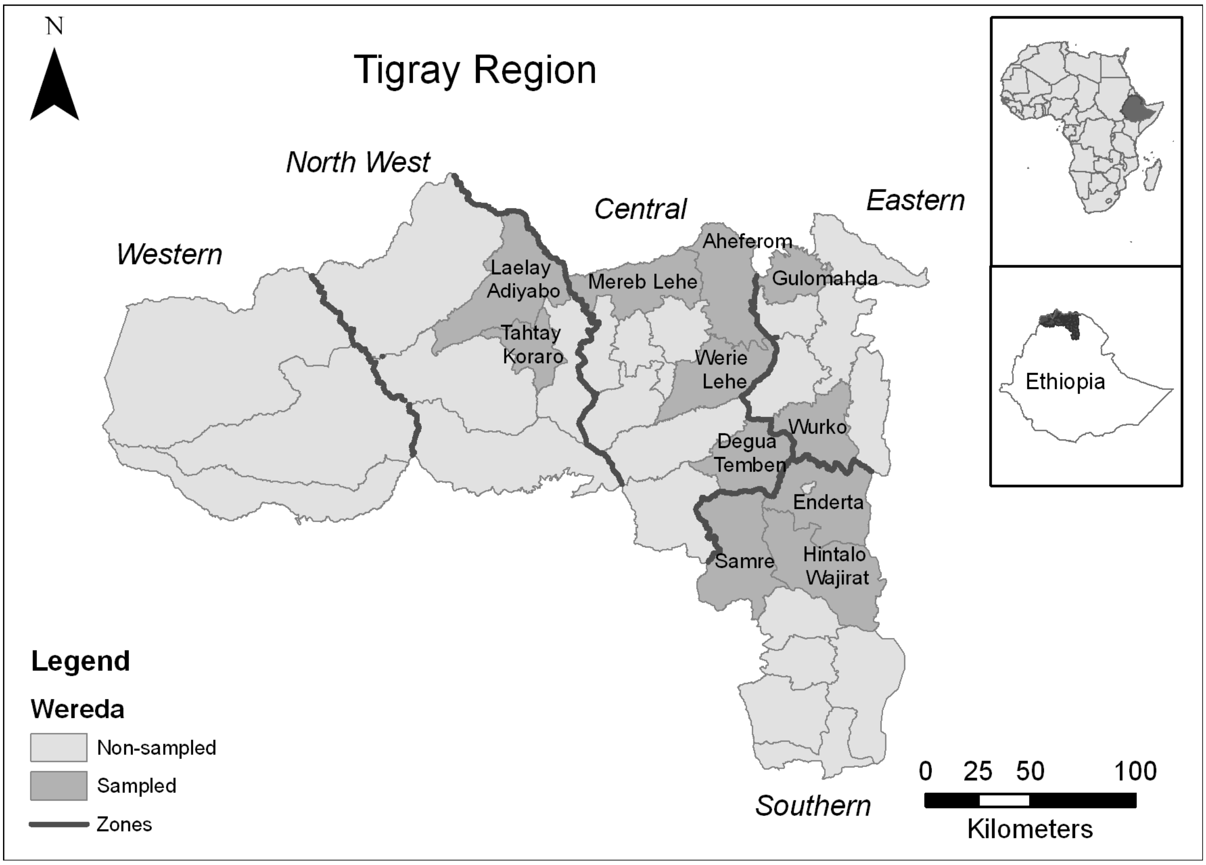
Zone	*Village (Tabia)*	*(Woreda) Branch name (see Figure 2.1)*	*Access to Woreda town/ market (yes if <10 km)*	*Access to irrigation*
Southern	*Hintalo*	*Hintalo-Wajerat*	No	Yes
	May-alem	*Enderta*	Yes	No
	Mahbere-genet	*Enderta*	Yes	Yes
	Samre	*Samre*	Yes	No
Eastern	*Kihen*	*Wukro*	No	No
	Genfel	*Wukro*	Yes	Yes
	Embasmena	*Wukro*	Yes	No
	Hagere-selam	*Gulo-mekeda*	No	No
Central	*Seret*	*Degua-tembien*	No	No
	Dibdibo	*Werie_leke (Enticho)*	Yes	No
	May-keyahti	*Ahferom*	No	No
	Addis-alem	*Mereb-lekhe*	No	No
North Western	*Hadegti*	*Laelay-Adiabo*	Yes	No
	Tsaéda-ambora	*Laelay-Adiabo*	No	No
	May-adrasha	*Tahtay-koraro*	Yes	Yes
	Adi-menabir	*Tahtay-koraro*	No	No

Geographically, this sampling covers most of the densely populated highlands (1500 meters above sea level) and hence credit provision is widely distributed (See Figure 2A) parts of the region. The western and southern lowlands that are less densely populated but are endowed with relatively better land resources and have unique climate are not included in this study. Note that the 2005 data is used only in chapter 3, because it lacked some information required in the rest of the chapters. In addition to the standardized questions, the 2006 survey included additional questions on the household relationship with DECSI and other sources of credit.

2.4.2 The cross-sectional data set

The cross-sectional survey was conducted in the summer of 2003 to study[9], specifically, the process and outcomes of group formation in chapter four. Again, sampling was done at two levels. First, based on their proximity to town markets, roads, and agro-ecology, six out of the 96 sub-branches of DECSI were selected from five *woredas*. This distribution roughly represents the sub-branches in the highlands with relatively immobile and more densely populated compared to the low lands. These are two important elements in group formation.

[9] This survey was conducted by the author of this thesis as part of his M.Sc. thesis work.



Source: Extracted from the International Livestock Research Institute (ILRI, 2007) GIS data (shape file) for Ethiopia

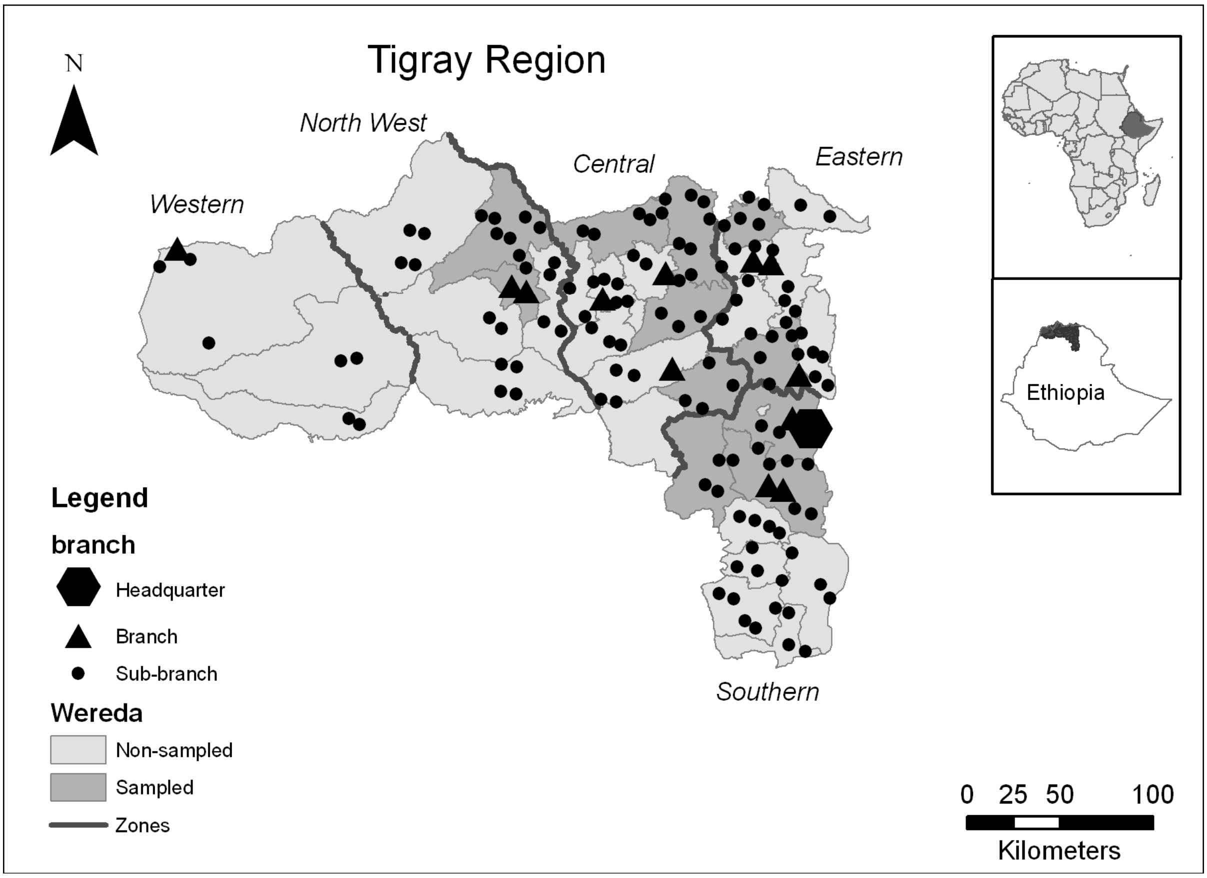
Figure 2.1 Study sites in Tigray (see legend).

The sample *woredas* also represent the relatively active economic activity areas in the region. Second, a total of 201 households that make up 57 randomly selected groups were surveyed. The number of households surveyed by sub-branch, *Woreda* and zones are given in table 2.2. A specialized structured household questionnaire that included household characteristics, main sources of income, assets, credit and saving history, group formation (e.g., screening, monitoring and enforcement) and social ties was administered. Besides, respondents were asked if they participated in local networks (e.g., Equb, Iddier, and religious gatherings) before and after the credit group formation. Open ended questions were included to accommodate unanticipated and broader responses. Moreover, discussion with key (client) informants, branch and sub-branch officers of DECSI was part of the survey, which gave useful insights into the overall social processes related to credit group formation. Summary statistics of key household characteristics are presented in chapter four.

Table 2.2 Distribution of sample households by DECSI sub-branch

Zone	*Woreda*	*Sub-branch name (see Figure 2.1)*	*Number of households*
Mekelle	*Debub Mekelle*	*Mekelle*	21
Southern	*Hintalo Wajerat*	*Adi-gudom*	26
		Hiwane	41
	Samre	*Samre*	34
Eastern	*Wukro*	*Agulae (close to Wukro)*	39
Central	*Degua Tembien*	*Hagereselam*	40
		Total	201

Appendix 2A



Source: Extracted from the International Livestock Research Institute (ILRI, 2007) GIS data (shape file) for Ethiopia

Figure 2A The distribution of DECSI's Branch (triangles) and Sub-branch (circles) offices

CHAPTER 3

JOINT LIABILITY BORROWING DECISIONS UNDER RISK: EMPIRICAL EVIDENCE FROM RURAL MICROFINANCE IN ETHIOPIA

Abstract: *This chapter investigates borrowing decisions of rural households from a microfinance institution in Tigray, Ethiopia, using household panel data and a dynamic panel probit model. The theoretical model takes two types of risks involved in joint liability lending explicitly into account: risk of partner failure and risk of losing future access to credit. Empirical results show that these risks are important in explaining borrowing decisions. Another finding is that the probability of repeat-borrowing is higher than the probability of new participation. This could imply that perceived joint liability threats deter participation and easing stringent punishments might help poor households' access to credit.*

Key words: *Microfinance, joint liability, risk, dynamic panel probit, maximum simulated likelihood, Ethiopia*

3.1 Introduction

The microfinance revolution is hailed for its innovative approaches that mitigate classical incentive and cost problems in the provision of credit (Ahlin and Jiang, 2007). A celebrated approach in microfinance is the joint liability contract, often combined with dynamic incentives (e.g. Ghatak and Guinnane, 1999). Joint liability requires borrowers to form self-selected groups in which all members are liable for all group members' loans. In most microfinance institutions (MFIs), this is combined with the dynamic incentive of rewarding successful borrowers with subsequent loans and denying future access to *strategic* defaulters (for example, Armendáriz de Aghion, 1999). This contractual design assumes a long-term relationship between borrowers and MFIs on which enforcement relies. However, since interest in MFIs often lies in building sustainable financial institutions, the focus has been mostly on refining these contractual designs that expedite institution building rather than realizing effective participation in specific borrower environments. In most cases, joint liability designs are replicated with little or no flexibility to specific borrower interests and given socio-economic and environmental conditions (Wright, 2001). In a similar fashion, much of the theoretical microfinance literature is devoted to fine-tuning of how and when such contractual designs can help discipline borrowers.

In many theoretical models (e.g. Armendáriz de Aghion and Gollier, 2000), the potentials for credit market efficiency and aggregate welfare gains due to the new MFI approaches are shown to exceed gains from standard lending methods, often under static incentive and participation constraints (Ahlin and Jiang, 2007). An important theoretical assumption is that borrowers are risk-neutral and insensitive to future income variability. This implies that risks can be effectively transferred onto poor borrowers without distorting borrowing incentives. Moreover, *individual* borrower welfare gains from accessing credit at lower interest rates are shown to exceed losses due to joint liability contracts (Ghatak, 1999). This argument has convinced donors and governments that microfinance can help to reduce poverty and as a consequence, there has been an unprecedented flow of resources into world wide microfinance in the last two decades (Morduch, 1999).

Given access to joint liability credit, an important empirical question is however to what extent this microfinance 'promise' can be kept by both lenders and borrowers. While there is some evidence that the mechanisms emphasized in joint liability theory indeed discipline borrowers and improve repayments (Hermes and Lensink, 2007), evidence on whether and how these contractual designs influence borrowing incentives, in particular in risky production environments, remains largely unknown (Armedáriz de Aghion and Morduch, 2005: 99-113). A recent World Bank study reported that despite the unprecedented flow of finance into microfinance, access to and participation in credit by the poor remains unsatisfactory worldwide. This study also emphasized that identifying the barriers that prevent poor households from accessing credit remains crucial to policy making (World Bank, 2008)[1]. At micro-level many studies stress that client-MFI relationships remain unstable everywhere and contractual designs are among the main barriers impeding participation (Hulme and Mosley, 1997; Wright, 2001)[2]. For example, based on a survey from Bangladesh, Evans et al. (1999) identify several (contractual) risk factors as reasons for nonparticipation, resulting in participation by less than a quarter of the eligible households. Diagne and Zeller (2001: xi) claim that most rural households in developing countries continue to rely on the informal credit market despite the increasing credit supply through MFIs. They also conclude from a study in Malawi that access does not matter if the institutional designs do not take into account the conditions under which households operate. Hulme (1999) and Wright (2001) observed high client dropout rates of 25 to 60 per cent per annum for East African MFIs and point to inappropriate product designs as main reasons.

Given such non-participation and dropout rates, a key question is whether certain characteristics of the joint liability lending contract also impede participation. More specifically, can the transfer of 'default risk' from the lender to the borrower and the dynamic incentive of conditioning future credit access on repayment partly explain nonparticipation? This is an empirical question that has not been addressed in the literature thus far. Our study is further motivated by two important observations from fieldwork on a MFI in northern Ethiopia: despite the unprecedented joint liability based credit supply, even at lower than global MFI interest rate (Morduch, 2008) or interest rate set by other sources in the area (e.g., Woldehanna and Oskam, 2002), the majority of 'eligible' households are not happy to participate in joint liability credit and, contrary to the theoretical literature (for example, Ghatak, 2000), the majority of rural households prefer credit on individual basis, even if that implies higher interest rates.

Starting from the standard joint liability model, this chapter conceptualizes borrowing decisions in a dynamic stochastic framework where the contractual risk of joint liability and future access to borrowing are taken explicitly into account. When potential borrowers are risk averse, as is the case for most rural households experiencing shocks, the optimality of these contracts depends on the trade-off between insurance and incentives to borrow. Our conceptual model shows that joint liability contractual designs may no longer be optimal in the absence of insurance mechanisms such as repeat-borrowing to cover consumption ex post and production shortfalls following shocks and when borrowers are over-stretched to repay loans at the cost of their subsistence.

In the empirical analysis a five-wave panel dataset (1997-2006) on 400 rural households in Tigray, Ethiopia is used. Participation decision is modeled in a dynamic random effects

[1] According to the International Poverty Centre, 47.8 per cent of the 193.6 million poor families worldwide in 2006 were within reach of MFIs (World Bank, 2008). For the same year, this figure was only 11.4 per cent for Africa and the Middle East.

[2] Traditionally, providing access to- and not necessarily participation in- credit is seen as a sufficient remedy for the credit-constraint problem. Diagne et al. (2000) discuss the distinction between access to and participation in credit in the context of rural credit markets in developing countries.

probit framework based on Heckman (1981a) that is implemented using recent quadrature estimation techniques (Stewart, 2006). Controlling for effects of observed and unobserved (initial) conditions and state dependence, we find that joint liability risk and household differences in future liquidity are important in explaining nonparticipation. Results also indicate that unobserved household heterogeneity is important in explaining participation but also that participation was state dependent. Moreover, systemic shocks such as rain failure strongly affected participation.

The rest of the chapter is organized as follows. Section 3.2 introduces a joint liability framework that mimics the practice in Tigray. It also conceptualizes the theoretical interplay of the contract with other household decisions. Section 3.3 presents the empirical model and estimation strategy and section 3.4 describes the study area and the data. Results are given in section 3.5. Conclusions and implications are given in section 3.6.

3.2 Joint liability lending and household borrowing decisions

This section consists of two parts. First, contractual risks in the joint liability design, mimicking the practice in Ethiopia are introduced. In the second part these contractual risks are conceptualized within a farm household decision framework.

3.2.1 Joint liability lending

Consider household i with endowment, W_t and access to joint liability credit with other sources of credit limited. Suppose households attach a value v_i to this 'expected borrowing potential' from the MFI, which the latter uses as a leverage to enforce loans. The MFI extends future loans conditional on previous repayment of the group loan. To simplify the household's borrowing decision problem, we do not consider strategic group interactions in repayment and assume that each group member, if able, is fully committed to repay her individual loan obligations. In other words, loan defaults are assumed to be non-strategic[3].

[3] Admittedly, strategic group interactions could matter for individual household decisions. Several theoretical papers have addressed this issue (for example, Besley and Coate, 1995). The focus in this paper is however on the role of borrower-lender related contractual risks under the condition that potential households are 'integrity-safe' to the MFI but success is exogenously determined. This is a crucial element often subsumed in the participation constraint of the joint liability theory. Moreover, in the context of immobile farm households such as in rural Ethiopia where communities lived together for years, intra-group strategic interactions can be theoretically assumed to implicitly be taken care of in the group formation processes. Empirically, this assumption is controlled by including social capital variables such as 'trust' and intra-group interactions.

Ability to repay depends on overall household's liquidity at the due date, which in turn depends on past and present household income and endowments. Income and endowments are however subject to systemic and idiosyncratic risks such as weather, pests, floods, disease or price variability, occurring with probability p_i and affecting the ability to repay. The MFI is however not only imperfectly informed about the borrowers' abilities but also about their intentions to repay and punishes defaults even when they are non-strategic, a threat well perceived by households in the study area[4]. Let this threat of wrong punishment, according to household beliefs, occur with probability τ , $0 \le \tau \le 1$. This introduces an information asymmetry problem into our analysis, which reduces the limited liability often assumed in the literature by a probability τ.

To study the possible outcomes of the contract and their subsequent effects on household income variability, let borrowers i and j form a joint liability contract $ij = (r,m)$ of the form described above, where r is the interest payment and m is a parameter for joint liability payment. The timeline is that first the MFI disburses a loan to the group at season t that, for simplicity, i and j share equally and L_{it} is the share of i to be repaid after harvest when outcomes are realized at $t+1$.

Both i and j can be affected by a shock, determining whether they can repay (S) or not (F), and the outcomes for i and j are assumed to be independent. This leads to four possible states of contract ij , i.e. $\{SS,SF,FS,FF\}$ that occur when both i and j succeed (with probability $p_i p_j$), when i succeeds but j fails (with prob. $p_t(1-p_j)$) and vice-versa and when i and j fail (with prob. $(1-p_i)(1-p_j)$), respectively. Repayment by i, affecting income in the next period, depends on the state of the contract and the resulting group loan repayment:

$$R_{i,t+1} = \begin{cases} (1+r)L_{it} & \text{if } SS \text{ with } (p_i p_j), \text{or if } FF \text{ with } \tau(1-p_i)(1-p_j), \\ \left[(1+r+m)L_{it}\right] & \text{if } SF \text{ with } p_i(1-p_j), \text{or} \\ 0 & \text{if } FS \text{ with } p_j(1-p_i) \end{cases} \quad (1)$$

Two potential contractual outcomes influence i's decision to borrow. First, the state of own success combined with partner's failure (SF) involves extra income risk of $p_i(1-p_j)m$, $m \le L_{jt}(1+r)$. In the standard joint liability theory with risk-neutral borrowers, which predicts the formation of self-selected homogenous groups as in Ghatak (2000), this

[4] According to discussions with key informants in the field, despite well noticed harvest failures, some households are forced to dissolve their assets to repay debts to the MFI. Others claim to have migrated to cities after shocks, hiding from their lender. According to some branch officers, this is done to prevent the precedence of running away in the future.

contractual risk is the opportunity cost 'safe' borrowers would like to forgo in order to access credit at relatively lower interest rate. However, when borrowers are risk averse, this income risk may outweigh the gains from the reduction of interest rate *ex ante* - an increase in *m* for a given decrease in *r* leaves risk averse borrowers worse off (Ghatak, 2000). In other words, potential borrowers could manage risks of joint-liability borrowing *ex ante* by deciding not to participate in group lending, with the foregone interest rate reduction of joint liability lending as a risk premium. This corresponds to the well established idea that small farmers, who are necessarily risk averse because they need to secure their subsistence from current production or face starvation (Lipton and Longhurst, 1989), avoid risky decisions of higher income levels and instead prefer safer decisions that smooth future income (Binswanger and Sillers, 1983; Morduch, 1994). A number of empirical studies (e.g. Rosenzweig and Binswanger, 1993; Morduch, 1995) have found that in marginal environments, *ex ante* income risk management strategies include risk avoidance even if that involves loss of profitable opportunities. A central argument in this chapter is thus, given exogenous shocks, contractual risks of joint liability approaches may limit credit uptake by inducing potential borrowers to manage risk *ex ante*, despite their promise of making credit available at relatively lower rates.

Second, conditional on both being considered a 'strategic' default by the MFI (state *FF* occurring with probability $\tau(1-p_i)(1-p_j)$), both i and j are compelled to liquidate productive resources to repay or else they are denied future access to credit. In other words, even when both borrowers fail to repay due to bad harvests (non-strategic defaults), group members might still be held accountable for their group debt or else lose future access to credit, depending upon τ that reflects the extent of MFIs state verification problems or its desire to punish defaults. Although this indeed might tighten the possibility of strategic defaults, it does so at the expense of introducing the risk of losing access to future borrowing. So, the decision to borrow today involves a risk of losing borrowing in the future. However, to the extent of *j's* success or ability to rescue the group, *i's* access to borrowing is persevered. Note that *i*'s gains (in the bad state) from *j*'s success is essentially an insurance to access future credit. The effect of this second risk element on the decision to borrow therefore depends on the balance between the two.

In sum, while access to credit is often seen as insurance against income variability, the decision to borrow also introduces two types of risks, i.e. the risk of partner failures that may offset the risk of own failure and the risk of loosing future options to borrow. This leads to trade-offs between incentives and insurance. It follows that households operating in risky environments would participate in borrowing only if they expect that the benefits of borrowing more than compensate for the combined negative effects of these risks. This is an interesting trade-off because poor farm households, in order to avoid further destitution, may choose not to borrow and therefore forgo an opportunity to move out of poverty.

3.2.2 Joint liability borrowing decisions under risk

Borrowing decisions have to be considered as part of the full set of decisions that the household makes in each period. Moreover, as discussed in the previous section, borrowing today has implications for income and borrowing options tomorrow. So, household borrowing decisions should be analyzed in a dynamic context. This is discussed in this section.

To conceptualize borrowing decision in a dynamic household decision framework, consider a household *i* that has a planning horizon *T*, choosing levels of consumption, production, and borrowings at *t* where choices are constrained by the possibilities available to the household in each *t*. The decision problem at each *t* can be summarized by assuming the household maximizes the expected value of time-separable utility, *u*(.), derived from consumption:

$$Max\ E_t \sum_{k=0}^{T-t} \beta^k u\left(C_{it+k}\right), \quad k = 0,...,T-t$$

subject to:

$$W_{it+k+1} = (1+r)\left(W_{it+k}\right) - C_{it+k} - I_{it+k} + Y_{it+k} + D_{it+k}, \qquad (2)$$

$$Y_{it+k} = f(\sigma I_{it+k}, \varepsilon_{it+k}) \qquad (3)$$

$$v_{it+k+1} = \rho v_{it+k} \geq 0 \qquad (4)$$

$C_{it+k} \geq \overline{C} > 0$, minimum consumption constraint (5)

$W_{iT} \geq 0$, the transversality condition (6)

where E_t is the expectations operator based on information available at *t*, β is the rate of time preference, abusing notation, for now *r,* is the deposit rate often different from the lending rate, C_{it+k} is consumption at *t+k*, *f*(.) is a concave production function, Y_{it+k} is expected gross household income at *t+k*, I_{it+k} is gross household investment and $\sigma \geq 1$ indicates the productivity increase due to borrowing, which equals one if there is no borrowing. We assume that credit is associated with higher expected yield. Furthermore, ε_{it+k} represents production shocks at period *t+k* and the net debt is defined as $D_{it+k} = L_{it+k} - E(R_{it+k})$ where $E(R_{it+k})$ is the expected repayment obtained from the sum of own and partner joint liability repayment multiplied by their respective probabilities as given in eq.(1). The probability that access to credit is kept is $\rho = 1 - \tau(1 - p_{it+k})(1 - p_{jt+k})$, $0 \leq \rho \leq 1$. Uncertainty enters into the household maximization directly through own as well as partner's income shocks.

The constraints in (2) and (4) provide the state transition functions for household endowment and future access to borrowing, respectively. W_{it+k+1} is defined as initial endowment plus net debt and income minus consumption and input expenditure. Since consumption occurs throughout the year but agricultural income is generated at the end of the year, consumption at $t+k$ depends on initial endowment at $t+k$. In the absence of credit, and given eq. (2), eq. (5) is binding. Production is non-negative and has a minimum input requirement and consumption is at least equal to its subsistence level. Whereas the wealthier poor in bad years can cope by lowering their consumption to a minimum (Zimmerman and Carter, 2003), the poorest households are dictated to deplete their productive endowments to fulfill their subsistence consumption requirements. Borrowing thus enters the household endowment transition so as to relax both production and consumption constraints. While most MFIs are *production-credit* oriented, a typical characteristic of the smallholder economy is that production and consumption decisions are non-separable (for example, Feder et al., 1990) and that *production-credit* is directly or indirectly used to smooth consumption. Borrowing therefore relaxes the household budget through its liquidity effect at $t+k$ as well as by updating the endowment through additional productivity gains from investment at $t+k+1$.

As noted earlier, liquidity and productivity gains of borrowing at $t+k$ come at the risk of sliding down into debt spirals at $t+k+1$ and beyond, mainly due to additional contractual risks in combination with stochastic production. Moreover, to the extent of unlimited liability borrowing at $t+k$ may entail changes in future borrowing status, removing the possibilities of repeat-borrowing for investment as well as ex-post budget smoothing. This makes borrowing a dynamic variable with trade-offs between present liquidity and future endowment and hence consumption. The decision to borrow is therefore evaluated in a stochastic dynamic discrete choice framework that falls into the general family of optimal stopping problems (Adda and Cooper, 2003: 175). With borrowing option Z, the household maximization problem can be summarized in the following Bellman equation where the value function $V\left(W_{it+k}, v_{it+k}; Z_{it+k}\right)$ gives the maximum attainable sum of current and future expected rewards given the current endowment W_{it+k} and expected access to credit v_{it+k}.

$$V\left(W_{it+k}, v_{it+k}; Z_{it+k}\right) = \max_{z \in \{B, NB\}} \left[V^{B}_{it+k}\left(W, v, Z\right), V^{NB}_{it+k}\left(W, v\right)\right] \tag{7}$$

where $V^{B}_{it+k}(.)$ and $V^{NB}_{it+k}(.)$ are expected discounted lifetime utilities with and without borrowing, respectively. After substituting the transition functions (2) and (4) and denoting gross income with and without borrowing by Y and Y' these can be written as:

$$\begin{aligned} V^{B}_{it+k}\left(W, v; Z\right) &= u\left(C_{it+k}, v_{it+k}; Z_{it+k}\right) + \beta E_t V_{it+k+1}\left(W_{it+k+1}, v_{it+k+1}\right) \\ &= u\left(W_{it+k} - I_{it+k} + L_{it+k}, v_{it+k}\right) + \beta E_t V_{it+k+1}\left((1+r)W_{it+k} - C_{it+k} - I_{it+k} + Y_{it+k} + D_{it+k}; v_{it+k}\right) \end{aligned} \tag{7a}$$

$$\begin{aligned} V^{NB}_{it+k}\left(W, v\right) &= u\left(C_{it+k}, v_{it+k}\right) + \beta E_t V_{it+k+1}\left(W_{it+k+1}, v_{it+k+1}\right) \\ &= u\left(W_{it+k} - I_{it+k}, v_{it+k}\right) + \beta E_t V_{it+k+1}\left((1+r)W_{it+k} - C_{it+k} - I_{it+k} + Y'_{it+k}; v_{it+k}\right) \end{aligned} \tag{7b}$$

In the expected lifetime utility with borrowing (eq. (7a)) uncertainty enters directly through production shocks and indirectly through net debt and value of future access to credit. In the no-borrowing case (eq. (7b)) uncertainty arises only due to the production shock. Both eq. (7a) and (7b) can be rewritten to reflect the stochastic income where expectation operators are substituted by their respective probabilities as follows:

$$V^{B}_{it+k}(W,v;Z)=u(W_{it+k}-I_{it+k}+L_{it+k},v_{it+k})+\beta\{V_{it+k+1}[p_{it+k}(Y_{it+k}-(1-p_{jt+k})(1+r+m)L_{it+k}) \\ +\tau(1-p_{it+k})(Y_{it+k}-(1+r)L_{it+k})]+\rho v_{it+k}\} \tag{8a}$$

$$V^{NB}_{it+k}(W,v)=u(W_{it+k}-I_{it+k},v_{it+k})+\beta\{V_{it+k+1}[p_{it+k}Y'_{it+k}+(1-p_{it+k})Y'_{it+k}]+v_{it+k}\} \tag{8b}$$

Although the distribution of production shocks remains the same under both borrowing and no-borrowing scenarios and expected gross income is higher with than without borrowing, expected income net of borrowing, given in the square brackets of eq.(8a) and (8b), is more variable with than without borrowing. The relative riskiness of borrowing given by difference between eq.(8a) and (8b) provides the explicit policy that governs the decision to borrow:

$$Z_{it+k}=B \quad iff \quad 0\le u(L_{it+k},x_{it})+\beta\{V_{it+k+1}\{[p_{it+k}(Y_{it+k}-(1-p_{jt+k})(1+r+m)L_{it+k}-Y'_{it+k})] \\ +[(\tau(Y_{it+k}-(1+r)L_{it+k})-Y'_{it+k})(1-p_{it+k})]\}+v_{it+k}(\rho-1)\} \tag{9}$$

$$Z_t=NB, \quad otherwise$$

where x_{it} is a matrix of other taste covariates. The household participates in borrowing if $V^{B}(.)\ge V^{NB}(.)$. The first term of the RHS of eq. (9) is the utility of having a loan size of L, given x, at $t+k$. This is the utility of having a relaxed budget derived from the extra liquidity that borrowing provides at $t+k$. It is either, the utility gain from higher consumption at t due to release of resources from production, or given the non-fungibility of credit, the amount of borrowing diverted for consumption. The second and third terms indicate how loan repayment, which is higher in the good state by the joint liability amount, and the punishment threat in the bad state influence income variability. The two terms in square brackets provide the relative income variance due to borrowing under the 'good' versus 'bad' states, which are represented by the corresponding probabilities. The last term gives the state of future access to borrowing, determined subsequently. Note that ρ is less than one means that v is negative indicating the expected reduction in welfare due to lose of access to credit. Borrowing is thus determined by the expectations formed on the net effect of the two terms *ex ante*. In sum, the theory developed in this section helps to understand how households evaluate the relative riskiness of borrowing by comparing the sum of present and discounted stochastic future benefits with and without borrowing.

3.3 Empirical model and estimation strategy

In the empirical analysis the goal is to investigate if, after controlling for other factors, contractual risks indeed hinder participation in borrowing. This section presents the econometric model and estimation strategy implemented. As implied by eq. (9), contractual risks of income and future access to credit, *ceteris paribus,* are two key factors hypothesized to determine participation in borrowing. In short, the dynamic decision to participate boils down to expectations on future income variability with and without borrowing as well as households' degree of dependence on MFI credit in the future.

Realizations of income (or consumption) seem to be good candidates to proxy contractual effects of borrowing. However, since realized outcomes are likely to be influenced by borrowing status, they are endogenous to borrowing decisions and thus cannot give consistent estimates. Instead, we assume that expectations about income and future access to borrowing depend on individual household risk preferences at the time the decision takes place. A central issue in our estimation is identifying the roles contractual risks play in the decision to borrow. Separate identification of the two risk components (that is, joint liability and future access) is however problematic because both are simultaneously determined. As motivated in the theory, the first component can be best compared to a standard individual liability, which unfortunately was not properly practiced in the study area. We therefore try to unbundle the two effects by comparing the joint liability to a hypothetical individual liability of the same future access incentive structure. In the survey, we elicited preferences related to joint liability borrowing by a hypothetical question comparing joint liability partner risk to a reduction from an individual liability interest rate. Specifically, we asked respondents "holding other things the same, how much extra (or less) interest rate will you pay over the existing joint liability rate to access an individual liability?"[5] This value, which apparently measures the 'risk premium' households would like to pay to avoid joint liability relative to a standard individual liability, is used to proxy joint liability risk along with other indicators for individual as well as systemic risk indicators. However, this predicted risk (based on self-reported information) can be endogenous to individual risk bearing capacities that may vary with individual characteristics that in turn depend on borrowing status such as participation in the extension programs and land size cultivated. Households may, for example, participate less in 'extension programs' or rent-in less land because both are credit dependent. It is thus instructive to use predicted value of our 'risk premium' using some exogenous characteristics as regressors.

[5] Note that the scale of the premium ranges between -1 and 1; a positive value indicates preference for individual liability and is the risk premium households would pay to avoid joint liability risk. Note that efforts were made to minimize measurement error by framing the question such that households elicit the extra payment to avoid only the 'joint liability' element in group lending, excluding other factors such as group meetings and hassles, and differences in contract flexibilities.

The second component, future access to credit, has to do with household's relative dependence on external finance in the future and can be proxied by indicators for availability and potentials of future liquidity such as safety nets and the natural log of livestock holdings, at t-1 to avoid endogeneity. The two components are thus used to explain participation decisions together with other indicators for intra-household preference differentials and systemic risk.

Participation is a binary indicator and a binary choice model is therefore an appropriate empirical framework[6]. Availability of panel data allows for modeling unobserved individual heterogeneity and dynamics but also raises the issue of initial conditions (Heckman, 1981b). However, estimation of such a non-linear panel data model is not straight forward because of the well known incidental parameter problem (with fixed T, $N \to \infty$), which is due to the presence of individual heterogeneity (Hsiao, 1986: 73-76). In a linear panel data model this heterogeneity is often wiped out by a within transformation of the data (Baltagi, 2001: 206). For the fixed effects logit model, Chamberlain (1980) suggested a solution to the incidental parameter problem by maximizing a likelihood function conditional on a minimally sufficient statistic for the individual heterogeneity parameter. However, the problem with this solution in our case is that it yields inconsistent estimates because observations that never change status over time are dropped (Verbeek, 2008: 395). Besides, we are interested not only in those that change borrowing status over the years but also in those that never change status because throughout the sample period they never or always borrowed. With a fixed effects probit model it is also not possible to get rid of the fixed effects and, besides computational difficulties, this approach also yields inconsistent estimates due to the incidental parameter problem in short panels like ours (Heckman, 1981b)[7].

A natural option is to estimate a random effects probit model. To elaborate this we first specify a static random effects panel probit model for the decision variable as follows[8]:

$$Z_{it} = \begin{cases} 1 \quad (= B) & if \quad Z_{it}^{*} \geq 0, \\ 0 \quad (= NB) & otherwise \end{cases}$$

$$Z_{it}^{*} = \beta_0 + \beta_2 \hat{m}_{it} + v_{it}^{'}\beta_3 + x_{it}^{'}\beta_4 + \alpha_i + e_{it} \qquad (i = 1,..., N;\ t = 1,..., T) \tag{10}$$

where the latent variable Z_{it}^{*} indicates the propensity to borrow, $\hat{m}_{it}$ is the joint liability risk indicator predicted from individual household risk preferences explained earlier, v_{it} is a vector

[6] Obviously, this ignores the intensity of participation in the event that households react to uncertainty by reducing amount of borrowing rather than avoiding it altogether. Nevertheless, the focus in this paper is on the extreme situation where households abandon borrowing for contractual risk reasons.
[7] For a brief review of this problem see Baltagi, 2001:209 and Cameron and Trivedi, 2005: 782.
[8] Although not formally derived from the theoretical model, our analysis here draws from eq. (9) to specify a reduced form empirical model capturing key features of the problem.

of indicators for future credit access, x_{it} are other exogenous covariates, β 's are parameters to estimate and e_{it}, for now, is Gaussian distributed with mean zero and σ_e^2=1. Furthermore, α_i is a household specific error component capturing heterogeneity, where $\lambda = \sigma_\alpha^2 /(\sigma_\alpha^2 + 1)$ is the proportion of variance due to individual heterogeneity, which is assumed equal across periods. This proportion can be used to test whether the specified random effects probit specification is more appropriate than a standard pooled probit model that neglects unobserved heterogeneity. Pooling is not appropriate if the hypothesis $\lambda = 0$ is rejected.

The standard random effects probit estimator yields consistent parameter estimates if the following assumptions hold: i) participation at t is independent to participation at t-1 (no true state dependence), ii) initial participation conditions, Z_{i1}, are uncorrelated to individual heterogeneity α_i (no spurious state dependence), and iii) e_{it} is serially independent. However, the nature of participation in MFI borrowing makes the validity of these assumptions doubtful. Borrowers select themselves into the program by joining a group of their choice on which the MFI decides to grant a loan based on its own criteria[9]. In both cases, selection can be based on observed and unobserved 'initial' household characteristics (Armendáriz de Aghion and Morduch, 2005: 199-223). Note that observables such as availability of partners, village-level trust, MFI eligibility criteria, group screening and group characteristics are easily controlled for. Nevertheless, initial participation can be based on unobserved household characteristics such as entrepreneurial skills and perceptions on credit that may persist over time. Moreover, as implied by the theory, in underdeveloped credit markets like in rural Ethiopia where formal borrowing is limited, previous participation may influence future participation (true state dependence) by reducing perceived risks and through learning or vice versa. Although changes in risk perception can be subsumed to be captured by the risk parameter, state dependence due to non-risk factors can still confound consistent estimation.

Consequently, assumptions (i) and (ii) are worth reconsidering in our context. True state dependence can be accounted for by including a one period lag of the dependent variable:

$$Z_{it} = 1\left(\gamma Z_{it-1} + \beta_2 \widehat{m}_{it} + v_{it}'\beta_3 + x_{it}'\beta + u_{it} \geq 0\right) \qquad (i = 1,...,N;\, t = 2,...,T) \qquad (11)$$

where, the error components in (10) are condensed to $u_{it} = \alpha_i + e_{it}$. If the hypotheses $\gamma \neq 0$ cannot be rejected, the dynamic specification in (11) is correct, indicating the presence of true state dependence. Again, if (ii) and (iii) can be assumed, eq.(11) is consistently estimated using a standard random effects probit. However, also assumption (ii) is expected to be too strong to hold in our context. If this is indeed the case the estimator is inconsistent and parameters tend to be overstated, particularly the degree of true state dependence, γ (Baltagi,

[9] Further discussions of the participant – non-participant classification is provided in section 3.4 (see also figure 3A.1, appendix).

2001: 216). Three approaches to this 'initial conditions' problem due to Heckman (1981a), Orme (1997) and Wooldridge (2005) are proposed to relax assumption (ii). Recent Monte Carlo evidence indicates that the Heckman approach, despite its relative computational complexity provides the least biased parameter estimates of these approaches (for example, Miranda, 2007). This approach is implemented in this study using recent simulation based estimators that facilitate estimation. The Heckman approach requires a separate specification for the initial condition, Z_{i1}, conditional on α_i, which is estimated as a system with eq.(11):

$$Z_{i1} = M_{i1}'\theta + \psi\alpha_i + e_{i1} \; (i = 1,...,N; \quad t = 1) \tag{12}$$

where M_{i1} is a vector of exogenous instruments that also includes x_{i1} and, if available, other pre-sample variables. θ and ψ are parameters and α_i and e_{i1} are assumed independent. Under assumption (iii), the Heckman approach uses the joint probability of the full observed sequence $(Z_{i1},...,Z_{iT})$ conditional on α_i to approximate the probability of observing the sequence for individual i:

$$P(Z_{i1},...,Z_{iT}) = \Phi[(M_{i1}'\theta + \psi\alpha_i)(2Z_{i1} - 1)]\prod_{t=2}^{T}\Phi[(\gamma Z_{it-1} + x_{it}'\beta + \alpha_i)(2Z_{it} - 1)] \tag{13}$$

This approach provides consistent estimates for the parameters of interest and enables to separately estimate the initial condition parameters that are useful to perform robustness tests. The likelihood function for the latter requires integrating the probability of α_i against its density, $\phi\ (\alpha_i\ ),$ which can be evaluated using a Gaussian-Hermite quadrature procedure (Butler and Moffitt, 1982). A program developed by Stewart (2006) reduces the computational difficulties of ML implementation of the quadrature for this Heckman estimator[10].

If however assumption (iii) does not hold, the Heckman estimator too is inconsistent. Extending it to incorporate an autocorrelated error structure makes estimation using ML Gaussian-Hermite quadrature infeasible because it involves evaluation of T-dimensional integrals of Normal densities (Contoyannis et al., 2004, Stewart, 2007). Stewart (2006, 2007) presented a simulation based implementation strategy, Maximum Simulated Likelihood (MSL), based on the GHK algorithm for this approach allowing autocorrelation of errors. Assuming an AR(1) process, we run this simulation for 150 replications to check for autocorrelation, specifying a quasi-random matrix to initiate a Halton sequence that facilitates convergence.

[10] Interested readers are referred to Heckman (1981b:114 -178) for detailed outline of the model and to Stewart (2007) for a concise presentation and implementation strategy.

3.4 Microfinance in northern Ethiopia and the data

Panel data used comes from 400 rural households randomly sampled from sixteen villages of four zones in the Tigray region, Ethiopia. This section gives a brief overview of the microfinance practice and underlying agro-climatic and economic conditions of the study area, the data collection processes and the data itself.

3.4.1 Microfinance practice and economic environment

Tigray is the north most region of Ethiopia, located in the semi-arid belt of the Sub-Sahara region. Erratic rainfall, decades of civil-war and conflicts, overpopulation and severe natural resource degradation characterizes its economy, generally classified as food-deficient. Smallholder agriculture, the main stay of 775 thousand rural households, is unpredictably subject to the vagaries of nature. Efforts to increase productivity by introducing new technologies such as high-yielding crops and fertilizer are often hindered by income shocks because formal insurance schemes to smooth consumption after shocks are absent and informal risk-sharing is limited.

A recent effort to mitigate the volatile economic situation and reduce poverty through improving input use and productivity includes the provision of financial services to farmers. The *Dedebit Credit and Saving Institution* (DECSI) is one of biggest and pioneer MFIs in Ethiopia operating in the region providing *working capital* credit to smallholders that are neglected by traditional banks. DECSI started trial operations in 1994. It officially launched in 1997 and expanded its client size to nearly 424,000 and average loan size to USD 217 between 1997 and 2006, encroaching to almost all parts of the region (see table 3A.1). By 2001, DECSI's network has grown to 96 sub-branches covering 91 per cent of villages in the region (Borchgrevink et al., 2003). While almost all respondents reported they knew DECI in 1997, only 42 per cent of them reported that a DECSI branch was in their 'nearest town' in the same year. This number grew to 74 per cent in 2000 and to 86 per cent in 2006, reflecting the rapid expansion of DECSI. Lending interest rates range between 12.5 and 15 per cent per year. Maximum loan size is close to USD 500. Joint liability loan repayment in the study period ranged between 1-2 years.

DECSI followed a 'Grameen-style' joint liability credit contract and implemented it in a stricter sense. A credit application is made by a group of 3 to 7 self-selected borrowers screened by a credit committee composed of DECSI as well as local officials. After approval, individual loan demands are awarded for which all group members become responsible and borrowing repayment is strictly conditional on previous (group) repayment. Anything less is considered default and consequently all group members, sometimes even their village, is denied access to future credit. Contrary to the 'limited liability' assumption in the microfinance literature, DECSI, with the help of local officials, tracks down defaulters to jail or local courts regardless of realized outcomes. Although not officially declared, this strategy is pursued to "*avoid bad precedence for run-away borrowers*" and is at the heart of DECSI's 'high' repayment performance.

3.4.2 The data

The panel data covers ten years (1997-2006) observed in five waves with intervals of about two years. The first four waves of data were collected by other researchers. In 2006, we took advantage of these earlier surveys and visited the same households. Note that the last survey included recall questions regarding key borrowing history to track down those that dropped out in any of the earlier surveys. This information is mainly used in this chapter. The data includes household characteristics, assets, income and expenditure, perceptions and attributes of the household head, and village and MFI information. Borrowing information include alternative sources of credit and nature of borrowing, perceptions on future access to credit and participation. The empirical identification benefits from the fact that the first round survey coincided with the extensive encroaching of DECSI into most of the (sample) villages. Moreover, the initial survey tracked some pre-survey information on households useful in the reduced-form Heckman model estimated.

To give an idea about general structure of household borrowing, the population with "*physical* access" to microfinance can be broadly classified as participants and non-participants (see Figure 3A.1 in the appendix). We define participants as those that were in a joint liability based borrowing relationship in a given year. Admittedly, the non-participant group is heterogeneous in terms of reasons for non-participation, which can be further classified as involuntarily and voluntarily excluded from borrowing. Involuntarily excluded are those that despite their preference to participate were excluded by the MFI because they are ineligible (for for example, age, risk considerations) or those that were eligible but discriminated for non-loan product reasons such as social status or political reasons.

Table 3.1 Yearly participation and repeat participation

Yearly participation			*Overall participation*		
Yearly	*Number*	*%*	*Repeat times*	*Number*	*%*
1997	121	30.3	Never	118	29.50
2001	130	32.5	Once	155	38.75
2003	89	22.3	Twice	66	16.50
2005	85	21.3	Thrice	48	12.00
2006	59	14.8	Four times	13	3.25

The voluntary excluded include those that never demand credit regardless of the nature of credit provision, for example, because of availability of other sources, or for religious reasons and those that need credit but decided not to participate due to contractual risks. While the rest are also included, this later group is the main target of this study.
In general, asked about their demand for credit in the years covered, nearly 95 per cent of households reported they needed credit during the study period, of which 73 per cent had at least once applied for joint liability credit. A negligible number of them (2%) reported their applications had been turned down by the screening committee for reasons they did not know. Four percent of the households excluded themselves for religious reasons (*haram*). When asked about 'why a household decided not to borrow while there was a need for credit', 59 per cent of them reported that they had feared failure to repay group loans and risk of sliding down into debt-trap even if their projects would have been profitable. Some seven percent of the participants had experienced "non-strategic" partner default. Very few of them reported that they had other sources of credit. Thus, regarding borrowing status, households in the sample had either borrowed repeatedly, participated at least once but dropped out, or never participated at all over the years (see Figure 3.1). In general, participation of sample households, although slightly increased from 1997 to 2001, declined over the survey period (see table 3.1). This is consistent with institution-level trends over the same period (see table 3A.1)[11]. The overall repeat participation between consecutive survey years is given in table 3.1. While 30 per cent never participated throughout, only three percent participated four times and none did more than four times over the years. Average group size during the study period was 4.7. Of participant households, 52 per cent reported their groups were formed among households heterogeneous in wealth but only 17 per cent of participants switched from their first group to another group[12]. Table 3.2 summarizes specific variables used in the empirical analysis of predicting the joint liability risk as well as the main participation decision model.

The estimated dynamic probit model contains the two groups of variables in the reduced form Heckman model, namely, $t > 1$ variables and initial period ($t=1$) variables, which include all x-variables and the pre-sample variables. The lagged dependent variable is one of the $t > 1$ variables. The pre-survey variables include an indicator for household ability to find credit partners if needed. To control for overall social capital and trust respondents were asked if they believed trust had deteriorated over time. These two variables provide information about the social capital on which joint liability heavily relies. An underlying factor in this study is that smallholder agricultural borrowing risks are predominantly derived from exogenous systemic as well as individual shocks.

[11] Recently, DECSI has introduced a variant of individual credit 'packaged' with specific inputs. Total borrower numbers in table 3A.1 include these borrowers, which are not included in the analysis. In the empirical analysis, this is controlled by including it in the 'other source of borrowing' variable.
[12] Descriptive information summarizing these questions not given here can be obtained from the authors.

Table 3.2 Summary statistic of variables used in the empirical analysis

Variable description	*Mean*	*Std. dev*	*Min*	*Max*
Participated in joint liability borrowing (yes=1)	0.24	0.43	0	1
Joint liability risk premium	0.03	0.08	-0.06	0.25
Predicted risk of joint liability	0.03	0.010	-0.01	0.08
Losing future credit access worst punishment (yes=1)	0.08	0.27	0	1
Unable to find a partner (yes=1), (t=1)	0.87	0.34	0	1
Partner repayment problem (if participated) (yes=1)	0.90	0.31	0	1
"Trust" deteriorated in the community (yes=1), (t=1)	0.51	0.50	0	1
Experienced partner repayment problem (yes=1)	0.06	0.23	0	1
Size of joint liability group (if participated)	4.75	1.38	3	7
Natural log of value of live stock holding (t-1)	7.45	1.47	2.99	11.00
Size of land owned in *Tsimad* (= 0.25 ha.)	4.62	2.94	0.25	18
Income from off-farm employment	692.26	1801.16	0	36,000
Annual transfers received (remittance, family)	192.44	602.90	0	9,612
Annual household consumption (log) (t-1)	7.46	0.85	2.57	11.21
Location/village is highland (=1; otherwise, lowland)	0.69	0.47	0	1
Annual rainfall (nearest station), in milliliters	697.14	150.70	419.05	1100.40
Systemic shock occurred (reported) (yes=1)	0.07	0.26	0	1
Idiosyncratic shock occurred to household (yes=1)	0.24	0.43	0	1
Participated in food-for-work and safety nets (yes=1)	0.51	0.50	0	1
Participation in extension program risky (yes=1)	0.62	0.49	0	1
Male headed household (male=1)	0.78	0.42	0	1
Age of household head (years)	52.76	14.50	20	90
Household size	5.30	2.39	1	13
Household head is literate (yes=1)	0.29	0.45	0	1

A well known source of systemic shock in Ethiopia is rain failure and annual rainfall information (measured at the nearest station to the village) is used. To account for intra-household differential impacts of the systemic shocks, respondents were asked if there was a major (systemic) shock such as draught, floods, locust swarms that affected the household in the year before the survey.

We also probed for information regarding idiosyncratic (for example, illness or death of key household member, animal loss) shocks in the just ended year. Copping mechanisms are diverse, which among others include investing in livestock holdings, remittances, transfers, and food-for-work (safety net) activities. Livestock is the most liquid asset for rural households in Ethiopia and is often considered as an insurance against downside shocks in the household. Another well known insurance policy in Ethiopia, which has been re-instituted as 'productive safety net program' recently is the 'food-for-work program', often available for households hit by drought.

3.5 Estimation, results and discussions

This section presents estimation results based on the econometric strategy outlined in section 3.3. Section 3.5.1 gives results of the joint liability risk indicator, which together with other variables is used in estimating the participation decision in the section 3.5.2.

3.5.1 Predicted joint liability (partner) risk

OLS estimates of this first-stage regression for the endogenous risk premium are presented in table 3.3. All variables as a group and the variables of interest individually significantly explain the joint liability risk preference at acceptable critical levels. White-robust standard errors are calculated to correct for possible heteroskedasticity. The predicted values from table 3.3 are used in the main model estimation.

3.5.2 The decision to participate in joint liability credit

Coming back to the estimation of the main model of borrowing participation, as a first step, we test whether the data can be pooled estimating a standard random effects probit for the same sample and specification as in the Heckman model. Pooling (λ=0) is strongly rejected in favor of the random effects estimator by the likelihood-ratio test ($\chi^2(01) = 25.02$, $p = 0.000$), implying that individual heterogeneity is an important element in our borrowing decisions. Unfortunately the standard random effects estimator simply assumes initial conditions as exogenous. Estimating the dynamic random effects model using the Heckman estimator resolves this problem. Results are presented in table 3.4, along with the standard estimates to investigate the bias due to the exogenous initial condition assumption in the latter. The parameters in the estimated reduced form model, excluding the *t=1* values for the initial conditions, are jointly highly significant with a χ^2 (11) Wald test statistics of 91.86. The hypothesis $\gamma = 0$ is rejected in both the standard and Heckman estimator, confirming the relevance of the dynamic specification. The estimate for γ is however overstated in the standard estimator both quantitatively (0.27) and qualitatively (highly significant). After properly controlling for initial conditions in the Heckman procedure, the estimate is almost a third smaller and less significant. The parameter for α_i in the linearlized (ψ) eq. (12) is significant at the 10 per cent critical level, once again confirming nonrandom initial participation in joint liability borrowing. The positive ψ sign means that even controlling for initial household, group and village characteristics (age, age-squared, trust, availability of partners and other sources of finance, the latter not reported in table 3.4), households with

Table 3.3 OLS estimates of predictors for joint liability risk

Dependent var.: elicited joint liability risk premium	*Coefficients*	*(Robust Std. errors)*
Male headed household (male=1)	-4.204*	(2.385)
Household size	-0.848*	(0.383)
Size of land owned in *Tsimad* (= 0.25 ha.)	-1.034***	(0.257)
Household head is literate (yes=1)	-1.976	(1.914)
Considers participation in the extension program risky (yes=1)	2.331*	(1.781)
Income from off-farm employment	-2.93×10^{-4}*	(3.72×10^{-4})
Amount of annual transfers received (remittance, family)	-1.76×10^{-3}**	(1.09×10^{-3})
Location/village is highland (=1; otherwise, lowland)	1.784***	(1.666)
Believed likely to experience partner repayment problem (yes=1)	18.037***	(2.514)
Size of joint liability group (if participated)	-2.388***	(0.452)
Intercept	43.845***	(2.660)
F(10, 1989)	10.69***	
R-squared	0.3970	
Number of obs.	2000	

Notes: (*), (**), (***), significant at 10%, 5% and 1% critical levels, respectively. Dependent variable: hypothetical willingness to pay risk premium to shift to individual liability, avoiding joint liability

some unobserved favorable conditions to joint liability borrowing were more likely to participate than otherwise. All other parameter estimates from both models are comparable or, as expected, slightly lower (in absolute value) for the standard than for the Heckman estimator. Results (not presented here) of the simulated Heckman estimator with correlated errors using the MSL tell a similar story and the hypothesis of no autocorrelation cannot be rejected at acceptable critical levels. Results from the Heckman estimator with no autocorrelation are therefore interpreted. Controlling for observable and unobserved household, group, and MFI–level effects, including state dependence, the two important hypotheses in this chapter are confirmed by our data.

First, predicted joint liability has a significant negative impact on borrowing confirming the unfavorable impact of joint liability downside risk on participation. Based on our elicited risk preference, we find evidence that households that are willing to pay a positive risk premium to avoid a joint liability contract in favor of an individual liability contract of the same structure, including the 'future access punishment upon non-repayment' arrangement, are less likely to participate in joint liability borrowing. Second, our key indicators for future liquidity and credit constraint, and hence future access to credit, i.e. the natural log of livestock at *t*-1, access to food- for-work and safety nets, explain participation significantly.

Table 3.4 Probability of the decision to participate in joint liability based MFI borrowing.

Variable description	*Dynamic random effects panel probit* *Standard estimator*	*Heckman estimator*
1 if participated in joint liability borrowing (*t-1*), zero otherwise	0.266***	0.186*
	(0.083)	(0.093)
Predicted risk of joint liability	-14.918***	-21.148***
	(3.971)	(5.200)
Annual rainfall (nearest station), in milliliters	-9.7×10^{-4}***	-8.0×10^{-4}***
	(2.5×10^{-4})	(2.5×10^{-4})
1 if shock (illness or death of key household member, animal loss) occurred to the household	-0.027	-0.033
	(0.081)	(0.082)
1 if losing future access to credit worst punishment	0.496***	0.495***
	(0.123)	(0.124)
Natural log of value of live stock holding (t-1)	0.057*	0.047*
	(0.026)	(0.026)
Live stock value (t-1) * systemic shock occurrence (reported by household)	-2.0×10^{-5}	-3.0×10^{-5}
	(2.0×10^{-5})	(2.0×10^{-5})
Natural log of annual household consumption (t-1)	-0.043	-0.048
	(0.044)	(0.044)
1 if participated in food-for-work and safety nets	0.161*	0.155*
	(0.074)	(0.074)
Age of household head (years)	0.015	0.012
	(0.017)	(0.017)
Age of household head squared (scaled by 100)	-0.022	-0.019
	(0.016)	(0.016)
1 if unable to find a partner (t=1)	-	-0.046
		(0.364)
1 if believed "trust" deteriorated in the community (t=1)	-	-0.222
		(0.281)
Constant	-0.054	-7.204
	(0.608)	(5.601)
λ, proportion of variance due to individual heterogeneity	-	-4.446
		(1.380)***
	-	2.560*
ψ, Parameter for initial conditions		(1.518)
♣Wald χ2(11)	100.48***	91.980***
LR test of proportion of panel-level variance= 0: χ2 (1)	25.02***	2.860*
♣Log likelihood	-791.557	-1022.037
Number of obs.	1600	2000

(***), (**), (*) significant at the 1%, 5% and 10% critical level. Standard errors are in brackets.

(♣) refers to parameters in the *t>1* reduced form model, (t-1) are variables measured with one year lag.

Controlling for other factors, the more a household owned livestock at the time of borrowing, and also the better the access to food-for-work, the greater the probability to participate. This is an interesting finding with implications for equity and the 'safety net' policy. To account for intra-household ability to cope with shocks of systemic nature, a dummy equal to one if reported it was bad year for all is interacted with natural log of livestock at *t-1* and shocks of idiosyncratic nature are included. Both are found insignificant but with the expected parameter sign. However, reported annual rainfall from the nearest station (in milliliters), another indicator for systemic shock turned out to be negatively highly significant[13]. Thus, risks of systemic but not idiosyncratic nature together with contractual risks of joint liability borrowing seem to impede the decision to participate in joint liability based borrowing in these villages. On the other hand, credit is less significantly used to smooth consumption gaps *ex post.* That is, the natural log of annual household consumption, measured at *t-1* to capture effects of lagged consumption on downside risk, reflects that joint liability credit is rarely used to smooth consumption shortfalls. It also means that there is no enough evidence of showing loan diversion for consumption in those particular years.

Another indicator for future access to credit, based on a question asking whether a respondent thinks that denial of credit is the worst punishment of all possible punishments, is also found to be significant. The positive sign indicates that the higher households value future credit access, the more likely they keep their relationship with the MFI by borrowing and repaying frequently, which is also in line with our state dependence finding. It may be argued that participation causes higher valuation. But again, this way of causality is captured by the state dependence parameter, which is an interesting addition to our findings. In fact, the positively significant lagged dependent parameter means that the probability of repeat-borrowing is higher than first-time entry (true state dependence). One implication of this is that first-time entry is more barred than repeat borrowing or that imagined contractual risks and punishment threats are more important for households considering borrowing for the first time.

The variables for availability of a reliable (preferred) partner and the general perception on trust are also significant and with expected parameter signs. Both capture the 'partner risk' associated with a joint liability contract. The smaller the chances of obtaining a reliable and trusted partner, the more likely to perceive encountering partner default and the less likely to join a joint liability contract. Age and age-squared are also included to capture eligibility criteria of the MFI but their parameters are insignificant.

[13] Thus, although qualitatively in the same direction of influence as the rainfall variable, we suspect that our dummy for systemic shock, which is reported at household level (=1 if bad year) might be measured with errors.

3.6 Conclusions and implications

This chapter investigates both theoretically and empirically how joint liability risk and the threat of losing future access to credit after failure to repay group credit influence households' borrowing decisions. Unlike most existing theoretical models in microfinance that consider participation in joint liability borrowing from an 'independent project' perspective, our theoretical framework examines participation decisions within the 'household entity' where production and consumption decisions take place inseparably. This approach is a novel attempt to examine recent microfinance contractual approaches within intertemporal household decisions.

Using a unique panel dataset from sixteen villages in Ethiopia, the chapter studies the impact of the two contractual risk factors on agricultural borrowings where exogenous shocks like rain failure, death and illness of productive inputs are explicitly taken into account. A dynamic random effects probit estimator is implemented allowing for endogeneity of initial conditions and including risk preference indicators and unbundling them from state dependence using recently developed econometric simulation techniques. Three main findings follow from the empirical investigation.

First, controlling for MFI selection, eligibility and persistence due to initial differences, we find that joint liability risk as measured by predicted risk deters borrowing in a highly significant way. Second, in line with this finding, we find evidence that intra-household differentials in future liquidity and insurance (such as livestock endowment- the most liquid asset, and access to food-safety nets- the last resort to bridge household resource gap after a shock) are important factors in participation. These findings have interesting equity and insurance policy implications for the MFI and regional policy makers. Specifically, even if joint liability is an innovative approach to remove the collateral requirement theoretically, in practice, it does so at the expense of rationing the poorest of the poor (as proxied, for example, by livestock) out of borrowing. Good news for policy makers is that the well known safety net program recently instituted in Ethiopia to cushion households against production shocks appears to promote use of credit.

Our results also indicate that joint liability borrowing, which is meant only for production, is also associated with downside consumption risk. This is not just a loan diversion story, but may well mean that some household consider MFI credits only as a last resort to bridge downside risks and not, as presumed by the lender, for improving productivity. However, rainfall, a more general village-level indicator for systemic major draughts, tends to reduce the probability of participation substantially. The more draught is felt across borrowers, the less the probability to borrow, controlling for ex-post coping mechanisms. Third, controlling for time-invariant unobserved initial households heterogeneity, which also mattered for participation, we find that households who happened to participate once are more likely to repeat borrowing. One possible implication of this is that punishment threats and risks associated with joint liability are perceived bigger than they are in reality and policies that help to ease perceived risks and threats might help to encourage households to benefit from MFI borrowings.

To conclude, one major reason microfinance credit is needed in those areas is to help cushion against risk. However, if some elements of the contract introduce and exacerbate aversion to risk, then it will continue to keep poor but potential households unable to protect themselves against downside risk stay in poverty by avoiding productive credit- just as the well known trade off between insurance and incentives. Ensuring physical availability of credit through MFIs without improving such contractual risks is therefore only a step but not a sufficient condition to access credit in poor and risky environments. For the borrowers of the MFI at stake, providing full-fledged credit services, including credit for consumption, may help to cope with risks after shock and thus encourage them to use credit to tackle poverty. Most importantly, the policy of 'one size fits all' does not seem to be the most effective way of providing access to credit in rural areas with diverse socio-economic and environmental conditions.

Appendix 3A

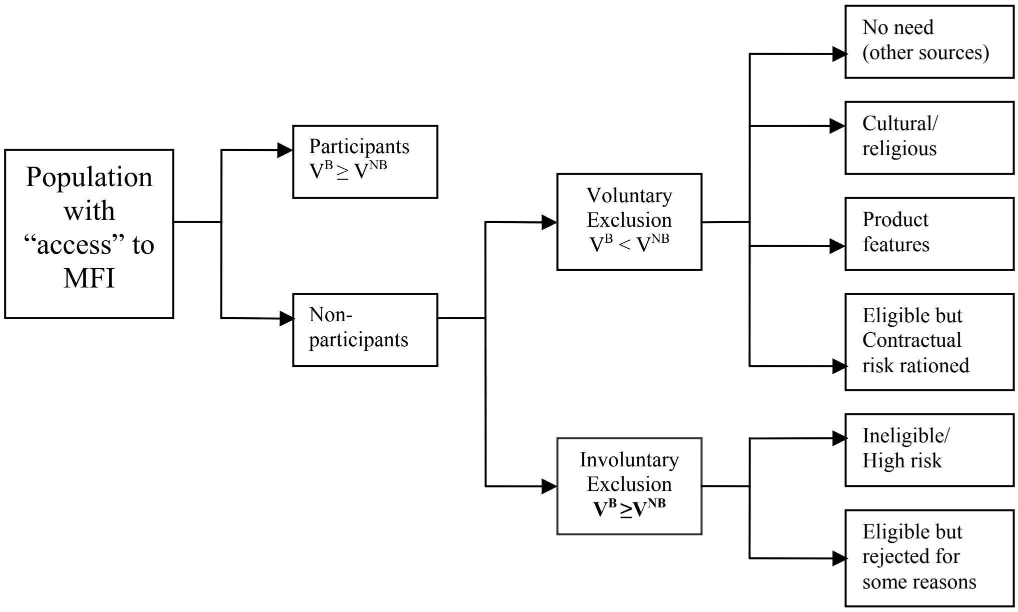


Figure 3A.1. Access to credit and participation in MFI borrowing: the contractual risk-rationed
Source: Adapted from World Bank (2008: 29)

Table 3A.1 Number of borrowers, average and total loan (2001-2005)

Year	*No of borrowers*	*growth rate (No of borrowers)*	*Average loan ($)*	*growth rate (average loan)*	*Total loan ($)*	*growth rate (total loan)*
1994	8,446	-	125	-	1,052,060	-
1995	13,881	0.643	98	-0.212	1,362,030	0.295
1996	20,515	0.478	118	0.204	2,423,007	0.779
1997	67,057	2.269	111	-0.060	7,449,692	2.075
1998	168,976	1.520	92	-0.167	15,616,007	1.096
1999	210,572	0.246	69	-0.253	14,543,162	-0.069
2000	187,470	-0.110	61	-0.118	11,427,221	-0.214
2001	158,883	-0.152	82	0.342	14,398,280	0.260
2002	225,996	0.422	109	0.333	24,685,525	0.714
2003	336,733	0.490	138	0.266	46,362,212	0.878
2004	419,052	0.244	186	0.348	77,886,681	0.680
2005	392,693	-0.063	217	0.167	85,304,139	0.095
2006	423,830	0.079	231	0.065	97,904,968	0.148

Source: Own calculations from (Mees, 2000) and MIX MARKET (2007).

CHAPTER 4

RISK-MATCHING BEHAVIOR IN MICROCREDIT GROUP FORMATION: EVIDENCE FROM NORTHERN ETHIOPIA[1]

Abstract: *Theoretical models on group lending assume the formation of groups of homogenous risk types. Recent theoretical and empirical findings challenge this view arguing that when markets for insurance are missing, risk homogeneity may not hold anymore and risk heterogeneity can be the optimal outcome. Using data from an MFI in Tigray (Ethiopia) this chapter examines the homogeneity hypothesis and reflects on implications for repayment. No evidence is found that supports risk homogeneity, even accounting for matching frictions. However, we also do not find an explicit link between the presence of risk heterogeneity and side-payments due to missing insurance as suggested in the literature. Instead, other trust based social networks seem to underlie heterogeneity. Such social networks are often synchronized with credit groups and influence the probability of repayment under heterogeneity. The implication is that successful repayment rates in group lending need not arise only under risk homogeneity but can also arise under risk heterogeneity. Heterogeneity may also serve to bridge missing insurance markets in poor rural environments. MFIs therefore need to consider such local conditions when designing their lending schemes.*

Key words: microfinance, group formation, risk matching, risk homogeneity, Ethiopia

[1] A paper published as Guush Berhane, Cornelis Gardebroek and Henk A.J. Moll (2009). "Risk-matching behavior in microcredit group formation: evidence from northern Ethiopia," *Agricultural economics,* 40 (2009):409-419.

4.1 Introduction

Microfinance has become a standard tool of poverty alleviation programs that take the provision of financial services at the centre of development policy. Provision of financial services to low-income households requires however solving two central problems: the information problem (how to establish group members' willingness to repay) and the cost problem (how to handle small financial transactions with a short duration cost-effectively).

Experience over the past twenty years has shown that some Microfinance Institutions (MFIs) focusing on low income households are able to overcome these two problems. One of the approaches adopted by some of the successful MFIs to overcome the information problem and the related possibility of default is joint liability in group lending. In group lending, borrowers are required to form small groups that are held jointly liable for the debts. Moreover, MFIs use the threat of banning the entire group from future loans if one or more of the group members fail to repay. The idea is that group members will either develop team-support and help each other in case of default or pressure members inclining to strategically default. Joint liability thus mitigates the information and cost problems by inducing borrowers to behave in the interest of the MFI through peer screening (e.g. Varian, 1990; Ghatak, 1999) and peer monitoring (Stiglitz, 1990; Besley and Coate, 1995). By effectively transferring costs of screening, monitoring and enforcement from the MFI to the borrower, group lending helps MFIs to reduce lending costs (Armendáriz de Aghion and Morduch, 2000).

In practice however the performance of MFIs is mixed and how differences in success arise is not clear (e.g. Morduch, 1999). A central issue in group lending is the group formation process and the group types that arise. Some argue that groups are formed among members of similar risk profiles, known as homogeneous risk matching: safe borrowers strive to select safe partners, and risky borrowers end up with other risky partners (Varian, 1990; Ghatak, 1999). Assuming borrowers are fully informed about each others' types, a number of theoretical studies (e.g. Varian, 1990; Ghatak, 2000) show homogeneity is necessary for welfare improvements under group lending. The central argument underlying the homogenous risk matching hypothesis arises due to differences in the effective borrowing costs faced by risky and safe borrowers. Since risky borrowers fail more often than safe borrowers and assuming that the risky cannot fully compensate for the extra risk safe borrowers would have to bear when joining risky borrowers, it is optimal for borrowers of similar success probabilities to select each other (e.g. Van Tassel, 1999; Ghatak, 2000). As a result, any observed risk heterogeneity within groups must be due to matching frictions and not an optimal choice of borrowers. Matching frictions may arise due to unavailability of similar risk types or information asymmetry in finding a perfect match. It is this notion of homogeneity within groups that is considered as strong social capital to deal with lending problems that may otherwise arise. Likewise, many empirical studies (e.g., Wenner, 1995; Sharma and Zeller, 1997) take homogeneity as given.

Exceptions are Armendáriz De Aghion and Gollier (2000), Sadoulet (1999) and Guttman (2008) who argue heterogeneity can arise under different conditions and the homogeneity result is not always necessary for welfare improvements in group lending. Armendáriz De Aghion and Gollier (2000) conclude that group lending can be optimal with heterogeneity. In their model, cross-subsidization among relatively mobile, anonymous and heterogeneous urban group members acts as collateral to enforce loans without borrowers' *ex ante* full information about their partners and without requiring the homogeneity outcome. Van Tassel (2000) also finds heterogeneous matching in unobservable business characteristics for urban credit groups in Bolivia.

Sadoulet (1999) challenged the homogenous risk matching hypothesis by arguing that homogeneity is not optimal when borrowers operate in risky environments where insurance markets are missing and other mechanisms such as side-payments are endogenous in group formation. Allowing for endogeneity of such insurance scheme, he presents a theoretical model in which non-monotonic matching patterns arise: safer types match with medium-risk types; medium-risk types match either heterogeneously with safe types or homogenously with their types if no safe type is available; and risky types match homogenously with their risky types because they are too risky to be accepted in such insurance schemes. He assumes that medium-risk types can make side-payments of higher surpluses to safer borrowers (than other safe borrowers would) for the guarantee the latter provides in the event of the former's failure[2]. Sadoulet's heterogeneity outcome arises because his model considers repeated interactions among group members, whereas the homogeneity outcome in other theoretical models such as in Ghatak (2000) arises because of the static consideration. By explicitly incorporating dynamic incentives due to *repeated* interactions on which Sadoulet's (1999) result is based, Guttman (2008) theoretically shows homogeneity does not necessarily hold if earlier models such as Ghatak (2000) are extended to include dynamic incentives, mainly, the threat of not being refinanced if the group defaults. Intuitively, since future borrowing is denied if both of them fail, which is less likely for a safe borrower, a risky borrower is willing to pay more to have a safe partner than another safe borrower will be willing to pay. In direct contrast to Ghatak's (2000) arguments, Sadoulet (1999) and Guttman (2008) emphasize heterogeneity is more likely when side-payments between borrowers are feasible.

Empirical research is required to determine which of these two theoretical results, i.e. the homogeneous risk matching model (e.g. Ghatak, 2000) or the heterogeneous risk matching model (e.g. Sadoulet, 1999) holds in specific settings. Such knowledge is also of practical relevance to MFIs. If heterogeneity in group lending is indeed observed, an important implication is that it can improve welfare through enabling risk-sharing and insurance mechanisms via heterogeneous groups in addition to solving information problems.

[2] Note that heterogeneity is incentive compatible to a safe borrower as well because Sadoulet (1999) assumes the marginal effect on safe borrower's surplus caused by lowering the risk of her partner decreases with her own risk and, in the extreme, homogenous matching yields her zero surplus.

Therefore, MFIs operating in poor rural areas, such as in rural Ethiopia, where formal and informal insurance is limited would need to re-design their lending mechanisms taking heterogeneous matching processes into account. The only empirical studies done so far are by Sadoulet and Carpenter (2001) using data from urban retailers in Guatemala and by Lensink and Habteab (2003) using data from petty-traders with some farming activities in Eritrea. Both follow the theoretical work by Sadoulet (1999) to examine risk matching among group members and test the causality between individual risk and the level of risk heterogeneity in groups, accounting for endogeneity between choice of group risk and choice of individual (project) risk. Both find evidence that supports heterogeneity. While Sadoulet and Carpenter suggest heterogeneity might be in line with the missing insurance theory by Sadoulet (1999), Lensink and Habteab (2003) propose to have further research to investigate why heterogeneity arises.

Following similar methodological approaches as in Sadoulet and Carpenter (2001) and Lensink and Habteab (2003), this chapter examines the validity of the homogenous risk matching hypothesis among credit groups of a microfinance operating in a different socio-economic setting. Whereas the above mentioned studies focus on urban and semi-urban poor, the data used in this chapter comes from credit groups among smallholder farmers in rural areas in Tigray (northern Ethiopia). The specific objectives of this study are the following. First, to identify the factors determining risk heterogeneity in group formation of microfinance; second, to test the often assumed homogeneous risk matching hypothesis; and third, to investigate the link between social networks and group formation and repayment. The contributions of this study are twofold. First, the rural setting in Ethiopia gives an ideal environment to test the risk homogeneity hypothesis where credit groups are operating not only under missing insurance markets but side-payments are also limited due to relative covariance of risks. It also offers a good contrast to an urban environment where matching frictions are relatively higher due to mobility and anonymity. Moreover, in rural areas group formation processes may be endogenous to risk-sharing arrangements that rely on trust developed over the years through clubs and networks (e.g., religious gatherings) typical of poor economies where legal institutions play little role (Fafchamps, 2006). Therefore a second contribution of this study is that it provides new empirical insights into the underlying social processes behind risk heterogeneity and its implications for repayment using a unique data set from rural households in Ethiopia. In general this study contributes to the understanding of microfinance credit, which is widespread in Ethiopia, one of the poorest countries in the world.

Consistent with the previous empirical findings by Sadoulet and Carpenter (2001) and Lensink and Habteab (2003), we find strong statistical support for heterogeneity after accounting for matching frictions. While there is not enough evidence from our data to support the 'side-payment' intuition held in theory, we find some evidence on endogenous links between the group formation processes and traditional credit groups and trust-fostering religious gatherings operating in the study area. The implication is that if borrowers are allowed to match freely, group lending may serve to mitigate other missing markets such as insurance. Thus, MFIs operating under similar circumstances may need to consider such local specificities and underlying social processes when designing their financial products.

The chapter is organized as follows. Section 4.2 motivates the risk matching hypothesis and presents the empirical method used. Section 4.3 describes the data and the MFI where the data comes from, the *Dedebit Credit and Saving Institution (DECSI),* one of largest MFIs in Ethiopia. Section 4.4 explains measurement of risk and risk heterogeneity within groups. In section 4.5 these risk measures are used to estimate the effects of matching friction and optimal risk and subsequently the homogeneity hypothesis is tested. Results are compared to other studies. Implications of observed risk matching on repayment are also given. Conclusions are drawn in the last section.

4.2 Conceptual model and estimation strategy

The empirical strategy in this section follows the method first implemented by Sadoulet and Carpenter (2001) and adopted by Lensink and Habteab (2003). As discussed above the homogeneous risk matching hypothesis states that microcredit joint-liability groups are formed among members of similar risk types and that any observed heterogeneity in group formation is due to matching frictions. Empirically, if this hypothesis is valid, there must be no systematic relationship between members' (observed) individual choice of risk and their choice of (observed) level of group risk heterogeneity. With matching frictions, estimation is not straightforward because individual preferences determine optimal choices of own (project) risk as well as partners' risk simultaneously. Sadoulet and Carpenter (2001) deal with this problem by specifying the risk matching model as follows. First, in a frictionless world, the structural equations for level of risk heterogeneity, h_i^* and optimal own risk, θ_i^*, are specified as:

$$h_i^* = H(\theta_i^*, 0) \tag{1}$$

$$\theta_i^* = \theta\left(X_i, h_i^*\right) \tag{2}$$

where X_i represents the set of borrower *i*'s exogenous individual characteristics, and *H* and θ are functions for h_i^* and θ_i^*, respectively. Equation (1) indicates that the risk heterogeneity in borrower *i*'s group is a function only of the optimal risk level. According to equation (2) the optimal chosen risk depends on the borrowers' individual characteristics and the level of risk heterogeneity. Both equations can be combined to:

$$\theta_i^* = \theta(X_i, H(0,\theta_i^*) = Z(X_i))$$

If there were no matching friction, it is instructive to see that optimal own risk only depends upon individual characteristics and simultaneous determination of h_i^* and θ_i^* is not an issue. In practice, however, there is not a frictionless world and these frictions affect not only the desired level of risk heterogeneity but also the choice of own risk compared to a non friction situation. Let h_i denote the observed risk heterogeneity in the presence of matching frictions and let f_i represent a vector of variables causing matching friction. The structural equations for observed optimal risk heterogeneity and observed own risk θ_i in the presence of matching frictions, respectively, are then specified as:

$$h_i = H(\theta_i^*, f_i) \quad (3)$$

$$\theta_i = \theta(X_i, h_i) \quad (4)$$

And the reduced form equation for θ_i includes matching frictions:

$$\theta_i = \theta(X_i, H(f_i, Z(X_i)) \equiv K(X_i, f_i) \quad (5)$$

The fitted value of θ_i from (5) is used to estimate the heterogeneity equation (3) from which the homogeneous risk matching hypothesis can be tested by setting:

$$\frac{\delta h_i}{\delta \theta_i^*} = 0$$

If this null-hypothesis can be rejected in the presence of matching frictions f_i, it indicates that heterogeneity relates to optimal risk choice, rejecting the homogeneous risk matching hypothesis. The test requires measurement of observed risk, observed heterogeneity and a proxy for matching frictions (Sadoulet and Carpenter, 2001).

4.3 Group lending and clients in Ethiopia

To implement the model described in section 4.2, we conducted a survey among borrowing households participating in group based credit of the *Dedebit Credit and Saving Institution* (DECSI), a MFI operating in Tigray Region, Northern Ethiopia. This section gives the basic characteristics of borrowers included in the study. But, first a description of the practice of group lending in DECSI that unconventionally operates in remote rural areas of Tigray is given.

4.3.1 The practice of group lending in DECSI

Established in 1994, DECSI provides financial services to poor borrowers, mainly farmers. DECSI is the biggest institution operating in this economically marginal area where millions are living at subsistence levels with limited economic opportunities and high risk (Mehan, 2001). DECSI adopted the (Grameen style) group lending method since its inception. Small initial loans, up to ETB[3] 5000 are disbursed to jointly liable poor but "capable" groups of 3 to 7 members for self-employment purposes and income generation activities. Groups are formed voluntarily among credit users: members simply select their partners. DECSI accepts individual applications under the eligibility requirements that borrowers come in groups, are above 18 years old, and are not from the same family. Loan periods differ among activities: a maximum of two years (with yearly repayments) for agricultural loans and one year (with monthly repayments) for non-agricultural loans.

DECSI pursues group pressure and social sanctioning to enforce its loans. In principle, further loans are extended after the previous group loan is entirely paid back. Group credit exclusion follows for subsequent loans when a loan is in default for more than the due date. Sometimes exclusion leads to denying credit to an entire village or '*Tabia'* (lowest local administrative unit) if one or more groups do not repay. Under certain circumstances, such gross exclusion serves as a pressure for local leaders to get involved in enforcing defaults, which often takes the form of linking privileges to repayments. Such sanctions are more authoritative and effective tools of enforcement. In terms of outreach, DECSI has now 9 main branches with 96 sub-branches throughout Tigray and covers 91 per cent of the communities in Tigray (Borchgrevink et al., 2003). DECSI's interest rates range from 12 to 15 percent for credit depending on the nature of economic activity and 3 per cent for saving. The repayment rate of the regular loans for the period was 97.6% (DECSI, 2003).

4.3.2 The data

In 2003, we conducted a survey on 201 borrowing rural households that constituted 57 credit groups selected from 45 villages where DECSI operates. The sampling method used in selecting the sites was one of convenience sampling. Out of the 96 DECSI sub-branches throughout Tigray, six representative sub-branches in terms of socio-cultural, agro-ecology,

[3] ETB stand for the Ethiopian currency, 'Birr'; USD=8.34 *ETB* during the survey period.

market access, and proximity to the formal banking facilities in nearest towns, and sub-branch bank performance were selected.

Respondents were asked selected household characteristics and credit mechanisms, mainly, group formation, monitoring and enforcement. Respondents are characterized by subsistence traditional farming (see table 4A.1 in appendix) with average land holdings of 0.75 ha and a reported average annual income of ETB 2217 as well as poor asset compositions. Average family size is 5.47. Only in periods with good harvest or good business group members can on average fulfil their repayment obligations (see table 4A.2 in appendix). Reported data shows an average borrower is unable to cover repayment obligations in bad years, regardless the nature of the repayment schedule. This is different for households with diversified income sources such as petty trading and petty production (34%) viz. handcrafts and honey production. Regarding group dynamics, about 20 percent of members of current groups have been in another group before. One reason is that smaller groups are preferred over larger ones and following a recent change in DECSI's minimum group size policy from 5 to 3, average group size declined from 5 to 4. Interestingly, according to key informants in the study, social and traditional networks such as *Equb*[4] seem to facilitate the effectiveness of joint liability and its repayment rates in DECSI.

4.4 Measuring risk and heterogeneity

A crucial element in the empirical strategy is measuring individual borrowers' risk, which then can be used to estimate the level of risk heterogeneity among group members. Risk is however a latent variable and requires proxy variables that are often not readily available. Both Sadoulet and Carpenter (2001) and Lensink and Habteab (2003) calculate the difference between outstanding payments and savings made before the due date as a proxy for risk. In section 4.4.1, we adopt this method, modifying it to capture savings in our setting. Section 4.4.2, measures group heterogeneity based on the risk measure.

4.4.1 Measuring borrowers' risk

The risk measure based on outstanding payments and periodical savings from the project financed by credit assumes a one-to-one correspondence between project financing and repayment. In other words, borrower risk is taken equivalent to the risk of the project for which the loan is used. However, in a rural household setting where fungibility of credit use is common, borrower risk may not necessarily coincide with project risk. Other household

[4] *Equb* is an Ethiopian variant of a Rotating Saving and Credit Association (*ROSCA*).

income or liquidity (de)stabilizing factors may change household's credit risk. Specifically, the higher households' potential to secure the required liquidity for repayment before the due date, the lower the risk. A risk measure that accounts for such 'potential', which may also include the 'borrowing potential' of the household from its informal lenders (see e.g. Moll, 1989:25), is difficult to measure without errors. Nevertheless, we try to minimize this error by estimating the household's expected liquidity (L_i) at the due date. This is a composite of household specific liquidity sources, mainly cash savings, (expected) income from animal (product) sales, petty trade, off-farm work, any form of near liquid wealth like gold and other household assets that can be liquidated or held in balance by group partners, potential side-payments, and harvest income.

Following Sadoulet and Carpenter (2001) and Lensink and Habteab (2003), the risk index, θ_i, is thus constructed from L_i and outstanding loan repayment P_i as follows:

$$\theta_i = \begin{cases} \dfrac{P_i - L_i}{P_i}, & \forall\ P_i > L_i \\ 0, & \forall P_i \leq L_i \end{cases} \tag{7}$$

The index is censored at zero from below because those with values below zero (or negative risk) are those that have liquidity in excess of their current repayment obligation. Likewise, it is also truncated from above when L_i is equal to zero, which makes sense because the greater the positive margin between P_i and L_i, the higher the risk of the borrower, and the closer θ_i is to unity, and vice versa. Summary statistics of this calculated θ_i and the variables used to construct it are given in Table 4.1.

4.4.2 Measuring risk heterogeneity in groups

The next step is to measure the risk heterogeneity within groups using the calculated individual risk index. The Euclidean Distance measure is applied (similar to Sadoulet and Carpenter, 2001) to determine the degree of heterogeneity. Since DECSI's group size ranges from three to seven we use standardized average Euclidean distances to estimate h_i such that the potential problems of non-standardization and outliers in the heterogeneity measure are minimized.

Table 4.1 Summary statistics of heterogeneity and indicator variables

Variable	*Obs.*	*Mean*	*Std. Dev.*	*Min*	*Max*
Loan Size	201	1365.547	1066.389	100.00	5000.00
Repayment (P_i)	201	448.816	434.199	10.00	2816.00
Observed/actual risk (θ_i)	201	0.386	0.39	0.00	0.99
Mean of individual group risk ($\overline{\theta}_k$)	201	0.426	0.25	0.00	0.93
Group size (N_k)	201	3.960	1.17	3.00	6.00
Heterogeneity (h_i) (censored)	201	-0.009	0.65	-1.00	1.00

Censored risk (θ_i)=0, if θ_i <=0) (74 left-censored).

The average (standardized) Euclidean distance measure is given by (see e.g. Sharma, 1996:218):

$$h_i = \left[\frac{\sum_{i,j \in k} (\theta_i - \theta_j)^2}{N_k - 1} \right]^{1/2} \cdot sign\left(\theta_i - \overline{\theta}_k\right) \tag{8}$$

where θ_i is the risk index for borrower *i* calculated from (7); *k* is the group in which *i* and *j* are members and has group size N_k and a group mean risk of $\overline{\theta}_k$. The formula measures the weighted variance of an individual borrower's risk from partners' risk. The closer the individual index to the average group risk, the smaller the variation in risk and the more risk homogeneous the group is. The sign of the heterogeneity measure indicates whether *i*'s risk is above or below the group average. A value of zero indicates homogeneity and values close to -1 or 1 indicate heterogeneity. Table 4.1 presents summary statistics for risk and risk heterogeneity measures together. The average loan size for the period is 1366 ETB and the average periodical outstanding repayment 445 ETB. The average group risk is 0.39. Group sizes range from 3 to 6 with average 3.96. Heterogeneity is within the range [-1,1] with mean -0.009. This censored variable is used to test the heterogeneity/homogeneity hypothesis in section 4.5.2.

4.5 Empirical analysis, results and discussion

In this part, the empirical strategy motivated in section 4.2 is implemented. Section 4.5.1 describes how predictions for optimal risk and matching frictions are obtained from the following empirical specification for the reduced form risk model introduced in section 4.2 by equation (5):

$$\theta_i = X'\alpha + f'\beta + \varepsilon_i \quad (9)$$

It is discussed how variables of this specification are defined and how the equation is estimated. From these equation predictions for matching frictions ($\hat{f}$) and optimal risk ($\hat{\theta}_i^*$) are obtained that are used as regressors in the empirical counterpart of the heterogeneity model (equation (3) in the theoretical model):

$$h_i = \alpha + \gamma\widehat{\theta}_i^* + \delta\widehat{f}_i + \varepsilon_i^h \quad (10)$$

Section 4.5.2 describes estimation issues of this heterogeneity model and how it is used to test the homogeneous risk matching hypothesis. A specific point of attention is how to deal with potential endogeneity of the optimal risk variable that arises since proxy variables are used, introducing measurement error. The results of our analysis are also compared with findings from other studies. Based on the outcome of the homogeneous risk matching hypothesis test section 4.5.3 empirically investigates the link between social networks and repayment.

4.5.1 Risk estimation: matching friction and optimal risk indicators

Before we can test the homogenous risk matching hypothesis, we need to separately identify f and θ_i^* (i.e., risk without matching frictions) that are used in estimating the heterogeneity model (10). This enables to disentangle the risk heterogeneity into matching friction and optimal risk components. However, the challenge is that 'matching friction' is unobservable and that its many potential proxies might be multicollinear with X variables that explain θ_i^*. This problem is dealt with using Principal Component Analysis (PCA), which is often used to reduce multicollinearity and dimensionality (Sharma, 1996). We use PCA to select good proxies for f that are orthogonal by construction to the X variables in equation (9). Thus, factor loadings of individual variables onto particular components that by definition indicate X and f help to establish identification of X and f. The procedure is also exploited to reduce dimensionality without loss of information relevant to the analysis. Details of the PCA analysis can be obtained from the authors.

Based on the PCA factor loading, two sets of variables are identified. In line with common sense, the two factor loadings indicate that the variables that explain optimal risk (risk without friction), have to do with borrowers' ability to pay while the matching friction indicators are related with group formation and monitoring elements. The following variables are selected using PCA to proxy for optimal risk: *FEMALE* for female group, *ILITRT* for educational background, *INFRAST* for proximity to selected basic infrastructures, *FARMY* for main income source, *LANDSZ* for the size of land owned. Proxy variables for matching frictions, *f*, include *KNOWBFR*, *BORN*, *KNOWINCM* and *EQUB*. *KNOWBFR* captures group members' knowledge of group partners before the group formation takes place. *BORN* is whether or not a member is born in the village and represents familial and social ties of group members. *KNOWINCM* is about whether or not borrowers know the income of their partners. While both *KNOWBFR* and *BORN* capture the extent of information (a) symmetry crucial for screening, *KNOWINCM* is related to information on partners' overall status that is helpful for monitoring in post group formation. *EQUB* refers to participation in Rotating Saving and Credit association (*ROSCA*).

Once the proxies for matching frictions are known, the next step is to predict optimal risk and matching frictions from the risk model. We therefore estimate the reduced form risk model in (9) using the *X* variables and indicators for *f* as regressors from which both *f* and θ_i^* are predicted. Due to the censored nature of the dependent variable θ_i, a Tobit specification (Verbeek, 2008: 230-240) is chosen to estimate equation (9). Before predicting values for optimal risk and matching friction from (9), it is instructive to examine the relevance of the variables in the risk model. Results are presented in table 4.2. All parameters except for *ILITRT* have the expected sign and are significantly different from zero at 95% or 90% level.

Table 4.2 Tobit estimates of the risk model

Variable name	*Variable Description*	*Coefficients*	*St. errors*
Optimal Risk Indicators			
FEMALE	1 if in a female group, 0 otherwise	0.220**	0.092
ILITRT	1 if borrower is illiterate, 0 otherwise	0.120	0.085
INFRAST	Mean distance to basic infrastructure	0.020**	0.005
FARMY	1 if main income source is farming, 0 otherwise	0.453**	0.124
LANDSZ	Total land size (in hectares)	-0.158**	0.072
Matching Friction Indicators			
BORN	1 if born in the same village, 0 otherwise	-0.200*	0.108
KNOWINCM	1 if borrower knew income of partners, 0 otherwise	-0.133*	0.076
EQUB	1 if participating in *ROSCA*s	-0.403**	0.104
KNOWBFR	1 if borrowers knew each other before group formation, 0 otherwise	0.226*	0.126
Intercept	Constant	-0.331**	0.162
N		186	

(**) significant at the 5% level, (*) significant at the 10% level.

Log likelihood = -117.32366, Prob > chi2 = 0.0000. Pseudo R2 = 0.2941.

Including quadratic terms for *FARMY* and *AGE*, as proposed by Lensink and Habteab (2003), does not improve our specification. Parameters for *FARMY*, *LANDSZ* and *INFRAST* are also highly significant with the expected sign, indicating that being dependent on farming as the main source of livelihood, farming on small size land and living further away from social infrastructure, respectively, increase risk. In DECSI, women groups are encouraged. *FEMALE* represents a women group and being a female group increases risk, which may have to do with their relatively low control over household resources. Being born in the same village *(BORN)* and knowing the annual income sources *(KNOWINCM)* reduces matching friction. As expected, knowledge of each other's income status helps to select the best match. Participation in the traditional Ethiopian *Equb* (*ROSCA*s) also helps to get to know integrity and reduces matching friction. In fact, it is discussed later in section 4.6 that this network is essentially part of households' borrowing structure and there are indications that it is synchronized with group lending. As indicated earlier, predicted values for optimal risk and matching-frictions are obtained from (9) such that $\widehat{\theta}_i^* = X'\widehat{\alpha}$ and $\widehat{f} = f'\widehat{\beta}$, respectively. In the next section, these values are used to estimate the risk heterogeneity model.

4.5.2 Testing for risk homogeneity in groups: results and discussions

Next, equation (10) is estimated using the heterogeneity measure calculated in section 4.4.2 as the dependent variable. As indicated earlier, this measure proxies how far individual borrowers' observed risk varies from their partners'. The main interest in this chapter is to investigate whether the observed heterogeneity is entirely explained by matching frictions or whether optimal risk also plays a role, violating the homogenous risk matching hypothesis. Again, because of the censored dependent variable, we estimate a Tobit model of the reduced form equation in (10). To see if the censoring of the heterogeneity variable has an effect on our distribution, the uncensored heterogeneity measure is also estimated using OLS.

A problem that was also noted by Sadoulet and Carpenter (2001) is measurement error in the predicted values for optimal risk and matching friction. If measurement error in these proxy variables is present and substantial, it leads to biased estimates. Since any proxy variable introduces some measurement error, the question is basically how good these proxies are. Since our four indicators for matching frictions all indicate knowledge on (potential) partners we think that a prediction based on these four variables gives a good proxy for matching frictions with minimal measurement error. Also note that the test on the homogeneous risk matching hypothesis is performed on the parameter γ for optimal risk in equation (10). Therefore, in testing for this endogeneity problem using a Durbin-Wu-Hausman test (Verbeek, 2008:144) and potentially solving for it we concentrate on the optimal risk proxy $\hat{\theta}_i^*$.

Testing and solving the potential endogeneity problem due to measurement error requires a set of variables that can serve as Instrumental Variables that are both valid and not weak. Validity implies that potential instruments are not correlated with the measurement error or measured with error themselves. Instruments are considered not to be weak when they strongly correlate with the variable to be instrumented, reflected in an F-test value exceeding 10 in the first-stage regression (Murray, 2006). The first requirement cannot easily be assessed and requires careful selection of instruments. Based on these considerations we choose two variables as instruments, i.e. households' self evaluations of their wealth status compared to people in their neighbourhood and their spread in agricultural plots. The first instrument indicates household's perceived ability to cope with risk and equals one if the household perceives it has a good wealth status and is zero otherwise. Spread in agricultural plots is also related to risk since one way of diversifying production risk (e.g. due to rainfall, flood, locust) in these areas is by having plots in different geographical locations. This instrumental variable indicates the extent of household's agricultural plot diversification and is equal to one if less-diversified and zero otherwise. Both variables are easily measured without errors, while at the same time *not* correlated to potential errors of the heterogeneity (dependent) variable. Regressing the first-best proxy for risk $\hat{\theta}_i^*$ on these two variables indicates that both are significantly related at the 5% level with an overall F-test value of 48.67, indicating that these instruments are not weak and can be used as instruments and to perform the Durbin-Wu-Hausman test. The Durbin-Wu-Hausman test indicated that the null hypothesis of exogeneity of the first best risk proxy $\hat{\theta}_i^*$ could not be rejected (p-value 0.88), indicating that measurement error is not a problem for this proxy variable. Results of Tobit, OLS and the (less efficient) IV estimation of equation (10) are given in table 4.3.

Estimates of the Tobit, OLS and IV models all show that the parameters for matching frictions and optimal risk are significantly different from zero, indicating that the observed heterogeneity is explained by both matching frictions and optimal risk choice. In fact, under all estimators, the optimal risk parameter is highly significant.

Table 4.3 Tobit estimates of risk heterogeneity

Variables	*Tobit*	*OLS*	*IV*
Optimal Risk $(\hat{\theta}_i^*)$	0.738 (0.131)**	0.679 (0.119)**	0.706 (0.232)**
Matching Friction $(\hat{f})$	1.042 (0.469)**	0.981 (0.486)**	0.997 (0.442)**
Intercept	-0.356 (0.070)**	-0.324 (0.078)**	-0.338 (0.124)**
Tobit, F(2,199); OLS, F(2,198)	16.09**	16.35**	5.56**
N	201	201	201

(**) significant at the 5 % level. Standard errors in parentheses.

However, matching frictions also matter. This implies, contrary to the commonly held view, that credit groups in group lending are formed among members of different risk types not only because members fall short of finding their perfect match but also because it is to their best interest to do so. Our result does not come as a surprise, given similar previous findings by Sadoulet and Carpenter (2001) and Lensink and Habteab (2003). Using data from a rural area our results supplement these findings and provide more empirical support to the theory posed by Sadoulet (1999) that states that risk heterogeneity might also be an optimal choice instead of being due to matching frictions.

OLS results indicate that the censoring has some effects on parameter estimates but not on their significance. IV estimates are rather similar to the OLS results, as was expected since the Durbin-Wu-Hausman test already showed absence of endogeneity due to measurement error. In view of the theoretical (e.g. Armendáriz De Aghion and Gollier, 2000) and empirical (e.g. Van Tassel, 2000) insights (see section 4.1), and given that our data comes from a rural setting where borrowers are expected to be less mobile with observable economic activities and anonymity is less likely compared to an urban setting, the result presents interesting as well as challenging questions regarding credit group formation. An important question is, apart from matching frictions, why groups are formed risk heterogeneously rather than risk homogeneously. In line with Sadoulet's (1999) and Guttman's (2008) theoretical insights, Sadoulet and Carpenter (2001) find some evidence from their (semi-) urban setting that this might be due to missing insurance markets in the area. They suggest that it may be optimal for those looking for insurance due to risk of failure and loss of future borrowing to arrange side-payments (e.g., labor) with those that are able to cover all group debts in case of failure. Therefore, our survey also included questions on such forms of side-payments among group members. However, only four households in the sample reported having such exchanges with group partners. This is small compared to those who had repayment problems, when such an arrangement could have helped to solve it immediately. Thus, based on respondents' direct reports, there is not enough evidence to conclude the insurance-side-payments claim holds among our borrowers.

Observation from key informants during the survey however reflects group members often engage in some form of traditional networks such as *Equb* (ROSCA) or other traditional and religious gatherings (e.g., *tsebel* or *mahber*)[5]. The descriptive analysis also shows nearly 40 percent of the respondents who are members of *Equb* are at the same time members of a credit groups. In fact, such networks are part of the group formation processes and provide the foundation for establishing trust or are in some way linked to borrowing and saving practices of borrowers. In several of the sample sites, particularly those closer to the village towns, borrowers engaged in petty trade activities reported they often synchronize their group credit

[5] *Tsebel* and *mahber* are religious gatherings, not necessarily, of the same neighbourhood or socioeconomic status, that include monthly sainthood-services and festivities common in Tigray; reciprocal in nature, where every participant is duty-bound to serve.

repayments to the allocation of the '*Equb* pot'. Studies (e.g. Besley et al., 1993, 1994) show that the ROSCA pot can be allocated either by random (drawing lots) or by bidding. A different type of the ROSCA in which allocation of the pot is made 'by consensus' among members is witnessed in the study area. The explanations for the existence of this type of ROSCA are beyond the scope of this chapter. However, it may be that the 'consensus' is explained by cooperative behaviour and trust developed over the years more than the 'insurance-side-payments' claimed to be associated with wealth or risk differences observed in credit groups. In the next section, we examine if these elements of social networks can partially explain the probability of individual borrowers' repayment, which in turn provide insights into the role they play in credit groups.

4.5.3 Social networks, group formation and repayment: any link?

The high repayment rate of successful MFIs is often attributed to the joint-liability in group lending. The notion that group lending is key for repayment success is associated with one of its unique features that induces borrowers to act responsibly through out the lending process, which yield desired outcomes for the lender. One desired outcome is assortative matching, which leads to homogenous matches and helps to separate risky and safe borrowers, so that the lender is able to provide discriminatory interest rates, a process that maximizes aggregate welfare (Ghatak, 2000). But, if the outcome of group formation is not homogenous any more, but rather heterogeneous, do elements of group formation matter for repayment? If not, given heterogeneity, what explains the high repayment rate of 97.6 per cent in DECSI in 2003? Can simultaneous participation in social networks and group lending tell something about repayment in heterogeneous groups?

To investigate these follow up questions, we estimate a logit model taking indicators for peer screening, peer monitoring or social pressure, trust and networks, and household characteristics as regressors. Information about variables used is given in the appendix. The dependent variable is one for group members that had unresolved repayment problems after due date (26%) and zero otherwise[6]. Table 4.4 reports the logit model estimates. Three variables (*ACTKNOW, OPT_GTYP,* and *GSZ_NOW*) are included to proxy peer screening. *ACTKNOW* captures knowledge on activities of partners before group formation, which is useful information to discern partners of their choice. *OPT_GTYP* captures the degree of optimality of the realised matches. *GSZ_NOW* measures effects of group size and is indeterminate *apriori.*

[6] Self-reported claims related to repayment problems have been cross-checked with local branch officers.

Table 4.4 Logit estimates of factors explaining repayment problems

Variable	*Description*	*Coefficients*	*St. errors*
	Elements of peer screening, monitoring and peer pressure		
ACTKNOW	1 if borrower knew other members' activities	-0.290	(0.857)
OPT_GTYP	1 if borrower thinks s/he joined its "optimum" group	0.242	(0.539)
GSZ_NOW	Current group size	0.093	(0.170)
GAGE	Age of the group (in months)	-0.144*	(0.082)
AVDSTNC	Distance (in KM) between major economic aactivities of group members	0.132*	(0.078)
MYOBLGN6	1 if repay only because of moral obligation	0.387	(0.424)
WPENALTY	1 if borrower feels asset confiscation /prison is worst penalty of upon default	0.785*	(0.474)
	Elements of trust/social networks		
EQUB	1 if participated in networks (e.g., *Equb, Edir, Tsebel, etc)* with one or more group members	-1.464*	(0.759)
OPTIONS	1 if other (informal) sources of credit exist	0.759*	(0.440)
FEMALE	1 if female group	-0.062	(0.469)
	Borrower characteristics		
LnLASTLOAN	(log)loan Size in last cycle	0.342	(0.313)
HHSIZE	Household size	0.031	(0.102)
LANDSZ	Total land size in hectares	-0.236	(0.310)
LnAVERGY	Household's average annual income (values)	-0.595**	(0.202)
ILITRT	1 if the respondent is illiterate	0.566	(0.480)
	The constant term	-0.219	(2.464)

Log pseudo-likelihood = -88.118953; Pseudo R2 = 0.1535; N= 176; (**) significant at the 5% level, (*) significant at the 10% level.

Larger groups may have more chances to dilute risk but may also increase chances of free-riding. None of these peer screening indicators is significant however. Peer monitoring is proxied by average distance between major economic activities of members and age of the group. Both indicators are statistically significant. Social pressure and enforcement is captured by whether or not the borrower feels obliged to repay either legally *(WPENALTY)* or morally (*MYOBLGN6)*. While *WPENALTY* is statistically significant and *MYOBLGN6* is not. It appears that borrowers feel more legally obliged than they are morally obliged.

Variables *EQUB, OPTIONS, FEMALE* are also included to explain social networks and associated trust. Except *FEMALE,* which tracks whether or not a group is female or male group (because DECSI's group lending is gender based), the rest two are statistically significant (with the expected sign), which support our earlier observation that social networks play crucial roles in resolving repayment problems and perhaps heterogeneity through the complex social processes among members. Note that availability of other borrowing options tends to aggravate the probability of facing repayment problems significantly. This is unexpected result but may signal that networks are not necessarily useful to the MFI. Lastly, it is important to note that of all the general household variables only (natural log of) income is statistically significant.

In sum, while many variables have the predicted parameter signs, only few, mainly monitoring and trust indicator variables significantly explain repayment problem. This may imply that there are some peer monitoring activities even when groups are heterogeneous in risk types and homogeneity is not a necessary outcome for monitoring to take place. Moreover, the non-significance of peer screening variables may indicate that screening might have taken place (such as during formation of *Equb*) long before credit groups are formed.

4.6 Conclusions

This chapter investigated the empirical relevance of the homogeneity hypothesis commonly held in the group lending literature. It also attempted to investigate the effects of the observed group risk structure on repayment. The data comes from credit groups in rural Tigray (northern Ethiopia). As first step in the analysis, risk is indexed from a comparison between outstanding repayments and ability to repay and a risk model is estimated. Being in a women group, non-proximity to infrastructure, and having farm income as main income source, *ceteris paribus*, increase risk of default. But, larger land area, participation in *Equb*, knowing each other's income, and village acquaintance reduce the probability of default risk.

Controlling for matching frictions, the empirical test on risk matching behavior of group members, after accounting for matching frictions, rejects the hypothesis that group members form homogenous groups in equilibrium. This suggests that heterogeneity is an important feature in credit groups and plays a role in group formation. The explanation given for the heterogeneity observed is not found to be consistent with the missing insurance argument. Rather, this chapter finds that networks linked with credit groups may help to build trust among group members over time. The link between formal and informal financial intermediation, specifically the link between credit groups and participation in the Ethiopian version of *ROSCA, Equb,* is one important finding in this chapter. Borrowers take the advantage of established networks through the years not only to smooth out information problems, develop reputation among each other or take advantage of the long-time accumulated experience on which they can count to form credit groups (regardless of homogeneity) but may also use credit groups as a reputation indicator strengthening the *ROSCA* stability.

The empirical evidence does not support the notion that homogeneity is a necessary condition for peer screening and peer monitoring to take place effectively. In fact, we find evidence that particularly peer monitoring reduces the probability of default even if groups are organized heterogeneously. The link with peer screening seems indirect. Peer screening in many cases takes place long before the establishment of credit groups because other informal networks function as building blocks for credit groups. This is implied through participation in other networks (like, *Equb*) that are linked with DECSI's credit.

A final note is that compared to previous similar studies, our sample comes from a remote rural socio-economic environment where other formal borrowing options are scarce and access to credit is limited. Nevertheless, results are consistent with findings from urban settings. It appears however that the underlying rationale for the findings is different than in these other studies. In our case, the absence of other borrowing options which often give way for traditional financial and insurance networks to play a greater role seem at the same time to set the grounds for microfinance group formation and credit transactions. The policy implication is that MFIs operating under similar circumstances may need to consider such socio-economic specificity when designing their financial products. Moreover, there is a need for further research on the link between formal credit groups and *ROSCAs* or other informal networks existing in rural communities. This chapter found evidence that this link exists but this is not well addressed in the microfinance literature.

Appendix 4A. Borrower characteristics, group formation and dynamics

Table 4A.1 Socio-economic characteristics of group members

Variable	*Mean*	*Std. dev.*
Age	42.0	12.09
Household size	5.47	2.14
Land size (in *'tsimdi'*) (*LANDSZ*)	3.00	2.94
Average Annual Income *(AVRGEY)*	2288.31	2794.67
% Female (*FEMALE*)	32.3	
% Single Respondents	19.5	
% of households members that earn an Independent Income	10.4	
% using irrigation	19.4	
% of households employed as 1st income source is:		
Farming	63.18	
Employment with fixed salary	1.49	
Daily labourer	3.98	
Service giving	4.48	
Petty trade	19.90	
Others	6.97	
% Households perceived their wealth status as:		
Extremely Poor	3.0	
Medium Poor	50.2	
Poor	40.8	
Rich	5.0	
% Educational level:		
Illiterate *(ILITRT)*	50.7	
Just read and write	14.0	
Primary school complete and Above	35.3	
N=201		

Table 4A.2 Borrowing, group formation and saving characteristics of group members [a]

Characteristic	*Number*	*Mean*	*Std. de*	*Min*	*Max*
Age of the group (in Months) *(GAGE)*	190	36.18	28.97	1	108
Group size at establishment	201	5.35	1.40	3	7
Group size at survey time	198	4.40	1.20	3	7
Optimum group size	196	2.98	1.28	2	7
Amount of loan applied for	197	580.8	1597.40	200	10000
Actual borrowings:					
last loan cycle (DECSI)	170	1255.6	843.70	100	5000
Present loan cycle (DECSI)	196	1378.4	1028.40	100	5000
Savings in Cash/Liquid form:					
Equb, Edir, DECSI, etc	158	1042.3	2906.3	2	25500
Only in DECSI	189	235.4	390.7	18	3000
In other forms	14	1012.7	2629.9	70	10000
Expected saving at due date when payment is every end of month[c]:					
Good harvest/business times	59	496.5	771.5	20	3,600
Bad harvest/business times	48	198.0	357.7	0	2,000
Expected saving at due date when payment is every end of year[c]:					
Good Harvest times	119	845.6	1,361.9	0	10,000
Bad Harvest times	119	0.0	0.0	0	0
Access to other sources of credit (%)[b]	*Number*	*%*			
yes	44	24.0			
no	139	75.9			
Faced repayment difficulties at least once					
yes	141	73.44			
no	51	26.56			
Defaulted at least once					
yes	173	86.07			
no	28	13.93			
Borrower thinks s/he joined optimum group type					
no	33	16.58			
yes	166	83.42			
Which form of credit arrangement do you like to have?					
group credit	31	15.42			
individual credit	161	80.10			
don't mind	9	4.48			

[a]Amount in ETB. [b] include borrowing potential from money lenders, relatives, friends and remittance
[c]valued in 2003 local market prices

CHAPTER 5

EVALUATING THE LONG-RUN CAUSAL IMPACT OF MICROFINANCE CREDIT USING PANEL DATA ECONOMETRICS: A CASE STUDY FROM ETHIOPIA[1]

Abstract: *This chapter evaluates the long-term impact of microfinance credit from the intensity of participation in borrowing. We use a four-round panel data set on 351 farm households that had access to microfinance in northern Ethiopia. Over the years 1997-2006, with three-year intervals, households are observed on key poverty indicators: improvements in annual consumption and housing improvements. The relatively long duration in the panel enables to measure household poverty changes between consecutive periods and see the long-run effects of exposure to microfinance from the intensity of participation in borrowing. The fixed-effects model is innovatively modelled to account for potential selection biases due to both time-invariant and time-varying unobserved individual household heterogeneities. Results show that microfinance borrowing indeed causally increased consumption and housing improvements. A more flexible specification that allows for the number of times the household has been in borrowing also shows that repeated borrowing is effectively increasing consumption: the longer the borrowing relationship, the larger the effect partly due to lasting credit effects. Impact estimates that do not account for such dynamic effects may therefore undermine the effect of MFI borrowing.*

Key words*: Microfinance, treatment effects, trend model, panel data*

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[1] Paper by Guush Berhane and Cornelis Gardebroek submitted to *American Journal of Agricultural Economics.*

5.1 Introduction

The microfinance revolution got considerable momentum around the world in the last two and half decades. The potentials of microfinance as an effective tool to break the vicious circle of poverty have been widely voiced. As a result, several microfinance schemes have gone operational around the world, providing financial access to millions of poor people both in rural and urban areas. Important questions are however if and to what extent microfinance credit over its long time existence has contributed in reducing poverty.

Despite efforts to measure this impact, evidence on the poverty reduction effects of long term microfinance credit remains unclear mainly due to the difficulty of measuring counterfactual outcomes and the lack of follow up data spanning over sufficiently long periods to measure the impact. Without experimental designs, evaluations based on simple comparisons between participants and non-participants are subject to biases from two sources (e.g., Pitt and Khandker, 1998; Ravallion, 2001). The first bias is due to program placement and occurs because microfinance institutions (MFIs) do not randomize over villages to place programs. They often choose on village characteristics that may not be observable to the researcher. The second bias is due to the tendency of individual borrowers to self-select into programs. From the nature of borrowing it is evident that potential applicants can choose themselves to apply for a loan. When selection into the program is based on unobservable individual attributes (e.g. entrepreneurial ability) that simultaneously affect the impact outcome, attributing observed differences to credit gives biased impact estimates.

But even if pre-designed experimental or quasi-experimental designs that randomize over potential sources of selection are implemented, estimates based on one-shot observations may fall short of capturing the complete picture because longer periods may be required before the full effects from credit are realized (Karlan and Goldberg, 2007). A recent review of the evaluation literature emphasizes the issue of 'timing and duration of exposure to programs' is as important but relatively less studied than the identification problems that often attract much of researchers' attention (King and Behrman, 2009). Long period data is, however, costly and largely unavailable. As a result, most studies so far (e.g., Coleman, 1999; Pitt and Khandker, 1998) exploited program specific designs and employed innovative quasi-experimental survey methods to generate control and treatment groups from cross sectional data. A few exceptions are Khandker (2005), Copestake et al. (2005) and Tedeschi (2008) who used two-period data to estimate impacts. Long-term panel data, under certain conditions, allows to measure impact from *intensity of participation* over time by overcoming selection biases. An attractive feature of panel data is the possibility to deal with unobserved *time-invariant* individual and village heterogeneity using fixed-effects. However, when the selection processes is based on time-varying unobservables, such as individual motivation which is likely to change over time and borrowing status, standard panel data methods like fixed-effects and difference-in-difference are biased (Armendárize de Aghion and Morduch, 2005: 210). Other less frequently used panel data techniques such as *random trend*, and *flexible random trend* models offer alternative approaches to mitigate this problem by allowing an arbitrary correlation between time-invariant unobservables as well as individual trends in time-varying unobservables to program participation (Wooldridge, 2002: 317).

This chapter uses unique four-round household survey data covering 1997-2006 to estimate the impact of participation in microfinance credit on annual household per capita consumption and housing improvements. The data comes from sixteen villages in northern Ethiopia. We first investigate the impact of credit using fixed-effects approaches that is standardly applied to account for time-invariant individual as well as village unobservables. Further, we use variants of the random trend model due to Heckman and Hotz (1989) that mitigates both time-invariant and individual trends in time-varying unobservables. We find that program credit has significant impact on household consumption and housing improvements of participants compared to non-participants. However, compared to the random trend approach, results from the standard fixed-effects approach that does not account for individual trends in time-varying unobservables overestimates credit impact. We also model program credit more flexibly by including the effect of loan-cycles and individual specific trends and find that credit impact on per capita consumption increases with frequency of borrowing. The effect of borrowing on the probability of housing improvement is realized after one-cycle but declines sooner after the third cycle borrowing. From the flexible approach, we conclude that borrowing effects last longer than one-period and cumulative effects are best captured the longer the time covered in the analysis. Besides, while household borrowing effects are multidimensional and cannot be captured by a single household outcome, we also conclude that effects on household outcomes are not monotonic over time. Impact estimates that do not account for such dynamic effects may therefore underestimate the effect of MFI borrowing.

The rest of the chapter is organized as follows. Section 5.2 provides a brief review of the main approaches followed in the literature on impact assessment. Section 5.3 describes the nature of the data and section 5.4 presents the empirical method used. Section 5.5 provides the estimation results and section 5.6 concludes.

5.2 A review of microcredit impact studies

This section presents a brief survey of the main methodological approaches of mitigating selection bias in microfinance impact evaluations.

Measuring the impact of microcredit programs is a challenging task because establishing 'causality' between credit effects and changes in the outcome of interest is complicated by the well known problems of self-selection and program placement biases that are inherent in such programs (e.g., Pitt and Khandker, 1998). Self-selection is a problem because, compared to non-participants, participants may already have initial advantages such as better entrepreneurial ability that can translate into higher outcome variables, even without credit. Using data from a Peruvian MFI, Tedeschi (2008) finds that "*selection into credit programs is a substantial problem: those who will eventually become borrowers have significantly higher incomes than those who will not become borrowers"*. The main challenge is therefore to address the counterfactual question 'how would participants have performed in the absence of program credit or 'how would non-participants have performed had they participated in the program'.

MFIs may also design their credit programs to fit into specific villages or specific groups and screening may be based on criteria that influence outcomes of interest. Self-selection and program placement decisions in principle do not pose problems if they are based on known and measurable variables, because then they can be easily controlled for empirically. The problem is however that these decisions are often based on unobservable variables. In the absence of "comparison" and "treatment" groups, credit impact assessments that do not account for these problems are likely to be biased (Armendáriz de Aghion and Morduch, 2005:200-223; Tedeschi, 2008).

How microfinance impact studies have dealt with these problems varies. One strand of literature that is common among MFI practitioners simply compares existing clients ('treatment group') with new entrants ('control group'). Although simple to implement, this method is criticized for attributing the mean difference between the two as impact without dealing with selection problems (Tedeschi, 2008).

A second strand of literature that relies on cross sectional data deals with the selection problem employing instrumental variable and quasi-experimental techniques that exploit the nature and timing of program designs. One of the earliest and most cited studies in this line is by Pitt and Khandker (1998) who used cross-sectional data from Bangladesh and employed a quasi-experimental survey design to instrument nonrandom program placement and self-selection. However, such instrumental and experimental designs are often coincidental and difficult to replicate. Moreover, these approaches assume that the initial conditions of control and experiment villages are identical. A final problem is often that it is difficult to come up with strong and valid instrumental variables.

An ideal credit impact evaluation would have been one that compares effects with and without the program. A third approach that received considerable attention in recent microfinance evaluation is a pre-designed randomized experimental approach (Karlan and Goldberg, 2007). Experimental designs that randomize over observable and unobservable attributes of participants and non-participants would, in principle, provide unbiased estimates. Such designs are however time consuming and costly to undertake. Besides, it can be difficult to implement on ethical and political grounds (Heckman and Hotz, 1989).

A fourth strand of recent literature uses panel data to mitigate the biases present in cross-sectional studies. Assuming strict exogeneity between selection variables and time-varying unobservables that could affect the outcome of interest, fixed effect panel data methods can provide consistent estimates by differencing out time-invariant unobserved individual and village effects (Wooldridge, 2002: 637). Khandker (2005), Copestake et al. (2005) and Tedeschi (2008) relied on this assumption and used a fixed-effects approach to analyze the impact of credit. The fixed-effects estimator is however critically dependent on this strict exogeneity assumption, particularly on the assumption that the time-invariant heterogeneity is the only potential source of selection bias. Literature in empirical labor economics that studies the effect of labor-training programs on earnings under nonrandom program assignment extends the evaluation literature by allowing for individual heterogeneity to vary over time according to a linear trend (Heckman and Hotz, 1989). This approach is used in this study and is explained in more detail in section 5.4.

5.3 Brief description of the MFI, survey design and the data

Data used in this study comes from rural households in northern Ethiopia where a microfinance program, Dedebit Credit and Saving Institution (DECSI), provides financial services only for production purposes. Although DECSI, under the auspices of a local NGO, started providing credit services in few trial villages since 1994, it officially launched credit and saving programs in 1997 and expanded quickly into almost all villages in Tigray. By 2000, it extended loans to 1.4 million rural households with total outstanding loans of 447 million ETB and savings of 74 million ETB[2]. As of 2002, DECSI covered more than 91% of the villages in the region and extended to about half a million borrowers (Borchgrevink, et al., 2003). Initially DECSI provided Grameen style joint liability based credit mostly used for farm inputs, which eventually diversified into micro and small enterprise loans and other off-farm activities. Loans are extended once a year because production is largely monsoon rain dependent, and depending on activity, mature between 6-12 months.

Table 5.1 Households' participation and changes in borrowing status over survey years

	Number of times borrowed up to the survey year				
Survey year	*Never*	*Once*	*Twice*	*Thrice*	*Always*
1997	140	211	-	-	-
2000	87	182	82	-	-
2003	61	143	112	35	-
2006	40	102	130	46	33

Source: Survey data (1997- 2006)

[2] ETB stands for Ethiopian currency, 'Birr'; (annual average) USD conversion rate of 6.32 ETB in 1997 and 8.94 ETB in 2006.

In 2003, DECSI started individual loans packaged with some specific farming activities such as bee-keeping and milk production activities. Loan maturity in this latter loan product ranges between 1-2 years. In this study, participation in borrowing is defined as being in a borrowing relationship with DECSI in the year preceding the survey and no attempt is made to make a distinction between the different loan products provided.

Four-rounds of surveys with three-year intervals (1997-2006) were administered on randomly selected 400 borrower and non-borrower rural households. The dataset covers household- and village-level information ranging from household characteristics, consumption, assets, credit and savings to village infrastructure, markets, and credit contracts. Asked about access to credit in 1997, only a few respondents indicated that they were ineligible to borrow mainly due to old age and physical unfitness, which DECSI implicitly considered as selection criteria[3]. These are excluded from our analysis. Respondent attrition was minimal, mostly related to the Ethio-Eritrea border war, which started in 1998 and ended in 2000. This chapter is thus based on a balanced panel of 351 households, out of which 211 borrowed and 140 did not in the 1997 survey. Table 5.1 gives a summary of the evolution of borrowing status over time. Borrowing status changed in subsequent years with some households joining, while others dropped out. In general, there were 33 households that borrowed in all four periods and 40 that never did. The other households borrowed at least once in one of these years but also had years without a loan.

An advantage in this data set to study impact is that the first survey coincided with the massive expansion of DECSI into most villages in the region, which gives the opportunity to identify impact using the 1997 as baseline information for both borrowers and non-borrowers. Moreover, due to the government's as well as donors' inherent interest to synchronize credit services with the regular input extension programs that was running through out the region, there is little reason to believe DECSI's quick and massive branching out to villages has been systematic and endogenous to village outcomes. All residents were, in principle, eligible to branches available in the nearest rural town. E.g., credit was available for all in the most nearest-to-town villages as well as remote villages in 1997. However, households may have self-selected into credit and participation can be endogenous at individual level, which we explicitly tackle in the empirical analysis.

Although credit is given for productive purposes (e.g. fertilizer, oxen), eventually this will lead to higher per capita consumption. Our survey interval of three years is considered as an advantage in this respect, since this higher consumption is expected to materialize in years after having experienced higher output due to increased input use made possible by borrowing.

[3] We also test if there was no significant difference between participant and non-participant groups in the base year in terms of our outcome variables.

Table 5.2 Summary statistics of household per capita annual consumption and housing improvements

Survey years	1997	2000	2003	2006
Participants	211	135	126	160
Annual per capita consumption				
Mean	442	683	651	1422
Std. Dev.	523	503	371	1051
Housing improvements				
Mean	0.033	0.193	0.429	0.594
Std. Dev.	0.180	0.396	0.497	0.493
Non-participants	140	216	225	191
Annual per capita consumption				
Mean	371	675	577	1087
Std. Dev.	215	543	496	715
Housing improvements				
Mean	0.027	0.042	0.102	0.115
Std. Dev.	0.167	0.200	0.304	0.320

The time lag needed to translate borrowing into outcomes also strengthens the usefulness of the first- round survey as baseline information to identify impact. We measure credit impact on two welfare indicators in Tigray, i.e. annual household consumption and housing improvements. Household consumption is a continuous variable and housing improvement is a binary indicator. Households were asked if they had improved their roof to corrugated- sheet of iron anytime between the last and the present survey year. Household consumption is aggregated from food and nonfood consumption of selected items, both from own sources or purchased over a period of one year. Necessary adjustments are made to make measured items and units comparable over the survey years. A consumer price index for the region is used to adjust for price changes over time (Central Statistical Agency of Ethiopia, 2008). To minimize measurement error from age structure heterogeneities among households, per capita adult consumption is used. Summary statistics of indicators are presented in table 5.2.

In general, compared to non-participants, an average participant enjoyed higher per capita consumption levels and more often improved her house in all years observed. Note however that average outcomes in table 5.2 are based on participation or non-participation status in each survey year, i.e., regardless of previous status. We take such contamination effects into account in our econometric modeling and estimation. Moreover, the table doesn’t indicate whether higher consumption and housing improvement can be ascribed to borrowing or whether they have increased due to other factors

5.4 Empirical Methodology

In this section the origins of selection bias in estimating impact from long term panel data and the panel data techniques to control for it are discussed. Consider the following generic specification for program evaluation:

$$C_{it} = X_{it}\beta + prog_{it}\gamma + M_i\alpha + u_{it} \qquad t = 1, 2...,T; \quad i = 1, 2,...N \tag{1}$$

where the outcome variable consumption, C_{it} for household i at time t, is determined by a vector of observable household-, village-, and MFI-level characteristics X_{it}, a binary program participation variable, $prog_{it}$ (=1, if participated in borrowing at t, zero otherwise), and a vector M_i of time-invariant unobservable variables[4]. Borrowing in turn depends on a set of observable (Z_{it}) and unobservable variables (W_{it}), i.e. $prog_{it} = Z_{it}\psi + W_i\phi + v_{it}$, where Z_{it} can be contained in X_{it}. Selection bias arises when unobservables W_i and residuals v_{it} determining borrowing, correlate with unobservables M_i and residuals u_{it} affecting consumption. Or households that select themselves for borrowing may do so on the basis of unobservable characteristics that may also determine the outcomes consumption and housing improvement (Heckman and Hotz, 1989). This is a testable hypothesis from the first year survey and we follow Tedeschi (2008) to test whether or not the 1997 consumption and housing improvement outcomes for those who eventually become borrowers or those who always borrowed were statistically different from those who never borrowed:

$$C_i = \beta_1 + X_i\beta_2 + \beta_3 Always_i + \beta_4 Dropout_i + \beta_5 New_i + \beta_6 Branch_i + \varepsilon_i \tag{2}$$

where X is a vector of household characteristics, the dummy variables *Always, Dropout,* and *New* provide the test against those that *Never* borrowed, and the dummy *Branch* is one if borrower knew there was a DECSI branch in the nearest town and instruments for bias due to branch assignment by the MFI[5]. If selection is indeed a problem the impact of borrowing on consumption or housing improvement cannot be consistently estimated from (1) by standard pooled OLS estimators. Panel data models with specifications that allow program participation decision to be correlated with unobservables affecting outcome variables provide unbiased impact estimates (Heckman and Hotz, 1989; Papke, 1994). Three such specifications, i.e. the standard fixed-effects model, the random trend model, and a flexible random trend model are elaborated below and used in our analysis.

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[4] We follow Wooldridge (2002:247) to use W and M to denote the 'unobserved heterogeneity' term is a random variable and not a parameter to be estimated and thus ignore ϕ and α in subsequent discussions.

[5] We assume the further away a branch was located from a village in 1997, the less known it would be for villagers.

The standard fixed-effects estimator provides a consistent estimate of the borrowing parameter, γ, under the assumption that all unobservables that influence the outcome of interest are time-invariant, since these unobservables are removed by a within or first-difference transformation (Wooldridge, 2002: 252). If such individual-specific unobservables change however over time, which may happen for various reasons, the estimate for γ is still biased. In our setting, there are two such potential reasons. First, unobserved negative economic shocks affecting households' input endowments may pressurize households for input-bridging borrowings or repeat-borrowings to settle earlier debts. Anecdotal evidence from our sample villages indicate that households indeed resort to microfinance borrowings after experiencing a negative shock. Moreover, some repeat-borrowings may follow failure on an earlier one. Second, as argued earlier, credit may have lasting effects on unobservables on which selection is based. E.g. unobserved household characteristics such as entrepreneurial abilities, which may condition credit demand, may change over time depending on previous exposure to microfinance credit. Under these conditions, a more robust specification is required to remedy bias in the parameter estimates of interest.

A more robust specification due to Heckman and Hotz (1989)- the individual-specific trend model- allows both household specific time-invariant unobservables and individual trends of time-varying unobservables to correlate with program participation (Wooldridge, 2002: 315). This model, also used by Papke (1994) to study the effect of nonrandom enterprise zone designation on unemployment and investment, is specified as:

$$C_{it} = X_{it}\beta + prog_{it}\gamma + M_i\alpha + g_i t + u_{it} \qquad (3)$$

where g_i is an individual trend parameter, which in addition to the level effect M_i, captures individual-specific growth rates over time. A consistent estimate for γ, viz. the treatment effect of borrowing, can be obtained by wiping out the time-varying unobservables and the trend in time-invariant unobservables that can potentially bias γ (Wooldridge, 2002: 315). First, eq. (3) is first-differenced to eliminate M_i, which gives a standard fixed-effects model:

$$\widetilde{C}_{it} = \widetilde{X}_{it}\beta + pro\widetilde{g}_{it}\gamma + \widetilde{g}_i + \widetilde{u}_{it} \qquad t=1,2,\ldots,T \qquad (4)$$

where $\widetilde{C}_{it} = C_{it} - C_{it-1}$, $\widetilde{X}_{it} = X_{it} - X_{it-1}$, $\widetilde{u}_{it} = u_{it} - u_{it-1}$ and $\widetilde{g}_i = g_i t - g_i(t-1)$. Second, eq. (4) is consistently estimated using a standard fixed-effects approach, i.e. using a within transformation or by differencing the equation (again) to eliminate g_i and then estimate by OLS. The latter is preferred if u_{it} after the first differencing cannot be assumed white noise but at the cost of losing one period information in each transformation (Wooldridge, 2002: 316). Note that γ can be estimated consistently from this specification only if $T > 3$. In short panels like ours, it may be reasonable to assume u_{it} to be serially uncorrelated after first-differencing. However, using a second differencing transformation has an extra advantage of not assuming homoskedasticity of the first-difference of u_{it} (Wooldridge, 2002:316). We therefore second-difference eq. (4) and estimate by pooled OLS.

Although we only have four rounds of panel data, still our data covers a period of ten years. An advantage of panel data covering a longer period is that it enables to estimate the impact from long-term rather than one-shot program participation. Repeated participation may, in addition to shifting the levels in each borrowing year, affect the rate of change of the outcome variables relative to nonparticipation. Following Papke (1994) and Friedberg (1998), we account for this by including $prog_{it} \cdot t$ in eq. (4):

$$C_{it} = X_{it}\beta + \gamma_1 prog_{it} + \gamma_2 prog_{it} \cdot t + M_i\alpha + g_i t + u_{it} \qquad (5)$$

This specification provides impact estimates robust to random periodical changes by allowing the individual-specific trend to vary on participation over time. Estimation follows the same procedures as in eq. (4).

The specifications in (3) and (5) however impose the restriction that each successive loan-cycle's borrowings have uniform effects as their preceding borrowing. Initial borrowings may however entail lasting effects on incentives as well as on consumption levels, which alter the scale of the effects of borrowings later. A more flexible specification suggested by Wooldridge (2002: 317) allows program indicators to reflect the frequency of participation in each possible participation year as presented in table 5.1. This is done by replacing $prog_{it}$ and $prog_{it}.t$ in eq. (5) with a series of program indicators for each loan-cycle the participant has been in the program:

$$C_{it} = X_{it}\beta + \gamma_1 prog1_{it} +,...,+ \gamma_k progk_{it} + g_i t + M_i\alpha + u_{it} \qquad (6)$$

where $progj_{it}=1$ if household i has been in the program for exactly j years in year t and zero otherwise; k is the maximum number of (observed) years a household can be in the program. Program indicators attach more weights to differences between households' degree of participation regardless of year of participation. More weights are also given to the timing of participation within each indicator[6]. As before, eq. (6) is first-differenced and then transformed again by a within or another difference procedure.

Finally, note that since one of our outcome variables, i.e. housing improvement, is a binary indicator, the model is basically a limited dependent with binary regressor of the type

[6] E.g., for household i and j that borrowed twice each, but i borrowed in the first two years and j borrowed in the last two years, the model attaches the same weights for both i and j (i.e., prog1 =1 and prog2 = 1, for i and j, i ≠ j). However, in the within observations, prog2 gives more weight to i (i will have more ones in prog2) than to j.

discussed in Angrist (2001). Binary choice models with panel data are problematic to estimate due to the incidental parameter problem. Angrist (2001) emphasizes rather than imposing distributional assumptions which may complicate estimation and yield inconsistent estimates, a simpler estimator such as the linear probability model (LPM) is attractive and consistent for answering the question of interest, mainly estimating the effect of binary regressor in models with limited dependent variables. Thus, we stick to the simple LPM specification, which also provides an estimate conveniently interpreted as effect on the mean of the dependent variable (Wooldridge, 2002: 454-457).

5.5 Estimation results

In this section estimation results from the models outlined in section 5.4 are provided. Selection bias test results are first presented. The test is carried out by estimating eq. (2) using OLS for the 1997 consumption expenditure outcome and using a logit model for the 1997 binary housing improvement outcome. The null hypothesis that all parameters of interest are simultaneously equal to zero is rejected by the *F-statistic* test at 1 per cent significance level for both the OLS and logit models, indicating that both fit the data set well. Results are given in table 5A.1, appendix 5A.

The most important test results are given by the parameter estimates for *Branch, New* and *Always.* First, in both models, the insignificance of the proxy for DECSI branch in 1997 suggests that there is no bias due to program placement. Second, the hypothesis that *New* is different from zero is also rejected at acceptable significance level in both models, but the same hypothesis cannot be rejected for *Always* at 10 per cent significance level in the consumption expenditure model. Thus, controlling for dropouts, we cannot confirm those who will eventually become borrowers in 2000 had higher consumption levels than those who never did. However, we find evidence that those who always borrowed had consumption levels higher than those who never borrowed. Thus, our analysis here after must account for potential bias due to self selection but not due to program placement.

The basic model given in eq. (1) is estimated by the standard fixed-effects estimator where instead of a binary participation variable, the number of years the household has been in a borrowing relationship is used to account for the *degree* of participation as suggested by Copestake et al., (2001). Since we are primarily interested in credit impact estimates, only household observables that may systematically correlate with selection even after controlling for effects of time-invariant unobservables are included. One implicit borrower screening criteria of DECSI is household head age. Besides, as household heads become older, they self-select out of borrowing activities. Since most household variables collected are time-invariant we included only time-varying variables that may be systematically correlated to participation, mainly, land size and its square, gender of household head, household head's age and its square as other explanatory variables related to selection into the program. Although land is state owned in Ethiopia, farmers are given user rights. 'Ownership' of land and size cultivated therefore determines amount of input use, including credit. A year dummy (equal to 1 for 2006, zero otherwise) is included to contrast the relatively stable and good harvest year 2006 to the earlier years that are characterized by adverse conditions such as war and drought. That 2006 was a very good year is also reflected in table 5.2 that shows that average deflated consumption in that year was much higher. Note that household head's gender and skills are time variant. This specification is similar to Tedeschi's (2008) fixed-effect model except that our specification considers the cumulative effect of several loan-cycles as compared to 'number of participation days' used in the former paper. Results are reported in table 5.3.

Table 5.3 Household fixed-effects estimates of the impact of credit

Dependent variables	*Per capita annual household consumption*	*Housing improvements*
Number of (observed) years borrowed	**414.665*****	0.273***
	(27.584)	(0.015)
Women headed household	61.058	-0.038
	(51.853)	(0.028)
Additional skills other than farming	62.136	0.039
	(60.823)	(0.033)
Year 2006 dummy	264.098***	-0.012
	(38.227)	(0.021)
Age of household head	10.216	0.004
	(9.597)	(0.005)
Age-squared	-0.059	-0.628×10^{-4}
	(0.090)	(0.491×10^{-4})
Cultivated land size (in *Tsimad = 0.25hectare)*	-11.735	-0.002
	(9.378)	(0.005)
Land size-squared	0.066	-0.139×10^{-3}
	(0.295)	(0.162×10^{-3})
Intercept	-289.897	-0.168
	(246.768)	(0.135)
Within R-squared	0.215	0.257
F (8, 1045)	35.77***	45.250***
Household fixed-effects	Jointly significant***	Jointly insignificant
Number of observations	1404	1404

*, ** ,*** significant at 10%, 5% and 1%, respectively; Standard errors in parentheses

The *F*-statistics (at 1 per cent significance level) indicate that for both household consumption and housing improvement models all parameters are not all jointly equals to zero.

Based on the fixed-effects estimation, credit has a significant positive effect on annual household consumption expenditure and housing improvements of borrowers compared to non-borrowers. After controlling for potential selection on unobservable fixed-effects, household per capita consumption for an average borrower household has increased by ETB 415 for each additional borrowing year. Moreover, the probability of improving the house increases on average by 0.27 per year of credit taken. Note that the parameter for the 2006 dummy is also statistically highly significant in the consumption equation, indicating differential impacts on participants and non-participants due to aggregate macroeconomic variability, which also includes specific events which may have occurred due to aggregate effects (e.g., death of livestock due to drought and death of key labor in the household due to war). Compared to non-participants, participants have seen ETB 264 more consumption in the good year 2006.

The individual household heterogeneity not picked up by the variables included is captured in the fixed-effects parameter. For the household consumption model there is evidence for household heterogeneity given the significance of the fixed-effects. This is not however the case for the housing improvement model, suggesting for a pooled estimation. Estimating it by pooled regression also provides qualitatively the same results as the fixed-effects results. Note that the FE within procedure also has the benefit of removing potential selection bias due to time-invariant unobservables. As indicated in section 5.4, error terms may correlate due to selection based on time- varying individual-specific unobservables. In that case the individual trend model as specified in eq. (3) is more robust than the standard FE model since it allows selection to be based not only on individual averages of unobservables (i.e., fixed-effects) but also on individual-specific unobservable *trends*. This model is estimated by OLS after differencing twice to eliminate the trend component. This is done for both consumption and housing improvement outcomes. Since results for the housing improvement model are very similar to the fixed-effects results presented in table 5.3 they are not reported here. Results for household consumption are reported in the first column of table 5.4.

In general, removing individual-specific unobserved dynamics by including an individual trend and differencing the data twice provides more conservative results. Specifically, according to this individual-specific trend specification, per capita annual consumption increases by ETB 199 per year of credit taken. This result is statistically significant and credit impact is substantially reduced (by a more than 50 percent a year) compared to the fixed-effects result. This difference is the bias in the standard fixed-effects result due to time-varying individual dynamics. Consistent results are obtained when the same specification is estimated by fixed-effects after first-difference.

Table 5.4 Household specific trend model results of credit impact on per capita annual consumption

Variables	*Individual trend model*	*Individual trend model, and trend based on participation*
Number of (observed) years in borrowing	**199.317****	**160.738****
	(77.065)	(79.016)
Random trend *borrowing participation	-	**33.858****
		(16.043)
Year 2006 dummy	323.439***	324.497***
	(32.594)	(32.517)
Age of household head	2.003	1.632
	(9.428)	(9.407)
Age-squared	-0.022	-0.017
	(0.089)	(0.089)
Cultivated land size (in *Tsimad = 0.25 hectares)*	-0.496	-1.739
	(13.249)	(13.229)
Land size-squared	0.139	0.193
	(0.463)	(0.462)
Intercept	-130.553	-113.738
	(88.088)	(88.230)
R-squared	0.164	0.169
F (6, 695); F (7, 694)	22.640***	20.14***
Number of obs.	702	702

*, ** ,*** significant at 10%, 5% and 1%, respectively; standard errors in parentheses

After the first-difference, the fixed-effects error component is however jointly insignificant favoring estimation by pooled OLS. Second-differencing eliminates the trend and provide results more robust to second-order serial correlation and heteroscedasticity.

A variant of the individual-specific trend model given in eq. (5) allows individual household consumption not just to vary at different *trends* but also allows borrowing effects to depend on these unobserved individual-specific *trends.* Note that in this case, *trend* is interacted with participation ($prog_{it}$) indicator and not with 'number of years in borrowing'. Results are reported in column 2 of table 5.4. The credit effect estimate is both quantitatively as well as qualitatively consistent to the results in column 1 in table 5.4, but again more conservative than the standard fixed-effects estimate. After controlling for both time-invariant and time-varying selection bias, each borrowing cycle increases per capita consumption by ETB 161 directly and by ETB 34 indirectly (by changing other unobserved time-varying individual characteristics).

Table 5.5 Result of flexible random trend model with participation indicators

Dependent variables	*Household per capita annual consumption*		*Housing improvements*	
One year borrowing	**273.936****	(107.526)	-0.004	(0.075)
Two years borrowing	**319.132****	(137.706)	0.244**	(0.097)
Three years borrowing	310.697	(213.204)	0.555***	(0.149)
Four years borrowing	**665.024****	(337.707)	0.457*	(0.237)
Year 2006 dummy	326.079***	(31.954)	-0.019	(0.022)
Age of household head	2.578	(9.432)	-0.007	(0.007)
Age-squared	-0.027	(0.089)	0.531×10^{-4}	(0.623×10^{-4})
Cultivated land size (in *Tsimad = 0.25hectare*)	-0.887	(13.250)	-0.004	(0.009)
Land size-squared	0.175	(0.463)	-0.159×10^{-3}	(0.3245×10^{-3})
Intercept	16.268	(70.153)	-0.017	(0.049)
R-squared	0.170		0.044	
F(9, 692)	15.76***		3.560***	
Number of obs.	702		702	

*, ** ,*** significant at 10%, 5% and 1%, respectively; standard errors in parentheses

Thus, after accounting for selection biases, credit has been responsible not just to change the *levels* but also the *rate* at which yearly per capita consumption grew for an average borrowing household in the ten years considered. Note that other results are also consistent across the two specifications presented in table 5.4. A consistently significant negative intercept in both specifications captures a general consumption decline trends not captured by our aggregate shock variable. The results in table 5.4 provide interesting insights into how effective microfinance can be for households trying to extricate themselves from poverty in those villages, other factors remaining the same, by keeping their relationship with the MFI.

Important follow-up questions from a policy point of view are whether impact can be associated to the extent of repeat-borrowing. This is analyzed using the flexible individual-specific trend model given in eq. (6), which assigns indicators for the number of times each household has been involved in borrowing. Results are given in table 5.5. Again, the double differencing estimation procedure reduces the potential of selection bias to a minimum. Results show once more that borrowing has a significant impact on (future) consumption, but interestingly enough the magnitude of impact substantially increases with the increase in the length of relationship with the MFI. Specifically, compared to non-participants and other participants, per capita consumption has significantly increased by ETB 274 for one year participants and by ETB 319 for two year participants (which for them adds to the first year effect of ETB 274). The effect is (slightly) statistically insignificant for three year participants (p-value 0.145). Since for most three year participation the third cycle coincided with the occurrence of one of worst droughts the country has seen, the effect seems to have been neutralized by the overall drastic consumption shortfall for participants and non-participants alike. However, having participated in the previous three years has had an inertia effect for four year participation such that per capita consumption has increased substantially (by ETB 665) only for households that participated in all four cycles. It means that while the cumulative effect of the pre-shock participation might have slightly helped to overcome consumption shortfalls for participants compared to non-participants, those that participated for four years, including after the shock, have benefited substantially in terms of per capita consumption increases. For the housing improvement model, the probability of improving the house has significantly increased after the second round borrowing and raises up to 0.244 if households borrowed for two periods, 0.555 if borrowed for three periods and 0.457 if borrowed in all periods. The relatively lower effect in the case of borrowing in 'all periods' would not be surprising as households eventually shift attention from improving their houses to other activities.

Compared to the average impact on the participant obtained from the individual-specific trend model, this finding supports the lasting impact of credit over time by uncovering the specific impacts on each cohort of participants. Thus, while the impact of one time borrowing is close to the average impact previously obtained, it also uncovers having borrowed three and four times leads to even higher increases in consumption and probability of house improvements. Such high percentage increases attributed to credit is not surprising given the importance of credit at such marginally low initial conditions (e.g. initial average per capita consumption is ETB 442 for participants and ETB 371 for non-participants) and the relatively long period covered in which 8-11 per cent GDP growth was registered in the country.

5.6 Conclusions

Impact evaluations are often prone to self-selection and program placement biases. This chapter uses panel data techniques to deal with these potential selection biases. Standard fixed effect models mitigate selection based on time-invariant unobservables, whereas the more advanced random trend model also account for individual trends in time-varying unobservables. The dataset used is a unique four-round panel data set among households in Tigray, Ethiopia that covers a period of ten years, so that lasting effects of credit can be established.

The analysis started with tests of program placement and self-selection biases. While there was no indication of bias due to systematic program placement, the data did not confirm absence of bias due to self-selection. The analysis therefore accounts for any potential selection bias. Results indicate that microfinance credit significantly raised annual per capita household consumption. It also significantly raised the probability of improving housing (roofs), which is an important welfare indicator in this area. The random trend model with flexible participation indicators, which considers frequency of participation, shows that per capita household consumption (except in the bad year 2003) and probability of improving the house substantially increased with the frequency of participation. One time borrowing has no impact on housing improvements but significant improvements in per capita consumption, which is plausible at such early stages of livelihood changes for households in those marginal areas. Repeat-borrowing did matter in both cases however, but with a slight decline of the probability of housing improvements for household that borrowed frequently.

These findings have both substantive as well as methodological significance. First, they reflect the effect of credit on livelihoods is multi dimensional and cannot be fully captured by just a single household outcome. Moreover, the effect is not monotonically the same over time on all livelihood indicators used to measure impact. Second, it is also imperative that the effect of borrowing lasts longer than one or two periods. It therefore takes time before the effect of borrowing on livelihoods is fully materialized. Methodologically, impact estimates that rely on a single household indicator and only one-cycle of borrowing may undermine the potentials of microfinance credit on overall livelihoods that could be achieved over time. Future research must focus on more robust specifications that incorporate temporal as well as multidimensional effects of credit on livelihoods.

The implication for MFI practitioners such as DECSI is that eligible households should not only be encouraged to borrow, but also, if successful, to stay longer in a borrowing relationship in order to realize the full potentials of borrowing. As such, early *graduation* from microfinance (in our case, as early as before ten years) might be pre-mature in terms of achieving the required goal of eradicating poverty and careful weighing is necessary before graduation takes place. The flexible specification results also suggest that those that were able to continue borrowing even after a major shock in 2003 have seen even higher consumption levels after that shock. This implies that rescheduling repayment so as to provide, rather than deny, access to future borrowing after a shock may help poor borrowers to bridge their consumption and regain economic normalcy after a shock. Finally, although the results of the fixed-effect and trend models deviate somewhat, due to different assumptions, specifications and estimation techniques, they all strongly suggest that microfinance in this part of Africa has been useful in terms of measured outcomes.

Appendix 5A.

Table 5A.1 Test results for selection bias using base year data

Variables	*Per capita Consumption expenditure*♣		*Housing improvements*†	
Intercept	312.295	(499.573)	-9.894*	(4.568)
Household characteristics				
Age of household head	55.171***	(19.222)	0.227	(0.161)
Age-squared	-0.551***	(0.179)	-0.002	(0.002)
Women headed (yes=1)	-707.499***	(113.386)	1.934*	(0.995)
Special skills other than farming (yes=1)	388.856	(281.885)	1.325	(1.102)
Household head's education (literate=1)	411.233	(268.719)	-0.020	(0.994)
Number of oxen owned	53.220	(56.508)	0.521	(0.460)
Per capita land size owned	431.512*	(212.639)	-3.851	(2.330)
Shock occurred (yes =1)	-206.042*	(100.323)	0.378	(0.755)
Village characteristics				
Micro dam available (yes=1)	229.822*	(125.988)	0.163	(0.652)
Village is remote (yes=1)	-237.003*	(103.117)	-0.270	(0.837)
Borrowing status				
Always	249.392*	(142.423)	1.505	(1.107)
Dropout (in 2000)	191.481	(126.477)	0.024	(1.089)
New (in 2000)	-91.490	(124.259)	0.859	(1.132)
Knew **branch** was available in nearest town (yes=1)	77.345	(110.607)	0.359	(0.720)
R-squared; Pseudo R-squared	1912		0.150	
F(14, 336); Wald χ^2 (14)	7.80***		70.370***	
Sample size	351		351	

♣ OLS estimates; †Logit estimates; *, ** ,*** significant at 10%, 5% and 1%, respectively; *Robust*-std. errors in parentheses

CHAPTER 6

ASSESSING LONG-TERM CREDIT IMPACTS FROM THE TIMING OF PARTICIPATION IN MICROFINANCE[1]

Abstract: *This chapter uses the concept of composite future counterfactuals to assess the long-term impact of farm households' participation in microcredit. A four wave panel data spanning over ten years for credit-eligible rural households in Ethiopia is used to assess impact of (timing of) participation on consumption. New in this method is that only households that did not participate up to the time of participation are considered as candidates for controls. Further, to account for counterfactuals between timing of participation and outcome measurement period in a panel data setting where only the outcome variable is time-varying, potential future paths of individuals in the control group are considered. The propensity score method is used to adjust for initial differences between participants and controls. The combined methodological innovation enables us to overcome biases due to selection as well as problems of accounting for dropouts and new participants inherent in microfinance impact assessments. Results suggest that the timing of participation matters: the earlier the participation the better the effect. Results are robust compared to standard matched pair wise effects. Such comparisons suggest that not accounting for future counterfactuals, for the most part, overestimate impact.*

Key words: *microfinance, impact, dropouts, composite counterfactuals, Ethiopia*

[1] Paper by Guush Berhane and Cornelis Gardebroek submitted to *Journal of Development Studies.*

6.1 Introduction

In recent years, microfinance is seen as a beacon of hope to help eradicate poverty and has been at the center of policy making in many developing countries. A central element in microfinance is providing small but progressively larger and repeated loans to those that lack the required collateral to access conventional lenders. Loans are expected to help lift borrowers out of vulnerability and poverty over time. In the case of Ethiopia, repeated loans are primarily intended to bridge short term working capital requirements so as to gradually build assets and improve the ability to mitigate aggregate shocks. If successful, such loans would eventually trickle down into measurable welfare gains such as increases in consumption (e.g., Menon, 2006), or reducing vulnerability to economic hardships (Morduch, 1998). Nevertheless, many years since these programs are in operation, questions still remain if and to what extent these successive loans have been successful in achieving their intended goals.

Existing studies focus on evaluating before and after effects, regardless of the timing of participation and dynamics between participation and outcome measurement periods[2]. There are however differences among target households when it comes to benefiting from availability of credit. One major difference is that not all targeted households start to use credit at the same time and in the same intensity (Berhane and Gardebroek, 2009). For some reasons, some join earlier than others; still some remain members for long time while others dropout quickly. As a result, the effect of microfinance credit varies across different client pools and therefore evaluating the trickled down effects of credit requires measuring the relative impacts across these pools (Karlan and Goldberg, 2007). The aim of this chapter is to evaluate the long-term impacts of MFI credit by overcoming heterogeneities across periodical participant pools. Of particular interest is whether and how differences in the timing (e.g., early versus late participation) of participation impacted livelihoods, particularly in the face of economic distresses such as droughts, after accounting for pre- and post-entry differences. This provides an insight into how Microfinance Institutions (MFIs) can improve the design and timing of their loan products given heterogeneities in entry and loyalty of target clients (Karlan and Goldberg, 2007).

Evaluating such effects is however an arduous task not only because pertinent data spanning over sufficiently long periods is scarce but also because obtaining an appropriate 'control group' to identify effects over time given heterogeneities due to the timing of participation is difficult (Karlan, 2001). Microfinance impact studies thus far have focused on either experimental (e.g., Karlan and Zinman, 2007) or quasi-experimental cross-sectional designs (e.g., Pitt and Khandker, 1998, Coleman, 1999), or classic two-period panel data fixed-effects methods (e.g., Tedeschi, 2008) to investigate causal credit effects. Cross-

[2] A recent study emphasizes the issue of timing in evaluations is "as important but relatively understudied" (King and Berhrman, 2009).

sectional designs are useful to tease out biases due to individual borrower as well as MFI (e.g., program placement) selection characteristics inherent in such programs. They however lack the time-dimension required to capture lasting credit effects. King and Behrman (2009) emphasize the duration and timing of evaluation matter in program impact assessments. Likewise, although the classical fixed-effects method captures long-term effects when panel data is available, it has fundamental flaws when applied to repeated observations that go beyond the classic two-period case. First, in the case of dichotomous treatment, fixed-effects model works on the condition that individuals change treatment status (i.e., assumes treatment status is reversible) across time (Wooldridge, 2002:637-38). However, reversibility of treatment status means that impact estimates are biased because dropouts from previous borrowings are included in the control group (contamination-effect) and excluded from the treated group (attrition-effect). An appropriate impact assessment includes dropouts in the comparison group and the control group includes only untreated individuals (Karlan, 2001). Second, in time-varying treatments with more than two-period observations, counterfactuals need to account for potential future paths of individuals in the control group (Brand and Xie, 2007). The fixed-effects method simply compares participants and non-participants pair wise and does not account for such future paths.

The classical parametric evaluation literature is thus silent about these effects and time dimensions in treatments except that agents are exposed to one of two possible conditions of the treatment at a given time and outcomes are measured subsequent to exposure (Brand and Xie, 2007). Owing to the difficulty of establishing appropriate counterfactuals when both the treatment and outcome variables are time-varying, long-term impact evaluations in many applications remain challenging even when panel data is available. Recent developments in nonparametric methods offer alternative ways to handle such identification problems. A body of literature in epidemiology (e.g., Robins, Hernan, and Brumback, 2000) and sociology (Brand and Xie, 2007) exploits the conceptual apparatus of the 'potential outcome approach' in experimental causal inference and extends it to non-experimental panel data. The strategy involves establishing a composite of 'forward-looking composite counterfactuals' for each group in each loan-cycle, considering participation as an *irreversible* treatment. These composite counterfactuals combine weighted averages of those who will borrow later between the treatment and the outcome measurement period and those who will never borrow by the end of the outcome measurement periods. This alternative method is used in this chapter to overcome the difficulty of identifying credit impact from a four-wave panel data that covers ten years. The data comes from (non-) borrower households of a rural microfinance in Tigray, northern Ethiopia.

Results indicate that compared to later participants, early participants are better off even after accounting for initial as well as future counterfactuals. Comparative results show that not accounting for future counterfactuals overestimate impact. It also suggests that the timing of participation matters when it comes to the capacities credit provides to overcome economic distresses: the earlier the better. The contributions of this chapter are threefold. First, we measure credit impact from long-term panel data accounting for counterfactuals in future pathways, reducing biases due to dropouts and time dimension. This contrasts with conventional methods that compare participants and non-participants pair wise. Second, the propensity score matching method is used to establish appropriate controls for participant groups in each period. Although this non-parametric method is not new, we believe, applying it to panel data to establish causality from sequential counterfactuals adds a new dimension to microfinance impact evaluations. Third, this chapter provides additional evidence on selection bias, mainly due to the timing of decision to participate and changes in the composition of participants and non-participant, and underlines the danger of employing parametric impact assessments that naively compare participants and non-participants pair wise.

The rest of the chapter is organized as follows. By way of describing the structure of the data set used, section 6.2 discusses the challenges to identify impact using conventional methods in time-varying settings. Section 6.3 introduces the concept of 'forward-looking composite counterfactuals'. The techniques to overcome identification problems using this concept and its empirical implementation in this chapter are discussed. Section 6.4 presents and discusses the results. Section 6.5 concludes.

6.2 Estimating long-term impacts of periodical participation in microfinance credit

The main objective of this chapter is to assess the long-term impact of credit with periodical participation of households, which in more than two-period panel data setting involve identification problems due to differences in the *timing of first-time participation*. Before proceeding to discuss why these heterogeneities cause identification problems, it is essential to elaborate on the dynamics of participation using the structure of the data set at hand. Therefore, section 6.2.1 presents the data set structure and section 6.2.2 discusses the challenges of identifying credit impact using conventional methods in more than the classic two-period settings. An alternative method proposed by Brand and Xie (2007) that exploits the timing of treatment and outcome measurement to mitigate these identification problems is discussed in section 6.3.

6.2.1 The structure of the data set used

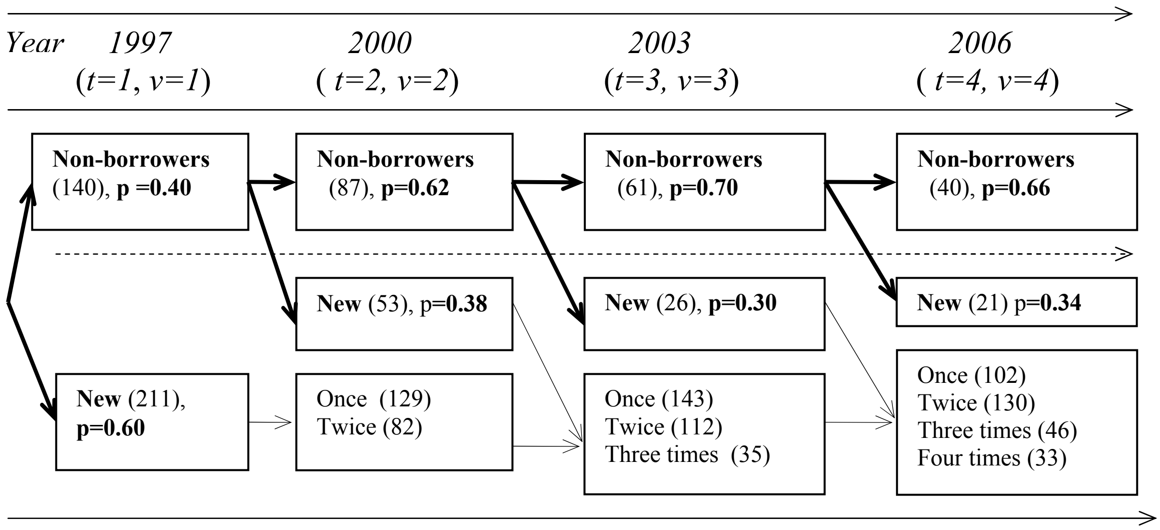
Panel data used in this chapter comes from farm households in Tigray, northern Ethiopia, whose livelihoods largely depend on rainfall based agriculture where production is only once a year. Data was collected during 1997-2006, in four-waves with a three-year interval.

Table 6.1 Summary statistics of yearly household consumption expenditure and other variables used in the propensity score matching Method.

Variable name	*Mean*	*Std.dev.*	*Min*	*Max*
Participation in program credit	0.450	0.498	0	1
Annual household consumption expenditure	3514	4271	229.82	72064
Family size	5.282	2.352	2	13
Male-headed (yes=1)	0.244	0.429	0	1
MFI Branch office close enough (yes=1)	0.762	0.426	0	1
Poorer village (yes=1)	0.239	0.427	0	1
Age of household head	52.875	14.852	19	92
Size of land owned (in *tsimad*=0.25 ha.)	4.408	3.835	0.25	10.5
Non-farm income dummy	0.742	0.438	0	1
Participation in extension programs (yes =1)	0.165	0.371	0	1
Micro-dam availability	0.514	0.499	0	1

Sample households include participants and non-participants of the Dedebit Credit and Saving Institution (DECSI), a MFI that provides credit services in the Tigray region of Ethiopia since 1994. First on trial basis in selected villages, but since 1997 it is present in almost all villages. The official launch coincided with the first wave of this study. Respondent households were selected using a multi-level sampling procedure. First, 16 villages were selected to represent regional differences, including access to credit. Second, 25 households were randomly selected from each village. Of the total 400 households, 351 are included in the analysis, a total of 1404 observations for the balanced four-year panel. The remaining were excluded because they either became non-targets (outliers) over time, mainly due to old age or dropped out from the survey. Respondents were surveyed on several household- (e.g., annual consumption levels, participation in MFI credit), village- (whether or not village had access to basic infrastructure) and MFI-level (e.g., ease of access to MFI credit) characteristics. Expenditure values (in local prices) on major household (food and non-food) consumption items were collected. Table 6.1 provides summary statistics of annual household expenditure and other variables used in the analysis.

An important issue in this data set for empirical identification is that households joined the MFI at different times during the survey. Of the sample households, 211 participated early (in 1997), while many others joined in later years. So, participation status of members fluctuated over the years. Some members dropped out after participating only once and others continued up to four times. Figure 6.1 presents the number (in parentheses), proportion and frequency of participation in each year. The fact that households switched their status over the survey years makes it possible to apply conventional (e.g., fixed-effects) methods to estimate the impact of participation. However, as will be made clear in section 6.2.2, these switches also make it difficult to establish appropriate controls for participants in each year.



Note: number of participants in parentheses, t = participation period, v = outcome measurement period, p=transition probabilities

Figure 6.1. Borrowing participation transition of sample households, 1997-2006

6.2.2 Problems of identifying credit impact in long panels with periodical participation

A straightforward method to estimate impact of periodical participation would have been to exploit the panel nature of the data and use the fixed-effects method. This method uses the time-varying nature of the treatment variable (i.e., participation) across observation periods to identify impact pair wise. Particularly, it exploits the fact that units change their treatment status across observation periods (Chamberlain, 1984: 1247-1317). In the case of borrowing, such status changes involve two-way transitions of members between participation and non-participation status over time, which essentially assumes *reversibility* of the treatment. In other words, when previous borrowers dropout at some point, they become part of the control group as if their previous participation does not have an effect on their outcome. Moreover, the fixed-effects method does not use variations of households that do not change status (i.e., always participants and never participants) in explaining outcome variables.

There are therefore three types of biases that arise in using standard parametric methods, such as the fixed-effects method, in panel datasets of more than two periods with dichotomous treatment as in our setting. First, not all participant households have become members of the MFI at the same time. For some reasons, some have joined earlier than others (see Figure 6.1). In other words, there are heterogeneities among the four *new* participant groups due to the timing of participation. One reason is that selection criteria by both the

MFIs and participants might have changed over time (Tedeschi and Karlan, forthcoming). Moreover, even the degree of participation over the years varies across members; some have continued to participate while others have dropped out. Second, estimates are biased if dropping out is non-random (Karlan, 2001)[3], i.e. if dropouts are those who became worse off or better off due to the program. Third, since they were once in the treated (participant) group, dropouts may contaminate the control group carrying over the effect of the treatment to the control group, particularly when the effect of the treatment lasts longer than the treatment period as in borrowing used to acquire durable inputs, e.g., oxen or draft animals. Thus, although long-term panels can be advantageous to capture long-term impacts in many applications, in this particular application the use of such periodical participation and the resulting two-way transition yields biased estimates. As such, partly owing to these problems, most credit impact assessments are limited either to the two-period classic panel data methods or quasi-experimental cross-sectional methods that compare ever-participants to never-participants on pooled data.

6.3 Empirical method

This section presents an alternative empirical method that overcomes the challenges of identifying impact of periodical participation in credit discussed in section 6.2.2. Section 6.3.1 briefly introduces a method proposed by Brand and Xie (2007) and its implementation. Section 6.3.2 discusses the Propensity Score Matching (PSM) method that is used to implement this method.

6.3.1 The forward-looking sequential counterfactual method

A central issue in our setup is that treatment effects are nonreversible and therefore our interest is to identify the causal effect from the timing of participation in credit on outcomes measured at several points in time subsequent to the treatment. However, when outcomes are measured at different periods other than just subsequent to treatment, it is no longer clear what the appropriate control group should include because the control group varies depending on the gap between the timing of the two events. The concept of 'forward looking sequential counterfactuals' proposed by Brand and Xie (2007) provides a framework to construct appropriate counterfactuals for treatments with lasting effects that vary over time. This concept is useful in applications where (a) exposure to treatment can take place at any point in

[3] Karlan, 2001 discusses the nature of the bias in cross-sectional designs that exclude dropouts from the treatment group. Tedeschi and Karlan (forthcoming) measure the extent of this bias using quasi-experimental method for two-period panel data from Peru.

time but once treated the effect stays on, and (b) the effect of the treatment varies over time subsequent to treatment, regardless of whether or not the treatment is repeated.

In this approach, the control group for individuals treated at *t* whose outcome is measured at *v* is composed of individuals that are (i) untreated by *t* but are treated any time up to *v,* (ii) never treated up to the end of the observation period but can be potentially treated any time in the future. Thus, individuals that participated prior to *t* can no longer serve as controls at *t* and the only controls for individuals that have participated at *t* are therefore those that have never participated up to *t*. However, control individuals at *t* may or may not participate after *t.* Likewise, participants at *t* would have had the same potential paths after *t* had they not participated at *t.* E.g., a household that participated in 1997, in our case, would have had two possible paths (i.e., participate or not) in subsequent loan cycles had this household not participated in 1997. This means, if outcome is measured in 2006, this household would have had three other participation chances up to 2006. Brand and Xie (2007) argue not accounting for such counterfactual possibilities biases impact measurement and therefore an appropriate counterfactual outcome must include all potential outcomes of the future paths of the control units considered. This is done by assigning transition probability weights to each potential future path of the controls (see Figure 6.1). A forward-looking sequential approach therefore composes a 'composite' of counterfactuals that are weighted combinations of those individuals later treated and those individuals never treated.

However participant and non-participant groups may also differ in their pre-treatment characteristics and participation may be based on these pre-treatment characteristics, in which case estimates are biased even after accounting for future paths. This problem is dealt using the propensity score method assuming the conditional independence assumption discussed in section 6.3.2. Under this assumption, the Average Treatment Effect on the Treated (*ATT*) of participation in borrowing at time *t*, *d=t,* on annual household consumption expenditure, *C*, measured at time *v, v≥ t,* is calculated using forward-looking counterfactual approach as:

$$E(\Delta C_v^{d=t} | X) = E(C_v^{d=t} | X) - E(C_v^{d>t} | X) \tag{1}$$

where the LHS is the *ATT* of participation in borrowing at *d*=*t* conditional on *X,* a vector of exogenous (or pre-treatment) characteristics that determine participation. The terms on the RHS are expected annual consumption expenditure after *d=t,* conditional on *X,* for participant and non-participant groups at *v*, respectively. In our case, these two terms are calculated using PSM, given as the average treatment effects on the participants and non-participants had the latter participated (more on this in section 6.3.2). The difference between the two provides the average treatment effect on the treated (*ATT*). Note that *d>t* in $E(C_v^{d>t} | X)$ indicates that those in the control group are not treated until *t* but may or may not be treated up to *v* and this term is a composite of non-participants' future outcomes expected between *t* and *v*. Depending on the causal question asked, this term is decomposed into its future components. That is, decomposition depends on the treatment (*d=t*) as well as the outcome measurement, *v* periods considered. Note that decomposition is needed only if *v>t*. Otherwise, (1) is reduced to standard pair wise comparison between participants and non-participants.

We illustrate this decomposition using two causal research questions in this chapter. First, consider 'what is the effect of *early* (1997) participation (*t=1*) in credit on annual household consumption measured in 2006 (*v=4*)?' As shown in Figure 6.1, in this specific problem, there are three possible paths for households who did not participate by *t=1*: to participate at *t=2*, to participate at *t=3*, to participate at *t=4* or not to participate at all, *d>4*. Let the probability to participate at any future path *t* be given by *p(t)*, otherwise *q(t)=1-p(t)*. Ignoring *X* for now, the composite term can be decomposed into its path dependent additive components as follows (Brand and Xie, 2007):

$$
\begin{aligned}
E(C_v^{d>t}|X) \cong E(C_4^{d>1}|X) &= \left[p(2)\cdot E\left(C_4^{d=2}\right)\right] + \left[q(2)\cdot p(3)\cdot E\left(C_4^{d=3}\right)\right] \\
&+ \left[q(2)\cdot q(3)\cdot p(4)\cdot E\left(C_4^{d=4}\right)\right] + \left[q(2)\cdot q(3)\cdot q(4)\cdot E\left(C_4^{d>4}\right)\right]
\end{aligned} \tag{2}
$$

where the terms in square brackets of the *RHS* give the weighted ATT of participation at *t=2*, *t=3,* and *t=4* and non-participation by *t=4*, respectively, measured at *v=4*. Note that the transition probabilities *p* (*t*) and *q* (*t*) in (2) assign the likelihood of individuals to transit to one of the two paths (participate or not participate) at *t* conditional on being non-participant prior to *t*; i.e., $p(t) = prob(d = t|d \geq t)$ and $q(t) = 1 - p(t)$. We therefore use the probability of being in the participant group in each path such that $p\ (t) + q\ (t) = 1$ (Brand and Xie, 2007). Combining eq. (1) and eq. (2) gives the ATT of *early* participation in MFI borrowing on household consumption expenditure measured in 2006.

$$
\begin{aligned}
ATT_1 = E(\Delta C_v^{d=t}|X) &= E(C_v^{d=t}|X) - E(C_v^{d>t}|X) \\
&= E(C_4^{d=1}|X) - \left\{p(2)\cdot E\left(C_4^{d=2}\right) + q(2)\cdot p(3)\cdot E\left(C_4^{d=3}\right)\right. \\
&\left. + q(2)\cdot q(3)\cdot p(4)\cdot E\left(C_4^{d=4}\right) + q(2)\cdot q(3)\cdot q(4)\cdot E\left(C_4^{d>4}\right)\right\}
\end{aligned} \tag{3}
$$

Second, consider the causal question, 'what is the effect of *late* (e.g., in 2003) participation (*t=3*) on annual consumption expenditure measured in 2006 (*v=4*)?' As before, the composite term in (1) is decomposed into its components[4]. For non-participants at *t=3* where *v=4* (see

[4] Analyzing the effect of participation at *d=t, t+1,…T* on outcomes measured at *v=t, t+1, …T* follows the same procedure. For a general formula that can be used in many other applications, the interested reader is referred to Brand and Xie (2007).

Figure 6.1), there is only one chance to participate before the survey period ends: to participate or not to participate at *t=4*.

$$E(C_v^{d>t}|X) \cong E(C_4^{d>3}|X) = \left[p(4)\cdot E\left(C_4^{d=4}\right)\right] + \left[q(4)\cdot E\left(C_4^{d>4}\right)\right] \tag{4}$$

Combining eq. (1) and eq. (4) gives the *ATT* of *late* participation (in 2003) in MFI borrowing on household consumption expenditure measure at v=4 (i.e., in 2006).

$$\begin{aligned} ATT_3 &= E(\Delta C_4^{d=3}|X) = E(C_4^{d=3}|X) - E(C_4^{d>3}|X) \\ &= E(C_4^{d=3}|X) - \left\{p(4)\cdot E\left(C_4^{d=4}\right) + q(4)\cdot E\left(C_4^{d>4}\right)\right\} \end{aligned} \tag{5}$$

This procedure is implemented for all ten possible counterfactual constructions in this chapter. A complete computation for all these possibilities is given in appendix 6A. Comparative results are summarized in the results section from which the causal effect of e.g. *early* versus *late* participation can be made by comparing results obtained from (3) and (5).

Note however that although non-reversible, participation in borrowing is repeatable. The time-varying treatment proposed by Brand and Xie (2007) does not give a way to incorporate the *ATT* of *repeat-borrowers* because treatment is assumed non-reversible and non-repeatable, or simply the treatment state is an *absorbing state* and once treated, individuals remain in the treatment state. The method discussed so far therefore provides the gross effect of credit after the onset of participation. To substantiate results from this method, effects of repeat-borrowing and dropping out are analyzed pair wise. These results are provided in section 6.4.3.

6.3.2 Propensity score matching method

The composite counterfactual method discussed in section 6.3.1 gives a way to construct appropriate *future controls* for each participant group in each treatment period. However, participant and non-participant households in each treatment period may not be directly comparable because participant households may self-select (or, be selected) into the program based on initial differences, including the outcome of interest, in which case the mean outcome of the two groups differ even in the absence of the program. Therefore, before proceeding to future counterfactuals, initial comparability must be established to avoid initial selection bias, at least, based on some common observable characteristics.

To deal with this problem, we use the Propensity Score Matching (PSM) technique that has gained popularity in recent years for its potential to remove substantial amount of bias from non-experimental data (e.g., Dehejia and Wahba, 1999). This technique helps to adjust for initial differences between a cross-section of participant and non-participant groups by matching each participant unit to a non-participant unit based on 'similar' observable characteristics. An advantage of PSM is that it summarizes all the differences in a single dimension, the propensity score, which is then used to compute treatment effects non-parametrically. The propensity score conveniently summarizes the conditional probability of participation given pre-treatment or exogenous characteristics (Rosenbaum and Rubin, 1983). An important assumption on which this technique builds is the Conditional Independence Assumption (CIA), which states that selection is solely based on observable characteristics and potential outcomes are independent of treatment assignment[5]. Under the assumption that initial differences between the two groups determining participation are captured by observable characteristics, the participants' counterfactual mean outcome had they not been participated is identified by non-participants' mean outcome. Besides CIA, another condition in PSM is the Common Support requirement, which ensures that individuals compared from the participant and non-participant groups are, to begin with, comparable. Specifically, it ensures individuals with the same observable characteristics have a positive probability of being in both participant and non-participant groups (Heckman, LaLonde, and Smith, 1999: 1865). This requirement can be imposed such that estimation is performed on individuals that have common support. The average treatment effect on the treated (*ATT*) is therefore given by the difference in mean outcome of matched participants and non-participants that have common support conditional on the propensity score.

The following practical steps are followed to implement the PSM technique in this chapter. The first step is to predict the propensity score for each group in each period using a probit model. Justifying the CIA requires that only variables that simultaneously influence the participation decision and consumption outcome but that themselves are not affected by participation are included (Heckman, Ichimura, and Todd, 1998). As such, variables included in our specifications are either measured before treatment or carefully selected exogenous characteristics. Specifications vary across participant groups, accounting for heterogeneities due to 'timing of participation'. Since there are four observed treatments (loan cycles) whose outcomes are measured four times, a total of thirty distinct matching specifications, one for each cross-section, are needed to construct the ten composite counterfactuals (see section 6.3.1).

The second step is to choose a method by which weights are assigned for matching. Four different matching algorithms are available in the literature (see Caliendo and Kopeinig, 2005). Throughout the chapter Kernel Matching (KM) is used. A major advantage of the KM method is that it ensures low variance because it uses weighted averages of all individuals in the control group to construct the counterfactual outcome. Its drawback is however the possibility of bad matches because it uses full information. A recommended solution is to

[5]Obviously, this is a strong assumption and there may be bias due to unobservables. Given the complexity of the problem we try to handle in this chapter, we hope that this bias is empirically less important compared to the magnitude of bias that this method eliminates.

properly impose the common support upon implementation (Heckman, et al., 1998). Once the propensity score is estimated and used to compute the matching, the third and critical step is to perform a 'balancing test' to check if the matching procedure was effective, i.e. to test if matching balanced observable covariates across treated and control groups. A t-test on equality of means in the treated and control households suggests the extent to which the difference in the covariates between the treated and control groups have been eliminated so that any difference in outcome variable between the two groups can be inferred as coming mainly from the treatment (Heckman and Smith, 1995).

6.4 Results and discussions

This section presents results based on methods discussed in section 6.3. Before proceeding to the main results, a pair wise comparison between matched and unmatched households discussed in section 6.4.1 highlights the bias due to initial differences. The main results are provided in section 6.4.2 where after matching among participants and non-participants in each loan cycle, average participation effects using composite counterfactuals are compared to average participation effects from simple pair wise comparisons. Finally, as a robustness check, section 6.4.3 provides comparative results between matched composite and unmatched pair wise comparisons.

6.4.1 Matched versus unmatched pair wise comparisons

The effect of participation for both matched and unmatched households is given in table 6.2. This table gives a simple pair wise comparison of average treatment effect of participants as compared to non-participants (controls) in each year with and without matching. Note that in this table each new participant group's (i.e., t=1, …, 4) annual consumption is observed over subsequent observation years (i.e., v=1, …, 4). E.g., average annual consumption of new participants in the first observation year (t=1) is given under v=1 to v=4, hence, the table is diagonal. There are two columns under each observation year, which give average impact estimates for the same participant group with and without matching. Comparing the matched against unmatched estimates for the same participant group (in each t) gives the bias reduction after appropriately accounting for initial differences using the matching method. It can be observed that the average effect on each *new* participant's annual consumption (see diagonal) is higher for the unmatched than for the matched. E.g., for new participants at t=1, average annual consumption increased by ETB 476 before matching but after matching, the increase is reduced to ETB 398. As such, comparing the new participants (on the diagonal of the table) against their appropriately matched controls reduces the average impact. One implication is that compared to an average non-participant that constituted the unmatched controls, better off households have self-selected into the program in each year. Clearly, comparing the new participants in each year against non-participants without accounting for initial differences, in our case, overestimate impact. The bias remains even after entry (off-diagonal). However, the direction of bias differs from year to year.

Table 6.2 Simple pair wise comparisons between matched and unmatched households

Average participation effects on yearly household consumption expenditure								
Timing of participation	***1997 (v=1)***		***2000 (v=2)***		***2003 (v=3)***		***2006 (v=4)***	
	Matched	***Unmatched***	***Matched***	***Unmatched***	***Matched***	***Unmatched***	***Matched***	***Unmatched***
t=1	398.05***	475.67***	529.72*	506.30*	371.16**	268.15*	1487.97***	1663.48***
	(109.12)	(112.67)	(281.95)	(282.55)	(146.65)	(1.82)	(546.15)	(540.30)
t=2			133.16	258.85	-296.51	-439.12*	1457.28*	1135.41**
			(428.50)	(380.23)	(263.56)	(247.02)	(871.11)	(751.85)
t=3					717.04*	739.88**	497.06	1398.76**
					(462.32)	(378.99)	(1178.49)	(918.44)
t=4							429.15	2041.47***
							(1591.53)	(934.48)

Note: ***, ***, **, *, significant at the 1%, 5%, and 10% levels, respectively. Standard errors in parentheses.

6.4.2 Matched composite versus matched pair wise comparisons

The pair wise comparison of participation effects between matched and unmatched groups in the previous section suggests that matching reduces a significant part of bias due to differences prior to entry. However, it does not account for how participating households would have fared in subsequent years if they had not participated. Such effect is captured by the composite counterfactual estimated according to methods provided in section 6.3.1 and 6.3.2 (detail calculations are given in appendix 6A). Main results are given in table 6.3. For comparison purposes, matched pair wise effects are also provided for each outcome measurement period along with the composite effects. The columns in table 6.3 provide these comparative results for each group of participant ($t=1,\ldots, 4$) and in each outcome measurement period ($v=1,..,4$). Note that standard errors are not given for composite results because they are calculated from several matching results according to methods in (2) and (4) (see appendix 6A).

There are two important findings in this exercise. First, regarding the main causal question of comparing the effect of early versus late participation, the composite counterfactual results suggest that early participants have consistently fared better than late participants. Specifically, after accounting for both initial differences and potential future changes in the composition of participants and their controls, long-term participants have enjoyed relatively higher average annual consumption than short-term participants. In table 6.3, this can be seen by comparing the composite effects for each new participant group (i.e., at each t) against its preceding participant column wise. Note that the composite effect, for the most part, declines going from top (early participants) to bottom (late participants) in each column. One reason is that the effect of borrowing lasts longer than the specific period it refers to and that long- rather than short-standing participants are more likely to enjoy higher effects in terms of capacity to smooth consumption over time. Another is since participation is state dependent, at least, in this data set (see results in chapter three); the chances of repeat participation and hence further increases in consumption are higher for early than late participants.

Second, in contrast to simple pair wise effects, the composite effects provide conservative results in all comparisons except for the initial year[1]. This is because the composite effects take future potential counterfactuals into account whereas the pair wise estimates do not. In other words, not accounting for future potential counterfactuals overestimates impact. This is so because not participating in any earlier year does not preclude the possibility of participating in any later year and given positive effects of participation, not accounting for these chances of later participation overestimates impact of

[1] Note that in each initial year, the pair wise effect is the same as the composite effect because $t=v$ and there is no need to account for future potential counterfactual.

early participation. This can be elaborated using the most early participants (i.e., t=1) whose outcome is measured in four period. The composite effect in the last outcome measurement period (v=4) takes into account the fact that some of their matched controls (i.e., non-participants at t=1) have been able to participate at t=2, t=3 or at t=4. This reduces the average effect from ETB 1488 to ETB 1238. Clearly, the difference is the counterfactual for early participants had they not participated at t=1. Thus, failing to account for the different future pathways between participation and outcome measurement periods overstates the effect of (early) participation.

Obviously, many factors other than borrowing dictate changes in consumption levels over time and, with a slight downturn in 2003, average consumption increased between v=1 and v=4 for both participants and non-participants, albeit at different pace. Specifically, except in the bad year 2003 in which case there was a consumption downturn, the pair wise causal effects, for the most part, overestimate impacts because the counterfactual paths are not taken into account.

Table 6.3 Matched composite versus matched pair wise comparisons

	Household consumption expenditure measurement period							
	1997 (v=1)		*2000 (v=2)*		*2003 (v=3)*		*2006 (v=4)*	
Timing of participation	*Composite*	*Pair wise*	*Composite*	*Pair wise*	*Composite*	*Pair wise*	*Composite*	*Pair wise*
t=1	**398.045**	398.045*** (109.118)	**388.733**	529.716* (281.947)	**859.674**	371.162** (146.650)	**1238.704**	1487.966*** (546.147)
t=2			**133.156**	133.156 (428.498)	**-568.872**	-296.506 (263.564)	**524.370**	1457.280* (871.109)
t=3					**717.044**	717.044* (462.315)	**-292.578**	497.059 (1178.491)
t=4							**429.150**	429.150 (1591.532)

Note: ***, ***, **, *, significant at the 1%, 5%, and 10% levels, respectively. Standard deviations in parentheses.

Evidently, conventional parametric impact assessments that compare ‘ever participants’ to ‘never participants’ without considering the timing of the decision to participate and the different potential future pathways an individual household might have followed in the absence of the program would yield biased estimates.

Finally, given the relatively longer period the data set covers, including two drought years (1999/2000 and 2003) in between, it is interesting to see the implications of the effects of these differences in timings of participation on household consumption and hence relative capacity to cope with vulnerability during and after the drought years. Composite effects of participation in the first three periods i.e., at $t = 1$, 2, and 3, on annual consumption during the last three outcome measurement periods, i.e., $v = 2$, 3, and 4, are of interest here. Compared to controls, results suggest that the average annual consumption of the earliest (t=1) participants has increased steadily, including during and post drought years. Intuitively, sufficient time is needed for the cumulative impact of credit to take effect (King and Behrman, 2009). This is however not the case for later (t=2 and t=3) participants. In fact, although both participant groups have seen increased average consumption in the year they participated (which happened to be the drought years for both), in both cases, it has declined a year after participation (post drought years). A possible explanation for this is that households might have diverted loans to smooth consumption in the drought years, a common phenomenon despite DECSI’s claims of ‘productive’ use of credit. A study on the same MFI by Borchgrevink et al., (2005:68-69) finds indications of use of credit given for production purposes diverted to consumption during drought periods. This is also inline with the claim in chapter three that for households that are borrowing risk constrained, credit might be only useful as a last resort in times of distress. Moreover, the fact that loans are repaid after one year seems to explain the relative decline in participant households’ consumption in the post drought periods. Nevertheless, for the $t = 2$ participants, the result suggests this decline has been reversed in 2006[1]. It can therefore be concluded that relative to non-participants, earlier participants gained better capacities to cope with shocks and the earlier the better. This conclusion has to be taken with caution though because the results explicitly compare variations of average consumption due to credit and not overall consumption variability due to shocks.

6.4.3 Effects of changes in the composition of treatment and control groups in time- varying treatments

In sections 6.4.1 and 6.4.2 interest centered on the effect of timing of first-time participation in MFI borrowing on annual household consumption in subsequent years. Thus, the analysis

[1] For $t = 3$ participants, the effect in subsequent years is not known because the observation period does not allow for this.

was mainly based on entry, considering borrowing as an irreversible regime, i.e., once households participate, they remain members thereafter. However, there are borrowing dynamics after this entry event took place. Particularly, once in the borrowing regime, some households participated repeatedly, and others participated occasionally or never repeated at all. Details about these dynamics are given in Figure 6.1. This section presents effects of such borrowing dynamics, mainly effects of dropping out and repeat-borrowing in a particular year. Due to the complexity of applying composite counterfactuals, effects are analyzed pair wise. However, comparative results are provided such that the biases in handling dropouts discussed in section 6.2.2 are also evaluated.

Table 6.4 presents comparative results of (i) including dropouts in control groups but not in treatment groups (comparison 1) in contrast to including them in treatment groups but excluding them from the control groups (comparison 2). This comparison shows the bias in using the standard fixed-effects method that would naively include dropouts in control groups; (ii) excluding dropouts from both and excluding new participants from treatment groups (comparison 3).

Table 6.4 Effects of repeat-participation and changes in the composition of participants: matched and unmatched pair wise Comparisons

Average participation effects (ATT) on yearly household consumption expenditure									
Composition of treatment & control groups	*periods*	*1997 (v=1)* Matched	*1997 (v=1)* Unmatched	*2000 (v=2)* Matched	*2000 (v=2)* Unmatched	*2003 (v=3)* Matched	*2003 (v=3)* Unmatched	*2006 (v=4)* Matched	*2006 (v=4)* Unmatched
Comparison 1:	*t=1*	398.05***	475.67***	529.72*	506.30*	371.16**	268.15*	1487.97***	1663.48***
Treatment group:		(109.12)	(112.67)	(281.95)	(282.55)	(146.65)	(1.82)	(546.15)	(540.30)
Repeat and new	*t=2*			443.19	590.50*	-191.69	-240.76	2350.14***	2382.83***
participants at *t*				(371.22)	(363.09)	(223.45)	(179.72)	(695.20)	(694.10)
Control group:	*t=3*					-32.79	492.15**	2113.53***	2886.44***
Non-participants at						(294.99)	(211.20)	(708.53)	(722.82)
t (dropouts	*t=4*							1058.21	1656.06
excluded)								(970.11)	(1347.41)
Comparison 2:	*t=1*	398.05***	475.67***	529.72*	506.30*	371.16**	268.15*	1487.97***	1663.48***
Treatment group:		(109.12)	(112.67)	(281.95)	(282.55)	(146.65)	(1.82)	(546.15)	(540.30)
Repeat and new	*t=2*			388.84	305.73	-235.22	-326.40	1371.13***	1548.64***
participants at *t*				(307.48)	(285.22)	(143.11)	(148.14)	(591.65)	(544.88)
Control group:	*t=3*					301.20**	386.78***	1177.21**	1620.21***
Non-participants at						(150.61)	(149.85)	(581.24)	(552.19)
t (dropouts	*t=4*							1129.25*	1311.09*
included)								(771.02)	(756.86)

(Table 6.4 continued)

Comparison 3:	*t =1*	398.05***	475.67***	529.72*	506.30*	371.16**	268.15*	1487.97***	1663.48***
Treatment group:		(109.12)	(112.67)	(281.95)	(282.55)	(146.65)	(1.82)	(546.15)	(540.30)
Only repeat	*t=2*			790.60*	804.85*	-130.69	-112.56	3148.09***	3189.09***
participants at *t*				(488.71)	(426.08)	(240.56)	(215.60)	(838.37)	(776.01)
	t=3					-208.26	348.36*	2193.49***	3273.24***
Control group:						(318.01)	(208.59)	(752.78)	(733.31)
Non-participants at *t*	*t=4*							1190.91	1837.33
(dropouts excluded)								(2177.47)	(1410.30)

Note: ***, ***, **, *, significant at the 1%, 5%, and 10% levels, respectively. Standard errors in parentheses.

In comparison 3, only repeated participants are compared to their periodical non-participant counterparts. This can be used as reference for comparisons 1 and 2. Besides, this last comparison provides the effect of repeated participation on outcomes measured in different periods.

Comparing comparison (1) and (2) in table 6.4, shows that including dropouts in control (but not in treatment) groups biases impact even after adjusting for initial conditions using matching. However, the direction of the bias depends on the relative condition of dropouts in each year, which in turn may depend on the economic conditions in the year before. That is, in years when dropouts are those that have became worse off after participation, excluding them from the treatment group overstates impact. On the other hand, in years when dropouts are better off households, excluding them from treatment groups overstate impact both due to attrition and contamination effects. For participants in 2000, result shows that excluding dropouts from the treated groups and including them in the control group understates impact in all outcome measurement periods (see matched columns, table 6.4). On the contrary, excluding dropouts from the treatment group (including them in the control group) overstates impact because not only were relatively worse off participants selectively dropped out in the bad year 2003, but also the new participants in the same year were relatively better off. The latter can be seen by comparing the average participation effect of ETB 717 for matched new participants (table 6.3) and ETB 301 for matched new and repeat participants (table 6.4) in 2003. The same is true to participants in 2006. This means, there is not only a strong selection processes at work but also the direction of selection depends on the underlying economic conditions households face in each period. Note that in the unmatched case, the same comparison provides a different picture, concealing the selection processes.

Lastly, the last part of table 6.4 (comparison 3) provides the effect of repeated participation, excluding dropouts and new participants. Once again, comparing the veteran participants to their non-participant counterparts by excluding dropout overstates impact even when new participants are excluded, e.g. for participants in 2000, because only worse off participants dropped out systematically. Thus, for the most part, results in comparison 3 are higher than in comparison 1, which are in turn, for the most part higher than results in comparison 2, reflecting the consistency of the results obtained.

6.5. Conclusions

This chapter dealt with the methodologically challenging question of assessing the impact of differences in the timing of participation in microfinance credit using a four-wave panel data covering ten years that comes from a rural microfinance in Ethiopia. Specifically, main questions addressed include (a) whether or not early participation, as opposed to late participation, matters in terms of increases in average annual household consumption, (b) a methodological issue of consideration of potential future path ways of control groups when the timing of participation and outcome measurement are different, and (c) overcoming impact estimation biases due to the dynamics of borrowing, mainly dropouts and new entrants.

In the empirical methodology the chapter argued that parametric impact assessment methods such as the fixed-effects method may yield biased estimates when the treatment variable is dichotomous and units are observed more often than in the classic two-period panel data case because such methods exploit on individuals being "on" and "off" the treatment over time, which contaminates the impact estimate. An alternative method is used that treats participation in credit as an irreversible regime and identify impact from the timing of onset of participation. As such, only non-participant households up to the time of entry of participants are considered as candidates for control. The propensity score matching is used to balance potential initial heterogeneities among participants and non-participant controls.

The results of the propensity score matching indicate that matching participants and non-participants on some basic pre-treatment characteristics reduces substantial amount of selection bias. Comparisons between matched and unmatched average treatment effects suggests that over the years 'better off' households tend to participate. The results from the main analysis indicate that early than late participants enjoyed higher average annual consumption over time. Comparing the composite effects against simple pair wise effects, the composite effects provide conservative results because they take future counterfactuals into account. Thus, not accounting for potential future path ways overstates impact. The results also suggest that compared to their respective control groups, credit has had better shock cushioning effect for earlier than later participants.

The analysis has also considered effects of the dynamics of borrowing once in the borrowing relationship. Particularly, pair wise effects of repeat-borrowing and dropping out are considered in a comparative way. Results suggest including dropouts in control (but not in treatment), as in fixed-effects, biases impact even after adjusting for initial conditions using matching. However, the direction of the bias depends on the relative condition of dropouts in each year. Further, the comparative analysis indicates that because of the selection processes at work when individuals dropout or repeat, impact assessments that compare participants and non-participants, covering longer periods but only adjusting for initial conditions but not for dynamics in mean time are likely to be biased.

Appendix 6A. Calculating ATT based on composite counterfactuals:

1) *t=1* and *v=1*

$$ATE_1 = E(\Delta C_v^{d=t}|X) = E(C_1^{d=1}|X) - E(C_1^{d>1}|X)$$

$$= 1960.363 - 1562.318$$

$$= 398.045$$

2) *t=1* and *v=2*

$$ATT_2 = E(\Delta C_v^{d=t}|X) = E(C_2^{d=1}|X) - E(C_2^{d>1}|X)$$

$$= E(C_2^{d=1}|X) - \left\{p(2)\cdot E\left(C_2^{d=2}|X\right) + q(2)\cdot E\left(C_2^{d>2}|X\right)\right\}$$

$$= (2950.014) - \{(0.38)(2643.837) + (0.62)(2510.682)$$

$$= 388.733$$

3) *t=1* and *v=3*

$$ATT_3 = E(\Delta C_v^{d=t}|X) = E(C_v^{d=t}|X) - E(C_v^{d>t}|X)$$

$$= E(C_3^{d=1}|X) - \left\{p(2)\cdot E\left(C_3^{d=2}|X\right) + q(2)\cdot p(3)\cdot E\left(C_3^{d=3}|X\right)\right.$$

$$\left. + q(2)\cdot q(3)\cdot E\left(C_3^{d>3}|X\right)\right\}$$

$$= (2383.919) - \{(0.38)(1889.588) + (0.62)(0.30)(2960.391)$$

$$+ (0.62)(0.70)(2243.347)\}$$

$$= 859.674$$

4) *t=1* and *v=4*

$$ATT_4 = E(\Delta C_v^{d=t}|X) = E(C_4^{d=1}|X) - E(C_4^{d>1}|X)$$

$$= E(C_4^{d=1}|X) - \left\{p(2)\cdot E\left(C_4^{d=2}|X\right) + q(2)\cdot p(3)\cdot E\left(C_4^{d=3}|X\right)\right.$$

$$\left. + q(2)\cdot q(3)\cdot p(4)\cdot E\left(C_4^{d=4}|X\right) + q(2)\cdot q(3)\cdot q(4)\cdot E\left(C_4^{d>4}|X\right)\right\}$$

$$= (7604.657) - \{(0.38)(6691.062) + (0.62)(0.30)(5961.886))$$

$$+ (0.62)(0.70)(0.34)(6537.704) + (0.62)(0.70)(0.66)(6108.554)\}$$

$$= 1238.704$$

5) *t=2* and *v=2*

$$ATT_5 = E(\Delta C_v^{d=t}|X) = E(C_2^{d=2}|X) - E(C_2^{d>2}|X)$$

$$= 2643.837 - 2510.682$$

$$= 133.156$$

6) *t=2* and *v=3*

$$ATT_6 = E(\Delta C_v^{d=t}|X) = E(C_3^{d=2}|X) - E(C_3^{d>2}|X)$$

$$= E(C_3^{d=2}|X) - \left\{p(3).E\left(C_3^{d=3}|X\right) + q(3).E\left(C_3^{d>3}|X\right)\right\}$$

$$= (1889.588) - \{(0.30)(2960.391) + (0.70)(2243.347)$$

$$= -568.872$$

7) *t=2* and *v=4*

$$ATT_7 = E(\Delta C_v^{d=t}|X) = E(C_4^{d=2}|X) - E(C_4^{d>2}|X)$$

$$= E(C_4^{d=2}|X) - \left\{p(3)\cdot E\left(C_4^{d=3}|X\right) + q(3)\cdot p(4)\cdot E\left(C_4^{d=4}|X\right)\right.$$

$$\left. + q(3)\cdot q(4)\cdot E\left(C_4^{d>4}|X\right)\right\}$$

$$= (6691.062) - (0.30)(5961.886) + (0.70)(0.34)(6537.704)$$

$$+ (0.70)(0.66)(6108.554)$$

$$= 524.370$$

8) *t=3* and *v=3*

$$ATT_8 = E(\Delta C_3^{d=3}|X) = E(C_3^{d=3}|X) - E(C_3^{d>3}|X)$$

$$= 2960.391 - 2243.347$$

$$= 717.044$$

9) *t=3* and *v=4*

$$ATT_9 = E(\Delta C_4^{d=3}|X) = E(C_4^{d=3}|X) - E(C_4^{d>3}|X)$$

$$= E(C_4^{d=3}|X) - \left\{p(4)\cdot E\left(C_4^{d=4}|X\right) + q(4)\cdot E\left(C_4^{d>4}|X\right)\right\}$$

$$= (5961.886) - \{(0.34).(6537.704) + (0.66).(6108.554)\}$$

$$= 292.578$$

10) *t=4* and *v=4*

$$ATT_{10} = E(\Delta C_4^{d=4}|X) = E(C_4^{d=4}|X) - E(C_4^{d>4}|X)$$

$$= 6537.704 - 6108.554$$

$$= 429.150$$

CHAPTER 7

CONCLUSIONS AND DISCUSSION

7.1 Introduction

The general objective of this thesis is to examine the mechanisms of providing credit through microfinance and assess the long-run borrowing effects on household welfare in Ethiopia. This general objective is broken down into four specific objectives that were dealt within separate chapters. This chapter presents a summary of the main conclusions from these chapters. It also outlines how these different issues integrate each other and contribute to the general discussion on improving the provision of financial services in risky rural environments of developing countries, such as Ethiopia. This chapter proceeds as follows. Section 7.2 summarizes the main conclusions and presents the lessons from chapter three to six. Section 7.3 integrates and places the main issues in this thesis into the general body of the literature on microfinance. Section 7.4 provides some suggestions for future research.

7.2 Summary of main conclusions

The first specific objective of this thesis was to investigate if risks involved in joint liability lending contracts, viz. risk of partner failure and the threat of being denied future access to credit in case of group repayment failure, impede participation of potential borrowers. This is analyzed in chapter three in a dynamic stochastic theoretical framework where, as opposed to previous related studies, household borrowing is considered within overall household production and consumption decisions given available endowments to the household. The theory developed in this chapter shows that households' decisions to participate in borrowing consider the relative riskiness of MFI borrowings by evaluating the sum of present and discounted stochastic future benefits with and without borrowing. Empirical results show that these risk elements embedded in the joint liability lending contract reduce the probability of participation in borrowing. Moreover, risk of losing future access to credit as proxied by inter-household differentials in major sources of future liquidity (e.g., livestock endowment) and insurance (e.g., access to government food safety nets) determine the probability to participate. It is also found that systemic shocks such as rainfall, an indicator for the recurring draughts in the area, influence the decision to participate in borrowing negatively. However, there is also evidence that participation is state dependent: those that happened to participate once are more likely to repeat it, implying that these risks are *perceived* higher by non-participants than participants. In general, it can be concluded that while the contractual methods used in microfinance are innovative approaches to deal with information and cost problems, perceived risks of such methods raise a hurdle for participation such that potential borrowers might stay away from borrowing. It should be noted that in this specific study, this effect is exacerbated by (a) the volatile economic conditions (e.g., covariant rainfall risks) in which households in the study area operate, and (b) the fact that the 'limited liability' waiver routinely assumed in theory, which provides insurance in case the whole group announces inability to repay is diminished or does not exist totally in practice. This latter effect makes the shift in borrowing risks from the lender to borrowers complete.

These findings have important lessons for MFIs operating in the Ethiopian as well as similar contexts. The group lending methods in microfinance, although seemingly helpful, do not fit everywhere. Particularly, there is a clear trade-off between providing credit access through methods that induce borrowers to behave responsibly and the additional (*perceived*) contractual risks these methods involve. These methods limit MFIs' outreach to poor but potential entrepreneurs to the extent that the disincentive effect of the additional risks outweighs the benefit. This is more so for the poorest households operating in risky economic environments (as in smallholder agriculture in Ethiopia) where insuring the very survival of the household is at stake and no additional credit is expected after a shock simply because additional credit is tied with repayment of previous group loans. Similar conditions were reflected in the discussion with key informants during the survey. Households face difficult borrowing decisions, which involve running the risk of selling assets or consumption shortages to repay for partners in drought years or give up all access to future loans. Recent theoretical extensions show that some of the innovative methods in microfinance (e.g., dynamic incentives) can be effectively used even with individual lending without having to depend on groups and associated partner risks (Armendáriz de Aghion and Morduch, 2000; Tedeschi, 2006). There is also some evidence of recent shifts to individual loans with similar contractual arrangements (Xavier and Karlan, 2006). It is therefore time for MFIs to go out of the one-size-fits-all box and look for alternative methods that can work in their specific conditions. In DECSI's case, reducing contractual risks (e.g., guaranteeing rescheduling or not obliging to repay in bad years) and providing full-fledged financial services, including consumption credit may help farmers to access production credit.

Given the contractual risks involved in group lending discussed in chapter three, an interesting issue to investigate was who borrowers choose as their partners. This was the subject of chapter four. It focused on the empirical analysis of the risk matching behavior of borrowers in microfinance group formation, and investigated the reasons behind this group behavior and its implications for repayment in the Ethiopian context. Specifically, the aim was to test if the groups that arise are homogeneous or heterogeneous and why. Instrumenting for potential endogeneity problems, results show strong statistical evidence that rejects the homogeneity hypothesis in favor of heterogeneity, even after controlling for problems in matching (e.g., inability to find a preferred match). Results are consistent with similar previous empirical results from urban (Sadoulet and Carpenter, 2001) and semi-urban (Lensink and Habteab, 2003) environments. However, this study brings new evidence on the synchronization of joint liability groups with community-based networks and religious gatherings. It is found that joint liability groups can be heterogeneous because, in this specific context, they are synchronized with these networks that do not necessarily need to be homogenous themselves but rely on trust, reputation, and reciprocity developed over the years. As such, neither is the group formation outcome ought to be homogeneous to promote repayments. In fact, in some instances ROSCA savings are deliberately synchronized with joint liability repayments. Again, the lesson is that when designing their products, MFIs need to consider such complex contexts and exploit them to their advantage.

While most economists are fascinated with the theoretical explanations of how microfinance can be successful in overcoming the well known banking problems and promote repayment rates, little is known yet about the actual impacts of microfinance on poverty. Chapter five and six of this thesis aim at contributing to the ‘impact evaluation gap’ in microfinance both methodologically and by evaluating the long-run benefits of participation in MFI credit from two important dimensions, namely, the degree or *intensity* of participation over the years and *timing* of entry or participation. Chapter five assesses impact from the degree or *intensity* of participation, regardless of the *timing* of participation. By using individual trends, the fixed-effects approach which traditionally mitigates only time-invariant unobservables is innovatively expanded to account for time-varying individual specific unobservables. This model is further specified to explicitly account for level of participation over the years. Credit impact is measured on two important household welfare indicators: per capita household consumption and housing improvements. Results show that having participated in credit at least once raises per capita household consumption and probability of housing improvements significantly. The flexible specification shows that one-time participation has no impact on housing improvements but does lead to a significant improvement in per capita consumption. Impact increased with length of participation in both cases however. The general conclusion is that not only is participation in credit useful to raise household welfare in the short term but also the effect lasts longer beyond the participation periods. There are three implications from these conclusions. First, estimates based on one or two period snapshots are likely to underestimate credit impact. Second, since the time needed for borrowing effect to materialize differs among welfare indicators and evaluating effect based on a single indicator measured in one-shot may therefore provide a partial picture of credit impact. Research aiming at rigorously evaluating the potentials of MFI credit must therefore take both its multidimensional and temporal effects into account. Third, since realizing the full benefits of borrowing in marginal environments such as the study area require longer periods, MFIs in those areas should be equipped to provide continuous loans rather than focusing on ambitious plans of reaching wider clients but with shorter graduation periods.

Chapter six assesses impact from the *timing* of first-time participation and outcome measured during participation thereafter. Specifically, annual consumption expenditure of participant households is compared to non-participant controls adjusting for initial differences using matching method and accounting for future potential participation paths of participants had they not participated. Two important conclusions are drawn from this approach. First, the pair wise versus composite counterfactual comparative analysis suggests that not accounting for initial differences and future potential paths yields biased estimates. The implication is that in long surveys with repeated outcome observations, standard parametric methods that only account for sources of bias due to differences in pre-participation characteristics are still subject to biases due to dynamics between participation and outcome measurement period. Second, considering the *timing* of participation, results suggest that in general early participants fared better than later participants. Moreover, although average consumption declined for both participants and non-participants after the shock in 2003, the very early participants among the early participants have seen increases in average consumption even after the shock due to the cumulative effects of borrowing. This again suggests that beyond the benefits reaped immediately, borrowing plays an important role in building resilience which would not be easily observed in studies that consider simple before and after comparisons.

7.3 Discussion

This section presents a brief discussion on how the different issues dealt in separate chapters of this thesis and the contributions in each of them relate to each other and to the larger body of literature on microfinance. Where necessary, innovative methodological approaches introduced to tackle specific empirical issues in this thesis are also discussed.

Understanding the genesis of MFIs helps to mirror on their present day performances as well as to visualize where they are heading today. Chapter one sets the general background for the analyses in this thesis. It briefly synthesizes the historical developments in rural financial markets and practical experimentations therein that eventually led to the emergence of present day MFIs. Two important historical backgrounds that shaped development thinking regarding rural finance are worth mentioning for the discussion here. First, donors' increased focus towards assisting the development of 'informal sector' micro enterprises (Dichter, 2007: 3) in the 1970s in one hand, and the overwhelming critique over subsidized, state-run, rural financial institutions (Armendáriz de Aghion and Morduch, 2005: 8-11) on the other, challenged the way rural finance was channeled. Second, on theoretical grounds, developments in information economics and agency theories that helped explain why conventional banks fail to reach the poor stimulated academics to shift attention to parallel experimentations in micro financing. The intersection of these two developments boosted the microfinance momentum to reach its unprecedented peak in recent years. Marking such a peak, the UN declared 2005 a 'Microcredit' year and Mohamed Yunus and his Grameen Bank won the 2006 Nobel Peace Prize. Microfinance has become a development catch word since. It is being spreading around the world. Nevertheless, many practical as well as theoretical questions remain.

Four specific objectives of this thesis within the framework discussed above are introduced in chapter one. The relatively long microfinance practices in rural areas of Ethiopia present a suitable context to examine these issues. Chapter two presents the operations of one of largest MFIs in Ethiopia in which the objectives of this thesis are empirically examined. This chapter also laid the ground for the rest of the chapters by describing the specific institutional and socioeconomic environment in which this MFI operates.

The theoretical and empirical analysis in this thesis begun by raising an important issue in microfinance discussed in chapter three. A core issue in microfinance credit is that it systematically exploits elements of social capital that inherently exist in poor areas into an incentive contract that substitutes collateral- a conventional requirement of lending that is virtually unavailable to the poor. This is a 'good intention' that gives the opportunity to provide millions of households *access* to credit that would otherwise remain unbankable. Unfortunately creating access to credit is not synonmous with actual use of credit, particularly when the 'good intentions' in microfinance go awry. Chapter three examines whether or not the well-intended contractual elements in microfinance can also deter realizing access to credit. How effective the collateral substitutes in microfinance are to achieve the intended goal of providing access to credit, particularly in risky environments as in the semi-arid areas of Ethiopia, is theoretically and empirically less clear (see e.g., Karlan and Goldberg, 2007). The theoretical and empirical analyses in this chapter provide fresh insights into how the specific risks of lending to groups combined with the ultra punishment threats can limit credit use.

Closest to the contributions of this chapter are theoretical work by Madajewicz (1997) who emphasized "the incentives used in microfinance are quite extreme" and Tedeschi (2006), who proposed improvements on contracts currently used in microfinance and underlined that "default punishment threats need not be lifetime". The fundamental point here is that as long as repayment is strictly enforced even when rains and harvests fail, these contractual risks simply add up to the overall risk households manage including by forgoing profitable but risky opportunities. This is inline with a recent empirical finding by Dercon and Christiaensen (2007) from Ethiopia that concluded adoption of modern inputs is reduced due to 'downside risk' related to strictly enforced input loans that disregard harvest outcomes. More specifically, since most input loans in rural Ethiopia are provided by MFIs using the same contractual mechanisms discussed in this thesis, it follows that part of the 'downside risk' of these loans is contractual. Broadly speaking, to the extent that such contractual risks are perceived important, poor households are forced to forgo opportunities to move out of poverty so as to avoid further distitution. The main conclusion from this chapter therefore runs consistent with Fafchamps' (2003: 30-31) general conclusion that says "*for credit to exist, credit contracts must allow for conditional default, that is, must mix an element of insurance with pure credit"*. The challenge remains of course how to design a credit contract that insures the borrower in case of harvest failure while providing the lender a guarantee to circumvent free-riding behavior of the borrower.

Chapter four discusses the group formation process and its effects on the resulting pool of borrowers. This issue is at the center of the joint liability theory in microfinance. Many theoretical studies (e.g., Ghatak 2000) show that joint liability induces borrowers to the extent they can, to form groups of homogenous (similar) risk types. Some recent studies (e.g., Sadoulet, 1999) however show in contexts where insurance is missing, that groups maybe formed with the purpose of intra-group risk-sharing and as a result can be heterogeneous. This alternative hypothesis is also supported by another recent theoretical work that considers behavioral effects of *repeated interactions* among group members (Guttman, 2008). This is an active research area for which the Ethiopian context is used as a natural experiment. This chapter brings additional evidence to this existing knowledge on group formation. As in the two other previously studied cases, Guatemala (Sadoulet and Carpenter, 2001) and Eritrea (Lensink and Habteab, 2003), the studied Ethiopian case reveals the formation of groups of heterogeneous risk types as opposed to the standard homogeneity assumption in the theoretical microfinance literature. But this chapter also brings empirical evidence that does not necessarily overlap with the previous findings. Specifically, previous studies sought to relate heterogeneity with insurance motivated by *direct* side-payments from risky to safe borrowers (Sadoulet, 1999). This chapter finds indications of links between microfinance credit groups and traditional networks, particularly rotating credit and saving associations (ROSCAs) and long standing religious gatherings that are common in Ethiopia. Even if there was no sufficient evidence of side-payment among groups in the sample, intuitively, trust and reputation developed within these traditional groups appears to be used as a foundation to formal credit groups in some of the study areas. This is basically in line with the original 'social capital' premise with which group lending is justified to function (e.g., Varian, 1990; Stiglitz, 1990; Banerjee et al., 1994; Besley and Coate, 1995).

Chapter five and six contribute to the largely missing long-term microfinance impact evidence. Existing studies show impact of credit on poverty is marginal, including in famous MFIs with the highest repayment rates such as the Grameen Bank (e.g., Morduch, 1998). However, most of these studies measure impact over a short time frame, which may undermine dynamic effects over time. Previous chapters (three and four) have indicated that most of the mechanisms in group lending work with dynamics; particularly borrowing is state dependent (chapter three) and group formation exploits reputation that is built on repeated interactions over many years (chapter four). Besides, donors and governments may also want to know what happened to microfinance clients in the long-run and also to those that accessed credit more than once. The existing microfinance evaluation literature has yet concentrated on determining the short-run marginal effects of MFI loans (e.g., Pitt and Khandker, 1998; Coleman, 1999) or on effects of observations between two points in time (e.g., Khandker, 2005, Copestake, 2001; Tedeschi, 2008), all of them on a single poverty indicator. As suggested by Copestake et al (2001), chapter five has taken this effort one step further by measuring effects of repeated borrowing over a relatively longer period as well as measuring it on two key welfare indicators in the study area. This chapter identifies impact from the degree or number of participation times in borrowing, regardless of timing of participation. Here, the comparison is not just, as it is often done, between borrowers and non-borrowers but also considers differences in the degree of participation among borrowers themselves. A data set covering a relatively long period (i.e., ten years) in which households are observed four times enables to identify long-term impact from variations in the degree of participation among households. Recent developments in econometric panel data techniques, namely the trend model along with standard fixed-effects are also employed to control for selection bias common in identifying credit impact.

Chapter six, on the other hand, identifies impact from timing of participation relative to potential outcomes of non-participants. This chapter builds on chapter five. Instead of depending on number of times the household has participated, this chapter assesses impact from first-time entry (or participation) where outcome is measured at several points thereafter, and compared to non-participants (control) at each entry time. Insights from chapter three indicate that participation or entry depends not only on initial household heterogeneities but also their dynamics over time. A recent method innovatively implemented in chapter six helps to attain comparability among participants and controls by accounting for initial characteristics as well as changes between the time of entry and outcome measurement. The propensity score matching method is used to attain initial comparability between participants and non-participants. The draw back in this modeling is that it does not reveal the separate effects of repeat-borrowing over time. However, an advantage in this approach is that it appropriately accounts for heterogeneities due to dynamics in borrowing, mainly timing of decision and dropping out that are often challenging to control in standard parametric methods (Karlan, 2001, Tedeschi and Karlan, 2006). To our knowledge, these two last chapters presented the first impact evidences from two distinct dimensions of such a long span of participation in microfinance. Besides, the empirical methods implemented in these two chapters are recent econometric methods rarely exploited to mitigate selection bias that is inherent in microfinance impact assessments.

To wind up, in a nutshell, this thesis has touched upon three core steps in the microeconomics of the provision of credit services to rural households of developing countries, i.e., the decision to participate, the process of participation (group formation), and welfare effects of participation.

Finally, it would be unfair to close this discussion without providing some pragmatic reflections about microfinance in the study area. First, although to a limited extent mostly due to the methodological limitations discussed in chapter three, microfinance has been usefully integrated into households' life in the region. DECSI has successfully internalized its presence as an alternative source of finance in rural areas of Tigray. Most importantly, it has succeeded to portray itself as a business-oriented financial institution as opposed to previous confusions towards it (e.g., confusions such as seeing it as an institution that distributes public or NGO finance). However, such a presence is just a potential far from required level of realization. As such, to many, DECSI's joint liability based credit is just as a 'ripe grape' in the garden that is not easy to reach. The conclusions from chapter three indicate that not all 'potential' households benefited from the presence of credit in their villages. Some (e.g., those with less asset endowments) have refrained from it, just to avoid indebtedness and potential future debt traps. Others have however afforded what it takes to borrow and the conclusions in chapter five and six indicate that those that accessed it have benefited, albeit to a limited extent (see results in table 5.5 and 6.2 in the context of three years interval between observations). Second, other than credit, DECSI has managed to promote a vibrant banking culture (e.g., saving, money transfer, pension payments) among villagers. These are remarkable developments that defy some of the 'pessimistic views' about the viability of rural microfinance. However, a lot remains to be done, particularly, with regard to sustaining these successes as well as reaching the poorest potential borrowers. As a last word, DECSI has to reconsider not just its joint liability lending method but all its one-size-fits-all policy and pragmatically look into specific client niches and adapt its methods to each niche (e.g., the emerging interest in micro-irrigation and water diversion is one niche). It is also time for DECSI to revise its simplistic approach of 'productive credit' and focus on holistic banking services including for consumption. We believe, transforming the existing 'sub-branches' into decentralized semi-banks with full-fledged services is a step in the right direction. The findings and lessons from this thesis can be taken as starting points towards this direction.

7.4 Future research

As has been emphasized throughout this thesis and elsewhere in the literature (e.g., Armendáriz de Aghion and Morduch, 2005; Karlan and Goldberg, 2007; Hermes and Lensink, 2007)[1], empirical research on microfinance lags far behind the theoretical fine-tunings. This

[1] Hermes and Lensink (2007) provide a survey of the most recent empirical research on microfinance. Karlan and Goldberg, (2007) discuss recent microfinance impact evaluations.

section outlines a future research agenda related to the specific analyses as well as conclusions of this thesis.

The analysis in chapter three concentrated on how contractual risks ration out potential microfinance borrowers. There are two important issues that follow from the analysis in this chapter. First, empirically, the focus has been on whether potential borrowers decide to participate or stay away from group borrowings in a given borrowing year. This however assumes the extreme situation where potential borrowers decide not to borrow. It may well be that households react to uncertainty by reducing the amount of borrowing rather than avoiding it altogether. Investigating such an 'amount rationing' effect of contractual risks is an interesting future research agenda. If information on amount borrowed is available, this can be done using a standard Heckman selection procedure.

Secondly, in order to separately analyze effects of contractual risks on potential borrowers that would like to use loans responsibly and remain committed to the MFI if they are able to repay (as opposed to those that would strategically default), the theoretical analysis in chapter three assumes no strategic group interactions among group partners. This avoids the analytical complications of intra-group interactions and simplifies the analysis to interactions between individual borrowers and the MFI. The inclusion of group interactions into the analysis may or may not exacerbate contractual risks and hence effects on individual participation. The empirical analysis has however attempted to capture this element by including indicators for intra-group interactions. Nevertheless, incorporating such intra-group strategic interactions in a unified theoretical analysis within the dynamic framework where risks are correlated and the household is considered as a single decision making unit helps to get further insights on the overall effects of contractual risks. This is an interesting exercise where theoretically motivated future empirical research can focus on.

Empirical insights from chapter four indicate that joint liability groups in microfinance might be formed based on other local groups established for other purposes long before microfinance. In the Ethiopian context, these include ROSCAs (e.g., farmers that engage in off-farm activities) and religious and cultural gatherings. A considerable theoretical literature on rotating credit and saving associations (ROSCAs) explains what holds them not to fall apart for many years (e.g., Besley et al., 1993). There is some evidence suggesting that some of the reasons that help ROSCAs to exist are also those same social factors that are expected to help MFIs to succeed. One common explanation is the enforcement mechanism both use. Both involve groups and use informal understandings among friends and acquaintances (Armendáriz de Aghion and Morduch, 2005: 57-67). However, each of them has a distinct contractual and financial arrangement. It is therefore not clear how both ROSCAs and formal credit groups can co-exist in a synchronized way, given these differences in arrangements as well as incentives for individual members. Varian (1989) and Besley (1995:2187) provide interesting introductory discussions to this issue but still has received insufficient attention in the literature. An important issue to investigate is the incentive mechanisms that link joint liability groups to ROSCAs, particularly with regard to the risk heterogeneity result in joint liability. This may help to redesign the provision of financial services in risky environments without relying on mechanisms that involve additional group risks.

Empirical research on the impact of microfinance, particularly of credit and its potential to eventually extricate households out of poverty in the face of high covariant risks in poor semi-arid agricultural environments is yet limited. Much of empirical research in microfinance credit impact comes from urban and semi-urban, non-agricultural, borrowers. This thesis has brought some insights into this missing knowledge. It also contributes on how to deal with the challenges of assessing credit impact from panel data covering long periods. Further research is required to determine if the empirical findings, particularly of long-run credit impacts, in this thesis can be established using similar approaches from other similar biophysical and socioeconomic environments. Moreover, impact analysis in this thesis has focused on participation or intensity of participation based on whether or not a household has participated in a particular loan period. Future research may however concentrate on amount of loans or cumulative receipt of loans over time rather than depend on dichotomous participation information.

Finally, applied research that can be of high practical value to MFIs in Ethiopia include 1) designing a localized, and client-niche based loan contracts (e.g., credit for irrigation motor pumps) that may exploit existing mechanisms in microfinance (e.g., dynamic incentives) but without necessarily depending on groups, 2) exploring and designing a more localized and simple screening mechanisms based on lessons learned from this thesis (e.g., ROSCA members often engaged in petty trading) or exploring the potentials of household assets (e.g., user-rights of land) as collateral are some of the outstanding issues to consider.

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SUMMARY

The poor's lack of access to credit is one major obstacle to economic development in poor countries. Providing credit access to the poor is however challenging and requires managing small and fragmented loan transactions cost-effectively as well as ensuring repayments. At the center of these challenges are two fundamental problems: lenders' lack of information about poor borrowers and poor borrowers' lack of collateral to pledge as security to lenders. Information problems are common to all lenders, including to conventional banks for which they use collateral. Understandably, since both problems – lack of information and lack of collateral – coexist in poverty, poor borrowers are unattractive to conventional lenders. The challenge of providing credit access to the poor therefore boils down to the poor's lack of collateral due to poverty itself.

In recent years, microfinance institutions (MFIs) have come up with the hope of meeting the credit access - collateral poverty deadlock by using innovative mechanisms that overcome information problems. Many MFIs use the group lending method where borrowers are required to form small groups in which they are jointly liable for each other's loans. Joint liability introduces group (social) pressure into the borrowing contract that induces borrowers to behave in the interest of the MFI. Specifically, group pressure encourages borrowers to self-select each other (*peer selection*), monitor each other's loan use (*peer monitoring*) and enforce repayments. In addition, lenders use the threat of banning the entire group (in some cases the entire village) from future loans if one or more of the group members fail to repay. This is an additional leverage for MFIs to enforce group loans. In effect, losses due to unsuccessful projects are greatly reduced because successful entrepreneurs within each group will cover part of these losses. As such, a significant part of the costs and risks involved in lending are transferred from the lender to borrower. Several theoretical papers argue that such reduction in cost and risk gives MFIs way to provide credit access to poor borrowers at relatively lower average interest rates, a hope that has been coined as a 'win-win' solution to the old problem.

This hope has received considerable attention internationally and generated immense support from global donors and enthusiast individuals. Partly driven by this global support, MFIs are now at the center of many poor countries' poverty reduction and development strategies. In most countries, the objective of MFIs is twofold: reducing the risk of income shocks to help reduce poverty, and raising asset accumulation to encourage private activity. According to the 2009 Microcredit Summit Campaign report, as of December 2007, the number of MFIs globally has jumped to 3,552 (from only 618 in 1997) serving over 150 million clients worldwide of which 106 million are in the poorest (with less than one dollar a day) category. According to this report, MFIs have now reached roughly over a third of the 1.3 billion poorest people globally. However, this figure may only tell part of the story (i.e. access) and despite some progress in access, evidence regarding the success of borrowers and

individual MFIs around the world is mixed. In many circumstances, potential borrowers are not attracted to group loans as expected. Little is known as to why many potential borrowers stay away from loan products offered. The extent of benefits gained by those that accessed these loans, particularly, for several years is also unknown. Generally, empirical research in microfinance lags far behind the theoretical fine-tunings. Particularly, whether and to what extent the innovative mechanisms of providing credit are successful in view of the diverse socioeconomic and biophysical settings in which MFIs operate is yet unclear.

The objective of this thesis is to examine the mechanisms of providing credit through microfinance and assess the long-run borrowing effects on household welfare in Ethiopia. The focus is on understanding and empirically investigating the behavioral responses of borrowers to some of the building blocks of the innovative methods in microfinance as well as evaluating observed household welfare effects of accessing these loans over a relatively longer period. From this general objective, four specific objectives are defined and analyzed in separate chapters.

The Ethiopian situation provides an interesting environment to meet these objectives. Two unique data sets – a panel and a cross-sectional - that come from a rural microfinance in Ethiopia are used in this study. Chapter two describes these data sets and the specific biophysical and socioeconomic context of the study area. Building on previous studies, a five-wave panel data set on 400 borrower and non-borrower households that spans ten years (1997-2006) with intervals of almost three years is used. These households are randomly selected from sixteen villages in Tigray state of Ethiopia, all of them covered with microfinance. This data set is used to meet the first, third and fourth objectives. For the second objective, a specialized group-based cross-sectional data is collected in 2003 on 201 borrower households from the same zones.

In chapter three a dynamic stochastic theoretical framework that takes two types of risks involved in joint liability lending, i.e., risk of partner failure and risk of losing future access to credit, explicitly into account is used to analyze if these contractual risks impede participation of households in MFI borrowing. Results from a dynamic panel probit model show that these risks reduce the probability of participation in borrowing. Other systemic risks (e.g., recurrence of droughts) in the area, level of endowments, and access to government safety nets also determine participation. Most importantly, it is found that the probability of repeat-borrowing is higher than the probability of new participation, which implies that these risks are perceived higher and hence more stringent to non-participants than participants. It is concluded that there is a clear trade-off between providing credit access through mechanisms that induce borrowers to behave responsibly and the additional (perceived) contractual risks that these mechanisms involve, particularly in risky environments such as the drought prone areas in northern Ethiopia.

Given the joint liability contractual risks, the type of groups that arise when households decide to participate in group loans is empirically analyzed in chapter four. Specifically, the hypothesis that groups formed are of homogenous rather than heterogeneous risk profiles is tested. The empirical analysis takes potential endogeneity between choice of own and partner risks as well as difficulties to find a preferred partner into account. Results from the cross-section of borrower households show strong statistical evidence rejecting the homogeneity hypothesis in favor of heterogeneity. This chapter further investigated if heterogeneity is the result of missing insurance as suggested in the literature and if this result has implications for repayment performance of group members. There is no sufficient evidence supporting the link between risk heterogeneity and side-payments among group members. Instead, other trust based social networks, and already existing traditional saving and credit groups seem to underlie group formation in these areas. Such social networks are often synchronized with credit groups and influence the probability of repayment positively.

Chapter five and six of this thesis focus on evaluating the impact of long-term participation in MFI credit, considering the *duration* and *timing* dimensions of participation. In chapter five impact is evaluated from the intensity of participation over the ten years on household welfare indictors, namely, per capita annual household consumption expenditure and housing improvements. The latter is an important local welfare indicator in Tigray. By using individual trends, the fixed effects approach which traditionally mitigates only selection bias due to time-invariant unobservables is innovatively expanded to also account for time-varying individual specific unobservables. It is also further specified to explicitly account for the number of times the household has been in a borrowing relationship. Controlling for potential sources of selection bias common in microfinance impact evaluations, results indicate that participation increased household consumption and the probability of housing improvements significantly. The flexible specification further uncovers that one time participation has no impact on housing improvements but does increase per capita consumption significantly. Impact increased with increases in the length (intensity) of participation in both cases though. It is concluded that participation in credit improves not only in the short-term but also effect lasts longer beyond participation periods and that impact estimates that do not account for periods beyond duration of exposure to programs may underestimate impacts.

In chapter six impact is evaluated from the *timing* of first-time participation (i.e., timing of participation), regardless of intensity or number of times of participation, on household consumption measured at different years thereafter. A new method based on a forward-looking sequential counterfactual that enables to account for selection bias due to timing of participation as well as potential counterfactuals between timing of participation and outcome measurement period is used. This method combines panel data techniques with the semi-parametric method of propensity score matching to address selection biases due to initial as well as future heterogeneities. Results show that the timing of participation matters, even accounting for selection and timing effects. That is, early than later participants fared better, particularly in the face of droughts, partly because effects last longer than the period for which credit is used. It is concluded that borrowing plays an important role in terms of building resilience which would not be easily observed in studies that consider simple before and after comparisons.

Finally, the last chapter of this thesis, chapter seven gives a summary and an integrated discussion of these findings. Moreover, it gives a brief synthesis of the findings in this thesis and their contributions to overall microfinance literature. A brief discussion on further research directions that come out of this thesis is also given at the end of this chapter.

SAMENVATTING

Het ontbreken van kredietmogelijkheden voor mensen met zeer lage inkomens is een van de obstakels voor ontwikkeling van arme landen. Het is een enorme uidaging om kredietmogelijkheden te creëren waarbij kleine en versnipperde leningen op een kosten effectieve manier worden georganiseerd en waarbij tevens terugbetaling kan worden gegarandeerd. Centraal hierin zijn twee fundamentele problemen: gebrek bij kredietverschaffers aan informatie over arme cliënten, en het ontbreken van onderpand bij dergelijke cliënten. Nu zijn informatieproblemen gemeengoed voor alle kredietverschaffers, inclusief reguliere banken die dat compenseren door onderpand te vragen. Omdat gebrek aan informatie zich tegelijk voordoet met gebrek aan onderpand in een omgeving van armoede, zijn arme cliënten niet aantrekkelijk voor reguliere kredietverschaffers. De uitdaging van kredietverschaffing voor armen zit dus in het ontbreken van onderpand veroorzaakt door armoede.

In de afgelopen jaren hebben microkrediet instituten echter de hoop gevoed dat het probleem van beperkte of geen toegang tot krediet door gebrek aan onderpand kan worden opgelost door het toepassen van innovatieve mechanismen die het informatieprobleem te boven komen. Veel microkrediet instituten gebruiken de methode van groepsleningen, waarbij kredietnemers verplicht zijn kleine groepen te vormen waarin men gezamenlijk aansprakelijk is voor elkaars leningen. Gezamenlijke aansprakelijkheid introduceert sociale druk in het leningcontract wat er voor zorgt dat leners zich gedragen overeenkomstig de belangen van het microkrediet instituut. Meer specifiek stimuleert groepsdruk leners in het zorgvuldig kiezen van partners (*peer selection*), in het controleren van elkaars omgang met de lening (*peer monitoring*), en in de terugbetaling. Daarbij komt dat kredietverschaffers vaak dreigen om de hele groep (in sommige gevallen zelfs het hele dorp) van toekomstige leningen uit te sluiten, indien een of meerdere groepsleden hun lening niet terugbetalen. Dit is een extra drukmiddel van microkrediet instituten om terugbetaling af te dwingen. Het gevolg is dat verliezen in geval van mislukte projecten zeer beperkt zijn omdat succesvolle ondernemers binnen de groep (een deel van) deze verliezen op zich nemen. Daardoor zijn een aanzienlijk deel van de kosten en risico's overgeheveld van kredietverschaffer naar leners. Enkele theoretisch-economische artikelen stellen dat dergelijke reducties in kosten en risico's het mogelijk maken dat microkrediet instituten krediet verschaffen aan armen tegen redelijk lage rente tarieven, iets wat als een win-win oplossing voor een oud probleem wordt aangeduid.

Microkrediet heeft internationaal aanzienlijke aandacht getrokken en heeft geleid tot aanzienlijke ondersteuning van wereldwijde donoren en enthousiaste individuen. Deels gedreven door wereldbrede ondersteuning zijn microkrediet instituten nu een centraal onderdeel in armoede bestrijding- en ontwikkelingsstrategieën van ontwikkelingslanden. In de meeste landen is de doelstelling van microkrediet instituten tweeledig: het terugbrengen van inkomensschok risico's om zo armoede tegen te gaan, en het stimuleren van opbouw van eigen vermogen om ondernemerschap aan te moedigen. Volgens een rapport van de 2009 Microkrediet Top is het aantal microkrediet instituten wereldwijd toegenomen tot 3552 per December 2007 (terwijl het er in 1997 nog maar 618 waren). Gezamenlijk bedienen zij wereldwijd meer dan 150 miljoen cliënten, waarvan 106 in de laagste inkomensgroepen (minder dan een dollar per dag). Volgens dit rapport zijn microkrediet instituten nu bereikbaar voor 1,3 miljard van de armsten wereldwijd. Deze cijfers vertellen echter maar een deel van het verhaal. Ondanks de toegenomen toegang tot microkrediet, is het bewijs omtrent succes van leners en individuele microkrediet instituten gemengd. In veel gevallen nemen in tegenstelling tot wat men verwacht potentiële leners niet deel aan groepsleningen. Er is weinig bekend waarom deze potentiële cliënten wegblijven bij de aangeboden microkrediet leningen. De inkomensverbetering door microkrediet, vooral gemeten over meerdere jaren, is vaak ook niet bekend. In het algemeen kan worden gesteld dat empirisch onderzoek naar microkrediet achterblijft bij de theoretische bijdragen. In het bijzonder blijft het onduidelijk of en in welke mate de innovatieve microkrediet mechanismen succesvol zijn, gegeven de diverse socio-economische en biofysische omstandigheden waarin microkrediet instituten werkzaam zijn.

Het doel van dit proefschrift is om mechanismen van kredietverschaffing door microkrediet instituten te onderzoeken en om de lange termijn effecten van microkrediet leningen op de welvaart van Ethiopische huishoudens vast te stellen. De nadruk ligt op het begrijpen en empirisch onderzoeken van het gedrag van microkrediet cliënten, in het bijzonder met betrekking tot sommige cruciale bouwstenen van microkrediet, en op het evalueren van waargenomen welvaartseffecten door microkrediet leningen over een langere periode. Vanuit deze algemene doelstelling zijn vier meer specifieke doelstellingen geformuleerd die in afzonderlijke hoofdstukken worden uitgewerkt.

De Ethiopische context biedt een interessante omgeving voor deze onderzoeksdoelstellingen. Twee unieke datasets, een panel dataset en een cross-sectie dataset, die afkomstig zijn van een ruraal microkrediet instituut in Ethiopië zijn gebruikt in deze studie. Hoofdstuk twee beschrijft deze datasets en de specifieke biofysische en socio-economische context of het studiegebied. Voortbouwend op voorgaande studies, is een panel dataset van vijf rondes over de periode 1997-2006 gebruikt die gegevens bevat van 400 huishoudens (leners en niet-leners). Deze huishoudens zijn willekeurig geselecteerd uit zestien dorpen in de Tigray regio in Ethiopië, die alle toegang hadden tot microkrediet. Deze dataset is gebruikt om de eerste, derde en vierde subdoelstelling waar te maken. Voor doelstelling twee is een specifieke cross-sectie dataset verzameld die individuele en groepsgegevens bevat van 201 deelnemers aan groepsleningen in 2003 in deze regio.

In hoofdstuk drie wordt een dynamisch stochastisch theoretisch model gebruikt dat twee verschillende soorten risico onderscheidt, namelijk risico van mislukte projecten van partners en risico van het verliezen van toegang tot toekomstige kredieten. Dit raamwerk is gebruikt om te analyseren of dergelijke risico's deelname aan microkrediet door huishoudens belemmeren. Schattingsresultaten van een dynamisch panel probit model laten zien dat deze risico's de kans op deelname aan microkrediet verkleinen. Andere risico's in het gebied (zoals droogte), maar ook de hoeveelheid eigen vermogen en toegang tot sociale overheidsprogramma's bepalen deelname. Een ander belangrijk resultaat is dat de kans op herhaalde deelname aan microkrediet groter is dan de kans op deelname voor de eerste keer, wat impliceert dat risico's van leningen hoger worden ingeschat door niet-deelnemers dan door deelnemers. Een conclusie is dat er een uitruil is tussen het aanbieden van krediet middels mechanismes die deelnemers aansporen te handelen overeenkomstig de belangen van het microkrediet instituut, en de contractrisico's die deze mechanismen met zich mee brengen, vooral in een risicovolle omgeving zoals de droogte gevoelige gebieden in het noorden van Ethiopië.

Gegeven de contractrisico's van groepsaansprakelijkheid, wordt in hoofdstuk vier empirisch onderzocht welk soort groepen ontstaan wanneer huishoudens deelnemen aan groepscontracten. De hypothese dat homogene groepen met dezelfde risico types worden gevormd is getest. De empirische analyse houdt rekening met potentiële endogeniteit tussen eigen risico's en risico's van partner, en de moeite van het vinden van de juiste partners. Resultaten op basis van een cross-sectie dataset laten zien dat er sterk statistisch bewijs is voor het verwerpen van de homogeniteit hypothese ten gunste van heterogeniteit. In dit hoofdstuk wordt verder onderzocht of heterogeniteit het gevolg is van ontbreken van verzekeringsmogelijkheden, zoals wel wordt gesuggereerd in de literatuur en of heterogeniteit gevolgen heeft voor terugbetaling van groepsleningen. Er is niet genoeg bewijs die de relatie tussen heterogeniteit in risico's en het betalen van risicopremies door groepsleden aan elkaar ondersteunt. Daarentegen blijkt dat andere op vertrouwen gebaseerde sociale netwerken en bestaande traditionele krediet groepen vaak ten grondslag liggen aan het groepsformatieproces in deze regio. Dergelijke bestaande sociale netwerken lopen vaak synchroon met krediet groepen en deze beïnvloeden terugbetaling op een positieve manier.

Hoofdstuk vijf en zes van dit proefschrift richten zich op de lange termijn impact van deelname aan microkrediet, rekening houdend met tijdstip en duur van deelname. In hoofdstuk vijf wordt over een periode van tien jaar de invloed van intensiteit van deelname op twee huishoud welvaartsmaatstaven onderzocht. Deze maatstaven zijn jaarlijkse consumptie per hoofd en woningverbetering. Deze laatste is een belangrijke lokale welvaartsmaatstaf in Tigray. Door gebruik te maken van individuele trends, wordt het traditionele panel fixed effects model, dat doorgaans alleen robuust is tegen selectie bias door tijdsinvariante variabelen, op een innovatieve manier uitgebreid zodat het ook rekening houdt met selectie op basis van niet waargenomen tijdsvariante variabelen. Het model houdt expliciet rekening met het aantal keren dat een huishouden heeft deelgenomen aan microkrediet. Rekening houdend met mogelijke oorzaken van selectie bias die gebruikelijk zijn in microkrediet studies, wijzen de resultaten erop dat deelname aan microkrediet leidt tot significante stijgingen in de huishoud consumptie en de kans op woningverbetering verhoogt. De flexibele modelspecificatie laat verder zien dat eenmalige deelname aan microkrediet niet leidt tot woningverbetering maar wel een significante stijging van de consumptie tot gevolg heeft. De impact neemt echter toe met de duur van deelname. De conclusie is dat deelname in microkrediet niet alleen leidt tot een verbetering op korte termijn, maar de effecten gaan ook verder dan alleen de periode van deelname. Impact studies die geen rekening houden met deze lange termijn effecten, onderschatten daarmee het effect van microkrediet programma's.

In hoofdstuk zes wordt impact gemeten aan de hand van het moment van eerste deelname, ongeacht het aantal daaropvolgende keren van deelname. Impact wordt gemeten op basis van consumptie in verschillende jaren na deelname. Een nieuwe methode van 'forward-looking sequential counterfactuals' wordt gebruikt, die het mogelijk maakt om rekening te houden met selectie bias op basis van tijdstip van deelname en mogelijke alternatieve situaties tussen moment van deelname en waarneming van impact. Deze methode combineert panel data technieken met de semi-parametrische 'propensity score matching' methode om zo selectie bias door oorspronkelijke en toekomstige heterogeniteit heet hoofd te bieden. De resultaten laten zien dat moment van deelname er toe doet, rekening houdend met selectie en timing effecten. Dat wil zeggen dat vroege deelnemers beter af waren dan latere deelnemers, deels omdat effecten van krediet langer doorwerken dan de periode waarin geleend is. Een conclusie is dat krediet een belangrijke rol speelt in het opbouwen van buffers, iets wat niet gemakkelijk gevonden wordt in geval van een simpele vergelijking van de situatie voor en na krediet.

Hoofdstuk zeven van het proefschrift geeft een samenvatting van de belangrijkste conclusies en een geïntegreerde discussie van de gevonden resultaten. In dit hoofdstuk wordt ook een synthese van de resultaten van dit proefschrift met bestaande literatuur gegeven. Dit laat ook de bijdrage van dit proefschrift aan de wetenschappelijk literatuur op het gebied van microkrediet zien. Een korte bespreking van mogelijkheden voor toekomstig onderzoek wordt aan het eind van dit hoofdstuk gegeven.

TRAINING AND SUPERVISION PLAN

Description	Institute / Department	Year	ECTS*
Courses:			
Mansholt Introduction course	Mansholt Graduate School of Social Sciences (MG3S)	2005	1.5
Techniques for Writing and Presenting a Scientific Paper	Wageningen Graduate Schools (WGS)	2005	1.2
Mobilizing your Scientific Network	WGS	2009	1
Interdisciplinary and Transdisciplinary Research: Intervision and Communication Skills	WGS	2009	1.1
Economic Theory I	Netherlands Network of Economics (NAKE)	2005	3
Economic Theory II	NAKE	2005	3
Behavioural Economics	MG3S	2005	4
Economics of Finance and Banking	NAKE	2006	3
Economic Models	MG3S	2004	6
Advanced Econometrics	MG3S	2004	6
Randomized Evaluations	MIT[1], Cambridge, USA	2005	2
The Bayesian Approach in Theory and Practice	MG3S	2008	1.5
NAKE Workshop	NAKE	2009	1.5
Generalized Methods of Moments - participation in the course	MG3S	2006	
Panel Data Econometrics - participation in the course - teaching assistant	MG3S	2009	
Presentations at conferences and workshops:			**5**
Mansholt Multidisciplinary seminar at 2nd European Ph.D. conference (Mansholt Ph.D. day)		2005	1
6th European Development Research Network (EUDN) Ph.D. Seminar, University of Göttingen, Germany		2008	1
12th Congress of the European Association of Agricultural Economists (EAAE), Ghent, Belgium		2008	1
First European Research Conference on Microfinance, Brussels, Belgium		2009	1
26th International Agricultural Economics Association (IAAE) Conference, Beijing, China		2009	1
1st European Microfinance Seminar, European microfinance Program, Brussels		2005	
2nd European Microfinance Seminar, European microfinance Program, Brussels		2007	
MG3S Cluster Economics Seminar, Wageningen, The Netherlands		2009	
WOTRO Research Day, Utrecht, The Netherlands		2009	
Total (minimum 30 ECTS)			**39.8**

*One ECTS on average is equivalent to 28 hours of course work

[1] Poverty Action Lab; Massachusetts Institute of Technology, USA.

CURRICULUM VITAE

Guush Berhane Tesfay was born on May 29, 1973 in Adi Abun, Tigray State, Ethiopia. He attended his primary school at Tub-Gorzo and middle high schools at Adi-Abeto Secondary Schools. He continued the first years of his Senior high school in Queen Sheba Secondary School, Adwa, Ethiopia and moved to Asmara, Eritrea, where he completed high school in Keih Bahri Secondary School. In 1994, he joined the University of Asmara, Eritrea, where he earned his Bachelor Degree in Economics and Finance in July 1998. In September 1998, he joined the department of economics, Mekelle University, and worked as assistant graduate and later as assistant lecturer until August 2002. During this period, he also worked as students' internship officer of the Faculty of Business and Economics, Mekelle University, and later as Planning and Programming head of Mekelle University. From September 2002 to May 2004, he studied Development Economics at Wageningen University, the Netherlands and obtained a master's degree with thesis titled 'Equilibrium risk-matching and repayment performance in group based microfinance institutions: the case of the Dedebit Credit and Saving Institution in Ethiopia'. In September 2004, he got the opportunity to study his Ph.D. at the Agricultural Economics and Rural Policy Group where he also earned a *WOTRO* Science for global development grant, 2005-2009. *WOTRO* is the science division within *NWO,* Netherlands Organization for Scientific Research. This grant gave him the opportunity to gain practical knowledge on fieldwork research in Ethiopia where he also combined it with teaching and thesis supervision of students at Mekelle University. During the Ph.D. study period, he also followed the Ph.D. education program in the Mansholt Graduate School of Wageningen University and the Netherlands Network of Economics (NAKE). He also followed an executive course on 'evaluating social programs' at the ABDUL LATIF JAMIL Poverty Action Lab (J-PAL) of Massachusetts Institute of Technology (MIT), USA. He has presented his work in several international congresses during the Ph.D. study period. Besides, he was involved as teaching assistant of M.Sc. and Ph.D. courses at the Agricultural Economics and Rural Policy Group of Wageningen University, The Netherlands. He is also an occasional reviewer of scientific articles for the *Agricultural Economics* journal of the International Association of Agricultural Economists.

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Front cover art & design:

A young and delicate tree handled with care from both sides, symbolizing infant microfinance institutions are as fragile and need careful handling from all sides - a core message in this thesis.

(Art by Kelly de Bruin and Graphic design by Paul de Bruin)

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