Group Lending or Individual Lending? Evidence from a Randomized Field Experiment in Mongolia*

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Abstract

Although microfinance institutions across the world are moving from group lending towards individual lending, this strategic shift is not substantiated by sufficient empirical evidence on the comparative impact of both types of lending on borrowers. We present such evidence from a randomized field experiment in rural Mongolia. We find a positive impact of access to group loans on food consumption and entrepreneurship. Among households that were offered group loans the likelihood of owning an enterprise increases by ten per cent more than in control villages. Enterprise profits increase over time as well, particularly for the less-educated. For individual lending on the other hand, we detect no significant increase in consumption or enterprise ownership. These results are in line with theories that stress the disciplining effect of group lending: joint liability may deter borrowers from using loans for non-investment purposes. Our results on informal transfers are consistent with this hypothesis. Borrowers in group-lending villages are *less* likely to make informal transfers to families and friends while borrowers in individual-lending villages are *more* likely to do so. Importantly, we find no significant difference in repayment rates between the two lending programs, neither of which entailed weekly repayment meetings.

Keywords: Microcredit; group lending; poverty; access to finance; randomized field experiment

JEL Codes: 016, G21, D21, I32

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1 Introduction

The effectiveness of microcredit as a tool to combat poverty is much debated now that after years of rapid growth microfinance institutions (MFIs) in various countries - including India, Bosnia and Herzegovina, and Nicaragua - are struggling with client overindebtedness, repayment problems, and in some cases a political backlash against the microfinance sector as a whole. This heightened scepticism, perhaps most strongly voiced by Bateman (2010), also follows the publication of the findings - summarized below - of a number of randomized field experiments indicating that the impact of microcredit might be more modest than thought by its strongest advocates. These studies have tempered the expectations many had about the ability of microcredit to lift people out of poverty.

Much remains unclear about whether, and how, microcredit can help the poor to improve their lives. Answering these questions is even more important now that the microcredit industry is changing in various ways. In particular, increased scale and professionalization has led a number of leading MFIs to move from group or joint-liability lending, as pioneered by the Bangladeshi Grameen bank in the 1970s, to individual microlending.[1]

Under joint liability, small groups of borrowers are responsible for the repayment of each other's loans. All group members are treated as being in default when at least one of them does not repay and all members are denied subsequent loans. Because co-borrowers act as guarantors they screen and monitor each other and in so doing, reduce agency problems between the MFI and its borrowers. A potential downside to joint-liability lending is that it often involves time-consuming weekly repayment meetings and exerts strong social pressure, making it potentially onerous for borrowers. This is one of the main reasons why MFIs have started to move from joint to individual lending.

Somewhat surprisingly, there as yet exists very limited empirical evidence on the relative merits of individual and group lending, especially in terms of *impacts on borrowers*. Both the ample theoretical and the more limited empirical literature mainly center on the impact of joint liability on repayment rates. Armendáriz and Morduch (2005, p. 101-102) note that: "*In a perfect world, empirical researchers would be able to directly compare situations under group-lending contracts with comparable situations under traditional banking contracts. The best test would involve a single lender who employs a range of contracts (...). The best evidence would come from well-designed deliberate experiments in which loan contracts are varied but everything else is kept the same.*"

This paper provides such evidence from a randomized field experiment among 1,148 poor women in 40 villages across rural Mongolia. The aim of the experiment, in which villages were randomly assigned to obtain access to group loans, individual loans, or no loans, is to measure and compare the impact of both types of microcredit on various poverty measures. Importantly, neither the group nor the individual-lending programs include mandatory public repayment meetings and are thus relatively flexible forms of microcredit.

The loans provided by the programs we investigate are relatively small, targeted at female

[1] Liability individualization is for instance at the core of 'Grameen Bank II'. Large MFIs such as ASA in Bangladesh and BancoSol in Bolivia have also moved towards individual lending. Cull, Demirguç-Kunt, and Morduch (2009) show that joint-liability lenders tend to service poorer households than individual-liability lenders.

borrowers, and progressive in nature: successful loan repayment gives access to another loan cycle, with reduced interest rates, as is the case with many microcredit programs. Our evaluation is based on two data rounds of collections: a baseline survey collected before the start of the loans, and a follow-up survey collected 18 months (and potentially several loan cycles) after the baseline.

Though the loans provided under this experiment were originally intended to finance business creation, we find that in both the group- and in the individual-lending villages, about one half of all credit is used for household rather than business goals. Women who obtained access to microcredit often used the loans to purchase household assets, in particular large domestic appliances. Only among women that were offered group loans do we find an impact on business creation: the likelihood of owning an enterprise increases for these women by ten per cent more than in control villages. We also document an increase in enterprise profits but only for villages that had access to microcredit for longer periods of time. In terms of poverty impact, we find a substantial positive effect of access to group loans on food consumption, particularly of fruit, vegetables, dairy products, and non-alcoholic beverages.

In terms of individual lending, overall we document no increase in enterprise ownership, although there is some evidence that as time passes women in these villages are more likely to set up an enterprise jointly with their spouse. Amongst women in individual-lending villages we also detect no significant increase in (non-durable) consumption, though we find that women with low levels of education are significantly more likely to consume more.

The stronger impact on consumption and business creation in group-lending villages, after several loan cycles, may indicate that group loans are more effective at increasing the permanent income of households, though we detect no evidence of higher income in either individual- or group-lending villages, relative to controls. If one were to take at face value the evidence on the larger impact of group loans, one would want to ask *why* such loans are more effective at raising consumption (and probably long-term income). One possibility is that the joint-liability scheme better ensures discipline in terms of project selection and execution, so that larger long-run effects are achieved. We document results on informal transfers that support this hypothesis: women in group-lending villages decrease their transfers to families and friends, contrary to what we find for women in individual-lending villages.

The remainder of this paper is structured as follows. Section 2 summarizes the related literature, and this is followed by a description of our experiment in Section 3. Section 4 then explains our estimation methodology, and Section 5 provides the main results. Section 6 concludes.

2 Related literature

This paper provides a comparative analysis of individual versus joint-liability microcredit and as such is related to the theoretical literature on joint-liability lending that emerged over the last two decades.[2] Notwithstanding the richness of this literature, the impact of joint liability on

[2]See Ghatak and Guinnane (1999) for an early summary. Theory suggests that joint liability may reduce adverse selection (Ghatak, 1999/2000 and Gangopadhyay, Ghatak and Lensink, 2005); ex ante moral hazard by preventing excessively risky projects and shirking (Stiglitz, 1990; Banerjee, Besley and Guinnane, 1994 and Laffont and Rey, 2003); and ex post moral hazard by preventing non-repayment in case of successful projects (Besley and Coate, 1995 and Bhole and Ogden, 2010).

risk taking and investment behavior remains ambiguous. For instance, on the one hand, group lending may encourage moral hazard if clients shift to riskier projects when they expect to be bailed out by co-borrowers. On the other hand, joint liability may stimulate borrowers to reduce the risk undertaken by co-borrowers since they will get punished if a co-borrower defaults.

Giné, Jakiela, Karlan, and Morduch (2010) find, based on laboratory-style experiments in a Peruvian market, that contrary to much of the theoretical literature, joint liability stimulates risk taking - at least when borrowers know the investment strategies of co-borrowers. When borrowers could self-select into groups there was a strong negative effect on risk taking due to assortative matching. Fischer (2010) undertakes similar laboratory-style experiments and also finds that under limited information, group liability stimulates risk taking as borrowers free-ride on the insurance provided by co-borrowers (see also Wydick, 1999). However, when co-borrowers have to give upfront approval for each others' projects, ex ante moral hazard is mitigated. Giné and Karlan (2010) examine the impact of joint liability on repayment rates through two randomized experiments in the Philippines.[3] They find that removing group liability, or introducing individual liability from scratch, did not affect repayment rates over the ensuing three years. In a related study, Carpena, Vole, Shapiro and Zia (2010) exploit a quasi-experiment in which an Indian MFI switched from individual to joint-liability contracts, the reverse of the switch in Giné and Karlan (2010). They find that joint liability significantly improves loan repayment rates.

To the best of our knowledge, there as yet exists no comparative empirical evidence on the merits of both types of lending from the borrower's perspective. Earlier studies that focus on the development impact of microcredit study either individual or joint-liability microcredit, not both in the same framework. In an early contribution, Khandker and Pitt (1998) and Khandker (2005) use a quasi-experimental approach and find a positive impact of joint-liability microcredit on household consumption in Bangladesh, though one must acknowledge the possibility of omitted variable and selection bias. Morduch (1998) and Morduch and Roodman (2009) replicate the Bangladeshi studies and find no evidence of a causal impact of microcredit on consumption. Kaboski and Townsend (2005) also use non-experimental data and document a positive impact of joint-liability microcredit on consumption but not on investments in Thailand. Based on a structural approach the authors corroborate this finding in Kaboski and Townsend (2011). Bruhn and Love (2009) use non-random opening of bank branches in Mexico to analyze the impact of access to individual loans on entrepreneurship and income. They find that branch openings led to an increase in informal entrepreneurship amongst men but not women. Because women in 'treated' municipalities start to work more as wage-earners they eventually increased their income too.

More recently, randomized field experiments have been used to rigorously evaluate development policies, including microcredit (Duflo, Glennerster and Kremer, 2008). Banerjee, Duflo, Glennerster and Kinnan (2010) randomly phase in access to joint-liability microcredit in the Indian city of Hyderabad. The authors find a positive impact on business creation and investments by existing businesses, while the impact on consumption is heterogeneous. Those that start an enterprise reduce their non-durable consumption so they can pay for the fixed cost of the start-up

[3] Ahlin and Townsend (2007) empirically test various repayment determinants in a joint-liability context.

(which typically exceeds the available loan amount). In contrast, non-entrepreneurs increase their non-durable consumption. Crépon, Devoto, Duflo, and Parienté (2011) find that the introduction of joint-liability loans in rural Morocco led to a significant expansion of the scale of pre-existing entrepreneurial activities. Here as well there was a heterogeneous impact on consumption with those expanding their business decreasing their non-durable and total consumption.

Two other field experiments focus on individual-liability loans. Karlan and Zinman (2011) instructed loan officers in the Philippines to randomly reconsider applicants that had been labelled 'marginal' by a credit-scoring model. They find that access to loans *reduced* the number and size of businesses operated by those who received a loan. In a similar vein, Augsburg, De Haas, Harmgart and Meghir (2011) analyze the impact of microcredit on marginal borrowers of a Bosnian MFI. In contrast to Karlan and Zinman (2011), they find that microcredit increased entrepreneurship although the impact was heterogeneous - similar to Banerjee et al. (2010) and Crépon (2011). Because microloans only partially relaxed liquidity constraints, households had to find additional resources to finance investments. Households that already had a business and that were highly educated did so by drawing on savings. In contrast, business start-ups and less-educated households, with insufficient savings, had to cut back consumption. These households also reduced the school attendance of young adults aged 16-19.

Our paper is the first to use the same experimental context to compare the impact of individual versus joint-liability microcredit on borrowers.

3 The experiment

3.1 Background

Microfinance, as it is known today, originated in Bangladesh - one of the most densely populated parts of the world with 1,127 people per km^2 - but has also taken hold in less populated countries. One of these is Mongolia, which encompasses a land area half the size of India but with less than 1% of the number of inhabitants. This makes it the least densely populated country in the world with just 1.7 people per km^2.[4] This extremely low population density means that disbursing, monitoring, and collecting small loans to remote borrowers is very costly, particularly in rural areas. Mongolian MFIs are therefore constantly looking for cost-efficient ways to service such borrowers.

Mongolian microcredit has traditionally been provided in the form of individual loans, reflecting concerns that the nomadic lifestyle of indigenous Mongolians had impeded the build up of social capital outside of the family. Notwithstanding such concerns, informal collective self-help groups (*nukhurlul*) have developed and some of these have started to provide small loans to their members, in effect operating as informal savings and credit cooperatives. This indicates that group lending might be feasible in rural Mongolia too. Moreover, recent theoretical work suggests that when group contracts are sufficiently flexible, group loans can be superior to individual loans even in the absence of social capital (Bhole and Oden, 2010). This implies

[4]Source: United Nations World Population Prospects (2005). Mongolia has a semi-arid continental climate and an economy dominated by pastoral livestock husbandry, mining, and quarrying. Extreme weather conditions - droughts and harsh winters with temperatures falling below -35º C - frequently lead to large-scale livestock deaths.

that group lending may also work in countries were social connectedness and the threat of social sanctions is relatively limited.

This paper describes a randomized field experiment conducted in cooperation with XacBank, one of Mongolia's main banks and the second largest provider of microfinance in the country, to compare the impact of individual and group loans on borrowers' living standards.[5] While XacBank provides both men and women with microcredit, our experiment focused on extending credit to relatively less well-off women in rural areas. This target group was believed to have considerably less access to formal credit compared with richer, male, and urban Mongolians. According to the Mongolian National Statistics Office (2006, p. 54): "*Microcredit appears to be unavailable to most of the poor living in the aimag and soum centers. Their normal channels for credit are to borrow from a shop or kiosk where they often buy supplies or from a relative or friend*".[6]

3.2 Experimental design

The experiment took place in 40 soum centers (henceforth: villages) across five aimags (henceforth: provinces) in northern Mongolia. Figure A1 in the Annex maps the geographical location of all participating villages and provinces. The experiment started in January-February 2008 when XacBank loan officers and representatives of the Mongolian Women's Federation (MWF) organized information sessions in all 40 villages. [7]The goal and logistics of the experiment were explained and it was made clear to potential borrowers that there was a 2/3 probability that XacBank would start lending in their village during the experiment and that lending could take the form of either individual or group loans. Women who wished to participate could sign up and were asked to form *potential* groups of about 7 to 15 persons each. Because of our focus on relatively poor women, the eligibility criteria stated that participants should in principle own less than 1 million Mongolian tögrög (MNT) (USD 869) in assets and earn less than MNT 200,000 (USD 174) in monthly profits from a business.[8] Many of these women were on official 'poor lists' compiled by district governments.

Various indicators show that the households in our sample lie markedly below the Mongolian average in terms of income, expenditures, and social status. Data from the Mongolian statistical office indicate that the average rural household in 2007 had an annual income of MNT 3,005,000 (USD 2,610) whereas the average household in our sample earned MNT 1,100,000 (USD 955) (we define earnings as profits from household enterprises plus wages from formal employment

[5]According to XacBank's mission statement, it intends to foster Mongolia's socio-economic development by providing access to comprehensive financial services to citizens and firms, including those that are normally excluded such as low-income and remote rural clients. The bank aims to maximize the value of shareholders' investment while creating a profitable and sustainable institution.

[6]Mongolia is divided into 18 aimags or provinces which are subdivided into 342 soums or districts. Each soum contains a small village or soum center of on average 1 kilometer in diameter. The average soum in our experiment had 3,853 inhabitants of which on average 1,106 people (314 households) lived in the central village. The average distance from a village to the nearest province center - small towns where XacBank's branches and loan officers are based - is 116 kilometers. Because the distance between a village and the nearest paved road is on average 170 km, travel between villages, and between villages and province centers, is time consuming and costly.

[7]The MWF is a large NGO whose representatives worked together with XacBank and the research team to ensure a smooth implementation of the experiment. They signed up participants, facilitated group formation in the group-lending villages, provided information to loan applicants, and assisted the survey company.

[8]We use a MNT/USD exchange rate of 1,150 which corresponds to the average exchange rate during the first half of 2008.

by all household members). Similar patterns emerge when we compare expenditure levels, using data from the Mongolian statistical office or the EBRD 2006 Life in Transition Survey, or when we compare livestock ownership, a primary wealth indicator in Mongolia.

After about 30 women had signed up in each village, a detailed baseline survey was administered to all 1,148 participants during March-April 2008. Face-to-face interviews were conducted by a specialized survey firm hired by the research team and independent of XacBank. Interviews were held at a central location in each village where respondents and interviewers had sufficient time to go through the questions without interruptions. Use of a central location also minimized the risk that the female respondents would give biased answers due to the presence of older and/or male family members (as had happened during piloting). Interviews lasted approximately one hour. At the time of the baseline survey we also collected information on the main socioeconomic, demographic, and geographic characteristics of the 40 villages.

The baseline survey measured variables that reflect households' standards of living and that could be expected to change over the 1.5 year interval of the experiment. These include income, consumption, and savings; entrepreneurial activity and labor supply; asset ownership and debt; and informal transfers. In addition, information was elicited about household composition and education; exposure to economic shocks; and respondents' income expectations. The surveys also collected information on more context-specific poverty indicators such as livestock ownership and the quality and size of the dwelling, most often a *ger*.[9]

Randomization took place *after* completion of the baseline survey so that at the time of the interview, respondents did not know whether or not they would be offered a group loan, an individual loan or no loan at all. Randomization took place at the village level, with 15 villages receiving access to individual loans, 15 receiving access to group loans, while in 10 control villages XacBank did not provide loans to the participating women for the duration of the experiment. In all three types of villages XacBank continued to provide individual microloans to regular, more wealthy clients most of whom were male. Randomization across rather than within villages was chosen because it was administratively and politically easier to manage. Moreover, randomization across villages avoids the possibility that the program affects even individuals who do not receive it directly, though informal transfers and connections. We also stratified at the province level because a completely randomized design could have resulted in a situation whereby some provinces contained only treatment or control villages, which was unacceptable to XacBank. Also, to the extent that geographical or economical differences between provinces are large, we might not have been able to detect treatment differences in an unstratified design.

After randomization, group formation proceeded in the 15 group-lending villages, but not in the individual-lending and control villages. Group formation consisted of the development of internal procedures, the election of a group leader, and the signing of a group charter. Groups were formed by the women themselves, not by XacBank. A maximum of two women per group were allowed to be from the same family. Group members lived in the same village and already knew each other to varying degrees. In many cases actual group composition differed substantially

[9] A ger is a portable tent made from a wood frame and felt coverings. Its size is measured by the number of lattice wall sections (*khana*). A basic ger consists of four or five khana, with larger and less common sizes including six, eight, or ten khana. Bigger gers are a sign of wealth as they are more costly to heat. A sufficiently insulated ger has two layers of protective felt, whereas poorer households often only have one layer. Gers are sometimes surrounded by (costly) wooden fences (*hashaa*) that offer protection from the wind.

from the potential groups that were identified at the very beginning of the experiment when women had to indicate their interest (or not) to participate in the project. After a group had collected enough internal savings it could apply for its first XacBank loan. We provide detailed information on the type of loans offered in Section 3.4 below.

The 'treatment period' during which XacBank provided loans in the group and individual lending villages lasted 1.5 years - from April 2008 to September 2009 - with some variation across villages. During this period participating women in treatment villages could apply for (repeat) loans, while XacBank refrained from lending in the control villages. In October-November 2009 we conducted a follow-up survey to again measure the poverty status and economic activity of our sample of participating women. We also obtained information on how women had used their XacBank loan(s). In addition, we conducted a second village-level survey to collect information on village characteristics that may have changed, such as the prices of important consumer goods. Lastly, XacBank collected repayment information on all of its loans for the period April 2008-June 2011. In October 2011 we revisited one individual-lending and two group-lending villages for structured interviews and discussions with a number of borrowers about how they had experienced the lending programs.

3.3 Randomization

Table 1 presents a statistical comparison between the control villages and the two types of treatment villages. We compare the means of various characteristics of the villages themselves and of the respondents and their households. Treatment and control villages are very similar overall, and in particular in terms of size, number of inhabitants, distance to the nearest province center and the nearest paved road, and the prices of various consumption goods (Panel A). Panel B shows that the respondents living in the treatment and control villages are on average very similar too. We find no significant differences in household structure, informal transfers, self-employment, wage earnings, the value of the dwelling, or consumption patterns. Households are also very similar in terms of a large number of other consumption and asset-ownership measures (not shown but available upon request).

[INSERT TABLE 1 HERE]

Panel C also shows no significant differences between control and treatment villages in terms of the number and type of businesses operated by our respondents and their households. We do find, however, some differences in terms of access to finance at the household level. A majority of the households had at least one loan outstanding at the time of the baseline survey and this percentage is higher in the individual-lending villages (67 per cent) than in the control villages (56 per cent). However, conditional on having at least one loan, there are no significant differences between the treatment and control villages in the average number of loans per household, the total debt value (in absolute terms and in percent of household income), and the debt-service burden.

These figures also indicate that at the time of our baseline survey the penetration of micro-credit was already well advanced in rural Mongolia. For our purposes, however, an important

question is whether households were already using their access to microcredit to finance entrepreneurial activities by our female respondents. Our baseline data show that this appears not to be the case. First, from Panel C we see that around 75 per cent of all outstanding loans were used for consumption, mainly to buy electric household appliances, instead of income generation. This picture is the same across all types of villages. Second, fewer than 20 per cent of households had invested part of their loan(s) in a business owned by the female targeted by the loan. Furthermore, while access to credit at the household level was somewhat higher in individual-lending villages, Panel C shows that the amount and percentage of funds used for female enterprises did not differ significantly between the three types of villages. In control villages households had invested on average 15 per cent of their outstanding debt in a female-run business, whereas these percentages were 11 and 10 per cent in individual and group-lending villages. These percentages, as well as the absolute amounts, do not differ significantly between control and treatment villages.

We conclude that the randomization process was successful: we find very few significant differences between treatment and control villages, despite considering a broad range of variables. The few differences that do exist are small and do not provide evidence of a systematic disparity between treatment and control villages along any particular dimension. We are therefore confident that randomization ensured absence of selection bias so that we can attribute any post-treatment differences in outcomes to the lending programs.

3.4 The loan products

The purpose of both group and individual loans was to allow women to finance small-scale entrepreneurial activities.[10] Given the focus on business creation and expansion, loans had a grace period of either two months (for loans exceeding six months) or one month (for shorter-term loans).[11] The interest rate varied between 1.5 and 2 per cent per month and was reduced by 0.1 per cent after each successful loan cycle. Other dynamic incentives included the possibility to increase the loan amount and/or maturity after each repaid loan (Table 2).

[INSERT TABLE 2 HERE]

Group-loan contracts stated that loans were based on joint liability and that XacBank would terminate lending to the whole group if that group did not fully repay a loan. Most group loans were composed of individually approved sub-loans with a maturity between 3 and 12 months depending on the loan cycle (within a group all sub-loans had the same maturity). Groups could also apply for a joint loan to finance a collective business, for instance to grow crops. The maximum size of the first loan to a group member was MNT 500,000 (USD 435). Group members had to agree among themselves who would get a loan and for what purpose. They

[10] Besides agriculture - both animal husbandry and crop growing - the main village industries are baking, wood-processing, retail activities, and felt making.

[11] Field, Pande, and Papp (2010) provide evidence from a randomized field experiment in India that indicates that a two-month grace period - instead of the regular two weeks - and the associated flexibility led to more business creation and investments but also to lower repayment rates.

then had to apply for the loan and XacBank screened each application independently.[12] If a borrower's project was deemed too risky XacBank could exclude it while the other members would still get a loan. If most projects were judged to be too risky then the total group loan was rejected. Contrary to individual loans the screening of group loans thus involved a two-stage process: first by co-borrowers and then by a XacBank loan officer.

Before applying for a loan, groups had to build up savings in a joint savings account equivalent to 20 per cent of the requested loan amount. Group members were in principle allowed to pledge assets instead of the compulsory savings although XacBank encouraged borrowers to use savings. The savings not only served as collateral but were also a means of ascertaining whether potential borrowers had sufficient financial discipline. Group leaders were responsible for monitoring and collecting loan repayments and handing them over to the loan officer on a monthly basis. There were no public repayment meetings or other mandatory meetings.[13] Groups decided themselves on the modalities of their cooperation, including the frequency of meetings (typically once per month).

Individual loans were similar to the sub-loans provided to group members, though larger on average. XacBank did not use predetermined collateral requirements but took collateral if available. As a result 91 per cent of the individual loans were collateralized, with the average collateral value close to 90 per cent of the loan amount. The maturity of individual loans ranged from 2 to 24 months, depending on the experience of the borrower and the type of business being invested in. Group loans had a somewhat shorter maturity (192 days on average) than individual loans (245 days) which reflects the smaller size of the former. Similar to group loans, individual loans did not involve any mandatory group activities such as repayment meetings.

3.5 Loan take-up

After the baseline survey XacBank started disbursing individual (group) loans in individual (group) treatment villages. All women who had signed up and expressed an initial interest in borrowing were visited by a loan officer and received a first loan after a successful screening. After 1.5 years, 54 per cent of all treatment respondents had borrowed from XacBank: 57 per cent in the group-lending villages and 50 per cent in the individual-lending villages. Although other MFIs were also lending in both the treatment and control villages during the experiment, our intervention led to a significant increase in borrowing. The probability of receiving microcredit during the experiment was 24 percentage points higher in treatment than in control villages (50 per cent of respondents in control villages versus 74 per cent in treatment villages).

We use information from the follow-up survey to better understand why a relatively large proportion of women in treatment villages did not borrow. First, the data show that of the 326 women who had initially signed up in the treatment villages but who did not get a loan during the experiment, 167 (51 per cent) never actually applied for a loan. At the time of signing up women did not know whether they would get access to an individual or a group loan (or end

[12] The loan officers were all female, between 21 and 27 years old, married with one or two children, and had completed at least a four-year university degree. They normally assess between 35 (Hentii province) and 50 (Hovsgol province) loan applications per month with an approval rate of about 90 per cent.

[13] Field and Pande (2008) randomly assign weekly and monthly repayment meetings and find that a more flexible schedule can significantly lower transaction costs without increasing defaults.

up in a control village). Some women may only have been interested in an individual (group) loan and may therefore not have applied when their village was assigned to group (individual) lending.

Second, of the non-borrowers who *had* applied for a loan, 47 per cent refused the offer made by XacBank. The main reasons stated for not taking up the loan were that the amount was too small, the interest rate too high, or the repayment schedule unsuitable. In total, about 75 per cent of the 'non-treatment' was therefore due to women who either did not apply for a loan or who applied for one but subsequently refused the offer. This leaves about a quarter of all 'untreated' women who were actually refused a loan by XacBank.

When we asked respondents during the follow-up survey why XacBank had refused them a loan, the main answers were 'too much outstanding debt' and 'insufficient collateral'. As discussed in Section 3.3, the baseline survey revealed that many households already had at least one microloan, mainly for consumption purposes. Interviews with loan officers indicated that existing debt at the household level made them hesitant to provide additional loans to female household members, even though these new loans were intended for entrepreneurial purposes rather than for consumption. At the time the Mongolian Central Bank had also become increasingly concerned about overindebtedness in rural areas. Loan officers may have been particularly conservative in lending to poorer-than-usual borrowers, despite having been explicitly instructed to do so by XacBank management.[14]

The experiment also partly coincided with the global financial crisis during which Mongolian financial institutions suffered from reduced access to foreign funding. Domestic funding constraints also tightened. The Mongolian Central Bank imposed higher reserve requirements in an attempt to stem inflation while deposit inflows were below average as herders suffered from low international cashmere prices. The confluence of these three factors made interbank liquidity dry up between March and late June 2008 and correspondingly XacBank reduced its credit supply. The year-on-year growth rate of business lending even turned negative in November 2008, not reverting to positive until July 2009.

Table 3 displays the results of reduced-form probit regressions to explain the probability of loan take-up in more detail. We find a higher probability of borrowing in group-lending villages (significant at the 10 per cent level). A closer inspection of the underlying data indicates that the higher lending probability in group-lending villages is not driven by XacBank covering some (group) villages earlier than others or by the follow-up survey being conducted earlier in individual-lending villages. Instead, demand for loans may have been lower in individual-lending villages either because the availability of microcredit was somewhat higher in the first place (see Panel C of Table 1) or because access to group loans (previously unavailable to anyone in these villages) was valued more than access to individual loans (previously available).

Interestingly, the number (or amount) of *outstanding* loans at the time of the baseline survey is not negatively associated with the probability of obtaining a loan during the experiment (for instance because households had already reached their borrowing capacity, either according to their own judgment or that of the loan officer). We do find a negative but imprecisely measured

[14]XacBank provided 375 out of 534 applicants with a loan, an approval rate of 70.2 per cent. This is below XacBank's regular approval rate, which is about 95 per cent according to its own management information system and about 90 per cent according to the answers of the loan officers during the loan officer baseline survey.

association with *previous* loans, i.e. loans that had been repaid at the time of the baseline survey. Prior use of loans could indicate borrower quality in which case one would expect a positive sign. A negative sign may indicate that previous borrowers no longer require loans, or that they were not satisfied with the loan product. Note that the prior loan variable is significantly negative in the group-village specification (when province fixed effects are included) indicating that borrowers with no or limited borrowing experience were particularly likely to participate in a group loan. This may indicate that even when individual loans are available some women may only be interested in applying for a group loan.

Lastly, we find that households who own a well, fence, or tools and machinery had a higher probability of getting a loan, either because they are more wealthy or could use these items as collateral.

[INSERT TABLE 3 HERE]

3.6 Attrition

The follow-up survey took place approximately 1.5 years after the baseline survey and 86 per cent of respondents were successfully re-interviewed. While an attrition rate of 14 per cent is relatively low, there is always the concern that non-response was not random across treatment and control villages, which could bias the estimated treatment effects. To investigate this, we estimate the probability of attrition as a function of treatment village dummies as well as a range of respondent, village, and household characteristics.

Table 4 shows that respondents in individual-lending villages are almost 7 percentage points more likely to attrit compared with those in control villages, and this is of borderline statistical significance at conventional levels (depending on the inclusion of control variables and/or province fixed effects). We detect no differential patterns in attrition between group and control villages. On further investigation, we find that the differential attrition is driven by two individual-lending villages where the wedding season was underway at the time of the follow-up survey, resulting in many respondents being away from home temporarily. We are thus reassured that the reason for higher attrition is unlikely to be related to the program, and so we retain these two villages in the analysis. While one might think that loan use might be distorted due to the wedding season, we note that we also estimate all models excluding these two villages and find that our results are robust.

[INSERT TABLE 4 HERE]

Lastly, we note that other variables have the expected association with attrition: respondents that own a fence or a well and families with more women and small children are less likely to attrit - as one would expect, given that these characteristics are generally associated with less mobility. Households that live further from the province center and/or own horses or camels are more likely to attrit, presumably because they are more likely to live a semi-nomadic lifestyle and are thus more difficult to locate for interviews. Households that experienced a recent death were less likely to participate in the follow-up survey too.

4 Methodology

In what follows, we report the results of an intention to treat (ITT) analysis where we compare *all* women who initially signed up in treatment villages, irrespective of whether they borrowed or not, with those who signed up in control villages.[15] The advantage of this conservative approach is that we can interpret the experimental intervention as a policy and learn about the impact on the population that XacBank initially targeted, and not just on those who actually borrowed. We also employed an instrumental variables (IV) methodology in which we instrument *actual* borrowing status of participants with a dummy indicating whether or not the village was randomized to be a treatment village. These IV results are very similar to the ITT findings described below and are available on request.

Results reported here use a difference-in-differences technique to compare respondents in treatment and control villages before and after the loan treatment.[16] Whilst in principle we could attribute post-treatment differences to the lending programs, we improve precision slightly when we take various baseline characteristics into account that are strong determinants of the outcome variables. All findings remain very similar if we use post-treatment data only. Our basic regression framework is:

$$Y_{ivt} = \alpha_0 + I_v \cdot (\alpha_1 + \alpha_2 \cdot F_t) + G_v \cdot (\alpha_3 + \alpha_4 \cdot F_t) + \alpha_5 \cdot F_t + \alpha_6 \cdot X_{iv0} + \epsilon_{ivt} \quad (1)$$

where:

- Y_{ivt} is the outcome variable of interest for individual i in village v at time t ($t = 0$ (1) at baseline (follow-up) survey);
- I_v is a binary variable equal to 1 for individual-lending villages (0 otherwise);
- G_v is a binary variable equal to 1 for group-lending villages (0 otherwise);
- F_t is a follow-up binary variable (0 for baseline observations);
- X_{iv0} is a set of baseline characteristics of respondents, their households, and their villages;
- ϵ_{ivt} is an i.i.d. error term clustered at the village level.

In this specification α_2 and α_4 measure the impact of the individual and group lending treatment, respectively. In addition, we also run more flexible specifications where we allow for heterogeneous impacts. We first allow for variation by education level of the respondent, which we consider to be an indicator of long-term poverty of the household:

$$Y_{ivt} = \alpha_0 + I_v \cdot (\alpha_1 + \alpha_2 \cdot F_t) + G_v \cdot (\alpha_3 + \alpha_4 \cdot F_t) + \alpha_5 \cdot F_t + H_i \cdot Z + \alpha_{12} \cdot X_{iv0} + \epsilon_{ivt} \quad (2)$$

[15] One can calculate the impact of access to microcredit on those women who actually borrowed - i.e. the average effect of the treatment on the treated (ATT) - by dividing the ITT effect by the probability of receiving treatment (57 per cent in the group-lending villages and 50 per cent in the individual-lending villages). A caveat is that this may not generalize, as those who receive the treatment may be systematically different from those who do not. As the (heroic) assumption underlying consistent estimation of ATT is that unobservable characteristics do not affect the decision to participate, we only show ITT parameters.

[16] We estimate using OLS for continuous dependent variables, a probit model for binary dependent variables, and a tobit model for dependent variables that are censored at zero.

where

$$Z = \alpha_6 + I_v \cdot (\alpha_7 + \alpha_8 \cdot F_t) + G_v \cdot (\alpha_9 + \alpha_{10} \cdot F_t) + \alpha_{11} \cdot F_t$$

and H_i is one for individuals with a high education level (grade 8 or higher, or vocational training) and zero for individuals with a low education level (less than grade 8). All other variables are as previously defined.

Second, because respondents in some villages received more loans than in others and for longer periods of time, we also analyze the impact of treatment intensity over and above the basic impact of access to credit. We allow impact to vary by treatment intensity Int_v at the village level, either measured as the average number of loans ($Number_v$) or as the average number of months between the date when the first respondents in a village received a loan and the follow-up survey ($Months_v$):

$$Y_{ivt} = \alpha_0 + I_v \cdot (\alpha_1 + \alpha_2 \cdot F_t + \alpha_3 \cdot Int_v) + G_v \cdot (\alpha_4 + \alpha_5 \cdot F_t + \alpha_6 \cdot Int_v) + \alpha_7 \cdot F_t + \alpha_8 \cdot X_{iv0} + \epsilon_{ivt} \quad (3)$$

where α_3 and α_6 give the additional effect of treatment intensity in individual-lending and group-lending villages, respectively.

We measure treatment intensity at the village level to avoid endogeneity problems: more motivated and entrepreneurial individuals may make sure to get exposed to the lending program early on, which would lead us to erroneously attribute the effect of these borrower characteristics to early treatment. We should stress that the intensity of the program was not purposely varied in a random fashion among the treatment sample. One should therefore interpret with caution the results obtained estimating equation (3), as the intensity of the program might vary with unobserved village and/or individual characteristics and induce biases in the estimation of the coefficients of this equation. Having said that, numerous conversations with XacBank officials make us believe that the variation in intensity of the program across villages was by and large induced by administrative quirks and is unlikely to be endogenous.

The mean number of months between the date when the first respondents in a village received a loan and the date of the follow-up survey is 5.2 months (6.3 months in group-lending villages, 4.2 months in individual-lending villages) with a standard deviation of 2.7 months. The mean number of loans received is 0.78 (0.99 in group-lending villages, 0.57 in individual-lending villages) with a standard deviation of 0.48. This indicates that not only is the probability of borrowing higher in group villages, but so also is the intensity of the treatment.

5 Results

5.1 Loan use

We first provide a picture of what borrowers reported having used their loans for. Table 5 shows that women used the individual and group loans in very similar ways. Assuming that the purchase of livestock, tools, and machinery are business expenses, we find that 67 (66) per cent of group (individual) borrowers used their first loan mainly to invest in a new or existing enterprise, putting between 70 and 80 per cent of the loan to this purpose, with the remainder

being used for household expenses. In the case of second loans, fewer women - 43 (51) per cent of the group (individual) borrowers - used the loan primarily for business purposes.

[INSERT TABLE 5 HERE]

We can also compare what women reported as the purpose of the loan at baseline and at follow-up. When we do this, we find that 86 (93) per cent of group (individual) borrowers who at follow-up stated that they had used their loan(s) mainly for business purposes, had consistently indicated at the start of the experiment that they would use the loan for entrepreneurial activities. However, 82 per cent of women in both types of treatment villages who used the loan mainly for consumption had reported at baseline that they would use it to invest in a business. We cannot say whether they intentionally misreported at baseline (as the loans were marketed as business loans) or whether they later on changed their minds.

5.2 Impact of the microcredit programs

A key objective of the microcredit programs was to encourage women to expand or invest in small-scale enterprises, with the ultimate aim of reducing poverty and improving well-being. To evaluate the extent to which the programme achieved these two objectives, we first look at the effect on enterprise creation and growth, and on whether enterprise profits increased. We then go on to estimate its effect on detailed household consumption, as a measure of well-being. To pre-empt, we find evidence of households in group villages increasing investment in enterprises, and corresponding increases in consumption. We detect no systematic effects in individual villages.

5.2.1 Did the programs affect business creation and growth?

As discussed, one of the main intermediate objectives of the programs was to encourage women to invest in new or existing small-scale enterprises. We have seen some suggestive evidence that this was the case, with a large majority of women reporting having used a substantial part of their loan(s) to invest in working capital and fixed assets. In this section we estimate the effect on business creation and growth. Table 6 shows estimates from equation (1) through (3). The odd (even) columns show the impacts for group (individual) loans.

We first estimate the basic impact using equation (1), and then estimate heterogeneous impacts by education level (equation (2)) and treatment intensity (equation (3)). Treatment intensity is measured as the number of borrowing months or as the number of loans, and is in both cases the average at the village level. In line with equation (3) the intensity effects measure the impact of longer *actual* exposure to loans over and above the basic ITT effect. We use the same estimation approach for the other outcome variables. All regressions include a standard set of baseline respondent and village-level covariates (listed in Table A1 in the Annex) and our results remain robust to the exclusion of these covariates.

[INSERT TABLE 6 HERE]

Columns (1) and (2) show the impact of access to microcredit on the probability that the household operates a small-scale business, whether the respondent's own one, her partner's, or

their joint one (65 per cent of respondents are married or cohabitating). Columns (3) and (4) show similar regressions but specifically for the respondent's own enterprise. We see that access to group loans has a significant positive impact on female entrepreneurship and this effect is largely driven by less-educated women (see row II). At the end of the experiment, these women had a 29 per cent higher chance of operating a business compared with women in the control villages. This difference is 10 per cent for highly educated women.[17] Rows III and IV show that a large part of these effects is driven by women who had been exposed to (repeat) loans for a longer period of time.

The results for access to individual loans are less strong. Columns (2) and (4) indicate no impact on female entrepreneurship, although there is a positive impact on total entrepreneurship over time (row III). This latter effect is driven by joint enterprises which become more prevalent in individual-lending compared with control villages. In individual-lending villages where respondents borrowed on average for six months, the probability that a household operates any type of business is 12 percentage points higher than in the control villages. Interestingly, the nature of the businesses operated by women themselves and those operated jointly with their spouses differ. The former are mostly sewing businesses and small-scale retail activities whereas the latter comprise mainly animal husbandry and crop production.

Figure 1 depicts how the *actual* loan exposure at the village level influences entrepreneurship (for a typical respondent with average covariate values). The left-hand (right-hand) panels show individual- (group)-lending villages. The upper panels focus on the likelihood that women run their own business, whereas the lower panels indicate the probability that households operate any kind of business. The starting point of each graph indicates the probability of business ownership for the average respondent in treatment villages where in practice virtually no XacBank lending took place. Due to the randomization these values do not differ significantly between both types of treatment villages nor do they differ from the values in the control villages (where XacBank did not lend *by design*). The graphs then show similar point estimates, surrounded by a 95 per cent confidence interval, for the probability of business ownership in treatment villages where the actual average exposure was 2, 4, 6, 8, 10, or 12 months.

While in all four graphs the probability of business ownership increases with loan exposure, the confidence intervals are narrowest for female enterprises in group-lending villages and for all enterprises in individual-lending villages. For example, a typical respondent in a group-lending village where respondents were only exposed to credit for a few days, had a 36 per cent probability of operating her own enterprise (the same as in a control village). A similar respondent in a group-lending village where respondents had been borrowing for a full 12 months had a 53 per cent probability of running a business. This 53 per cent is outside the 95 per cent interval surrounding the point estimate of 36 per cent for respondents in relatively less treated villages. These results mirror those in Table 6: female enterprises became more prevalent in group-lending villages (compared with the control villages) whereas in individual-lending villages there was a gradual and significant increase in the number of businesses operated jointly by borrowers and their spouses.

[17] This also translates into a higher likelihood of operating any type of enterprise (column (1)). Unreported regressions show that there is no strong impact of access to group loans on enterprise ownership by, or jointly with, the borrower's partner. The effect in column (1) is thus driven by an increase in female entrepreneurship.

[INSERT FIGURE 1 HERE]

Columns (5) to (8) in Table 6 analyze whether access to credit resulted in more profitable enterprises. Even though enterprise profitability decreased in both treatment and control villages between the baseline and follow-up surveys, mainly due to the economic crisis, access to credit seems to have partly shielded borrowers from this impact. Columns (5) and (7) show that over time and after repeat borrowing, enterprises in group-lending villages were significantly more profitable than those in control villages. After half a year of exposure to credit, the difference in yearly profitability amounts to over 200,000 tögrög, or almost one third of the average annual enterprise profits at baseline. We find a similar positive impact on business profits in individual-lending villages, although here again the impact is mainly due to enterprises that are operated jointly with the borrower's partner.

Lastly, we look at whether households increased labour supply in line with this increased business creation. About a quarter of respondents were employed in wage activities at the time of the baseline interview and they received an average wage of MNT 130,000 (USD 113) per month. During the experiment the share of wage employment remained unchanged and there was a marked drop in salary levels, most likely due to the global crisis. We find no clear impact of the programs on total labor supply or income at the household level, nor do we find an impact when we split labor supply into wage labor and hours worked in own enterprises (Table 7). There is weak evidence (at the 10 per cent significance level) that over time group borrowers work less for a wage, which would be in line with the increase in female self-employment. We do not find a significant impact on enterprise labor for these group borrowers though. In contrast, there is some evidence that households in individual-lending villages start to work more in enterprises over time, in line with the evidence on gradual (joint) enterprise creation. Despite these impacts we do not find any significant effect on overall household income (or on wage income and income from benefits separately).

[INSERT TABLE 7 HERE]

5.2.2 Did household well-being increase? The impact on consumption and asset ownership

In order to assess whether borrowers' increased engagement in entrepreneurial activities fed through to improving household well-being - a key objective of the program - we next estimate the effects of the program on household consumption. We use detailed information on consumption patterns elicited in the surveys, in which food consumption is measured over the past week (at a disaggregate level as well as overall), and non-durable and durable consumption over the past month and year, respectively.

Interestingly, we find robust evidence that access to group loans led to more and healthier food consumption, in particular of fresh items such as fruits, vegetables and dairy products (Table 8). With the exception of dairy these effects are not only due to increased home production: we also see treated clients purchasing more. The probability that a household consumed dairy products, fruits and vegetables, and non-alcoholic drinks in the last week was 5, 10 and 13 percentage points higher in group-lending than control villages. Total food consumption was 17 percentage

points higher. To put this into context, the average loan per borrower in group-lending villages is 300 USD and the average monthly pre-treatment food consumption in group-lending (and control) villages was 108 USD per household. So the estimated effect implies that over time food consumption increased by 19 USD more per household in group villages, i.e. 6.3 per cent of the loan amount. Over time we also see an increase in the use of combustibles and additional felt for ger isolation as well as other non-durable and total consumption. In line with Banerjee et al. (2010) we find a negative impact on the probability of smoking and the amount spent on cigarettes, a typical temptation good.

In contrast to households in group-lending villages, households in individual-lending villages do not experience much change in their consumption as a result of access to credit. We do not find any effects on aggregate consumption and expenditure variables - not even with increased exposure to treatment.

[INSERT TABLE 8 HERE]

Our evidence on consumption and business creation somewhat contrasts with recent evidence from other microcredit field experiments, such as Banerjee et al. (2010) in India and Augsburg et al. (2011) in Bosnia, who find that clients who start new businesses reduce consumption, at least in the short run and probably to be able to finance the new business. Our results could be explained by the fact that our follow-up survey is conducted 18 months after the start of the program and after several loan cycles. This would imply that the women who did start a new business might be already reaping the returns and the higher (permanent) income of such an activity.

We also consider whether asset ownership increased, and find evidence that overall asset wealth does increase over time in group-lending villages, but not in individual-lending villages - see Table 9. In particular, we detect a significant increase in the ownership of VCRs, radios, and large household appliances for both treatment types. At the end of the experiment the probability of owning a VCR or radio was 17 and 14 per cent higher in the group and individual-lending villages, respectively. For large household appliances the corresponding figures are 9 and 7 per cent.

[INSERT TABLE 9 HERE]

In unreported regressions we do not find a robust impact of access to either type of loan on the likelihood of owning the main dwelling or on the value of this house or ger. There is thus no evidence that loans encouraged borrowers to buy new property or invest in their existing main property. However, in columns (5) and (7) we do find some evidence that less-educated women in group-lending villages disinvest in second gers, land, and vehicles. This may indicate that less-educated women sold some of these assets in order to combine the proceeds with the loan amount and invest in small-scale businesses (see Section 5.2.1). In line with this interpretation, the results in column (17) show that these women are 30 per cent more likely to own tools at the end of the experiment, which closely matches the 29 per cent higher chance of operating a business (Table 6). Over time we document an increase in unsold stock and raw materials,

cattle, and riding equipment in group-lending villages, again in line with an expansion of business activity.[18]

We find fairly similar results for individual-lending villages: over time a reduction in second houses and an increase in the ownership of land and second gers. We also find an increase, relative to control villages, in the ownership of VCRs/radios, large household appliances and also of televisions (over time). Lastly, there was a gradual increase in the ownership of tools in the individual-lending villages, in line with the increase in the (general) business activity that we document for these villages in Table 6 and Figure 1.

5.2.3 Do the programs crowd out transfers?

The results just shown paint a different picture of the impact of the program in group and individual villages, with evidence that the group loans were relatively more effective at achieving their objectives. One interesting question is the extent to which interpersonal transfers are affected by the programs, and whether they are affected differently in group and individual villages: as in many developing countries, access to informal credit/transfers from friends and family is important in Mongolia, in particular for women (National Statistics Office, 2006). Kinship and social networks are confined to relatively small groups of people as they derive from the traditional *khot ail* support system in which a small number of nomadic households travelled, camped, and herded together for one or more seasons (Enkhamgalen, 1995). Within *khot ail* and similar social networks rural Mongolians often share income from entrepreneurial activities as well as pensions and other allowances.

Access to formal credit may have changed informal lending and transfer behaviour in two different ways. On the one hand, the increased availability of formal credit in treatment villages may have strengthened informal support networks as additional funds could be shared. On the other hand, informal networks may have weakened as borrowers substitute formal for informal credit, thereby crowding out insurance systems based on implicit reciprocal agreements.

The survey asked households about their informal - monetary and in-kind - transactions with friends and family during the past year and the most recent month. Although we do not find an overall ITT effect of either lending program on informal transfers, we document that over time group borrowers received less transfers both from friends and family members (Table 10). They were also less likely to make transfers to friends. Those that had been exposed to group loans for at least six months were 6 percentage points less likely to receive transfers from friends, 14 percentage points less likely to receive transfers from family, and 8 percentage points less likely to make transfers to friends.

[INSERT TABLE 10 HERE]

Interestingly, we find opposite effects in individual-lending villages. For individuals exposed to more loans and over a longer period of time, we detect an increase in the probability of making transfers to and receiving transfers from friends during the past year. We also find an increase

[18] We do not find a significant increase in the total number of animals as measured by the number of standardized Mongolian livestock units or bod (one horse, yak, or cattle equals one bod; one camel equals 1.4 bod; one sheep equals 1/6 bod; and one goat equals 1/7 bod).

in such transfers to and from family members over the past month. The relationship between the intensity of exposure to credit and the probability of receiving or giving transfers is shown in Figures 2a and 2b.

[INSERT FIGURES 2A AND 2B HERE]

These results may indicate that group borrowers partly substitute their informal networks with the formal network of the borrowing group. The associated discipline may make them less amenable to use part of their loans to help friends and family smooth consumption. In contrast, individual borrowers increase their informal financial transactions with friends and family, perhaps using part of their new loan to help others out.

Such an interpretation would be in line with recent evidence for Sri Lanka and Ghana by De Mel, McKenzie and Woodruff (2009) and Fafchamps, McKenzie, Quinn, and Woodruff (2011), respectively. The latter paper finds that women who received cash transfers did not increase their business profits as large portions of the cash grants ended up in household consumption and, to a lesser extent, transfers to others. Self-control problems, i.e. borrowers' inability to commit themselves to invest large parts of the cash grants into their enterprises and to resist the temptation to spend money on competing demands, including from friends and family, were a core explanation for the ineffectiveness of cash grants. Our results are also in line with Karlan and Zinman (2011) who find that individual-liability loans may increase access to informal credit from friends and family in the case of emergencies. Lastly, our finding that cigarette consumption increased far less in group-lending villages than in control villages, may reflect similar mechanisms. Just like group discipline can reduce the temptation to pass on part of the new loan to friends and family, it may also reduce spending on temptation goods (see also Banerjee and Mullainathan, 2010).

5.3 Repayment

In the preceding sections we documented a positive impact of access to group loans on consumption and business activities as well as some weaker effects of access to individual loans on business activity. In this section, we analyze the repayment behavior of both types of borrowers. Giné and Karlan (2010) also compare repayment rates between group and individual lending programs - both with mandatory weekly repayment meetings - and find no significant differences. In contrast, Carpena, Vole, Shapiro and Zia (2010) find that joint liability is associated with better loan repayment.

To construct our repayment data we use monthly reporting files that XacBank compiled on the basis of its administrative software. These files contain for each borrower the loan amount, interest rate, disbursement and due dates, loan purpose, collateral, overdue principal and interest, paid penalties as well as whether the client defaulted on the loan (defined as customers that were at least 90 days late in repaying one or more loan installments).

Table 11 presents probit regressions to explain the probability of loan default. The dependent variable is a dummy that indicates whether a borrower defaulted ('1') or not ('0'). The first two columns are based on a sample of first-time XacBank loans whereas the third and fourth columns are based on the full sample that includes repeat loans.

[INSERT TABLE 11 HERE]

We find, regardless of whether we control for borrower and loan characteristics, no difference between the probability of default in group-lending and individual-lending villages. This confirms the findings of Giné and Karlan (2010) although in our case *neither* loan program included mandatory repayment meetings whereas in their experiment *both* programs included such meetings.

The covariates in columns (2) and (4) give additional information on the borrower and loan characteristics that influence default probability. While the size of the loan does not influence the likelihood of repayment, there is a negative impact (at the 10 per cent significance level) of the amount of outstanding debt at the time of the baseline survey. Respondents with outstanding debt at baseline where thus *more* likely to (be able to) repay the subsequent XacBank loan. Borrowers that had already successfully passed the screening of another bank, where less risky compared with first-time borrowers.[19] In addition, column (4) indicates that repeat borrowers were significantly less risky, possibly because they had already successfully passed XacBank's own screening procedures and subsequently paid on time. For both first-time and repeat loans we also find that as loans mature (increasing number of months since disbursement) the risk of default increases, all else equal (see also Carpena et al., 2010).

Interestingly, a number of covariates are only of importance for first-time loans. Those that owned land or operated an enterprise at baseline were less risky borrowers as were the relatively highly educated. Ownership of a TV at baseline increased the risk of default, perhaps because this identifies women who use(d) debt for consumptive purposes. None of these variables is statistically significant at the 5 per cent level in the regression based on the whole loan sample (column 4). For repeat borrowers these variables are less important compared to the information that is contained in the variable that measures the number of successful previous loans with XacBank during the experiment.

Lastly, in unreported regressions we look at interaction effects between the liability structure and the number of previous loans of the borrower. We find no evidence for such a differentiated impact of repeat borrowing under the two programs. We also try other interaction terms but none of these is statistically significant, implying that there is no apparent heterogeneity between group and individual borrowers in terms of their repayment behavior.

6 Conclusions

We present results from a randomized field experiment in rural Mongolia where group-lending and individual-lending programs were randomly introduced across villages. The aim of the study was to measure and compare the effectiveness of these two types of microcredit in reducing poverty – a topic that still lacks unequivocal evidence, in particular for rural settings. While

[19] To the extent that multiple borrowing and overindebtedness were a problem in rural Mongolia this is not picked up by our default analysis. The fact that we do not find differences in repayments rates does not imply, however, that borrowers with initial debt did not experience any difficulties; it just shows that in the end they managed to repay as well as first-time borrowers. High repayment rates can point to successful projects with high returns but may also mask underlying problems where borrowers need to borrow from other sources or sell assets in order to be able to repay.

earlier papers have separately assessed the poverty impact of group lending (Banerjee et al., 2010) and individual lending (Karlan and Zinman, 2011) this is the first field experiment to compare both in the same (rural) setting.

Our findings on the poverty impact of different modes of microcredit are mixed. In line with previous studies, we document that participants in both programs used part of their loans to acquire assets – VCRs, radios, and large household appliances. A second finding that holds for both treatment programs is that women with lower education seem to benefit more from the intervention than women with higher education. We interpret the level of education as a proxy poverty measure, more reliable than a wealth indicator given that it is not affected by the program and is more stable over time. The results therefore suggest that it is the poorer part of the targeted population that benefits more from the microcredit intervention, independent of how it is being delivered.

For group loans we also find a positive impact on food consumption and entrepreneurship though not on current income. Enterprise profits increase over time as well. Among households that were offered group loans the likelihood of owning an enterprise increases by ten percentage points more than in control villages (and even close to 30 percentage points for less-educated women).

Our findings for individual lending are weaker. We find no significant increase in consumption or income although over time there is an increase in the probability that women operate a business jointly with their spouse. Over time these joint enterprises, which engage in different types of activities compared with the female-operated enterprises in group-lending villages, also become more profitable. More generally, we find that effects observed for group borrowers are also experienced by women in individual-lending villages if they are exposed to credit for longer periods of time. For example, their likelihood of starting a business is higher the longer they have access to loans. Nevertheless, it is not clear whether these longer-term effects will translate in the same way as they do for group clients. For instance, we find no evidence that food consumption goes up with exposure in individual-lending villages.

Importantly, we find no difference in repayment rates between the two lending programs, both of which did not include weekly repayment meetings. This casts doubt on the hypothesis that microcredit repayment rates are high mainly due to the effect of weekly group meetings. Our results indicate that, at least in our context, even without such regular meetings group and individual microcredit can have similar and high repayment rates (also note that both our loan products required some form of collateral).

There is at this stage no evidence on changes in income as a result of either of the programs, though it may be too early for such effects to be observed. The more sustained and more generalized increase in consumption (of both non-durable consumption and the service of durable items) in group-lending villages seems to indicate that these loans are more effective at increasing the permanent income of households. It would be interesting to test this hypothesis further by considering long-run income levels.[20]

If one were to take at face value the evidence on the stronger impact of group loans, one would

[20]There might also be a measurement issue. In developing countries income is notoriously harder to measure than consumption and might be more affected by measurement error, therefore making the detection of relatively small impacts harder.

want to ask *why* such loans are more effective at raising consumption (and probably long-term income) than individual loans. One possibility is that the joint-liability scheme better ensures discipline so that larger long-run effects can be achieved.[21] Group discipline may not only prevent the selection of overly risky investment projects, it may also ensure that a substantial part of the loans is actually invested in the first place (instead of used for consumption or transfers to others). Our results on informal transfers can be interpreted to support this hypothesis: we find that women in group-lending villages decrease their transfer activities with families and friends, opposite to what we find in individual-lending villages. This could reflect that groups replace some of their informal financial networks but further analysis is needed to explore this. Such an analysis would also be important to assess the welfare impact of access to group loans for the borrowers as well as their friends and families. Increased within-group financial discipline may come at the cost of disrupting informal credit and insurance systems based on kinship and other social ties.

Lastly, to some extent our weaker results for individual loans may also reflect that borrowing at baseline was somewhat higher in individual-lending villages compared with group-lending villages. Moreover, since group-lending was an innovative way of lending in the Mongolian context, the unmet demand for such a product - and consequently its marginal impact - may have been higher. Loan take-up was indeed higher in group-lending villages. This could indicate that some women, in particular the less-educated, had not been comfortable with borrowing on an individual basis but *were* willing to borrow within the framework of a group. This would imply that group and individual lending are complementary financial services for which the demand may differ across borrower types. The continuing process of liability individualization by MFIs may therefore run the risk that certain borrowers, those that are not able or willing to borrow and invest on their own, may gradually lose access to formal financial services.

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[21] The savings requirement of the group product may also have helped to select disciplined borrowers.

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Table 1. Randomization and treatment-control balance

This table provides t-test results for means comparisons of household and village characteristics in individual-lending versus control villages and in group-lending versus control villages. P-values are reported between brackets. ***, **, * denote significance at the 0.01, 0.05 and 0.10-level. In case of household characteristics, the standard errors are clustered at the village level. Table A1 provides the definitions and sources of all variables. *N* indicates the number of villages (Panel A) or respondents (Panel B and C) for whom information about a given variable is available. *Conditional N* indicates the number of respondents for whom the value of the respective variable is strictly positive in the case of conditional variables. E.g. 1,148 women answered the survey question about wage earnings and 266 of them reported positive wage earnings.

Panel A. Village and district characteristics

	People in village	People in district	District area	Livestock in district	Banks in district	SCCs in district	Distance to paved road	Time to paved road	Distance to province center	Time to province center	Price		
											Milk	Mutton	Bread
Control	1,017	3,530	2,823	128,747	1.7	0.6	185	220	113	218	628	2,967	1,035
Treatment	1,136	3,961	3,415	167,728	2.2	0.7	165	272	117	200	797	2,833	790
P-value	(0.35)	(0.63)	(0.24)	(0.08)*	(0.13)	(0.55)	(0.73)	(0.64)	(0.82)	(0.7)	(0.19)	(0.53)	(0.25)
N	40	40	40	40	40	40	29	24	39	36	39	33	39

Panel B. Household characteristics: general, consumption, assets

	Children <16	Age respondent	Education respondent	HH death	Received transfers	Given transfers	Self-employed	Wage earnings	Value of dwelling	Consumption			
										Milk	Red meat	Vegetables	Fuel
Control	1.5	40.4	9.3	6.0	155	241	32.4	29.4	1.43	3.4	5.4	2.2	22.8
Individual	1.6	38.9	9.4	6.4	174	153	33.4	31.8	1.52	4.0	5.2	2.0	18.9
P-value	(0.65)	(0.16)	(0.66)	(0.84)	(0.73)	(0.17)	(0.78)	(0.39)	(0.71)	(0.32)	(0.78)	(0.57)	(0.42)
Group	1.6	39.7	9.6	5.1	196	158	33.5	30.1	1.57	3.2	5	2.0	23.3
P-value	(0.82)	(0.48)	(0.38)	(0.58)	(0.73)	(0.21)	(0.76)	(0.79)	(0.55)	(0.86)	(0.54)	(0.45)	(0.93)
N	1,148	1,147	1,143	1,147	1,147	1,147	1,148	1,148	1,147	1,146	1,139	1,143	1,055
Conditional N					103	174		266					

Panel C. Household characteristics: entrepreneurship and borrowing

	Operates business	Female business	Hours hired	At least one loan	Outstanding loans	Debt value	Debt/HH income	Debt service	Interest rate	Secured loans	Percentage private use	Percentage female business	Amount female business
Control	58.9	64.8	40.9	56	2.6	1.7	0.9	31.7	2.2%	73%	72%	15%	158
Individual	59.8	62.6	54.1	67	2.7	2.0	0.9	45.1	2.1%	77%	74%	11%	140
P-value	(0.88)	(0.71)	(0.40)	(0.00)***	(0.48)	(0.44)	(0.24)	(0.07)*	(0.43)	(0.44)	(0.73)	(0.13)	(0.71)
Group	60.3	59.3	35.1	62	3.0	1.9	1.1	40.8	2.3%	73%	79%	10%	140
P-value	(0.80)	(0.31)	(0.74)	(0.13)	(0.25)*	(0.53)	(0.27)	(0.29)	(0.53)	(0.95)	(0.13)	(0.07)*	(0.71)
N	1,148	1,148	1,148	1,148	1,148	1,148	1,148	1,148	1,148	1,148	1,148	1,148	1,148
Conditional N		686			591	584	553	518	553	615	614	714	714

Table 2. The loan products

This table describes the main characteristics of the individual and the group loan products. Average loan size is calculated conditional on having a loan. Average loan size of group loans refers to loans per borrower not per group. Loans were disbursed in tögrög not US$. Source of data on maturities and loan size: XacBank.

	Individual loans	**Group loans**
Progressive?	Yes: larger loans, lower interest rate, and longer maturity after each successfully repaid loan	
Monthly interest rate	1.5% to 2%	
Grace period	One or two months depending on loan maturity	
Repayment frequency	Monthly, no public repayment meetings. In case of group loans, the group leader collects and hands over repayments to the loan officer	
Liability structure	Individual	Joint
Collateral	Yes but flexible approach	Joint savings (20% of loan) sometimes supplemented by assets
Available maturity	2 to 24 months	3 to 12 months
Average maturity 1st loan	224 days	199 days
Average maturity 2nd loan	234 days	243 days
Average size 1st loan	US$ 411	US$ 279
Average size 2nd loan	US$ 472	US$ 386

Table 3. Loan take-up

This table presents probit regressions to explain the probability of loan take-up in the individual and group lending villages. Standard errors are reported in brackets. ***, **, * denote significance at the 0.01, 0.05 and 0.10-level. Table A1 provides the definitions and sources of all variables.

	All villages		**Group villages**		**Individual villages**	
	(1)	(2)	(3)	(4)	(5)	(6)
Group village	0.120*	0.120*				
	(0.0692)	(0.0638)				
Outstanding loans	-0.00414	-0.00207	-0.0525	-0.00377	0.0457	0.0349
	(0.0296)	(0.0285)	(0.0377)	(0.0393)	(0.0386)	(0.0407)
Prior loans	-0.00566	-0.00899	-0.00760	-0.0130**	-0.00335	-0.00488
	(0.00738)	(0.00777)	(0.00650)	(0.00569)	(0.0155)	(0.0164)
Highly educated	0.0435	0.0309	-0.0526	-0.0774	0.111*	0.110*
	(0.0577)	(0.0559)	(0.0982)	(0.0948)	(0.0608)	(0.0637)
Owns dwelling	0.0778	0.0887	0.0961	0.131	0.0431	0.0565
	(0.0730)	(0.0743)	(0.137)	(0.149)	(0.0792)	(0.0854)
Owns fence	0.0946**	0.0690	0.195***	0.0968*	0.00530	0.0249
	(0.0458)	(0.0424)	(0.0649)	(0.0543)	(0.0521)	(0.0504)
Owns well	0.142***	0.109**	0.109	0.145**	0.163***	0.0711
	(0.0547)	(0.0535)	(0.0829)	(0.0712)	(0.0505)	(0.0627)
Owns vehicle	-0.00679	-0.0234	0.00294	-0.00606	-0.00793	-0.0371
	(0.0419)	(0.0401)	(0.0602)	(0.0530)	(0.0576)	(0.0574)
Owns tools/machinery	0.0793*	0.128***	0.0268	0.117**	0.124**	0.148***
	(0.0405)	(0.0344)	(0.0522)	(0.0464)	(0.0528)	(0.0455)
Owns animals	0.00364	-0.0193	-0.0250	-0.0746*	0.0273	0.0366
	(0.0415)	(0.0408)	(0.0354)	(0.0393)	(0.0741)	(0.0707)
HH death	-0.0223	-0.0307	-0.153	-0.141	0.0716	0.0625
	(0.0789)	(0.0816)	(0.110)	(0.115)	(0.105)	(0.110)
Province fixed effects?	No	Yes	No	Yes	No	Yes
Observations	830	830	397	397	433	433
Pseudo R-squared	0.03	0.06	0.04	0.13	0.03	0.06

Table 4. Attrition

This table presents probit regressions to explain the probability of non-participation in the follow-up survey. P-values are reported in brackets. ***, **, * denote significance at the 0.01, 0.05 and 0.10-level. Table A1 provides the definitions and sources of all variables.

	(1)	(2)	(3)	(4)
Individual village	0.0696	0.0663*	0.0688**	0.0640*
	(0.106)	(0.0969)	(0.0392)	(0.0570)
Group village	0.0155	0.0145	0.0325	0.0322
	(0.726)	(0.708)	(0.388)	(0.356)
Highly educated			0.0253	0.0223
			(0.467)	(0.517)
Male adults in HH			0.0190	0.0203
			(0.142)	(0.117)
Female adults in HH			-0.0255**	-0.0250**
			(0.0158)	(0.0181)
Children < 16			-0.0193*	-0.0173
			(0.0628)	(0.104)
Age respondent			-0.00333**	-0.00337**
			(0.0174)	(0.0138)
Distance to province center			0.000390*	0.0004**
			(0.0647)	(0.0411)
Owns dwelling			0.0263	0.0254
			(0.145)	(0.161)
Owns fence			-0.0813***	-0.0761***
			(0.000)	(0.000)
Owns other property			-0.0339	-0.0342
			(0.189)	(0.173)
Ownes well			-0.0801**	-0.0823**
			(0.0235)	(0.0283)
Owns cattle			-0.0210	-0.0151
			(0.444)	(0.607)
Owns horses or camels			0.0634***	0.0649***
			-0.003	(0.003)
Owns other animals			-0.0184	-0.0220
			(0.399)	(0.323)
HH death			0.110**	0.111**
			(0.0401)	(0.0384)
Province fixed effects?	No	Yes	No	Yes
Observations	1,115	1,115	1,115	1,115
Pseudo R-squared	0.01	0.01	0.07	0.07

Table 5. Loan use

This table presents an overview of how borrowers used their loans. Borrowers could state more than one type of loan use. Source: Follow-up survey.

	Percentage of borrowers that used part of the loan for this purpose		**Percentage of loan amount when used for this purpose**	
	1st group loan	***2nd group loan***	***1st group loan***	***2nd group loan***
Other business expenses	0.57	0.37	0.89	0.78
Other household expenses	0.28	0.22	0.73	0.56
Mixed expenses	0.14	0.17	0.60	0.60
Education	0.06	0.06	0.74	0.54
Purchase tools/machinery	0.06	0.01	0.87	100
Purchase livestock	0.04	0.05	0.60	0.69
	1st individual loan	**2nd individual loan**	**1st individual loan**	**2nd individual loan**
Other business expenses	0.51	0.47	0.82	0.83
Other household expenses	0.28	0.19	0.70	0.68
Mixed expenses	0.12	0.08	0.71	0.75
Education	0.08	0.07	0.65	0.53
Purchase tools/machinery	0.06	0.03	0.73	100
Purchase livestock	0.09	0.02	0.73	0.45

Table 6. Impact on business creation and growth

This table presents the results of difference-in-differences ITT regressions to measure the impact of group (G) and individual (I) loans on business creation and growth. *Base effect* refers to the basic difference between the treatment and the control villages. *High education* refers to an interaction term between a dummy for highly educated women and the base effect. *Intensity: Months* refers to an interaction term between intensity measure *Months* and the base effect. *Intensity: Number* refers to an interaction term between intensity measure *Number* and the base effect. Regressions also include a standard set of unreported pre-treatment covariates (see Table A1). The standard errors are clustered by village and reported in brackets. ***, **, * denote significance at the 0.01, 0.05 and 0.10-level. Table A1 provides the definitions and sources of all variables.

		Probability of any type of business		**Probability of female business**		**Profit of any businesses combined**		**Profit of female business**	
		G	**I**	**G**	**I**	**G**	**I**	**G**	**I**
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
I.	Base effect	0.080	-0.028	0.105*	-0.018	-2,125	-8,169	-2,125	-24,569
		(0.055)	(0.061)	(0.063)	(0.060)	(118,787)	(89,233)	(118,787)	(40,061)
II.	Base effect	0.284***	-0.001	0.289**	-0.105	-277,351*	-110,834	-88,405	-21,485
		(0.090)	(0.123)	(0.141)	(0.137)	(161,751)	(98,292)	(80,372)	(61,399)
	High education	-0.277**	-0.031	-0.186*	0.106	316,773	122,015	80,882	-2,933
		(0.124)	(0.126)	(0.110)	(0.143)	(221,398)	(129,769)	(113,427)	(89,685)
III.	Base effect	0.079	-0.029	0.103	-0.019	-7,658	-10,137	-20,514	-25,505
		(0.055)	(0.061)	(0.063)	(0.059)	(118,932)	(89,197)	(55,142)	(40,222)
	Intensity: Months	0.007	0.021**	0.014**	0.017	41,503**	26,255***	25,894***	10,428***
		(0.007)	(0.010)	(0.006)	(0.012)	(15,874)	(9,629)	(7,740)	(3,539)
IV.	Base effect	0.008	-0.028	0.103	-0.019	-6,018	-10,028	-19,855	-25,325
		(0.056)	(0.061)	(0.063)	(0.059)	(118,719)	(89,031)	(55,095)	(40,130)
	Intensity: Number	0.005	0.102	0.058*	0.010	201,679**	136,893*	135,560***	24,564
		(0.047)	(0.103)	(0.033)	(0.126)	(81,670)	(75,678)	(38,970)	(46,477)
	Observations	2,055	2,055	2,055	2,055	2,052	2,052	2,054	2,054

Figure 1. Treatment intensity and business creation

This figure shows the probability of enterprise ownership by an average respondent in the individual lending villages (left-hand side) and group-lending villages (right-hand side) as a function of the number of months respondents in a village borrowed on average from XacBank. The top two graphs show the probability of female-owned businesses whereas the two graphs at the bottom show the probability that the average household operates any type of business (operated by the respondent, her spouse, or jointly). The blue lines indicate the expected probability while the white lines indicate a 95 per cent confidence interval.

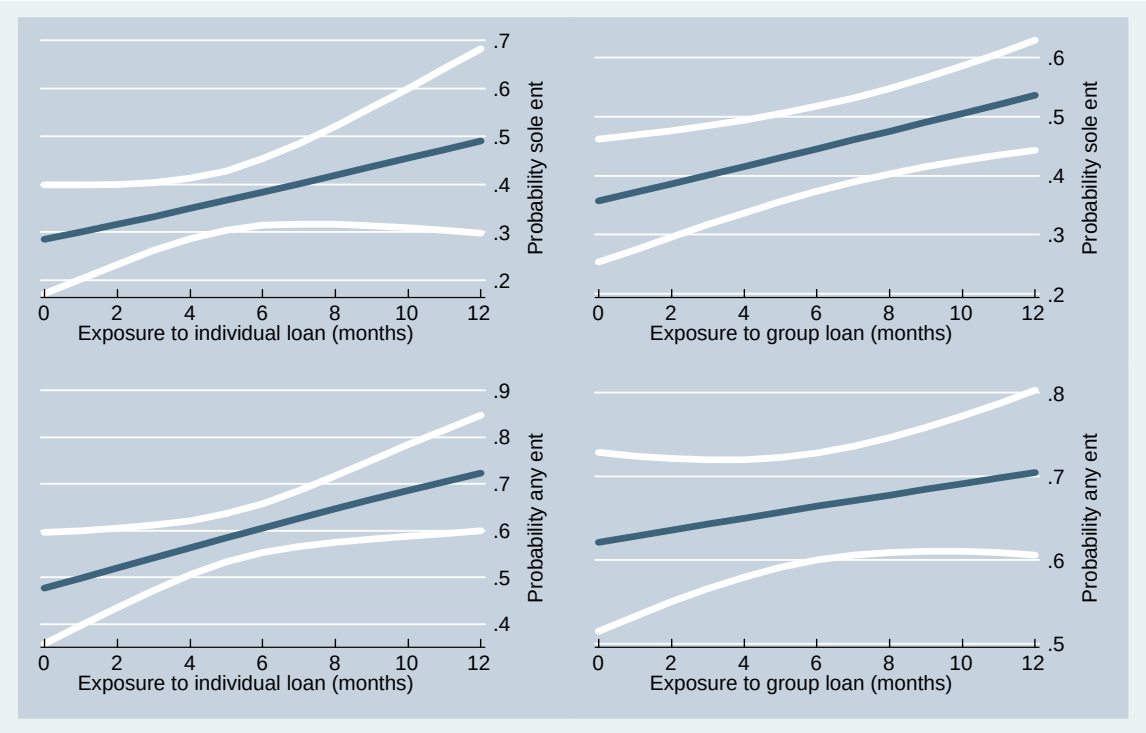

Table 7. Impact on labour supply and income

This table presents the results of difference-in-differences ITT regressions to measure the impact of group (G) and individual (I) loans on labour supply and income. *Base effect* refers to the basic difference between the treatment and the control villages. *High education* refers to an interaction term between a dummy for highly educated women and the base effect. *Intensity: Months* refers to an interaction term between intensity measure *Months* and the base effect. *Intensity: Number* refers to an interaction term between intensity measure *Number* and the base effect. Regressions also include a standard set of unreported pre-treatment covariates (see Table A1). The standard errors are clustered by village and reported in brackets. ***, **, * denote significance at the 0.01, 0.05 and 0.10-level. Table A1 provides the definitions and sources of all variables.

	Hours of wage labour by HH in average week		**Hours of enterprise labour by HH in average week**		**Total household income**	
	G	**I**	**G**	**I**	**G**	**I**
	(1)	(2)	(3)	(4)	(5)	(6)
I. Base effect	-4.914	8.409	6.135	-8.472	-110,788	-131,659
	(9.775)	(10.03)	(12.98)	(13.99)	(204,082)	(209,531)
II. Base effect	-45.090	0.037	21.23	-24.68	-224,480	91,786
	(28.950)	(25.24)	(37.24)	(33.18)	(224,003)	(229,403)
High education	44.180	9.591	-16.80	18.83	146,491	-252,523
	(27.360)	(26.25)	(37.55)	(32.99)	(288,917)	(307,018)
III. Base effect	-4.402	8.416	5.949	-8.495	-115,802	-133,925
	(9.717)	(10.04)	(12.99)	(13.94)	(203,265)	(210,005)
Intensity: Months	-2.166*	-0.019	1.207	5.708***	45,995	24,518
	(1.217)	(3.278)	(1.626)	(1.580)	(33,618)	(33,512)
IV. Base effect	-4.637	8.406	6.266	-8.463	-111,418	-134,153
	(9.706)	(10.01)	(13.05)	(13.96)	(203,382)	(209,871)
Intensity: Number	-7.353	8.605	-2.213	38.18**	187,612	186,060
	(6.864)	(29.83)	(12.17)	(16.40)	(197,646)	(265,296)
Observations	2,055	2,055	2,055	2,055	2,007	2,007

Table 8. Impact on consumption

	Total (log)		Food (log)		Non-durable (log)		Durable (log)		Dairy				Fruit and vegetables			
									Probit	*Tobit*	*Probit*	*Tobit*	*Probit*	*Tobit*	*Probit*	*Tobit*
	G	I	G	I	G	I	G	I	G	G	I	I	G	G	I	I
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
I. Base effect	0.116	0.0347	0.173**	0.0183	0.0113	-0.00283	-0.0974	-0.0570	0.047**	22,031	0.0474***	-1,235	0.0960*	1,112*	0.0764	803.0
	(0.0805)	(0.0759)	(0.0712)	(0.0668)	(0.157)	(0.144)	(0.118)	(0.121)	(0.0189)	(18,544)	(0.0170)	(2,532)	(0.0570)	(634.8)	(0.0545)	(497.5)
II. Base effect	0.276	0.230	0.444*	0.367*	0.119	-0.137	-0.550*	-0.385	0.0603	28,877	0.0829***	10,932	0.142	1,192	0.132	1,276
	(0.238)	(0.204)	(0.220)	(0.204)	(0.393)	(0.396)	(0.326)	(0.246)	(0.0414)	(20,562)	(0.0288)	(9,215)	(0.101)	(1,156)	(0.0952)	(875.3)
High education	-0.185	-0.227	-0.317	-0.407*	-0.116	0.156	0.530	0.389	-0.0336	-7,922	-0.101	-14,020	-0.0838	-84.42	-0.0873	-541.0
	(0.272)	(0.246)	(0.239)	(0.229)	(0.425)	(0.418)	(0.332)	(0.235)	(0.0973)	(13,378)	(0.109)	(10,913)	(0.160)	(1,084)	(0.149)	(908.2)
III. Base effect	0.110	0.0339	0.166**	0.0163	0.00297	-0.00253	-0.102	-0.0571	0.0462**	21,295	0.0473***	-1,361	0.0975*	1,100*	0.0779	801.8
	(0.0800)	(0.0759)	(0.0703)	(0.0667)	(0.158)	(0.144)	(0.119)	(0.121)	(0.0184)	(18,263)	(0.0158)	(2,508)	(0.0565)	(632.6)	(0.0542)	(497.4)
Intensity: Months	0.049***	-0.00146	0.055***	0.0193	0.037**	-0.0184	0.035	-0.0114	0.0145***	7,110	-0.0160	-74.49	-0.0108	62.43	0.0227**	108.6
	(0.0128)	(0.0180)	(0.0160)	(0.0173)	(0.0174)	(0.0255)	(0.0225)	(0.0335)	(0.00475)	(4,535)	(0.0146)	(1,518)	(0.00881)	(53.30)	(0.0113)	(105.8)
IV. Base effect	0.111	0.0335	0.166**	0.0163	0.0075	-0.00287	-0.0992	-0.0569	0.0472**	21,137	0.0471***	-1,528	0.0966*	1,102*	0.0784	801.5
	(0.0802)	(0.0762)	(0.0707)	(0.0671)	(0.158)	(0.144)	(0.119)	(0.121)	(0.0183)	(18,353)	(0.0155)	(2,562)	(0.0568)	(633.5)	(0.0541)	(496.5)
Intensity: Number	0.272***	0.00143	0.359***	0.0581	0.123	-0.0816	0.0910	-0.0649	0.0790***	56,965*	-0.147	1,420	-0.0362	330.8	0.176*	1,061
	(0.0689)	(0.160)	(0.0907)	(0.194)	(0.102)	(0.186)	(0.141)	(0.233)	(0.0206)	(31,544)	(0.115)	(15,570)	(0.0419)	(311.9)	(0.0944)	(726.6)
Observations	2,055	2,055	2,050	2,050	1,993	1,993	2,048	2,048	2,034	2,034	2,034	2,034	2,034	2,034	2,034	2,034

	Non-alcoholic drinks				Combustibles				Cigarettes				Felt for ger	
	Probit	*Tobit*	*Probit*	*Tobit*	*Probit*	*Tobit*	*Probit*	*Tobit*	*Probit*	*Tobit*	*Probit*	*Tobit*	*Probit*	*Probit*
	G	G	I	I	G	G	I	I	G	G	I	I	G	I
	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)	(27)	(28)	(29)	(31)
I. Base effect	0.125**	1,426**	0.0700	786.6	0.0221*	-264.1	0.00442	6,015	-0.0681*	-2,644**	-0.0630	-943.5	-0.00483	-0.00452
	(0.0583)	(557.3)	(0.0604)	(555.0)	(0.0115)	(6,867)	(0.0224)	(7,474)	(0.0348)	(1,043)	(0.0440)	(957.0)	(0.0100)	(0.00903)
II. Base effect	-0.0196	-272.2	0.0844	995.2	-0.554**	11,140	0.000597	3,182	-0.0635	-3,685*	-0.0474	-658.3	0.972***	0.966***
	(0.178)	(1,885)	(0.173)	(1,562)	(0.256)	(26,035)	(0.00362)	(26,611)	(0.0963)	(1,927)	(0.110)	(2,496)	(0.0472)	(0.0271)
High education	0.163	1,867	-0.0146	-246.9	0.0222	-13,059	-0.00582	3,420	-0.00969	1,164	-0.0204	-270.9	-0.0327***	-0.0331***
	(0.230)	(2,094)	(0.188)	(1,849)	(0.0912)	(26,517)	(0.0281)	(26,692)	(0.105)	(2,150)	(0.106)	(2,580)	(0.0101)	(0.00656)
III. Base effect	0.122**	1,393**	0.0704	788.6	0.0159*	-848.3	0.00346	5,961	-0.0678*	-2,629**	-0.0621	-902.2	-0.00364	-0.00400
	(0.0580)	(560.1)	(0.0604)	(554.7)	(0.00913)	(6,839)	(0.0179)	(7,450)	(0.0350)	(1,059)	(0.0436)	(960.0)	(0.00934)	(0.00750)
Intensity: Months	0.00839	129.2*	-0.0114	-79.40	0.00728**	2,735***	-0.00120	-944.3	-0.00528	-270.0	-0.0125	-337.7	0.00337***	0.00155
	(0.00752)	(70.16)	(0.00890)	(117.5)	(0.00321)	(1,003)	(0.00170)	(1,074)	(0.00688)	(290.2)	(0.0197)	(492.2)	(0.000812)	(0.00141)
IV. Base effect	0.123**	1,397**	0.0708	787.9	0.0191*	-574.0	0.00385	5,966	-0.0677*	-2,636**	-0.0625	-917.6	-0.00361	-0.00412
	(0.0581)	(560.0)	(0.0604)	(555.0)	(0.0105)	(6,839)	(0.0200)	(7,456)	(0.0348)	(1,051)	(0.0436)	(964.8)	(0.00966)	(0.00783)
Intensity: Number	0.0363	588.8	-0.0991	-643.2	0.0282**	10,244**	-0.00990	-3,635	-0.0265	-1,163	-0.0412	-1,523	0.0166***	-0.00238
	(0.0399)	(389.9)	(0.0628)	(882.8)	(0.0143)	(5,029)	(0.0162)	(8,240)	(0.0339)	(1,425)	(0.174)	(4,238)	(0.00437)	(0.0128)
Observations	2,034	2,034	2,034	2,034	2,034	2,034	2,034	2,034	2,034	2,034	2,034	2,034	2,034	2,034

Table 9. Impact on asset ownership

This table presents the results of difference-in-differences ITT regressions to measure the impact of group (G) and individual (I) loans on asset ownership. *Base effect* refers to the basic difference between the treatment and the control villages. *High education* refers to an interaction term between a dummy for highly educated women and the base effect. *Intensity: Months* refers to an interaction term between intensity measure *Months* and the base effect. *Intensity: Number* refers to an interaction term between intensity measure *Number* and the base effect. Bod are standardized Mongolian livestock units. One horse, yak, or cattle equals one bod; one camel equals 1.4 bod; one sheep equals 1/6 bod; and one goat equals 1/7 bod. Regressions also include a standard set of unreported pre-treatment covariates (see Table A1). The standard errors are clustered by village and reported in brackets. ***, **, * denote significance at the 0.01, 0.05 and 0.10-level. Table A1 provides the definitions and sources of all variables.

	Value of all assets (incl. main dwelling)		Probability 2nd house		Probability 2nd ger		Probability land/well		Probability vehicle		Probability VCR or radio		Probability television	
	G	I	G	I	G	I	G	I	G	I	G	I	G	I
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
I. Base effect	-129,482	-325,163	0.001	0.071	0.009	0.064	-0.017	-0.105	0.062	0.018	0.172***	0.137**	-0.022	-0.001
	(527,000)	(542,918)	(0.072)	(0.071)	(0.05)	(0.042)	(0.125)	(0.113)	(0.050)	(0.046)	(0.054)	(0.054)	(0.022)	(0.014)
II. Base effect	-1,148,000	-905,922	-0.057	0.148	-2.08***	0.072	-0.335***	-0.124	-0.297***	-0.237***	0.169	0.192*	-0.005	-0.010
	(1,188,000)	(831,094)	(0.134)	(0.115)	(0.0611)	(0.122)	(0.113)	(0.151)	(0.065)	(0.083)	(0.143)	(0.107)	(0.036)	(0.037)
High education	922,123	357,832	0.069	-0.080	0.406**	-0.006	0.307**	0.023	0.516***	0.360**	0.004	-0.062	-0.012	0.012
	(1,367,000)	(1,019,000)	(0.142)	(0.105)	(0.178)	(0.106)	(0.131)	(0.157)	(0.118)	(0.146)	(0.161)	(0.140)	(0.054)	(0.041)
III. Base effect	-164,484	-331,615	0.005	0.072	0.008	0.062	-0.120	-0.110	0.0613	0.017	0.171***	0.136**	-0.020	-0.001
	(520,573)	(539,958)	(0.074)	(0.072)	(0.035)	(0.042)	(0.124)	(0.113)	(0.05)	(0.046)	(0.054)	(0.055)	(0.021)	(0.014)
Intensity: Months	264,751**	31,276	-0.03***	-0.03***	0.02***	0.022**	0.02***	0.045***	0.008	0.006	0.004	0.024*	-0.003	0.011***
	(103,886)	(202,940)	(0.007)	(0.010)	(0.005)	(0.010)	(0.007)	(0.0170)	(0.012)	(0.023)	(0.015)	(0.014)	(0.003)	(0.003)
IV. Base effect	-147,759	-335,491	0.004	0.072	0.07	0.063	-0.118	-0.111	0.062	0.018	0.172***	0.135***	-0.021	-0.001
	(522,313)	(540,709)	(0.073)	(0.072)	(0.035)	(0.042)	(0.124)	(0.113)	(0.05)	(0.046)	(0.054)	(0.055)	(0.021)	(0.014)
Intensity: Number	987,927*	880,953	-0.15***	-0.185**	0.081**	0.047	0.087**	0.399***	-0.03	0.043	0.010	0.173*	-0.070	0.098***
	(574,456)	(1,440,000)	(0.036)	(0.088)	(0.032)	(0.101)	(0.042)	(0.112)	(0.06)	(0.171)	(0.088)	(0.094)	(0.014)	(0.026)
Observations	2,055	2,055	2,055	2,055	2,055	2,055	2,055	2,055	2,055	2,055	2,055	2,055	2,055	2,055

	Probability large household appliances		Probability tools		Probability unsold stock and raw materials		Probability riding equipment		Number of cattle		Number of animals (in bod)	
	G	I	G	I	G	I	G	I	G	I	G	I
	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)
I. Base effect	0.085**	0.070*	0.060	0.161	0.011	-0.090	0.039	0.017	-0.601	-1.884*	-1.330	0.956
	(0.036)	(0.041)	(0.113)	(0.109)	(0.043)	(0.046)	(0.044)	(0.044)	(1.255)	(1.083)	(1.126)	(1.362)
II. Base effect	-0.048	-0.180	0.306**	0.366***	0.037	-0.037	-0.106	-0.131*	-3.356	-3.827	0.420	1.234
	(0.139)	(0.126)	(0.132)	(0.135)	(0.178)	(0.153)	(0.116)	(0.074)	(2.467)	(2.509)	(2.085)	(2.170)
High education	0.147	0.258**	-0.313*	-0.290*	-0.027	0.021	0.166	0.174**	3.135	2.237	-2.542	-0.410
	(0.131)	(0.105)	(0.160)	(0.158)	(0.159)	(0.170)	(0.127)	(0.083)	(2.621)	(2.644)	(2.400)	(2.264)
III. Base effect	0.084**	0.070*	0.059	0.161	0.010	-0.020	0.034	0.016	-0.822	-1.876*	-1.330	0.956
	(0.037)	(0.041)	(0.112)	(0.109)	(0.043)	(0.046)	(0.044)	(0.043)	(1.264)	(1.067)	(1.127)	(1.362)
Intensity: Months	0.013	0.020	-0.01	0.027**	0.012**	0.014	0.036***	0.014	1.268***	0.127	0.139	0.651
	(0.014)	(0.019)	(0.01)	(0.012)	(0.005)	(0.012)	(0.005)	(0.010)	(0.262)	(0.067)	(0.685)	(0.711)
IV. Base effect	0.084**	0.069*	0.058	0.161	0.010	-0.019	0.036	0.015	-0.777	-1.871*	-1.330	0.956
	(0.036)	(0.041)	(0.113)	(0.109)	(0.04)	(0.046)	(0044)	(0.043)	(1.256)	(1.064)	(1.127)	(1.362)
Intensity: Number	0.027	0.210	-0.078	0.207*	0.064**	0.120	0.143***	0.151*	6.047***	0.233	4.952	2.393
	(0.073)	(0.146)	(0.050)	(0.111)	(0.029)	(0.103)	(0.041)	(0.089)	(1.746)	(4.787)	(6.422)	(3.529)
Observations	2,055	2,055	2,053	2,053	2,055	2,055	2,055	2,055	2,051	2,051	1,874	1,874

Table 10. Impact on informal transfers

This table presents the results of difference-in-differences ITT regressions to measure the impact of group (G) and individual (I) loans on informal transfers to and from family and friends. *Base effect* refers to the basic difference between the treatment and the control villages. *High education* refers to an interaction term between a dummy for highly educated women and the base effect. *Intensity: Months* refers to an interaction term between intensity measure *Months* and the base effect. *Intensity: Number* refers to an interaction term between intensity measure *Number* and the base effect. Regressions also include a standard set of unreported pre-treatment covariates (see Table A1). Standard errors are clustered by village and reported in brackets. ***, **, * denote significance at the 0.01, 0.05 and 0.10-level. Table A1 provides the definitions and sources of all variables.

	Probability of *receiving* transfers from *friends* during the last *year*		**Probability of *making* transfers to *friends* during the last *year***		**Probability of *receiving* transfers from *family* during the last *year***		**Probability of *making* transfers to *family* during the last *year***		**Probability of *receiving* transfers from *family* during the last *month***		**Probability of *making* transfers to *family* during the last *month***	
	G	**I**	**G**	**I**	**G**	**I**	**G**	**I**	**G**	**I**	**G**	**I**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
I. Base effect	0.0454	-0.00322	-0.0216	0.0210	-0.0201	0.0389	-0.00984	0.115	-0.0364	-0.000815	-0.0367	0.0244
	(0.0499)	(0.0369)	(0.0526)	(0.0566)	(0.0644)	(0.0594)	(0.0599)	(0.0704)	(0.0447)	(0.0451)	(0.0444)	(0.0624)
II. Base effect	0.0537	-0.0369	-0.0683	-0.0698	0.144	-0.0973	0.190	0.184	0.138	-0.0746	0.0269	0.0487
	(0.0944)	(0.0553)	(0.0784)	(0.0549)	(0.130)	(0.0930)	(0.130)	(0.170)	(0.154)	(0.0664)	(0.218)	(0.195)
High education	-0.00794	0.0462	0.0710	0.134	-0.155	0.174*	-0.213	-0.0832	-0.125**	0.109	-0.0593	-0.0266
	(0.0715)	(0.0895)	(0.0996)	(0.106)	(0.101)	(0.0943)	(0.132)	(0.160)	(0.0605)	(0.111)	(0.198)	(0.166)
III. Base effect	0.0491	-0.00222	-0.0194	0.0213	-0.0133	0.0389	-0.00680	0.115	-0.0329	-0.000895	-0.0348	0.0241
	-0.0509	-0.0366	-0.0527	-0.0561	(0.0647)	(0.0592)	(0.0594)	(0.0706)	(0.0447)	(0.0445)	(0.0443)	(0.0623)
Intensity: Months	-0.0102***	0.00706	-0.0155***	0.0146**	-0.0256***	0.00866	-0.0156	0.0140	-0.0126***	0.0141***	-0.00854*	0.0264***
	-0.00253	-0.00433	-0.0059	-0.00736	(0.00683)	(0.00762)	(0.0119)	(0.0112)	(0.00399)	(0.00486)	(0.00499)	(0.00937)
IV. Base effect	0.0491	-0.00186	-0.0194	0.0217	-0.0137	0.0390	-0.00788	0.115	-0.0336	-0.00103	-0.0358	0.0239
	(0.0511)	(0.0367)	(0.0526)	(0.0558)	(0.0646)	(0.0591)	(0.0597)	(0.0706)	(0.0444)	(0.0447)	(0.0443)	(0.0624)
Intensity: Number	-0.0585***	0.0973**	-0.101***	0.166***	-0.136***	0.0805	-0.0718	0.126*	-0.0582**	0.0828*	-0.0271	0.179***
	(0.0128)	(0.0413)	(0.0337)	(0.0606)	(0.0355)	(0.0646)	(0.0642)	(0.0762)	(0.0230)	(0.0494)	(0.0293)	(0.0657)
Observations	2,054	2,054	2,055	2,055	2,054	2,054	2,055	2,055	2,054	2,054	2,055	2,055

Figure 2a. Treatment intensity and informal transfers in group-lending villages

This figure shows the probability of receiving or giving informal transfers for an average respondent in the group-lending villages as a function of the number of months respondents borrowed on average from XacBank. The top two graphs show the probability of giving (left) and receiving (right) transfers to and from friends, while the bottom two graphs show the same for transfers to and from family members. The blue lines indicate the expected probability while the white lines indicate a 95 per cent confidence interval.

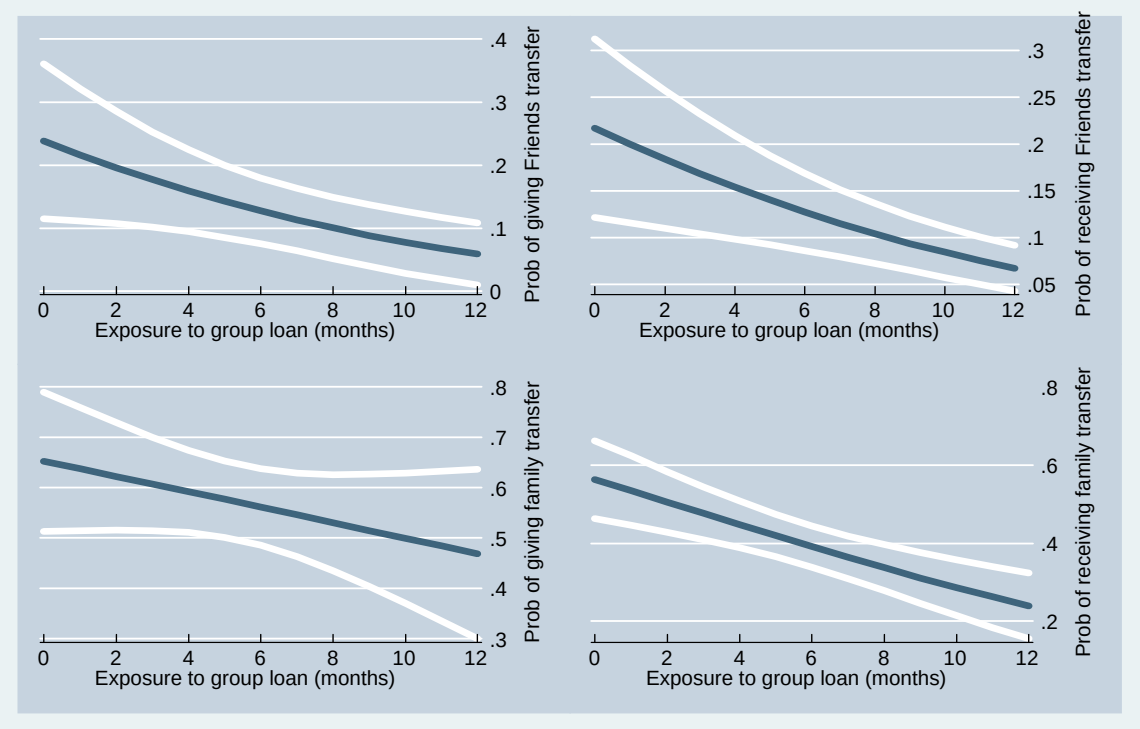

Figure 2b. Treatment intensity and informal transfers in individual-lending villages

This figure shows the probability of receiving or giving informal transfers for an average respondent in the individual-lending villages as a function of the number of months respondents borrowed on average from XacBank. The top two graphs show the probability of giving (left) and receiving (right) transfers to and from friends, while the bottom two graphs show the same for transfers to and from family members. The blue lines indicate the expected probability while the white lines indicate a 95 per cent confidence interval.

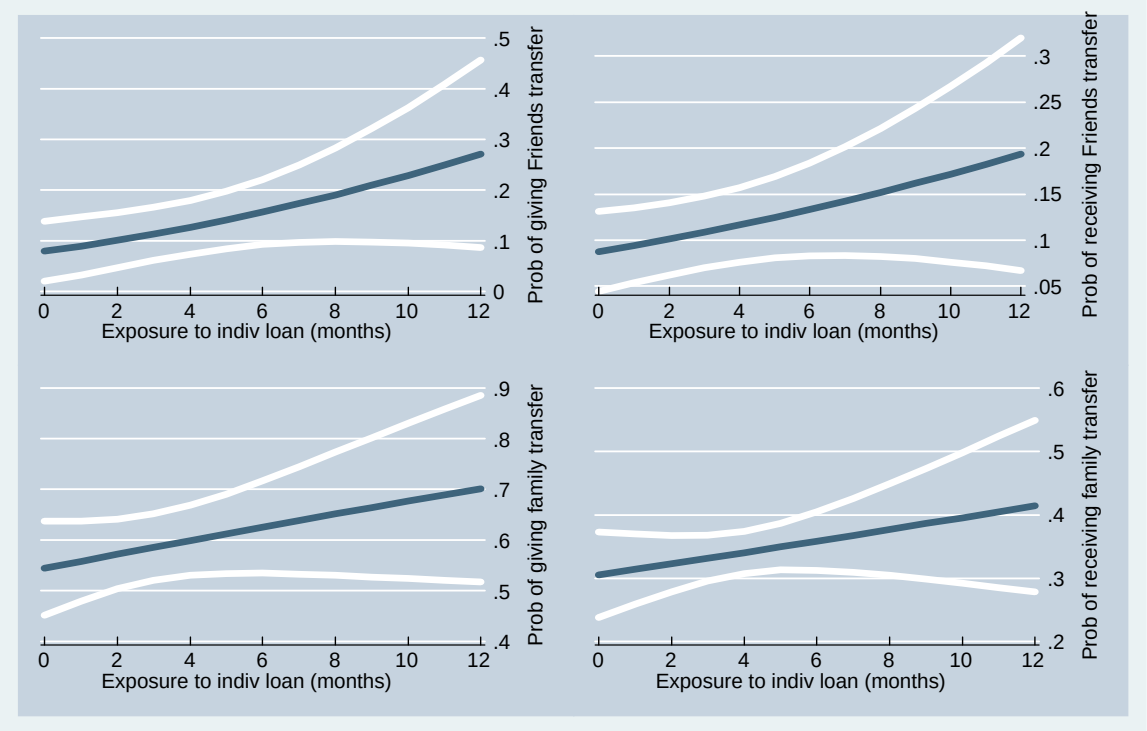

Table 11. Determinants of loan default

This table presents probit regressions to explain loan default. The dependent variable is a dummy that indicates whether a borrower defaulted (1) or not (0). *Loan amount* and *Debt at baseline* are measured in millions of tögrög. The following additional covariates were included but are now shown (all insignificant): *Household size*, *Collateral value*, *Male HH members >16*, *Female HH members >16*, *Children <16*, *Owns fence*, *House or flat*, *Owns vehicle*, *Saver*, *HH crop disaster*, *HH death*. Standard errors are clustered by village and reported in brackets. ***, **, * denote significance at the 0.01, 0.05 and 0.10-level. Table A1 provides the definitions and sources of all variables. Source of repayment data: XacBank.

	First loan		**All loans**	
	(1)	(2)	(3)	(4)
Group loan	0.029	-0.144	0.289	0.387
	(0.398)	(0.144)	(0.339)	(0.360)
Loan amount		-0.790		0.444
		(0.636)		(0.584)
Debt at baseline		-0.200*		-0.200*
		(0.140)		(0.117)
No. prior loans with XacBank				-0.161***
				(0.040)
Months since disbursement		0.096***		0.109***
		(0.024)		(0.021)
Owns land		-0.590***		-0.263
		(0.222)		(0.208)
Owns TV		1.262**		0.152
		(0.643)		(0.318)
Owns enterprise		-0.403*		-0.093
		(0.221)		(0.153)
Grade VIII education		-0.868***		-0.370*
		(0.297)		(0.218)
Vocational education		-0.809***		-0.359
		(0.325)		(0.225)
Age		-0.088		-0.023
		(0.090)		(0.066)
Age squared		0.001		0.000
		(0.001)		(0.001)
Buddhist		0.465		0.178
		(0.390)		(0.262)
Hahl		-0.763**		-0.707**
		(0.377)		(0.329)
Married		0.192		0.034
		(0.266)		(0.188)
Natural disaster		0.752*		0.300
		(0.404)		(0.277)
Observations	327	302	638	612
Pseudo R-squared	0.009	0.321	0.009	0.29

Figure A1. Overview of participating villages and provinces

This figure shows the geographical location of the 10 control soum centers (villages) as black dots, the 15 individual-lending villages (grey dots), and the 15 group-lending villages (white dots) across the five Mongolian provinces that participated in the experiment.

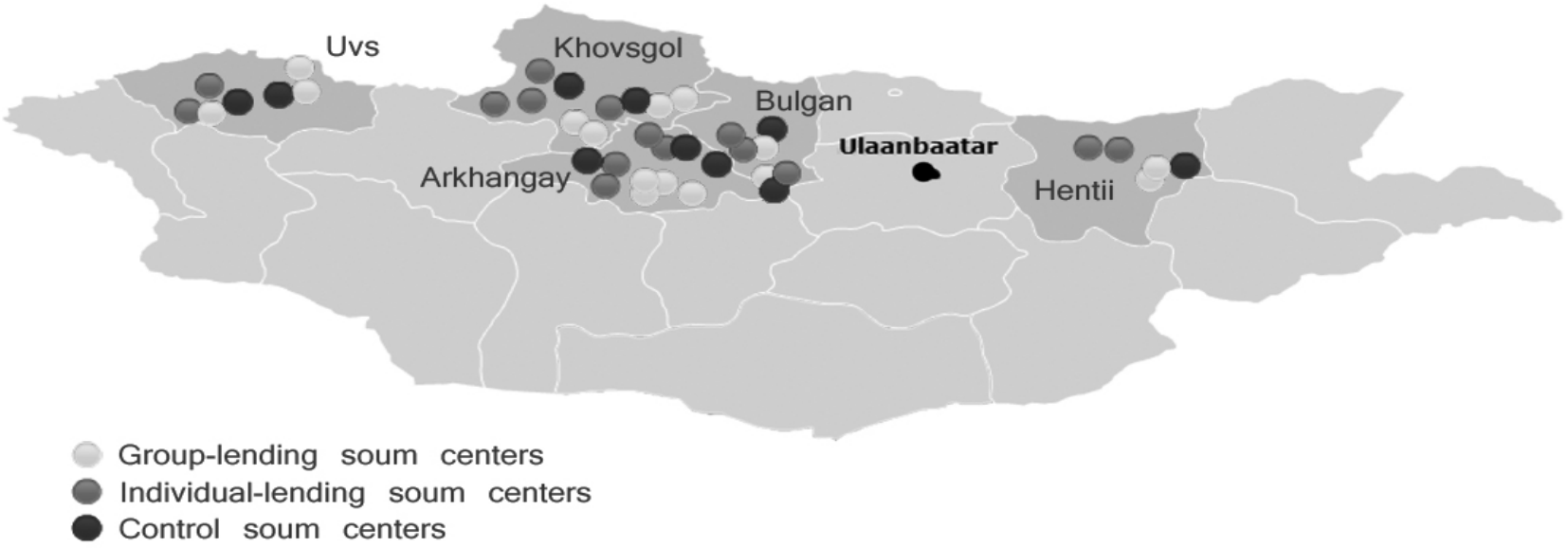

Table A1. Variable definitions

This table provides the names, definitions, and data sources of the variables used in the empirical analysis in alphabetical order. MNT = Mongolian t ögrög.

Variable name	Description	Standard control variable in impact analysis?
	Respondent and household (HH) level data (# respondents = 1,148). Source: Baseline survey	
Age	Age in years of respondent	X
Age squared	Age in years of respondent squared	X
Amount female business	Loan amount (in 000's MNT) that is used for a female-owned business	
At least one loan	Dummy variable that is '1' if the HH had at least one loan outstanding	
Buddhist	Respondent is of the Buddhist religion	X
Children <16	Number of children in the HH younger than 16 years	X
Collateral value	Estimated market value of the collateral (in 000's MNT)	
Consumption fuel	Quantity of fuel burned by the HH in the past week (in liters)	
Consumption milk	Quantity of milk consumed by the HH in the past week (in liters)	
Consumption red meat	Quantity of red meat consumed by the HH in the past week (in kilograms)	
Consumption vegetables	Quantity of vegetables consumed by the HH in the past week (in kilograms)	
Debt at baseline	Amount of loans outstanding at time of baseline survey (in million MNT)	
Debt service	Loan+interest (re)payment at HH level over past month (in 000's MNT) conditional on at least one loan outstanding	
Debt value	Amount of debt (in million MNT) at HH level that is still outstanding conditional on at least one loan outstanding	
Debt/HH income	Outstanding debt amount as proportion of annual HH income conditional on at least one loan outstanding	
Education respondent	Number of years of education of the respondent	
Education high	Dummy variable that is '1' if the respondent completed grade VIII or higher or vocational	
Education >VIII	Dummy variable that is '1' if the respondent completed grade VIII or higher	X
Education vocational	Dummy variable that is '1' if the respondent completed vocational training	X
Female business	Dummy variable that is '1' if the respondent operates her own business conditional on at least one HH business	
Female HH members >16	Number of female HH members aged 16 or older	X
Given transfers	Value of monetary and in-kind transfers given in last 12 months from non-relatives (in 000's MNT) conditional on giving	
Hahl	Respondent ethnicity is Hahl	X
HH crop disaster	Dummy variable that is '1' if the HH experienced severe crop losses during the previous year	
HH death	Dummy variable that is '1' if the HH experienced death of a HH member in the previous year	
HH robbery	Dummy variable that is '1' if the HH experienced a robbery in the previous year	
Highly educated	Dummy variable that is '1' if the respondent has completed vocational training or grade VIII or above	
Hours hired	Average number of hours worked per week in peak season by non-HH members in the respondent's enterprise	
Household size	Number of children and adults in the household	
House or flat	HH lives in a house, flat or apartment rather than a *ger*	
Interest rate	Monthly interest rate on a loan	
Joint enterprise	Dummy variable that is '1' if the respondent operates an enterprise together with her spouse	
Male HH members >16	Number of male HH members aged 16 or older	X
Married	Dummy variable that is '1' if the respondent is married or living together with partner	X

Loans at baseline	Dummy variable that is '1' if the HH had at least one loan outstanding at the time of the baseline interview	X
Operates business	Dummy variable that is '1' if the HH operates at least one business	
Outstanding loans	Number of loans taken by the HH that are still outstanding, conditional on at least one loan outstanding	
Owns animals	Dummy variable that is '1' if the HH owns animals for business purposes	
Owns dwelling	Dummy variable that is '1' if the HH owns at least one dwelling (ger, house, and/or apartment)	
Owns fence	Dummy variable that is '1' if the HH owns a fence around the dwelling	
Owns HH appliances	Dummy variable that is '1' if the HH owns large household appliances (refrigerator, cooler, washing machine)	
Owns tools/machinery	Dummy variable that is '1' if the HH owns tools and/or machinery for business use	
Owns vehicle	Dummy variable that is '1' if the HH owns a vehicle (car, lorry, tractor and/or motorbike)	
Owns well	Dummy variable that is '1' if the HH owns a well near the dwelling	
Partner enterprise	Dummy variable that is '1' if the respondent's spouse operates an enterprise but not jointly with the respondent	
Percentage female business	Percentage of total outstanding loan amount of the HH that is used for a female-owned business	
Percentage private use	Percentage of total outstanding loan amount of the HH that is used for private purposes	
Prior loans	Number of loans taken by the HH over the last five years that had been fully repaid at the time of the baseline survey	
Received transfers	Value of monetary and in-kind transfers received in last 12 months from non-relatives (in 000's MNT) conditional on receipt	
Saver	Respondent indicated that she saves	
Secured loans	Percentage of loans that is collateralized	
Self-employed	Dummy variable that is '1' if the respondent is self-employed	
Sewing or shop	Dummy variable that is '1' of the respondent operates a sewing business or shop conditional on having a business	
Sole enterprise	Dummy variable that is '1' if the respondent operates an enterprise independent from her spouse	
Value of dwelling	Value of the dwelling the HH lives in (in million MNT)	
Wage earnings	Average weekly wage earnings for wage earners (in 000's MNT)	
Years in existence	Number of years since the establishment of the respondent's business	
	Village-level data (# villages = 40). Source: Village survey in Spring 2008	
Banks in district	Number of bank branches in the district	
Distance to paved road	Distance (in km) from the village to the nearest paved road	
Distance to province center	Distance (in km) from the village to the province center	X
District area	Total surface are of the district in km2	
Doctors in district	Number of doctors in the district	X
Livestock in district	Number of livestock (cattle, camels, horses, sheep, goats) in the district	
Months	Average number of months between the date when respondents in a village received the first loan and the follow-up survey	
Number	Average number of loans received by the respondents in a village	
People in district	Number of people living in the district surrounding a village as well as that village itself	
People in village	Number of people living in a village	
Price bread	Price of a loaf of bread (in MNT)	
Price milk	Price of a liter of milk (in MNT)	
Price mutton	Price of a kilo of mutton meat (in MNT)	
Primary schools district	Number of primary schools in district	X
SCCs in district	Number of Savings and Credit Cooperatives in the district	
SS teachers	Number of secondary school teachers in the district	X
Time to paved road	Time (in minutes) to travel from the village to the nearest paved road by car or motorcycle	
Time to province center	Time (in minutes) to travel from the village to the province center by car or motorcycle	