Anchoring and the adverse effects of progressive lending in microfinance

A framed field experiment in Bolivia

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Abstract

This study looks at the role anchoring effects might play in the potential adverse effects of progressive lending in microfinance through a framed field experiment in twelve communities of rural Bolivia. We find that even though anchoring effects do not occur when borrowers face a high credit limit compared to not facing any credit limit, participants assigned to the progressive lending treatment overborrow and (unstrategically) default significantly more overall (and in the final round) than participants who had always faced this final credit limit. This indicates that progressive lending might not only restrain learning effects, but also induces greater default risk as a result of persistent anchoring. These findings shed new light on a practice in microfinance which – while often unchallenged both in literature and in practice – had seen growing evidence of potentially adverse side-effects. By testing these descriptive relationships to the lab we help to attribute the mechanisms through which progressive lending may actually lead to overborrowing in microfinance.

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Contents

1. Introduction

For decades, microfinance was regarded an effective tool to create economic development (Dunford, 2006), empower women (Littlefield, Morduch, & Hashemi, 2003; Orso & Fabrizi, 2016) and target the 'poorest of the poor' (The Norwegian Nobel Institute, 2006). Muhammad Yunus, the inventor of microfinance, was even rewarded the Nobel Peace Prize in 2006. The numbers also seemed promising. By 2007, the Grameen bank, Yunus' microfinance institution (MFI) in Bangladesh had loaned \$3.9 billion, with a repayment rate of 98%. Approximately 90% of its loans were financed from within the organization, by savings of depositors, who are usually also borrowers (Yunus, 2007). Some even talk about the microfinance sector providing access to financial services to over 200 million people worldwide. More recently, the positive impacts have however been questioned (Duvendack et al., 2011; Karlan & Goldberg, 2011). Critics have accused microfinance institutions (MFIs) of charging usurious interest rates (Sandberg, 2012) and doing more harm than good for the poor by bringing them into a situation of over-indebtedness (Schicks, 2014). This study explores whether anchoring effects in the presence of progressive lending systems widely used in microfinance can be seen as a possible explanation for this over-indebtedness. Anchoring effects have been proven to influence decision making in a variety of contexts (e.g. Courant, Gramlich, & Laitner, 1986; Johnson, Kotlikoff, & Samuelson, 1987; Kotlikoff, Samuelson, & Johnson, 1988), and is tested as a potential mechanism behind overborrowing in microfinance in this paper. Progressive lending is widely used in microfinance, and means that credit limits grow over loan cycles.

This study takes place in Bolivia, one of the countries in the world where microfinance is most widespread. It is a landlocked country in South America with a population of approximately 10.5 million people. Parts of the country are located at very high altitudes in the Andes mountains. It is considered one of the poorest countries of the continent, with a GDP per capita of 3095 US$ in 2015. 39.3% of the population is considered to live below the national poverty line. However, these figures have greatly improved over the last years, since GDP per capita was only 1007 US$ in 2000 and the poverty headcount ratio at that time was 66.4% (World Bank, 2016). Of the 10.5 million inhabitants of Bolivia, 1.1 million are microfinance borrowers (Microfinance Information Exchange, 2012b). Penetration rates of Bolivian microfinance are among the highest of the world (Schipani, 2012). Furthermore, unlike the traditional microfinance concept, many microfinance loans provided in Bolivia are individual loans rather than group loans (Porteous, 2006).

This study uses a framed field experiment in Bolivia to gauge the potential adverse effects of progressive lending in microfinance. A total of 271 subjects participated in an experimental game reflecting a microfinance setting in twelve rural communities of the municipality of Coroico. Participants could borrow money to buy buckets of unsorted four-colours pasta, which could be sold at a profit once sorted by colour within a set time. They faced either no credit limit, a non-constraining high credit limit or credit limits according to a progressive lending schedule. The game randomly included either one or three separate banks, and lasted four rounds (although participants were blind to duration), indicating different loan cycles. Participants were divided into one of six treatment groups, reflecting situations with different kinds of credit limits and markets with either one or three MFIs.

This study shows how anchoring effects can limit learning effects in the presence of credit limits set according to the principle of progressive lending, and therefore lead to increased overborrowing among microfinance borrowers. Contrary to our expectations, participants in our experiment did not show direct anchoring to the credit limits set by the bank(s) in the experiment. However, borrowers facing credit limits according to a progressive lending schedule did overborrow more in later loan cycles compared to borrowers who always faced a non-constraining credit limit, due to prolonged anchoring effects limiting learning effects. This overborrowing eventually leads to more defaults among the group of borrowers in the progressive lending system. The results of this research also show borrowers to be defaulting on their loans more often in the presence of multiple banks.

The structure of this paper is built up as follows; first, we will give an overview of the existing literature on the topic of overborrowing in microfinance and the effects of progressive lending systems on borrower behaviour. Second, we will discuss the context of the experiment and the background of the microfinance sector in Bolivia. Third, we will discuss the experimental design and sampling methods. Fourth, we will explain the methodology for the analyses, the results of which are shown subsequently. Lastly, we will present a discussion and conclusion of the results, together with some recommendations for future research.

2. Literature review

Several studies have shown that microfinance has led to over-indebtedness of their borrowers (Gonzalez, 2008; Lascelles & Mendelson, 2012; Schicks, 2013, 2014). Schicks (2014), for example, found that over sixty per cent of the Ghanaian borrowers in her sample deem their returns insufficient to repay their loans. Even if borrowers do not become over-indebted and are able to repay their loans, they may still borrow more than they actually need for productive investment purposes. When a borrower borrows more than strictly necessary for production, these funds will not yield a return. MFIs often charge high interest rates. EIU (2010) for example reports that microfinance interest rates in Latin America range from 15 to 109 per cent. The majority of the microfinance institutions charges 20-45% interest. These interest rates, combined with loans not being used productively, are likely to cause repayment problems, eventually leading to over-indebtedness and default.

One possible explanation for overborrowing is the presence of anchoring effects. The anchoring effect is one of the most researched sources of bias in decision making. Tversky and Kahneman (1975) define the anchoring effect as "the disproportionate influence on decisions makers to make judgements that are biased toward an initially presented value". This means that when a decision maker is presented with some value, the decision made in the end is biased towards the anchor, which is the initially presented value. This effect has been tested in many different settings, in experimental lab contexts as well as real life situations. Anchoring effects are found to be stronger when the problem is more ambiguous, less familiar, relevant or when the decision maker is less personally involved. Additionally, when the anchor is set by a more trustworthy source, or can be considered a reasonable estimate, anchoring effects are also stronger (Van Exel, Brouwer, van den Berg, & Koopmanschap, 2006). The anchoring effect has been applied in research on credit decisions by Soman and

Cheema (2002). They build upon previous findings (Courant et al., 1986; Johnson et al., 1987; Kotlikoff et al., 1988) showing that people are unable to correctly predict their future incomes. Thereupon, they argue that borrowers use any information provided as an indicator for their earnings potential and therefore their creditworthiness. This information provided can be for example a credit limit set by a bank or credit institution. Borrowers who face a high credit limit are found to expect their future income to be high, whereas borrowers who face a low credit limit expect to earn little in the future. In line with the findings on familiarity discussed above, Soman and Cheema (2002) find that borrowers who have experience with loans are not as strongly influenced by the credit limit than naïve borrowers. If credit limits are sometimes set above the optimal loan size of borrowers in microfinance, anchoring effects might be an explanation for overborrowing.

Even though microfinance generally involves low credit limits, they do not always remain so low. A tool that was developed to cope with the risk inherently involved in providing loans to the poor is the use of dynamic incentives. Giné, Jakiela, Karlan, and Morduch (2010) experimentally show that dynamic incentives are an effective way of reducing risk taking. Two different types of dynamic incentives are widely used. One is the use of rising credit limits over loan cycles, or progressive lending. This means that in the first loan cycle, a borrower only has access to a very small loan, which grows in the next loan cycle after repayment (Armendáriz & Morduch, 2010; Godquin, 2004; Maitra, Mitra, Mookherjee, Motta, & Visaria, 2012). Progressive lending enables the bank to cut average costs, screen borrowers with small loans before taking the risk of lending larger amounts and increase the incentive for the borrowers to repay their loans (Armendáriz & Morduch, 2010). In such a system, a future outlook on larger loans provides an incentive to repay. Godquin (2004) shows that in such a situation, repayment performance decreases in later loan cycles. She claims this might be due to a decreasing credibility of dynamic incentives over time, causing borrowing groups to default more often as they age. Similarly, Kirschenmann (2015) shows that borrowers whose credit was rationed perform worse ex-post than borrowers whose credit was not rationed, since they increase their demand more gradually over loan cycles. These findings indicate that in the case of progressive lending, repayment might become problematic for borrowers later on, when credit limits have become non-constraining. The other dynamic incentive is punishment after default, a mechanism that excludes defaulting borrowers from future loan cycles. If a borrower does not repay its loan, he is blacklisted and unable to receive future loans from this financial institution for a given period of time.

The microfinance sector has greatly expanded over the last decades (McIntosh & Wydick, 2005). For example in Bangladesh, the country where microfinance as we know it today was founded, there are 64 different MFIs (Microfinance Information Exchange, 2012a). In Bolivia, there are 28 (Microfinance Information Exchange, 2012b). Between 2003 and 2013, the number of MFI offices in both South Asia and Latin America and the Caribbean has become almost ten times larger (Microfinance Information Exchange, 2016). For such a competitive market to function properly, a well-functioning central credit registry is crucial (Bos, De Haas, & Millone, 2015; Giné, Goldberg, & Yang, 2012). When several MFIs are active in the same market but do not communicate perfectly, the information asymmetry over borrower indebtedness increases, which causes the most impatient borrowers to take loans from several banks (double-dipping). This is a result of the less favourable loan terms borrowers receive in such a situation, something that the MFIs use to manage the risk inherent to lending without collateral to borrowers in a competitive market with imperfect information sharing (McIntosh & Wydick, 2005).

In a market where borrowers have the option to borrow from several MFIs, strategic default becomes a serious issue (Guha & Chowdhury, 2013). When progressive lending systems are used, strategically defaulting borrowers will wait until the loan size has grown to a substantial amount before defaulting. Many microfinance borrowers have limited liability, which means that the bank cannot enforce their repayment through anything else than a dynamic incentive. However, since dynamic incentives often include exclusion from a certain bank in the future, having other banks available greatly reduces the effectivity of this incentive (Armendáriz & Morduch, 2010). McIntosh, de Janvry, and Sadoulet (2006) prove that repayment and savings rates declined after an increase in competition. Still, if there is a well-functioning credit registry available in the market, this provides an extra incentive for borrowers to repay their loans (Armendáriz & Morduch, 2010; De Janvry, McIntosh, & Sadoulet, 2010). If repayment data is shared in real time, the punishment of exclusion from credit will not only count for one bank, but for several. In that sense, the market will function again like it would if there were not so many suppliers of credit, as loan officers will now have complete information of the indebtedness of loan applicants. Bos et al. (2015) show that when information sharing is mandatory, lending becomes more conservative at both the intensive and extensive margin. This means that more loans get rejected (extensive margin) and that they become shorter, smaller, and more costly (intensive margin). This increases loan and repayment quality. Since microfinance loans traditionally do not require collateral, this conservatism will most likely show in the loan sizes, leading to a further rationing of credit. Shapiro (2015) on the other hand argues that this information sharing is still insufficient. He claims that as long as banks are uncertain over how much borrowers value future loans, all borrowers will eventually default.

Following from the literature discussed above, we formulated three hypotheses. The first hypothesis (1) is that naïve borrowers anchor to credit limits. Borrowers who do not have previous experience, and are therefore naïve, use any information provided by the bank, such as a set credit limit, as a source of information while determining their optimal loan size. The second hypothesis (2) is that we expect that progressive lending limits learning effects and leads to overborrowing once credit limits have become non-constraining. Eventually, after a certain amount of loan cycles, the credit limit in a progressive lending system rises above the optimal loan size of the borrower, and is therefore not constraining any more. A borrower who has always been able to fully use the loan becomes used to interpreting this credit limit as a relevant source of information while determining the optimal loan size. After a few loan cycles, the permitted loan size has grown and the borrower's production capacity is not large enough anymore to fully use the loan. This leads to overborrowing and eventually to over-indebtedness. The third hypothesis (3) is that we expect borrowers to default more often in the presence of multiple banks and when a progressive lending system is used. When borrowers face multiple non-communicating banks the threat of exclusion from future loan cycles after default is no longer credible. The dynamic incentive of this punishment is no longer strong, and it becomes less problematic for a borrower to default on his loan. If the second hypothesis is confirmed, progressive lending leads to overborrowing. If borrowers overborrow, they eventually have no other option than to default on their loan, because they simply will not have earned sufficiently to repay their loan and the interest rates charged. Hence, the defaults in this hypothesis actually consist of two different types of default. On the one hand there are borrowers who can make the decision not to repay their loan and face the consequences, and on the other hand there are borrowers who have not earned sufficiently to pay back their loan.

3. Context

This experiment was conducted in Bolivia. The microfinance market in Bolivia is very competitive. There are 1.1 million borrowers who have a total outstanding debt of 6.6 billion USD (Microfinance Information Exchange, 2012b). MFIs in Bolivia have different legal statuses. Some are NGOs, whereas others are banks. On the one hand, the MFIs that have the status of banks are associated under ASOFIN, and regulated by ASFI. On the other hand, FINRURAL is the non-profit association of the NGOs supplying microfinance loans in Bolivia. It also includes credit registration among members. Together, the ASOFIN MFIs have 577 agencies throughout the country and provide loans to approximately 750.000 borrowers (ASOFIN, 2016), and the MFIs associated to FINRURAL have 335 agencies and 450.000 borrowers (FINRURAL, 2016). Even though most financial institutions in both associations are MFIs, loan sizes vary greatly. FINRURAL reports that 16% of all loans supplied are smaller than 1000 US$, and 30% are under 2000$. In 2013, 61% of the provided loans were smaller than the average loan. In 2010, this was still 78%, showing that bigger loans are being supplied. This could be a result of the new financial services law implemented in 2013, which includes interest rate caps (Heng, 2015).

In Bolivia, both a public (CIRC, connected to the ASOFIN MFIs) and a private credit registry bureau (FINRURAL) are active. The public credit registry covers 15% of the adult population, whereas the private covers 43.2% (World Bank, 2016). However, these credit registries are not updated in real time. De Janvry et al. (2003) state that the credit registry is updated once a month. He claims that many (56% in 1998-1999) microfinance clients do not appear in the registry. ASOFIN (2016) reports that loans of a total value of more than 7.5 million USD are blacklisted.

In 2003, only 30% of loans in Bolivia were group loans, like in the classical microfinance model (Porteous, 2006). Still, 72% of individual loans provided by MFIs associated with FINRURAL had a social aspect, like repayment in groups.

Bolivia provides a perfect context for this experiment, since this experiment tests what happens when loans are offered and provided by multiple banks who do not fully communicate with one another. Also, Bolivia is classically one of the countries where microfinance is most widely used. This means that the participants are used to microfinance loans and the way they function, like the use of dynamic incentives and progressive lending. Furthermore, many microfinance loans provided in Bolivia are individual loans, just like the loans in this experiment. Repayment in the experiment was visible by other participants in the group, which reflects the social aspect often used in individual loans.

4. Experiment

Sample

The data used for this thesis comes from a lab-in-the-field experiment conducted in Bolivia in December 2015. Additionally, we use data from a household survey carried out during August-November 2015. 271 people participated in the experiment, of which 37% also participated in the household survey. An additional 19% were household members of participants of the survey. All participants of the experiment lived in one of 12 communities in the municipality of Coroico, a town in the region of Yungas, close to La Paz. In every community we randomly selected 20 households for the household survey. For the experiment, we tried to find back as many of these subjects as possible. The participants did not participate in any previous experiments. In Coroico, 5 MFIs are active, but the participants reported to be aware of on average 3.4 MFIs.

A slight majority (57%) of the sample was female, and the average age was 43. On average, participants had followed 7.3 years of education. In 82% of the households of experiment participants that were also included in the household survey there was at least one household member with an outstanding loan.

We carried out the experiments in every community, and once in the town of Coroico itself, adding up to a total of 12 sessions. Every day, two communities were visited to conduct the experiment. One community in the early morning, and one in the evening, in order to make it easier for the participants to attend the sessions. All selected households were notified a day in advance by the community head. Depending on the outcome of the experiments, participants earned between 6 Bs (€0,75) and 40 Bolivianos (approximately €5). The average payoff was 16.6 Bs. The sessions were held in a central location in the community, usually at the ‘sede social’, where community meetings are also held.

Design

The participants were asked to do a simple task to simulate production, which was to sort four colours of pasta into plastic cups. They needed to buy every cup for 2 Bs (slightly less than €0,30). However, they did not start the game with money, so they had to borrow from the available bank(s). At the end of every round played, they had to repay their loans with an interest rate of 50%, so 3 Bs for every cup purchased. Every cup correctly filled with one single colour of pasta could be sold for 5 Bs, meaning 2 Bs could be earned per filled and sorted cup. The game was played in four rounds of four minutes each. However, participants were not told how many rounds they were going to play. The numbers were designed in such a way that if borrowers borrow more than 50% too much, they will have to default. The dynamic incentive of punishment after default was in place, so strategic default was an option, but the borrower would be excluded from borrowing from that particular bank for the next rounds. At the end of the four rounds, the experimenter revealed that the last round had now been played and one round was to be selected for payment by picking a coloured coin out of a blind bag. The money earned in this round was added to the participation fee of 6 Bs, and paid in real money to the participant. All participants who had not participated in the household survey were asked for their age, gender and years of education.

The experiment has a 3x2 between-subjects design. There are three different kinds of credit limits, and two different kinds of microfinance markets, one with one and one with three banks. The experimental setting with a market with only one bank could also be argued to reflect a situation in which there are actually multiple banks, but with a perfectly functioning credit registration bureau, enforcing communication between banks.

Table 1: Sample size per treatment group

	No limit	High limit	Progressive lending
One bank	45 (9)	45 (9)	45 (7)
Three banks	46 (9)	45 (9)	45 (7)

Number of communities in between brackets

Due to capacity constraints, it was impossible to play all of the six treatments in every community. Therefore, four out of the six treatments were selected for every community. Since the sample size in each of the treatment groups is not very large, it was important for every treatment to be played an equal amount of times to ensure sufficient power of the analyses. However, it sometimes turned out to be practically impossible to play the four selected treatments in the community. Therefore, some treatments were played in more communities than others. This was done in order to still have (almost) equal sample sizes among treatment groups, to maximize the power of the analyses. This will be controlled for by adding community dummies in the analyses, which is further explained in Section 5. Once it was clear which four treatments would be played in the community, participants were randomised into these treatment groups. However, it unfortunately turned out not to be feasible to perfectly randomise the participants into these groups, as participants did not arrive at the same time. Participants were not willing to wait until all others had arrived before starting the experiment and said they would leave if we would not start the experiment soon. Therefore, we decided to randomise the order in which the different treatment groups would play. The first five participants to arrive would then play the same treatment, which was randomly determined. This order was randomised in every community on every day. Therefore, we correctly randomise at the group level (we verify this in Section 5, Table 2) but might suffer from intra-group correlation of standard errors if similar types tend to arrive at similar times. Throughout the analysis we cluster standard errors at the group level to control for this possible confound.

Treatment 1: No limit – one bank

In this treatment, borrowers could borrow as much as they wanted from one single bank. If they defaulted, they could not borrow from this bank again in the future, meaning they were excluded from the game. However, to receive their payoffs, or at least their participation fee, they had to wait until the end of the game. This was the same in the other treatments where there was only one available bank.

Treatment 2: High limit – one bank

In this treatment, the credit limit was set to be approximately twice as high as the expected necessary amount for optimal production, calculated based on a few field tests. We expected people to, on average, be able to fill six cups in four minutes, so the credit limit was set at 24 Bs.

Treatment 3: Progressive lending – one bank

This treatment reflects the progressive lending system most MFIs use. In the first round, borrowers were allowed to borrow 6 Bs, in the second round 12 Bs, 18 in the third round and finally 24 Bs in the fourth round, reflecting the limit of the previously discussed treatment.

Treatment 4: No limit – three banks

Now, again, there is no credit limit. However, there is not just one bank borrowers could borrow money from, but three. Participants can borrow from only one bank if they want to, but it is also possible to borrow from two, or even from all three banks. This means that if they did not repay their loan, they were only excluded from borrowing from this bank in the future. They still had access to loans from the other two banks. This is also the case in the treatments discussed below.

Treatment 5: High limit – three banks

The credit limit for each bank in this treatment was set to be one third of the high limit in the one bank treatment. This was done in order to ensure the total credit limit to be the same as in the treatments including one bank only. Therefore, the credit limit was 8 Bs per bank.

Treatment 6: Progressive lending – three banks

In this treatment, the limits are also a third of the limits in the rising limit – one bank treatment. Hence, the limit starts with 2 Bs per bank in round 1 and increases up to 8 Bs per bank in round 4.

5. Methodology

Since the sample sizes of the different treatment groups are not very large, it is possible that the treatment groups have some differences in characteristics. Therefore, we collected some additional information from all participants, to use as control variables. If we look at the differences between treatment groups, we find no significant differences. Therefore, the groups can be considered similar in terms of age, gender and years of education.

Table 2: Treatment groups

	N	Age	Female (fraction)	Years of education
No limit – one bank	45	42.07 (17.80)	0.55 (0.50)	7.98 (3.92)
High limit – one bank	45	40.81 (16.74)	0.58 (0.50)	5.45 (3.90)
Progressive lending – one bank	45	46 (16.05)	0.62 (0.49)	5.82 (3.94)
No limit – three banks	46	40.66 (16.15)	0.54 (0.50)	7.98 (4.75)
High limit – three banks	45	44.62 (17.39)	0.58 (0.50)	7.76 (5.06)
Progressive lending – three banks	45	46.43 (16.39)	0.56 (0.50)	7.43 (4.46)
Total	271	43.53 (16.76)	0.57 (0.50)	7.105 (4.46)
F-test		1.01 (0.4133)	0.14 (0.9820)	5.36 (0.374)

Standard errors in parentheses for mean, p-value in parentheses for F-test

Nevertheless, we will include these control variables. Additionally, we will add community dummies to the specifications to control for community characteristics. There might be unobserved village characteristics influencing the behaviour of the participants. Besides, there might be session-specific effects. Since every community was visited only once, both are captured by the inclusion of village dummies in the estimated models. In every estimation, at least a few of the village dummies were significant. This confirms the need for including these parameters.

Three main models will be estimated to obtain the results of this study. These models are made up of different combinations of the variables specified in Table 3. All three of these models will be estimated using various estimation techniques and both including and excluding the control variables of age, gender, education and community dummies. The specifications below depict the most complete version of the models estimated. T2, the treatment group facing a high limit and one bank will be used as the reference group in all models to simplify interpretation.

Table 3: Variables

Variable	Information	Type
T1	Treatment 1: No limit – one bank	Dummy
T2	Treatment 2: High limit – one bank	Dummy
T3	Treatment 3: Progressive lending – one bank	Dummy
T4	Treatment 4: No limit – three banks	Dummy
T5	Treatment 5: High limit – three banks	Dummy
T6	Treatment 6: Progressive lending – three banks	Dummy
Overborrowing	Fraction of the total loan size not used productively	Between 0 and 1
Default	1 if a borrower defaulted on a loan in at least one round	Dummy
Age	Age of participant	
Gender	1 = Female, 0 = Male	Dummy
Education	Years of schooling	

Hypothesis 1: Naïve borrowers anchor to credit limits

The first hypothesis will be tested using the following specification.

$$Overborrowing = \beta_1 + \beta_2 * T1 + \beta_3 * T3 + \beta_4 * T4 + \beta_5 * T5 + \beta_6 * T6 + \beta_7 * Age + \beta_8 * Gender + \beta_9 * Education + \beta_{10} * Community_{j^1} + \epsilon$$

[1] Where j is the community number

To test whether naïve borrowers actually anchor to credit limits, we are interested in comparing the overborrowing of participants in the *high limit (T2)* treatment with participants in the *no limit (T1)* treatment in the first round. If an anchoring effect would occur, borrowers facing a high limit would anchor to this credit limit and therefore borrow more on average than borrowers not facing a credit limit. Since production capacity could differ, it is actually much more relevant not to look at the size of the loan, but at overborrowing.

As shown in Table 3, the *overborrowing* variable is defined as the fraction of the total loan size that is not used productively. This means that if a borrower takes out a loan of for example 8 Bs, with which he buys 4 cups to fill with the pasta in the experiment, he needs to fill up all 4 cups during the four-minute period that constitutes a round in order not to have overborrowed. If he instead only fills up three of the four cups, he will have overborrowed 2 Bs (the price of the empty cup) / 8 Bs (total loan size) = 0.25. Since this variable is a fraction, it will always return a value between 0 and 1. This makes a regression model including this variable a censored regression model. If a censored regression model would be estimated with OLS, this would yield inconsistent estimates. OLS would overestimate the intercept and underestimate the slope of the coefficients (Verbeek, 2012). Therefore, for estimations including this variable, we will include a Tobit regression in order to correct for the inclusion of a truncated variable such as *overborrowing*.[2]

Hypothesis 2: Progressive lending limits learning effects and leads to overborrowing once credit limits have become non-constraining

$$Overborrowing = \beta_1 + \beta_2 * T1 + \beta_3 * T3 + \beta_4 * T4 + \beta_5 * T5 + \beta_6 * T6 + \beta_7 * Age + \beta_8 * Gender + \beta_9 * Education + \beta_{10} * Community_j + \epsilon$$

[2] We also tested for random effects in all specification; but these were not found, therefore random effects estimation was unnecessary

To test the second hypothesis, the final round is used as we are interested in the difference between participants in the *high limit (T2)* and the *progressive lending (T3)* treatments. These two treatment groups are comparable for the fourth and final round, since the credit limit of the *progressive lending* treatment has risen to the same level of the limit the *high limit* borrowers have faced throughout the rounds of the game.

Hypothesis 3: Borrowers default more often in the presence of multiple banks and when a progressive lending system is used

$$Default = \beta_1 + \beta_2 * T3 + \beta_3 * T5 + \beta_4 * T6 + \beta_5 * Age + \beta_6 * Gender + \beta_7 * Education + \beta_8 * Community_j + \epsilon$$

To test whether the co-existence of multiple banks leads to increased defaults we will test the specification shown above. There are several interesting aspects of this specification. Firstly, the coefficients β_3 will show whether *progressive lending (T3)* borrowers who face one bank default more often than *high limit (T2)* borrowers who face one bank, which is the standard in this estimation. Secondly, a Wald test on β_2 and β_3 shows whether there are differences between *high limit (T2)* and *progressive lending* borrowers. Thirdly, β_4 will show whether borrowers who face multiple banks default on their loans significantly more often than borrowers who face one bank only if these banks do not set credit limits. Lastly, when comparing β_2 with β_4 using a Wald test the effect of facing multiple banks if progressive lending is used will show.

In this estimation, we excluded the treatment groups without a credit limit (T1 and T4). The reason for this is that for testing the third hypothesis, we were more interested in the comparison of the *high limit* and *progressive lending* treatment groups. The *no limit* treatment is the most unrealistic, as it is very unlikely that the MFIs do not set a credit limit. This is an interesting treatment to test the first hypothesis, but is much less relevant for this hypothesis.

However, this does mean that there are less group clusters, which requires the results to be stronger in order to be statistically significant.

The *default* variable is a dummy showing whether a participant defaulted on his loan in any of the four rounds. As this binary variable cannot be analysed using simple OLS regressions, binary choice models such as the logit and probit need to be used. For both these methods, the marginal effect of any variable is not constant and therefore need to be calculated separately. From the regression output, only the direction of the relation can be used for interpretation, and not the size of the coefficients (Verbeek, 2012).

6. Results

The first hypothesis discussed is that naïve borrowers anchor to given credit limits. We tested this by looking at the differences in overborrowing between participants in the *no limit* and *high limit* treatments who only faced a single bank, as shown by β_2 in the regression specification shown above. If anchoring would occur, people would borrow more in the *high limit* treatment in the first round of the experiment. Since the participants were randomised into the different treatment groups, we can assume there not to be any differences in the average production capacity of participants. Therefore, borrowing larger amounts would lead to more overborrowing. However, there was no significant difference in overborrowing between borrowers in the *no limit* and *high limit* treatments in the first round. This result is robust to various estimation methods (OLS and Tobit regression) and adding control variables to the estimation (both shown in Table 4). It seems that borrowers are risk averse, as they greatly increase their loans after the first round. In the second round, average loan sizes for borrowers in the *no limit* or *high limit* treatments increase from 7.98 to 10.13. For the borrowers who did not face one bank but three, there were also no significant differences between the overborrowing of the *no limit* and *high limit* borrowers (p-value Wald test 0.5504[3]). Therefore, our first hypothesis is rejected. In this setting, naïve borrowers do not anchor to credit limits.

In this experimental setting it would not be necessary for microfinance banks to implement a system to limit overborrowing. However, a progressive lending system is actually common practice in the microfinance market in many countries, including Bolivia. Hence, it is interesting to study the effects of such a system. In the first round, we see that the average total loan of participants in both the *no limit* and *high limit* treatments are higher than the

[3] Wald test on Tobit coefficients

credit limit in the first round of the *progressive lending* treatment (the credit limit was 6 and participants on average borrowed approximately 8), illustrating borrowers are actually credit constrained in this first round. This demonstrates that risk aversion does not play a role in the credit decisions of borrowers in the *progressive lending* system, as they are credit constrained to below average loan size demanded by borrowers in other treatment groups.

Table 4

Hypothesis 1: Naïve borrowers anchor to credit limits [4]

	(1) Overborrowing OLS	(2) Overborrowing OLS	(3) Overborrowing Tobit	(4) Overborrowing Tobit
No limit - One bank	-0.0198 (0.0454)	-0.0261 (0.0508)	-0.00107 (0.226)	0.00901 (0.243)
Progressive lending - One bank	-0.0522 (0.0372)	-0.0554 (0.0427)	-0.358 (0.254)	-0.348 (0.269)
No limit - Three banks	-0.0321 (0.0401)	-0.0315 (0.0476)	-0.108 (0.195)	-0.0807 (0.227)
High limit - Three banks	-0.0634 (0.0581)	-0.0647 (0.0634)	-0.239 (0.285)	-0.201 (0.301)
Progressive lending - Three banks	-0.0544 (0.0353)	-0.0609 (0.0427)	-0.528^{*} (0.278)	-0.545^{*} (0.303)
Community f.e.	Y	Y	Y	Y
Age		0.000878 (0.000564)		0.00363 (0.00389)
Gender		-0.00509 (0.0168)		-0.0322 (0.130)
Education		0.000719 (0.00211)		-0.00411 (0.0184)
Constant	0.0631^{*} (0.0369)	0.0256 (0.0620)	-0.634^{**} (0.260)	-0.810 (0.506)
Sigma			0.578^{***} (0.0912)	0.583^{***} (0.0873)
N	271	255	271	255
(Pseudo) R^2	0.210	0.207	0.2080	0.2065

Standard errors in parentheses. Clustered on the group level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[4] T1 is used as the reference group

To study the effects of a progressive lending system and test our second hypothesis, we look at the difference between the *high limit* and *progressive lending* treatments in the fourth round, where the only difference between the two was the lending history of the borrowers. In that round, the amount participants could borrow in the two scenarios was exactly the same. Therefore, there is no other reason for differences in behaviour among the two treatment groups than having faced a *progressive lending* system in the past rounds of the experiment. This enables us to analyse the effect of having gone through a progressive lending process in the past. When we compare overborrowing in round four, we find that participants in the *progressive lending* treatment (over)borrow on average more than participants in the *high limit* treatment, as shown in Table 5. This result only shows if we correct for the control variables. It also holds if we look at borrowers who faced three banks instead of one (Wald test on *high limit – three banks* and *progressive lending – three banks* gives a p-value of 0.0008[5])

A possible explanation for this is the lack of learning effects and a certain degree of anchoring to credit limits. In a progressive lending system, borrowers do not have to be very careful when deciding how much they want to borrow, since the limit is constraining in the first few credit cycles. Therefore, they can simply borrow the maximum amount allowed. However, at the point when the limit is no longer constraining, they are already used to borrowing the maximum amount, and have become incapable of deciding on the optimal loan size. Since the credit limit in the *high limit* treatment is high enough that many borrowers immediately realize this is too large for them to start with. Hence, they learn to critically assess their optimal loan size instead of easily anchoring to the limit, and become more experienced in this before the fourth loan cycle (or round in the experiment). This shows that even though there is no significant problem of overborrowing in the first round of this experiment, in a

[5] Wald test on Tobit coefficients

progressive lending system this problem does occur, at a later stage, due to a limiting of learning effects, confirming our second hypothesis.

Table 5

Hypothesis 2: Progressive lending limits learning effects and leads to overborrowing once credit limits have become non-constraining

	(1) Overborrowing OLS	(2) Overborrowing OLS	(3) Overborrowing Tobit	(4) Overborrowing Tobit
No limit – One bank	-0.0173	0.00605	-0.0315	0.0158
	(0.0336)	(0.0260)	(0.0955)	(0.0798)
Progressive lending - One bank	0.0533	0.0798^{**}	0.116	0.188^{**}
	(0.0392)	(0.0356)	(0.0935)	(0.0838)
No limit - Three banks	-0.00177	0.0226	-0.00978	0.0415
	(0.0294)	(0.0243)	(0.0822)	(0.0768)
High limit - Three banks	-0.0400	-0.00951	-0.136	-0.0513
	(0.0322)	(0.0259)	(0.0911)	(0.0803)
Progressive lending - Three banks	0.0422	0.0675^{*}	0.138	0.206^{**}
	(0.0377)	(0.0361)	(0.0954)	(0.0884)
Community f.e.	Y	Y	Y	Y
Age		-0.000833		-0.00362^{**}
		(0.000704)		(0.00168)
Gender		-0.0254		-0.0605
		(0.0173)		(0.0449)
Education		-0.000756		-0.000829
		(0.00259)		(0.00698)
Constant	0.114^{*}	0.109^{**}	-0.0312	0.0217
	(0.0615)	(0.0486)	(0.139)	(0.145)
Sigma			0.270^{***}	0.258^{***}
			(0.0195)	(0.0209)
N	271	255	271	255
(Pseudo) R^2	0.126	0.163	0.1519	0.2122

Standard errors in parentheses. Clustered on the group level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As the game also allowed for defaults, a borrower could decide not to repay his loan, either because he had earned too little in the round of the experiment to be able to repay, or because of strategic reasons. As shown in Table 3, if we analyse the total amount of borrowers who defaulted in at least one round of the experiment, we find that participants default more often when they face a market with several non-communicating banks.

From the table, only the difference between *high limit – one bank* and *high limit – three banks* can be determined. However, significant differences were observed between *progressive lending – one bank* and *progressive lending – three banks* (p-value Wald test 0.0244). Since the multiple banks in this experiment do not communicate, the dynamic incentive of punishment after default is less credible. If one bank will not provide a future loan, there are still two other banks to borrow from. This makes it easier for participants to decide not to repay their loan, since the consequences for the rest of the experiment are not as serious as if there would have been only one bank. This confirms the importance of a well-functioning credit registration bureau.

A more fascinating outcome of this analysis is that borrowers in the *progressive lending* treatment also default significantly more often than borrowers who face no, or a non-binding, credit limit. This result is found only in the estimations with control variables, but both with one and three banks[6], even though it is stronger in the presence of only one bank. The smaller amount of clusters due to the exclusion of the treatment groups without a credit limit especially shows in the probit regression without control variables, where the *progressive lending – three banks* treatment group dummy is insignificant. Recalculating the estimate omitting the clustering of the standard errors maintained a significant effect (p-value 0.021).

The differences in overborrowing between *progressive lending* and *high limit* borrowers are actually driven by borrowers who defaulted on their loan because they did not earn sufficiently to repay their loan. There is no significant difference in terms of pure strategic

[6] Wald test p-value (Probit): *high limit – three banks* and *progressive lending – three banks*: 0.0935

defaults[7] between borrowers in the progressive lending treatment and the other borrowers, but there is in terms of 'forced' defaults[8]. This is directly linked to the previously discussed problem of overborrowing in a progressive lending system. If borrowers overborrow too much (more than 50% of their loan), they will eventually not be able to repay their loans because of the interest rate charged and face the consequence of having to default on their loan. These findings are in line with our third hypothesis.

Table 6
Hypothesis 3: Borrowers default more often in the presence of multiple banks and when a progressive lending system is used

	(1) Default OLS	(2) Default OLS	(3) Default Probit	(4) Default Probit
Progressive lending - One bank	-0.00546 (0.0875)	0.109 (0.0751)	-0.127 (0.435)	4.469*** (0.248)
High limit - Three banks	0.0365 (0.0856)	0.186*** (0.0569)	0.0703 (0.453)	4.689*** (0.289)
Progressive lending - Three banks	0.239** (0.105)	0.322*** (0.111)	0.728 (0.477)	5.345*** (0.418)
Community f.e.	Y	Y	N	N
Age		0.000139 (0.00228)		-0.0000790 (0.0130)
Gender		-0.106* (0.0542)		-0.531** (0.258)
Education		-0.00463 (0.00829)		-0.0435 (0.0428)
Constant	0.282 (0.242)	-0.0627 (0.150)	-1.221*** (0.381)	-5.280*** (0.748)
N	175	160	175	160
(Pseudo) R^2	0.194	0.240	0.0621	0.1776

Standard errors in parentheses. Clustered on the group level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[7] Strategic defaults are defaults on loans when the borrower actually earned sufficient to be able to repay the loan and the interest charged but decided not to
[8] Defaults are considered 'forced' if the borrowers had no other option than defaulting on their loan since they did not earn enough in the round of the experiment

7. Discussion

There are three main limitations to this study. Firstly, even though the total sample size is sufficient (271), the experiment consists of six different treatment groups reducing the sample size per group (±45), affecting the power of the analyses. The aforementioned results are strong enough to also appear in such a small setting. Since randomization was done at the group level instead of individually, there is a possibility of intra-group correlation. Due to practical constraints, only the order in which the different treatment groups started playing was randomised. Therefore, it was the case that the five people who arrived first always played together, as well as the five people who arrived last. This could lead to biases, as there may be differences in behaviour between people who are in general more punctual and those who arrive a few hours late. The inclusion of clustered standard errors at the group level corrected for this. Furthermore, there might have been session-specific effects, such as the time of day in which the experiment was conducted, or some locations being more public than others. The inclusion of community dummies in the analyses correct for such potential session-specific characteristics, as well as village-specific characteristics, since every community was visited only once.

Secondly, the sample selection for this study had some limitations. We mainly included the same households that had participated in another study, consisting of a large household survey. The participants of this survey were selected based on lists provided by the *secretaria general*, the elected head of the community, and lists of clients of Sembrar Sartawi, a microfinance institution active in the region. The community heads were asked to include every household head living in the community. This required a lot of care, as in this particular region in Bolivia, many people are affiliated with a different community than the one they live in. Some families living in communities do not cooperate in communal labour or do not attend community meetings. Therefore, there is a possibility that the community head does not know all families actually living in the community, despite claiming to do so. Additionally, he or she might not want certain households to be included in a research providing them with a financial reward, since they do not contribute to the community. Due to practical reasons, we depended quite heavily on the information provided by the community head. Furthermore, the lists provided by the microfinance institution were not completely reliable, where at times they were outdated or included people from other communities, potentially affecting the randomness of participant selection. At times, they were outdated, or they included people living in other communities. These issues may have influenced the randomness of participant selection. Even with the aforementioned lists and contact details, it was often challenging to contact households and convince household heads to participate in the experiment because some refused to participate in the study due to other obligations. These factors made it hard to use information from the household survey while analysing the results of the experiment.

Finally, there are some limitations of the design of the experiment. Obviously, an experiment is always a simplified version of reality. For example, all rounds are independent of one another meaning that the rewards from one round cannot be used in another whilst in real life people can use income from one year in another, thus not independent. In order to successfully evaluate learning effects, the results of the rounds were required to be independent. Additionally, there can be other reasons for the rejection of our first hypothesis besides actual absence of any anchoring effect. For example, the anchor could have been unrealistically high, as we set the credit limit twice as high as the average production capacity of the conducted field tests. Soman & Cheema (2002) concluded that the credibility of credit limits is a very important determinant for the extent to which borrowers use it as an anchor.

Thus, if it was indeed set the limit to an unrealistically high value, the borrowers did not see it as a credible credit limit, and therefore did not anchor to this limit. In addition, the participants in Bolivia turned out to be risk averse in their decisions in the first round, since they borrowed substantially less in the first round than in the subsequent rounds. A possible explanation for this is that the risk aversion of the participants was stronger than the anchoring effect. This was not covered in the current experiment and would be an interesting topic for further research.

8. Conclusion

This study significantly adds to the existing literature on experimental evidence of the functioning of microfinance. It demonstrates how anchoring effects can limit learning effects in the presence of credit limits set according to the principle of progressive lending, and therefore lead to increased overborrowing among microfinance borrowers. To do this, we tested three different hypotheses in this research.

Firstly, we expected naïve borrowers to anchor to credit limits and therefore overborrow. However, this was not seen in our experiment as there was no significant difference in overborrowing between borrowers who faced a credit limit and those who did not. Secondly, we hypothesized that a progressive lending system could limit learning effects and therefore lead to overborrowing at the moment credit limits became non-constraining. Indeed, borrowers who had faced rising credit limits over the earlier loan cycles overborrow more when the credit limit grows to a non-constraining level, comparative to borrowers who had always faced this high credit limit. Thirdly, we expected borrowers in a progressive lending system to default on their loans more frequently, as well as borrowers facing multiple banks. This hypothesis is confirmed by our experiment. In all treatment groups where multiple banks were involved, there were significantly more defaulting borrowers than among their counterparts facing the same credit limits, but in treatment groups involving only one bank. Besides, there were significantly more defaulting borrowers in the progressive lending treatment groups than in the treatment groups with high credit limits.

A randomised control trial involving treatment groups with and without progressive lending systems would be the ultimate way to test whether the findings of this study hold outside of an experimental setting. Besides, further research is required to gain a greater understanding of the relationships observed that were not anticipated while designing the study, such as, the relationship between risk aversion and anchoring effects. In addition, including more qualitative research methods to explore the reasons behind the behaviour of borrowers to determine whether borrowers are deliberately using credit limits as a source of information of creditworthiness, or whether this happens subconsciously.

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10. Appendix

Picture 1: Experimental set-up T6: Progressive lending – three banks

Picture 2: Materials: Four-colour pasta and plastic cups

Picture 3: Women participating in the experiment

Picture 4: A full group sorting pasta