The Impacts of Microfinance: Evidence from Joint-Liability Lending in Mongolia

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Abstract

We present evidence from a randomized field experiment in rural Mongolia to assess the poverty impacts of a joint-liability microcredit program targeted at women. We find a positive impact of access to group loans on female entrepreneurship and household food consumption but not on total working hours or income in the household. A simultaneously introduced individual-liability microcredit program delivers no significant poverty impacts. Additional results on informal transfers to families and friends suggest that joint liability may deter borrowers from using loans for non-investment purposes with stronger impacts as a result. We find no difference in repayment rates between both types of microcredit. (*JEL* 016, G21, D21, I32).

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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 are struggling with client over-indebtedness and repayment problems. This heightened skepticism also follows the publication of the findings, summarized in the introductory article to this Special Issue, of a number of randomized field experiments that indicate that the impact of microcredit might be more modest than advocated by its strongest proponents. These studies have tempered the expectations many had about the ability of microcredit to lift people out of poverty.

Yet, much remains unclear about whether, and how, microcredit can help the poor to improve their lives. Microcredit encompasses many different models and modalities and the evidence on the relative effectiveness and on the role played by different components is limited. Answering these questions is particularly important now that the microcredit industry is changing in various ways. One important recent trend has seen increased scale and professionalization leading a number of established MFIs to move from group or joint-liability lending, as pioneered by the Bangladeshi Grameen bank in the 1970s, to individual micro-lending.[1]

This paper provides 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 from our implementing partner XacBank, 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. We focus mostly on the joint-liability program as these loans are closest to the canonical microcredit product.

[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. See also Cull, Demirguc-Kunt, and Morduch (2009).

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 frequent and time-consuming 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.

The loans provided by the programs we investigate are relatively small, targeted at female 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 rounds of data collection: a baseline survey before the start of the lending programs and a follow-up survey 19 months later.

Although the loans provided under this experiment were intended to finance business creation, we find that about half of all credit is used for household consumption rather than business investment. 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 almost ten percent more than in control villages. In terms of poverty impact, we find a positive effect of access to group loans on food consumption, particularly of milk, bread, and non-alcoholic beverages. In terms of individual lending, we document neither an increase in enterprise ownership nor any poverty impacts. The stronger and significantly different impact on consumption and business creation in group-lending villages 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 group borrowing fosters self-discipline and in doing so ensures that a substantial part of the loans is actually invested in the first place (instead of used for consumption or transfers to others). This may ultimately lead to larger long-run effects. Our findings on informal transfers 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. This suggests that group borrowing may come at the cost of more informal risk-sharing arrangements with (other) family members and friends.

This paper 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 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. Gine et al. (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 (2013) 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)

[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).

for empirical evidence from Guatemala on intra-group insurance). When co-borrowers have to give upfront approval for each others' projects ex ante moral hazard is mitigated.

Gine and Karlan (2010) examine the impact of joint liability on repayment through two 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 et al. (2013) exploit a quasi-experiment in which an Indian MFI switched from individual to joint-liability, the reverse of the switch in Gine and Karlan (2010). They find that joint liability significantly improved repayment rates. Our paper is the first to use the same experimental context to compare the impact of individual versus joint-liability microcredit on borrowers.

A number of caveats apply to our analysis. First, our trial took place in an environment where microcredit was already available to parts of the population. As we describe in the next section, at least one microfinance bank was active in our villages. However, our target population consisted of relatively poor Mongolian women who hitherto had been excluded from all but the smallest consumer loans.

Second, to increase statistical power we offered credit to women who had expressed an interest in borrowing during initial information sessions in each village. This means that our results apply to women who were keen to borrow even though they did not yet know which loan type (individual or joint liability) they would be offered. These results may or may not generalize to the broader population.

Third, notwithstanding attempts to maximize power through the aforementioned sign-up process, we document - as do many other microcredit impact evaluations - in some cases quite substantial but imprecisely estimated impacts.

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

While large standard errors make it difficult to provide convincing evidence for positive impacts we cannot rule out such impacts either.

Fourth, the attrition rate between the baseline and follow-up surveys is 16 percent. While not excessive, one may worry about possible imbalances. In Section II.C we show that while attrition was somewhat higher in the individual treatment than in the control group, attrition levels did not differ significantly between the group-lending and control group. The latter comparison is the main focus of this paper.

Fifth, the tables in this paper present tests of various individual hypotheses. This implies a potentially large probability of rejecting a true null hypothesis by chance alone. Classical methods to account for such multiple testing are based on p-values and rely on assumptions about the dependence structure.[4] A recent literature improves on these methods by using re-sampling to implicitly estimate the unknown dependence structure.[5] We follow Romano and Wolf (2005) who develop a stepwise multiple-testing procedure that asymptotically controls the family-wise error rate. We note upfront that, with the exception of our results on loan take-up (Table 2), few of our results survive this correction for multiple-hypothesis testing.

We proceed as follows. In Section I we describe the loan program and target population after which Section II sets out our experimental design. Section III then discusses our main results after which Section IV compares the impact of the group and individual lending products. Section V concludes.

[4]Best know is the Bonferroni correction (Dunn 1961). See also Holm (1979) and Hochberg and Tamhane (1987).

[5]See Westfall and Young (1993), Hansen (2005), and Romano and Wolf (2005).

I. The program

A. Target population

Microfinance as it is known today originated in Bangladesh 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 percent of the number of inhabitants. This low population density means that disbursing, monitoring, and collecting small loans to remote borrowers is costly, particularly in rural areas.

Mongolian microcredit has traditionally been provided as 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, some collective self-help groups (*nukhurlul*) 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.

We conducted our experiment with XacBank, the second largest microlender in Mongolia. While XacBank lends to both men and women, our experiment focused on economically disadvantaged women in rural areas. With the exception of a few small NGOs, microcredit in Mongolia is provided by two commercial banks, Khan Bank and XacBank. Khan Bank has an extensive branch network inherited from the communist era. According to the Mongolian National Statistics Office (2009) these small branches have little incentive to diversify their lending and mainly lend to relatively well off herders with high-quality collateral in the form of animals. At the time of our experiment, poorer and female borrowers were almost entirely excluded from access to business credit. 78 per cent of all bank lending (including microcredit) took place in Mongolia's capital Ulaanbataar even though only 36 per cent of all Mongolian households resided there (IFC-KfW, 2009). The only credit available to rural women were informal

or small consumer loans to buy mobile phones or small electrical appliances. The purpose of XacBank's expansion was to open up access to business credit for this hitherto excluded group of poor rural women.

B. The loan products

The purpose of both group and individual loans was to allow women to finance small-scale entrepreneurial activities. Given the focus on business creation and expansion, loans had a grace period of either two months (loans exceeding six months) or one month (shorter loans). The interest rate was around 2 percent per month and was reduced by 0.1 percent after each successful loan cycle. Other dynamic incentives included the possibility to increase the loan amount and/or maturity after each repaid loan (Appendix Table A2).

Group-loan contracts stated that loans were based on joint liability and that XacBank would terminate lending to the whole group if a group did not fully repay a loan. Most group loans were composed of 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. The maximum size of the first loan to a group member was MNT 500,000 ($435). Group members had to agree among themselves who would get a loan and for what purpose. If a borrower's project was deemed too risky XacBank could exclude her 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. Before applying for a loan, groups had to build up savings equivalent to 20 percent of the loan amount. Group members were allowed to pledge assets instead of the compulsory savings although XacBank encouraged borrowers to use savings.

Group leaders were responsible for monitoring and collecting repayments and handing them over to the loan officer each month. There were no public repayment meetings or other mandatory gatherings. Groups decided themselves

on the modalities of their cooperation, including whether to meet regularly or not, and if so, how frequently (typically once per month). The group loan product was therefore more flexible than "traditional" group lending, which borrowers often consider burdensome due to the associated frequent and lengthy repayment meetings (e.g. Wydick 2001).[6]

Individual loans were similar to the sub-loans provided to group members, though larger on average. XacBank did not use strict collateral requirements but took collateral if available. As a result 91 percent of the individual loans were collateralized. Group loans had a somewhat shorter maturity (192 days on average) than individual loans (245 days) which reflects their smaller size. Similar to group loans, individual loans did not involve any mandatory group activities such as repayment meetings.

Appendix Table A2 shows that women used the individual and group loans in similar ways. Assuming that the purchase of livestock, tools, and machinery are business expenses, we find that 66 (67) percent of individual (group) borrowers used their first loan mainly to invest in a new or existing enterprise, putting between 70 and 80 percent of the loan to this purpose, with the remainder being used for household expenses. In the case of second loans, fewer women - 52 (43) percent of the individual (group) borrowers - used the loan primarily for business purposes.

II. The experiment

A. Experimental design and loan roll-out

The experiment took place in 40 soum centers (henceforth: villages) across five aimags (henceforth: provinces) in northern Mongolia (see Figure A1 in the Ap-

[6] In Gine and Karlan (2010) weekly meetings were held in both individual-lending and group-lending villages.

pendix).[7] The experiment started in February 2008 when XacBank loan officers and representatives of the Mongolian Women's Federation (MWF) organized information sessions in all 40 villages. The MWF is a large NGO whose representatives worked together with the research team to ensure a smooth implementation of the experiment.

The goal and logistics of the experiment were explained and it was made clear to potential borrowers that there was a 75 percent 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. In particular, out of the 40 study villages, 15 would be allocated to group lending, 15 to individual lending and 10 to control status. Women who wished to participate could sign up and were asked to form *potential* groups of about 7 to 15 persons each. Because of the focus on relatively poor women, the eligibility criteria stated that participants should own less than 1 million Mongolian togrog (MNT) ($869)[8] in assets and earn less than MNT 200,000 ($174) in monthly profits from a business. Many of these women were on 'poor lists' compiled by district governments. The MWF representatives enforced these eligibility criteria.

We were able to check compliance by using the information on asset ownership and income that we collected during the baseline survey. This confirmed that the eligibility criteria had been enforced. 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 ($2,610) whereas the average household in our sample

[7]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 one kilometer in diameter. The average soum in our experiment had 3,853 inhabitants of which 1,106 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.

[8]We use an exchange rate of 1,150 MNT/$, the average rate during the first half of 2008.

only earned MNT 1,100,000 ($955). Similar patterns emerge when we compare expenditures using data from the Mongolian statistical office or from the EBRD-World Bank 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 and had declared their interest in receiving a loan, a detailed baseline survey was administered during the second and third week of March 2008 (Figure A2 in the Appendix provides a timeline). There were three survey teams in the field simultaneously to ensure that respondents in the three types of villages were interviewed at the same point in time.[9] The total number of women interviewed was 1,148. The 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. A central location also minimized the risk that the female respondents would give biased answers due to the presence of older and male family members. 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' living standards 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. We also elicited information about household composition and education; exposure to economic shocks; and respondents' subjective income expectations. Finally, we collected information on more context-specific poverty indicators such as livestock ownership and the quality and size of the dwelling, most often a *ger*.[10]

[9]Each team handled all three types of villages in their area. Statistical tests show no significant differences in the timing of the baseline interview across village types.

[10]A ger is a portable tent made from a wood frame and felt coverings. Its size is measured by

Randomization took place *after* completion of the baseline survey so that at the time of the interview respondents did not know whether they would be offered a group loan, an individual loan, or no loan at all. Village-level randomization was performed in a central location, using a random number generator in Stata, as decentralized and public randomization was not feasible given that the villages were spread over a large geographical territory. 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, through informal transfers and connections, even individuals who do not receive credit directly. We 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 from the potential groups that were identified at the very beginning of the experiment when women had to indicate their interest to participate in the project. After a group had collected enough internal savings it could apply for its first XacBank loan.

The treatment period during which XacBank provided loans in the group

the number of lattice wall sections. Bigger gers are a sign of wealth as they are more costly to heat.

and individual-lending villages lasted 1.5 years - from March 2008 to September 2009. During this period participating women in treatment villages could apply for (repeat) loans[11], while XacBank refrained from lending in the control villages. Because XacBank started marketing its loans in both types of treatment villages at the same time, there is no significant difference between village types in the time lag between the start of the baseline survey and the disbursement of the first loan (p-value: 0.90).

In October 2009 we conducted a follow-up survey to measure again the poverty status and economic activity of the participating women. We also obtained information on how women had used their XacBank loan(s). There were again no significant timing differences in the interview process: the amount of time between the day that the first loan was distributed in a village and the day that the follow-up survey started does not differ significantly between group-lending and individual-lending villages (p-value: 0.89) and was on average 13.7 months. At this time, we also conducted a second village-level survey to collect information on village characteristics that may have changed, such as the prices of important consumer goods. XacBank collected repayment information on all loans for the entire period March 2008-June 2011. Lastly, in October 2011, we revisited one individual-lending and two group-lending villages for structured interviews and discussions with a number of borrowers about their experience of the lending programs.

B. Treatment-control balance

Table 1 presents a statistical comparison between the control villages and the group-lending villages. For each variable we present the baseline mean for the control group (in the post-attrition sample) as well as the difference in means between the control and the group-lending treatment groups (with a p-value for a t-test of equality of these means).

[11] Of all borrowers 47 per cent received at least one repeat loan during the experiment.

This shows that the respondents in the control and treatment villages were very similar in terms of household composition, employment and consumption patterns (Panel A). Households were also very similar in terms of a large number of other consumption and asset-ownership measures (available upon request). As we consider many variables, we expect some statistical differences between the groups and this is the case for access to finance at the household level. A majority of the households had at least one loan at the time of the baseline survey and while this percentage does not differ significantly between both groups, we find that the amount borrowed in the treatment villages was slightly higher (p-value 0.06). Along all other dimensions, the treatment and control respondents were very similar.

These figures also indicate that at the time of our baseline survey the penetration of small retail-type loans was relatively advanced in rural Mongolia.[12] For our purposes, an important question is whether households were also using this credit to finance entrepreneurial activities by our female respondents. Our baseline data show that this is not the case. Around 75 percent 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 at baseline. Second, fewer than 20 percent of households had invested part of their loan(s) in a business owned by the female targeted by the loan. Furthermore, the amount and percentage of funds used for female enterprises did not differ significantly between both types of villages. In control villages households had invested on average 19 percent of their outstanding debt in a female-run business, whereas this percentage was 14 percent in the group-lending villages. These percentages, as well as the absolute amounts, do not differ significantly between control and treatment villages.

[12]Most poorer households and women in rural areas only had access to short-term consumer credit, backed by non-entrepreneurial income sources such as pensions or salaries, that was used to buy small household goods.

[Insert Table 1 here]

We conclude that the randomization process appears 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.

C. Attrition

The follow-up survey took place approximately 1.5 years after the baseline survey and 84 percent of respondents were successfully re-interviewed. A possible concern is that non-response was not random across treatment and control villages, which could bias the estimated treatment effects. Reassuringly, Table 1 shows that there is no significant difference between attrition levels in the control and the group-lending villages.

To investigate this in more detail, we estimate the probability of attrition as a function of a group-treatment dummy as well as a range of respondent and household characteristics (Annex Table A3). Overall, these results are reassuring as the coefficient of the treatment dummy is never statistically significant. However, a few covariates (such as respondent age) appear to be correlated with attrition status and we therefore reject the null hypothesis that the covariates are jointly insignificant.

In a final step, we also check whether the characteristics of attritors are similar in the group-lending treatment and the control group. This reveals very few differences between attritors and stayers in the control group whereas in the group-treatment group we find a number of differences. In particular, attritors in the group villages have on average 0.6 more household members (p=0.03)

and were 19 percentage points more likely to have a bank loan at the time of the baseline survey (p=0.02). In any case, we control for these variables in our analysis.

D. Estimation approach

In what follows, we report the results of an intention to treat (ITT) analysis where we compare *all* women who initially signed up in the group treatment villages, irrespective of whether they borrowed or not, with those who signed up in control villages.[13] 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.

As our sample was balanced at baseline, the question whether one should use only post-treatment data or a difference-in-differences approach boils down to whether the variance of time-invariant individual effects is greater or smaller than the variance of transitory shocks. If the former is smaller, using post-treatment data only is the appropriate strategy. If it is greater, we should use difference-in-differences. Ex-ante we do not have information on the relative size of these variances. McKenzie (2012) shows that difference-in-differences may limit statistical power if autocorrelation in the outcomes is limited. In our

[13] 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 percent in the group-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 assumption underlying consistent estimation of ATT is that unobservable characteristics do not affect the decision to participate, we only show ITT parameters.

case, autocorrelation is non-negligible but generally below 0.5.[14]

McKenzie (2012) suggests that in case of a single baseline and follow-up survey and autocorrelations below 0.5, power is highest when regressing the outcome variable at follow-up on a treatment indicator, a set of baseline covariates, and the baseline value of the outcome variable. We hence follow this approach and note that our results remain quantitatively and qualitatively unchanged when we use difference-in-differences.[15] Lastly, we also include strata dummies in the form of province fixed effects (cf. Bruhn and McKenzie 2009). Our main motivation for this particular choice is to improve the precision of our estimates.

Statistical power can also be held back in case of high intra-cluster correlation and only a limited number of clusters (in our case villages). Throughout the paper we therefore report cluster-robust standard errors. Since we have 40 clusters (that is, more than the often used rule of thumb of 30) cluster-robust standard errors should in principle be sufficiently conservative (see also Bertrand, Duflo, and Mullainathan 2004). We nevertheless also calculate standard errors based on the even more conservative wild cluster bootstrap-t procedure (Cameron, Gelbach, and Miller 2008). All our main results continue to hold at the same significance levels if we use these bootstrapped standard errors.

Our basic regression framework is:

$$Y_{iv1} = \alpha_0 + \alpha_1 \cdot G_v + \alpha_X \cdot X_{i0} + Y_{iv0} + \varepsilon_{i1} \tag{1}$$

where α_1 measures the impact of access to joint-liability credit and:

- 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);

[14] Enterprise ownership: 0.35, hours worked: 0.41, total consumption: 0.30, ownership of large household appliances: 0.50.

[15] We use OLS and a probit model for continuous and binary dependent variables, respectively.

- G_v is a binary variable equal to 1 for group-lending villages (0 otherwise);
- X_{i0} is a set of baseline characteristics of respondents and their households;
- ε_{i1} is an i.i.d. error term clustered at the village level.

III. Results

In this section we report our main results. We start with loan take ups, to move on to self-employment and income, hours of work and consumption and saving. We conclude the section with results on schooling and informal transfers.

A. The intervention and access to liquidity

After the baseline survey XacBank started disbursing loans in the 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. Although other banks (primarily Khan Bank) were also lending in both the treatment and control villages during the experiment, our intervention led to a significant increase in borrowing. Column 4 in Table 2 shows that the probability of receiving any type of loan during the experiment was substantially higher in group treatment than in control villages: 76 percent in group lending villages versus 50 percent in the control villages.[16] Column 1 of Table 2 confirms that the large difference in loan take-up between the treatment and control villages is driven by XacBank's lending programs. While even in the control villages a small number (6 percent) of respondents reported to have received a XacBank loan, this number is much higher for the group lending villages: 57 percent. The strategy of inviting people to sign up and express an initial interest in microcredit therefore led to a relatively large difference in take-up

[16]By way of comparison, Banerjee et al. (2013) report a difference in short-term take-up between treatment and control group of 8.3 percentage points while in the study by Crépon et al. (2011) the difference was 10 or 16 percentage points depending on whether MFI or survey data are used.

between treatment and control villages, resulting in additional statistical power. Of course, this strategy also defines our population of interest more narrowly as those that were interested in accessing microcredit in the first place.

Notwithstanding the relatively large difference in borrowing activity between treatment and control villages, we note that after 1.5 years only 57 percent of all group treatment respondents -all of whom had indicated an interest in microcredit during our initial information sessions- had borrowed from XacBank (this percentage was only 50 percent in the individual lending villages). We can use information collected through the follow-up survey to better understand why a significant proportion of women in treatment villages did not borrow. These data show that about 75 percent of the 'non-treatment' was due to women who either did not apply for a loan or who applied but subsequently refused the offer. This leaves only about a quarter of all 'untreated' women who were actually refused a loan by XacBank.

The last four columns of Table 2 provide some insight into late repayment and default behavior. Columns 5 and 7 are based on administrative data from XacBank while columns 6 and 8 are based on respondents' answers to our follow-up survey. Columns 5 and 6 show that defaults on XacBank loans were minimal in both types of treatment villages. Delayed repayment (here defined as loans that were at any time at least 30 days late) occurred in about 7 percent of all group loans.[17] We analyze loan repayment in more detail in Section IV.B.

[Insert Table 2 here]

B. Impact on self-employment and income

A key objective of the microcredit program was to encourage women to expand or invest in small-scale enterprises, with the ultimate aim of reducing poverty

[17] According to our survey data. This number was 12 percent based on XacBank's administrative data.

and improving well-being. To evaluate the extent to which the program achieved these two objectives, we first look at the effect on enterprise creation and growth, and on whether enterprise profits and income more generally increased. We then go on to estimate the effect on detailed household consumption, as a measure of well-being.

Column 3 in Table 3 shows the impact of access to joint-liability microcredit on the probability that the household operates a small-scale business, which could be owned by the respondent, her spouse, or by the couple jointly (65 percent of respondents are married or cohabitating). Access to this type of credit led to an 8 percentage points higher probability of entrepreneurship, at the household level, compared to the control group. This is also reflected in increased business asset ownership as measured by an index that captures whether the household owns tools and machinery; a tractor or lorry; riding equipment; and/or unsold stock (column 2). We find no significant impact on the (self-reported) total value of these assets (column 1).

[Insert Table 3 here]

Column 6 then shows that access to group loans had a positive impact on female entrepreneurship in particular. We find an overall increase of 9 percentage points (compared to an end-line level of female entrepreneurship of 39 per cent in the control group). Unreported results show that this effect is driven by less-educated women: at the end of the experiment these women had a 31 percentage points higher chance of operating a business compared with women in control villages.[18] We do not find that access to credit resulted in more profitable enterprises (Table 3, columns 5 and 7) or changes in other sources of household

[18]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 3 is thus driven by an increase in female entrepreneurship.

income (Table 4). If anything, the point estimates for enterprise profits are negative but imprecisely estimated.[19] We note that profits may have been low during our observation period due to the relatively high interest payments on the still outstanding loan balance. To the extent that newly established enterprises continue to generate revenues after full loan repayment, future profitability may increase.[20]

[Insert Table 4 here]

C. Impact on hours worked

In Table 5 we look at whether households increased their labor supply as a result of the lending program. In line with the increased probability of enterprise ownership in the group treatment villages, column 2 shows an average increase of almost 6 hours per week per adult worked in the female-run household business. This increase is completely due to a higher labor supply by prime-age adults (column 10).[21] There is a 35 percent increase of the number of hours worked on the female business compared to the control group. Column 5 shows a decline, of almost 3 hours, in the number of hours worked by teens. Overall, we thus find strong evidence that access to group loans allowed adult female Mongolians to set up new small-scale enterprises and to spend significantly more hours working in these businesses. At the level of the household as a whole, there was no significant change in working hours.

[19]The profit variable is zero for individuals without a business. We also ran tobit regressions for columns 5 and 7 and these confirm the absence of significant impacts on enterprise profitability.

[20]The impact of access to group loans on revenues from female-owned businesses is positive (p=0.107).

[21]In line with our previous (unreported) results, this effect is concentrated among the lower-educated women. Within these households, adults spend an average 19 more hours on the female-run business compared to the control villages.

[Insert Table 5 here]

D. Impact on consumption and savings

In this section we analyze whether borrowers' access to joint-liability credit and the resulting increased engagement in entrepreneurial activities fed through to improved household well-being - a key objective of the program. To do so, we first estimate the effects of the program on household consumption expenditures and the likelihood of consuming certain items. We use detailed information on consumption patterns elicited in the surveys, in which food consumption is measured over the past week (at a disaggregated level as well as overall) and non-durable and durable consumption over the past month and year, respectively. Table 6 reports the results.

We find robust evidence that access to group loans led to more (and healthier) food consumption (column 4). To put this into context, the average monthly food consumption in group-lending (and control) villages was $130 per household. The estimated effect implies that food consumption was on average $18.46 (i.e. 14.2 percent of baseline food consumption) higher per household per month for households in group villages than for households residing in control villages.

A closer inspection of the underlying data reveals that households in particular increased their consumption of milk, bread, and non-alcoholic beverages. With the exception of dairy, a staple in the Mongolian diet, these effects are not only due to increased home production: we also see treated clients purchasing more.[22] Indeed, we find a strong positive correlation between a household's food production at home and the monetary value of its total food consumption

[22]The expenditure share of food and non-alcoholic beverages in individual consumption in Mongolia amounted to 35.9 per cent in 2005 (source: World Bank International Comparison Program). According to 2009 FAO data on food supply decomposition by energy value, wheat (bread) accounted for 40 per cent of the average Mongolian's food energy supply (as measured in kcal per capita per day). Together with milk (11 per cent) bread thus constitutes one of the main Mongolian dietary components.

(i.e. home-produced plus bought food) at endline. This correlation is about the same in the control and the group-lending villages (0.69 and 0.61) and in both cases significant at the 1 percent level. This suggests that if access to credit allowed households to produce more food, this may have led to an increase in total food consumption as higher home production is not fully offset by less food purchases.

The increase in food consumption also translates into higher total per capita consumption at the household level. We do not find any effects on expenditures on durables, non-food non-durables, education expenses, or savings.[23] We also consider whether the program affected ownership of household goods. We construct an index of key households goods in the Mongolian context: computer, land-line telephone, mobile telephone, TV, VCR, small electric appliances, and large electric appliances. We find no impact of access to group credit on ownership of these household goods (but access to group credit did have an impact on ownership of *business* assets, see Table 3).[24] A comparison of Tables 1 and 6 shows a rapid increase in the household index from 0.13 to 0.30. Our results indicate that this increase reflects a secular trend, in particular of increased ownership of electrical appliances, that was not caused by improved access to group loans.

[Insert Table 6 here]

[23]We are mainly interested in the impact on consumption as a whole rather than impacts on specific food items. However, given the importance of bread and dairy in the Mongolian diet it makes sense to look at these staples separately as well. Yet, when we adjust the p-values for individual outcome variables that are interesting in their own right but plausibly belong to the same family, while taking into account the observed correlation structure between these outcomes, most of the individual impacts are no longer statistically significant (with the exception of the impacts on the consumption of non-alcoholic beverages and the ownership of VCRs/radios).

[24]We do not find a significant increase in the total number of animals, taking the actual number of animals owned or when looking at 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).

E. Social impacts: schooling and informal transfers

Table 7 summarizes the impacts of access to credit on schooling and the labor supply of children aged 6-15 and 16-20 (teens). We do not find any evidence of schooling impacts or of clear effects on child labor. There is some weak evidence at the 10 percent level that among the low-educated group borrowers, where our main impacts so far have been concentrated, there is some substitution away from outside labor by children to helping out in the newly established female-run enterprises. For teenagers we find positive impacts on schooling among the higher-educated households.

Our results so far provide evidence that the group loans were relatively effective at achieving their objectives of increasing entrepreneurial activities and improving household well-being. However, since we do not find an accompanying effect on household income, an 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 other 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 limited number of nomadic households traveled, 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 behavior in two 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.

Our survey asked households about their informal - monetary and in-kind - transactions with friends and family during the past year and the most recent month. The right-hand side of Table 7 shows that we do not find an overall ITT effect of group lending on the probability of informal transfers taking place (columns 6 and 8). On average about 40 (59) percent of our control respondents received (made) informal transfers to family members and friends. Columns 7 and 9 show that impacts in the group-villages are consistently negative although the standard errors are large.

[Insert Table 7 here]

Unreported results show that in individual-lending villages, access to credit actually had opposite impacts: here informal transfers went up. Figure A3 in the Appendix depicts the relationship between the intensity of exposure to credit and the probability of receiving or giving transfers. The difference between the intensity effects on transfers between both types of treatment villages is always significant at the 1 or 5 percent level. The graphs show the probability of making (left) or receiving (right) informal transfers to and from friends for an average respondent in the group-lending (top) and individual-lending villages (bottom) as a function of the average number of XacBank loans taken by respondents in the village. These results are suggestive of group borrowers partly substituting 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 et al.

(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.

IV. Comparing joint-liability to individual-liability loans

A. Borrower impacts

In Table 8 we summarize some evidence on the impacts of the individual-lending program that was simultaneously introduced in 15 other randomly selected treatment villages. The first two columns show that this treatment too led to a significant increase in borrowing. At follow-up the probability of having a loan from XacBank (any loan) is 42 (19) percentage points higher than in the control villages. Remember that the increase in borrowing among participants in group-lending villages was higher at 51 (XacBank loan) and 26 (any loan) percentage points.

Not only the probability of borrowing but also the intensity of the treatment in terms of number of loans was higher in group villages. The mean number of loans was 0.99 in group-lending villages and 0.57 in individual-lending villages. This means that conditional on borrowing women took out on average two (consecutive) loans in the group-lending villages and one in the individual-lending villages. We already discussed that there were no economically or statistically

meaningful differences between both village types in terms of the timing of the interviews or the *start* of the roll-out of XacBank's lending programs. However, on average loans were disbursed 46 days later in the individual villages compared to the group villages (p-value: 0.06). This suggests that the (s)lower loan take-up in individual-lending villages is mainly the result of lower demand.

The lower loan take up in the individual-lending villages is also reflected in the fact that we do not find impacts on the beneficiaries. Columns 3 through 6 in Table 8, show no positive impacts on either self-employment or consumption - in contrast to what we find for the group-lending villages. One explanation may be that individual-loans required more collateral so that women with slightly riskier (but potentially high-return) projects did not apply for fear of losing the collateral. On the supply side, loan officers may have been stricter too in the absence of joint-liability. In such as scenario, the individual loans may have acted more as consumption loans, taken out by women with access to a secure income stream from other sources. Unreported results indeed indicate a significant increase in household assets in the individual-lending villages.

[Insert Table 8 here]

B. Loan repayment

We have documented a positive impact of access to group loans on business activities and consumption but no such impacts of access to individual loans. It is also interesting to compare both loan products from the point of view of the lender. In this section, we therefore analyze the repayment behavior of both types of borrowers. Gine 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 et al. (2013) 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 9 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 disbursed as part of the experiment whereas the last two columns reflect the full sample, that is including repeat loans to the experiment participants.

[Insert Table 9 here]

We find, regardless of whether we control for borrower and loan characteristics, no significant difference between the probability of default in group- and individual-lending villages. This confirms the findings of e and Karlan (2010) although in our case *neither* program included mandatory repayment meetings whereas in their experiment *both* programs included such meetings. 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., 2013).

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 percent significance level) of the amount of outstanding debt at the time of the baseline survey on the likelihood of default. Respondents with outstanding debt at baseline where thus *more* likely to (be able to) repay the

subsequent XacBank loan. This suggests that borrowers that had already successfully passed the screening of another lender, were less risky compared with first-time borrowers.[25] In a similar vein, column 4 indicates that also repeat XacBank borrowers were significantly less risky in terms of default, possibly because they had already successfully passed XacBank's own screening procedures and subsequently paid on time.

A number of covariates are only of importance for first-time loans. Those that owned land or 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 percent 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.

V. 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.

Our findings on the impact of joint-liability lending are mixed. In line with some other RCTs, we document an increase in entrepreneurship due to access to group loans. Among households that were offered these loans the likelihood

[25]To the extent that multiple borrowing and over-indebtedness were a problem in rural Mongolia this is therefore 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 repay.

of owning an enterprise increases by almost ten percentage points more than in control villages (and even by 30 percentage points for less-educated women). Unlike most other randomized impact studies, we also find a positive effect on food and total consumption (though not on current income). The simultaneously introduced individidual-liability microcredit program did not yield significant poverty impacts.

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 frequent 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 (note that both our loan products required some form of collateral).

An important question is *why* joint-liability loans may have been more effective at raising consumption (and probably long-term income) in our context. One possibility is that the joint-liability scheme better ensures discipline so that larger long-run effects can be achieved. Group borrowing may foster self-discipline and 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: 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.

More generally, our results caution against a widespread move from joint-

liability to individual-liability microcredit. While (collateralized) individual-liability loans may for some be a good alternative to burdensome group loans, our results suggest that this does not hold for everyone. In particular, we document less repeat borrowing in the individual-lending villages and most of these loans were not used for business expansion and, relatedly, did not seem to have any impact on borrower welfare. This may reflect that some women, in particular the less-educated, were uncomfortable with borrowing on an individual basis (and put up the necessary collateral) but *were* willing to borrow as part of a group when XacBank made that option available in their village.

This would imply that group and individual lending are complementary financial services for which the demand may differ across borrower types and across different risk environments. 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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Appendix Tables and Figures

[Insert Tables A1-A2-A3 and Figures A1-A2-A3 here]

Table 1. Summary statistics

		Control			Group Treatment – Control	
	Obs	Obs	Mean	St. Dev.	Coeff.	*p-value*
Panel A. Post-attrition household sample						
Household composition						
# members	611	260	4.888	1.828	0.047	*0.712*
# adults (>=16 years old)	611	260	1.754	1.255	0.005	*0.950*
# children (<16 years old)	611	260	3.158	1.530	0.032	*0.746*
Age of respondent	611	260	40.881	9.360	-0.506	*0.337*
Education of respondent (1=at most grade VII)	611	260	0.150	0.358	-0.021	*0.289*
Religion of respondent (1=Buddhist)	611	260	0.758	0.429	0.000	*0.998*
Access to credit						
Loan from bank	611	260	0.477	0.500	0.032	*0.364*
Loan from relatives	611	260	0.023	0.150	-0.002	*0.801*
Loan from friends	611	260	0.046	0.210	-0.007	*0.359*
Any other loan	611	260	0.065	0.248	0.014	*0.549*
Any type of loan	611	260	0.573	0.496	0.035	*0.290*
Amount borrowed from ('000s MNT)						
Bank	606	260	362	637	82	*0.066*
Relatives	601	256	0.5	6	-0.2	*0.461*
Friends	599	256	1.3	8	-0.4	*0.084*
Other	598	255	3.3	18	2.7	*0.252*
Total	605	260	389	641	81.3	*0.058*
Self-employment activities						
Any type of enterprise	611	260	0.60	0.490	0.000	*0.998*
Respondent has own enterprise	611	260	0.396	0.490	-0.016	*0.566*
Revenue of respondent's enterprise	611	260	515.0	1,388	5.605	*0.940*
Expenses of respondent's enterprise	611	260	390.0	966.0	7.711	*0.909*
Profit of respondent's enterprise	611	260	125.0	898.3	1.085	*0.972*
Business asset index	611	260	0.03	0.910	0.030	*0.701*
Distance to province center (in km)	611	260	113	52.27	-12.04	*0.237*
Employment activities (except self-employment)						
# of income sources	611	260	0.546	0.742	0.062	*0.227*
Wages from agricultural work (0/1)	611	260	0.088	0.285	0.021	*0.227*
Wages from private business (0/1)	611	260	0.100	0.301	0.010	*0.521*
Wages from mining (0/1)	611	260	0.023	0.150	0.011	*0.332*
Wages from teaching (0/1)	611	260	0.112	0.315	-0.014	*0.369*
Wages from government (0/1)	611	260	0.100	0.301	0.003	*0.882*
Any other wage income (0/1)	611	260	0.131	0.338	0.024	*0.248*
Income from benefits (0/1)	610	259	0.950	0.219	-0.003	*0.700*
Wages and benefits ('000s MNT)						
Wages from agricultural work	611	260	26.0	279.8	12.86	*0.501*
Wages from private business	610	260	122.7	442.1	13.80	*0.595*
Wages from mining	609	260	50.3	411.3	12.02	*0.596*
Wages from teaching	611	260	246.3	748.4	-25.46	*0.522*
Wages from government	610	260	202.3	673.7	12.46	*0.745*
Any other wage income	611	260	173.5	542.4	55.42	*0.133*
All wage income	611	260	821.0	1,249.2	80.31	*0.351*
Income from benefits	608	257	485.2	618.7	-29.96	*0.505*
Consumption ('000s MNT)						
Total consumption expenditures (yearly)	589	252	2,800	2,200	6.49	*0.979*
Durable consumption (yearly)	602	255	710	700	84.00	*0.067*
Non-durables consumption (monthly)	604	259	89.87	110	-1.80	*0.779*
Food consumption (weekly)	602	257	21.06	22.32	-1.10	*0.764*
Log total consumption expenditures (yearly)	589	252	14.53	0.88	0.014	*0.881*
Log durable consumption (yearly)	599	253	13.03	1.00	0.105	*0.101*
Log non-durables consumption (monthly)	597	259	10.84	1.15	-0.010	*0.910*
Log food consumption (weekly)	561	243	9.37	1.29	0.004	*0.980*
Household asset index	611	260	0.13	0.79	0.028	*0.611*
Panel B. Attrition						
Not surveyed at endline	710	299	0.130	0.337	0.008	*0.689*

Notes: Unit of observation: household. Panel A: sample includes only households also surveyed at endline. Panel B: sample includes all households surveyed at baseline. In case of household characteristics, the standard errors are clustered at the village level. Table A1 provides the definitions and sources of all variables. *Wages from private business* includes wages from working in a shop, market, bank, finance company, or other private business. *Household (business) asset index* : Calculated for a list of home electrical appliances (business assets). Each asset is given a weight using the coefficients of the first factor of a principal-component analysis. Each index, for a household *i* , is calculated as the weighted sum of standardized dummies equal to 1 if the household owns the durable good. '000s MNT: Thousands of Mongolian tögrög. The exchange rate at baseline was USD 1 to MNT 1,150.

Source: Baseline household survey and author calculations.

Table 2. Credit

	(1) XacBank	(2) Other bank or MFI	(3) Informal loans	(4) Any loan	(5) Default XacBank loan (admin data)	(6) Default XacBank loan (survey data)	(7) Delay XacBank repayment (admin data)	(8) Delay XacBank repayment (survey data)
Panel A. Credit access†								
Treatment	0.508***	-0.135***	0.009	0.257***	0.071**	0.011	0.119***	0.073***
	(0.051)‡‡	(0.044)‡	(0.008)	(0.041)‡‡	(0.026)	(0.010)	(0.030)	(0.017)
Observations	611	611	611	611	609	611	609	611
Control mean follow-up	0.0615	0.454	0.004	0.504	0.000	0.015	0.000	0.015
Panel B. Loan amounts (in MNT)††								
Treatment	365,932***	-74,130	10,714	361,034***				
	(44,233)‡‡	(79,611)	(6,317)	(43,585)‡‡				
Observations	611	611	611	611				
Control mean follow-up	37,204	486,436	19.23	53,075				

Notes: Outcome at follow-up is regressed on treatment indicator, baseline covariates, and baseline measure of outcome variable to measure the impact of providing access to group loans on borrowing (upper panel) and borrowing amounts (lower panel). All variables measured at the household level. All regressions include a set of unreported pre-treatment covariates. Standard errors are clustered at the village level and reported in parentheses. The exchange rate at baseline was USD 1 to Tögrög 1,150. ***, **, * indicate significance at 1, 5, and 10 percent, based on p-values unadjusted for multiple-hypothesis testing. ‡‡, ‡, ǂ indicate significance at 1, 5, and 10 percent when correcting for multiple-hypothesis testing. Specifically, in this table we include the variables in columns (1) to (4) of both Panel A and B. Critical values refer to the final step (two steps at most) that allows us to still reject any of the hypotheses. *Delay XacBank repayment* : delayed loans are those that were at least 30 days late at any point in time. Table A1 provides the definitions and sources of all variables.

Source: Baseline and follow-up household surveys, XacBank, and author calculations.

Table 3. Self-employment activities: revenues, assets and profits

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All household businesses					*Respondent business*		
	Assets (stock in '000s MNT)	Business asset index	Has a self-employment activity	No. self-employment activities	Profit ('000s MNT)	Has a self-employment activity	Profit ('000s MNT)	Business started
Treatment	-29.292	0.137*	0.077**	0.021	-4.789	0.085**	-7.852*	0.014
	(249.636)	(0.077)	(0.033)	(0.031)	(5.302)	(0.038)	(4.230)	(0.018)
Observations	611	611	611	611	611	611	611	611
Control mean follow-up	2236	-0.165	0.585	0.331	-26.85	0.392	-12.11	0.0654

Notes: Outcome at follow-up is regressed on treatment indicator, baseline covariates, and baseline measure of outcome variable. Coefficients and standard errors (in parentheses). Columns (1)-(5) are at the household level and columns (6)-(8) at the respondent level. All regressions include a set of unreported pre-treatment covariates. Standard errors are clustered at the village level. The exchange rate at baseline was USD 1 to Tögrög 1,150. ***, **, * indicate significance at 1, 5, and 10 percent, based on p-values unadjusted for multiple-hypothesis testing. ###, ##, # indicate significance at 1, 5, and 10 percent when correcting for multiple-hypothesis testing. Specifically, in this table we include the variables in columns (1) to (8). Critical values refer to the final step (two steps at most) that allows us to still reject any of the hypotheses. *Business asset index* : Calculated for a list of four key business assets: tools and machinery, riding equipment, lorry or tractor, and unsold stock. Each asset is given a weight using the coefficients of the first factor of a principal-component analysis. The index, for a household *i* , is calculated as the weighted sum of standardized dummies equal to 1 if the household owns the durable good. *Business started* = 1 if the respondent at the time of the follow-up survey had a business that was less than 20 months old. Table A1 provides the definitions and sources of all variables.

Source: Baseline and follow-up household surveys and author calculations.

Table 4. Income

	(1) Self-employment (profit)	(2) Daily labor/salaried	(3) Household benefits	(4) Income from food production
Treatment	-4.8	-252.8	1.6	701.7
	(5.3)	(185.0)	(35.5)	(619.9)
Observations	611	611	611	610
Control mean follow-up	-26.85	413.9	393.1	506.1

Notes: Outcome at follow-up is regressed on treatment indicator, baseline covariates, and baseline measure of outcome variable. Coefficients and standard errors (in parentheses). All regressions include a set of unreported pre-treatment covariates. Standard errors are clustered at the village level. ***, **, * indicate significance at 1, 5, and 10 percent, based on p-values unadjusted for multiple-hypothesis testing. ~~###~~, ~~##~~, ~~#~~ indicate significance at 1, 5, and 10 percent when correcting for multiple-hypothesis testing. Specifically, in this table we include the variables in columns (1) to (4). Critical values refer to the final step (two steps at most) that allows us to still reject any of the hypotheses. Table A1 provides the definitions and sources of all variables. All variables expressed in '000s Tögrög. The exchange rate at baseline was USD 1 to Tögrög 1,150.

Source: Baseline and follow-up household surveys and author calculations.

Table 5. Time worked by household (HH) members

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Hours worked per adult member over the past 7 days of age group† :							
	all adults and teens				**teens**			
		of which:				*of which:*		
	total	*respondent's business*	*other HH business*	*outside activities*	total	*respondent's business*	*other HH business*	*outside activities*
Treatment	-2.410	5.675*	-5.650	-2.485	-2.584**	-0.278	-1.487*	-0.808
	(3.807)	(3.008)	(3.905)	(3.274)	(1.065)	(0.609)	(0.750)	(0.688)
Observations	611	611	611	611	611	611	611	611
Control mean follow-up	88.41	19.83	37.52	31.06	5.535	1.277	2.396	1.862

	(9)	(10)	(11)	(12)
	prime age adults			
		of which:		
	total	*respondent's business*	*other HH business*	*outside activities*
Treatment	1.909	6.135**	-2.074	-1.875
	(3.023)	(2.469)	(3.525)	(2.956)
Observations	611	611	611	611
Control mean follow-up	80.63	17.72	33.71	29.19

Notes: † Includes hours worked on average per member in self-employment and outside activities (housework excluded). Households were asked during the endline survey about the no. of hours worked by each member over the past 7 days. Teens include all household members of age 16-20 inclusive. Prime age adults are all members older than 20 years. Households with no teenage or prime-age adults are coded as having zero hours for these potential household members. Data source: baseline and follow-up household survey. Outcome at follow-up is regressed on treatment indicator, baseline covariates, and baseline measure of outcome variable. Coefficients and standard errors (in parentheses). All regressions include a set of unreported pre-treatment covariates. Standard errors are clustered at the village level. ***, **, * indicate significance at 1, 5, and 10 percent, based on p-values unadjusted for multiple-hypothesis testing. ##, #, + indicate significance at 1, 5, and 10 percent when

Source: Baseline and follow-up household surveys and author calculations.

Table 6. Consumption

	(1) Total per capita	(2) Durables	(3) Non-durables	(4) Food	(5) Education	(6) Temptation goods and entertainment	(7) Savings	(8) Household asset index
Treatment	0.109*	0.020	-0.071	0.144**	-0.179	0.213	0.003	0.007
	(0.061)	(0.072)	(0.093)	(0.069)	(0.124)	(0.363)	(0.003)	(0.038)
Observations	611	609	584	609	611	611	611	611
Control mean follow-up	10.95	10.82	10.73	10.34	1.079	1.056	0.0176	0.304

Notes: Outcome at follow-up is regressed on treatment indicator, baseline covariates, and baseline measure of outcome variable. Coefficients and standard errors (in parentheses). All variables measured at the household level. All regressions include a set of unreported pre-treatment covariates. Standard errors are clustered at the village level. ***, **, * indicate significance at 1, 5, and 10 percent, based on p-values unadjusted for multiple-hypothesis testing. ǂǂǂ, ǂǂ, ǂ indicate significance at 1, 5, and 10 percent when correcting for multiple-hypothesis testing. Specifically, in this table we include the variables in columns (2) to (8). Columns (1)-(4) show logs of monthly HH expenditures in tögrög. Columns (5)-(6) show amounts scaled by the mean amount for the control group at baseline. (7): log of the stock of household savings. (8): the index is based on a list of electrical appliances where each asset is given a weight using the coefficients of the first factor of a principal-component analysis. The index is calculated as the weighted sum of standardized dummies equal to 1 if a household owns the durable good.

Source: Baseline and follow-up household surveys and author calculations.

Table 7. Social effects

	(1)	(2)	(3)	(4)	(5)
	Share of kids aged 6-15 in school	Hours worked per child aged 6-15 over the past 7 days in:			Share of teenagers (aged 16-20) in school
		Respondent self-employment activities	Other HH self-employment activities	Total working hrs (any HH business and outside activities)	
Treatment	-0.025	-0.222	-0.818	-1.059	0.031
	(0.027)	(0.385)	(0.517)	(0.629)	(0.035)
Observations	611	611	611	611	611
Control mean follow-up	0.705	0.831	1.419	2.250	0.261

(6)	(7)	(8)	(9)
Received transfers from	Amount received	Transferred money to	Amount transferred
-0.004	-0.001	-0.029	-0.015
(0.038)	(0.003)	(0.038)	(0.019)
611	238	611	611
0.400	0.0327	0.588	0.196

Notes: Outcome at follow-up is regressed on treatment indicator, baseline covariates, and baseline measure of outcome variable. Coefficients and standard errors (in parentheses). All variables measured at the household level. All regressions include a set of unreported pre-treatment covariates. Standard errors clustered at the village level. The exchange rate at baseline was USD 1 to Tögrög 1,150. ***, **, * indicate significance at 1, 5, and 10 percent, based on p-values unadjusted for multiple-hypothesis testing. ~~###~~, ~~##~~, ~~#~~ indicate significance at 1, 5, and 10 percent when correcting for multiple-hypothesis testing. Specifically, in this table we include the variables in columns (1) to (5) and, separately, (6) to (9). Columns (6)-(9): Transfers between the household and family or friends over the past year (in '000s tögrög).
Source: Baseline and follow-up household surveys and author calculations.

Table 8. Impacts of the individual-liability program

	(1)	(2)	(3)	(4)	(5)	(6)
	XacBank	Any loan	Has a self-employment activity		Consumption	
			All HH	*Respondent*	Total per capita	Food
Treatment	0.416***	0.194***	-0.008	-0.019	0.021	-0.013
	(0.060)~~###~~	(0.050)~~###~~	(0.047)	(0.044)	(0.036)	(0.032)
Observations	610	610	610	610	610	606
Control mean follow-up	0.0615	0.504	0.585	0.392	10.95	10.34

Notes: Outcome at follow-up is regressed on treatment indicator, baseline covariates, and baseline measure of outcome variable. Coefficients and standard errors (in parentheses). All regressions include a set of unreported pre-treatment covariates. Standard errors are clustered at the village level. ***, **, * indicate significance at 1, 5, and 10 percent, based on p-values unadjusted for multiple-hypothesis testing. ~~###~~, ~~##~~, ~~#~~ indicate significance at 1, 5, and 10 percent when correcting for multiple-hypothesis testing. Specifically, in this table we include the variables in columns (1) to (6). Table A1 provides the definitions and sources of all variables.

Source: Baseline and follow-up household surveys and author calculations.

Table 9. Liability structure and loan default

	(1)	(2)	(3)	(4)
	Default dummy (>90 days late)			
	First loan		*All loans*	
Joint liability	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)
Observations	327	302	638	612
Pseudo R-squared	0.009	0.321	0.009	0.290

Notes: Default dummy is 1 if a borrower was at least 90 days late in repaying one or more loan instalments. *Joint liability* is a dummy variable that is 1 (0) in case of joint (individual) liability loans. The following additional covariates were included in the probit regressions but now shown (all insignificant): *Age*, *Age squared*, *Buddhist*, *Household size*, *Hahl*, *Collateral value*, *Married*, *Male adults*, *Female adults*, *Children <16*, *Owns fence*, *Owns dwelling*, *Owns vehicle*, *Saver*, *HH crop disaster*, *HH natural disaster*, *HH death*. Standard errors are clustered at the village level and reported in parentheses.***, **, * indicate significance at 1, 5, and 10 percent. Table A1 provides the definitions and sources of all variables.

Source: XacBank administrative data (dependent variable) and baseline household survey (controls).

Table A1. Variable definitions

Variable name	Description	Standard control variable
	Respondent and household (HH) level data. Source: Baseline survey	
Age	Age in years of respondent	X
Age squared	Age in years of respondent squared	X
Assets (stock)	Total value of assets in ('000s MNT)	
At least one loan	Dummy variable that is '1' if the HH had at least one loan outstanding	
Business asset index	Index of a list of four key business assets: tools and machinery, unsold stock, lorry or tractor, riding equipment (stock, not flow). Each asset is given a weight using the coefficients of the first factor of a principal-component analysis. The index is calculated as the weighted sum of standardized dummies equal to 1 if a household owns the durable good.	
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)	
Durable consumption	Total value of durable consumption over the last month in logs MNT	
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 adults	Number of female household members aged 16 or older	X
Food consumption	Total food consumption of the household over the last week in logs MNT	
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 illness	Dummy variable that is '1' if at least one HH member experienced a serious illness in the previous	
HH natural disaster	Dummy variable that is '1' if the HH experienced a natural disaster, e.g. *dzud* , in the previous year	
HH robbery	Dummy variable that is '1' if the HH experienced a robbery in the previous year	
Hours enterprise labor	Total number of hours worked per week by all household members in the respondent's enterprise	
Hours hired	Average number of hours worked per week in peak season by non-HH members in the respondent's enterprise	
Hours wage labor	Total number of hours worked per week by all household members outside the own business in return for a formal wage	
Household asset index	Index of a list of electrical appliances: computer, land-line telephone, mobile telephone, TV, VCR, small electric appliances, large electric appliances (stock, not flow). Each asset is given a weight using the coefficients of the first factor of a principal-component analysis. The index is calculated as the weighted sum of standardized dummies equal to 1 if a household owns the durable good.	
Household size	Number of children and adults in the household	
Joint enterprise	Dummy variable that is '1' if the respondent operates an enterprise together with her spouse	
Loan amount	Loan amount (in 000's MNT)	
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
Male adults	Number of male household members aged 16 or older	X
Married	Dummy variable that is '1' if the respondent is married or living together with partner	X
Months since disbursement	Number of months since the loan was disbursed	
Non-durable consumption	Total value of non-durable consumption over the last month in logs MNT	
No. prior XacBank loans	Number of prior XacBank loans taken by the HH as part of the experiment	
Ownership any business	Dummy variable that is '1' if the HH operates at least one business	
Ownership female business	Dummy variable that is '1' if the respondent operates at least one business herself	
Ownership partner enterprise	Dummy variable that is '1' if the respondent's spouse operates an enterprise but not jointly with the respondent	
Outstanding loans	Number of loans taken by the HH that are still outstanding, conditional on at least one loan outstanding	
Profit any business	Total profits of all household business activities over the last year	
Profit female business	Total profits of respondent-owned business activities over the last year	
Total consumption	Value of total household consumption over the last year in logs MNT	
Total income	Total annual income of the household in the previous year in logs MNT	
Transfers given family	Value of monetary and in-kind transfers given in last 12 months to relatives (in 000's MNT) conditional on giving	
Transfers received family	Value of monetary and in-kind transfers received in last 12 months from relatives (in 000's MNT) conditional on receipt	
Transfers given friends	Value of monetary and in-kind transfers given in last 12 months to non-relatives (in 000's MNT) conditional on giving	
Transfers received friends	Value of monetary and in-kind transfers received in last 12 months from non-relatives (in 000's MNT) conditional on receipt	
Wage earnings	Average weekly wage earnings for wage earners (in 000's MNT)	
	Village-level data. Source: Village survey in Spring 2008 and baseline survey	
Buddhist %	Percentage Buddhist households in the village	
Crop disaster %	Percentage of households in the village that experienced a crop disaster in the previous year	
Dairy village	Dummy variable that is '1' if dairy production is among the three main business activities in the village	
Death %	Percentage of households in the village that experienced a death in the previous year	
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	
District area	Total surface are of the district in km2	
Doctors in district	Number of doctors in the district (including the village)	
Felt village	Dummy variable that is '1' if felt production is among the three main business activities in the village	
Households in district	Number of households (nuclear families) living in the district (including the village)	
Households in village	Number of households (nuclear families) living in the village	
Illness %	Percentage of households in the village that experienced an illness in the previous year	
Job loss %	Percentage of households in the village that experienced a job loss in the previous year	
Livestock in district	Number of livestock (cattle, camels, horses, sheep, goats) in the district (including the village)	
Money transfers %	Percentage of households in the village that engaged in money transfers (receiving or giving)	
Number of loans	Average number of loans received by the respondents in a village	
Number of months	Average number of months between the date when respondents in a village received the first loan and the follow-up survey	
Over 60	Average number of household members over 60 in the village	
People in district	Number of people living in the district (including the village)	
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 litre of milk (in MNT)	
Primary schools district	Number of primary schools in district (including the village)	
Robbery %	Percentage of households in the village that experienced a robbery in the previous year	
SCCs in district	Number of Savings and Credit Cooperatives in the district (including the village)	
Secondary school teachers	Number of secondary school teachers in the district (including the village)	
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	
Under 16	Average number of household members under 16 in the village	

Notes: This table provides the names and definitions of the variables used in the empirical analysis in alphabetical order. MNT= Mongolian tögrög.

Table A2 The loan products and their use

Progressive traits	Larger loans, lower interest rate, and longer maturity after each successful repaid loan	
Monthly interest rate	1.5 to 2 percent	
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	
	Individual loans	Group loans
Liability structure	Individual liability	Joint liability
Collateral	Yes but flexible approach	Joint savings (20 percent of loan) sometimes supplemented by assets
Average maturity 1st loan	224 days	199 days
Average maturity 2nd loan	234 days	243 days
Average size 1st loan	$411	$279
Average size 2nd loan	$472	$386
Percentage of 1st (2nd) loans that are mainly used for:		
- *Other business expenses*	51 (47)	57 (37)
- *Other household expenses*	28 (19)	28 (22)
- *Mixed expenses*	12 (8)	14 (17)
- *Education*	8 (7)	6 (6)
- *Purchase tools/machinery*	6 (3)	6 (1)
- *Purchase livestock*	9 (2)	4 (5)

Notes: This table describes the main characteristics of the individual and group loans. Average loan size is 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 USD. Source of data on maturities and loan size: XacBank. Source of data on loan use: follow-up survey (borrowers could indicate multiple loan purposes).

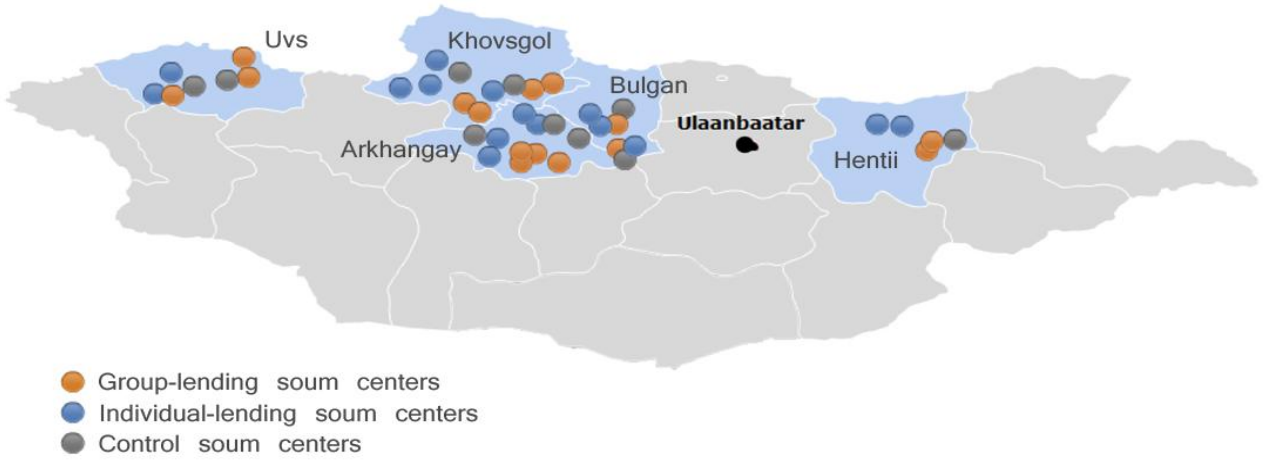
Source: XacBank.

Table A3. Attrition

	Dependent variable: HH attrited between baseline and endline					
Joint-liability treatment	0.035	0.032	0.037	0.032	0.033	0.021
	(0.060)	(0.061)	(0.062)	(0.062)	(0.062)	(0.064)
Household composition	No	Yes	Yes	Yes	Yes	Yes
Access to credit (dummy variables)	No	No	Yes	Yes	Yes	Yes
Access to credit (amounts)	No	No	No	Yes	Yes	Yes
Employment activities	No	No	No	No	Yes	Yes
Consumption variables	No	No	No	No	No	Yes
Observations	710	710	687	681	673	652
F-Stat (test of joint significance) - incl treatment		12.34	22.08	25.65	43.95	48.03
Prob>F		0.090	0.024	0.012	0.028	0.026
F-Stat (test of joint significance) - excl treatment		12.15	21.98	25.46	43.88	47.84
Prob>F		0.059	0.015	0.008	0.021	0.021

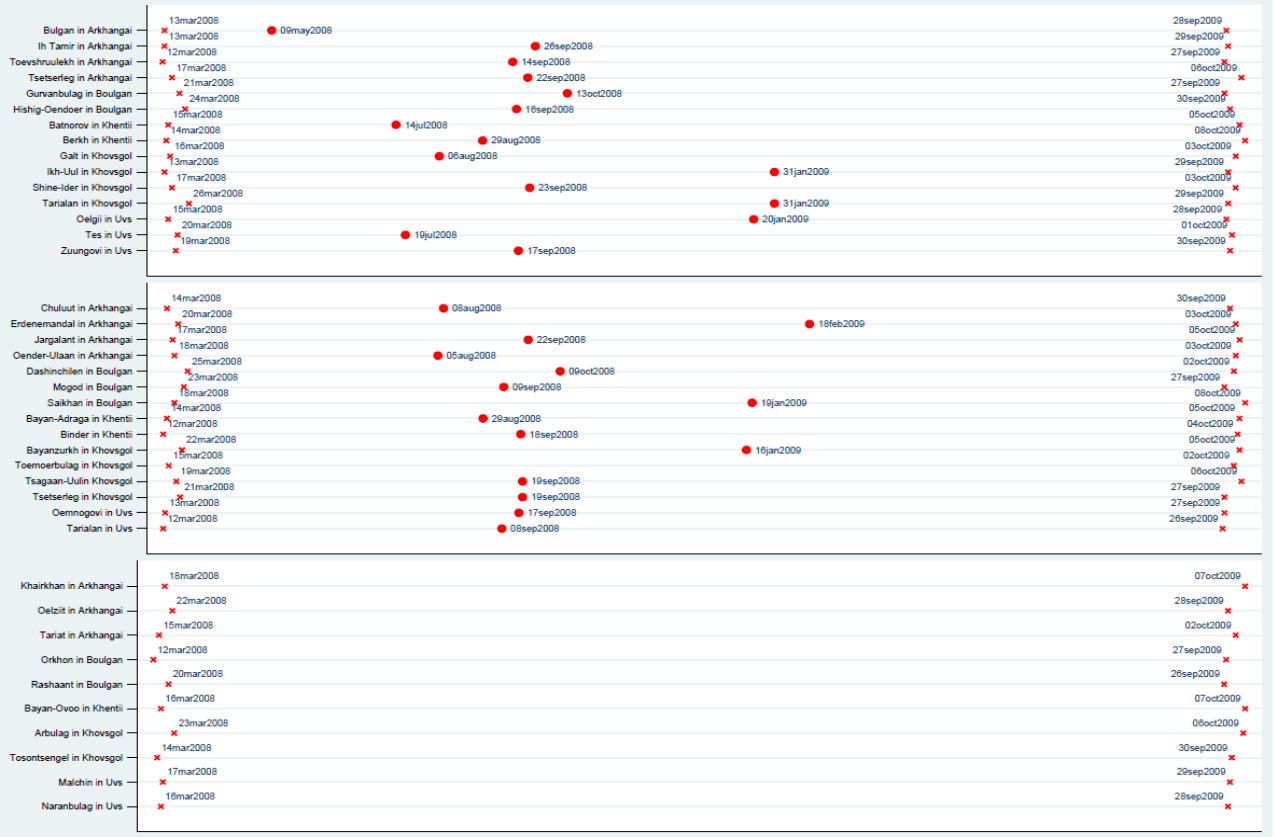
Notes: Unit of observation: household. Coefficients and robust standard errors (in parentheses) from a probit regression where the dependent variable is an indicator of whether the household attrited or not.
Source: Baseline and follow-up household surveys and author calculations.

Figure A1 Overview of the participating villages and provinces



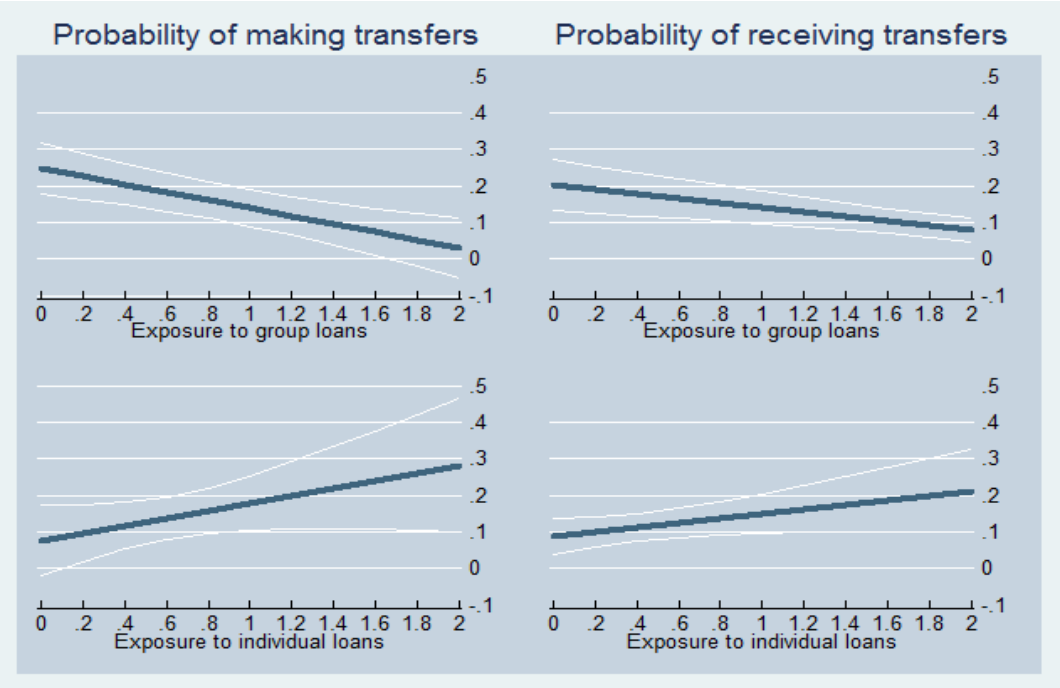
Notes: This figure shows the geographical location of the 10 control soum centers (villages) as grey dots, the 15 individual-lending villages as dark blue dots, and the 15 group-lending villages as orange dots across the five Mongolian provinces (highlighted in light blue) where the experiment took place.

Figure A2 Timeline of the experiment



Notes: This figure shows the roll out of the experiment across the 15 group-lending villages (upper panel), 15 individual-lending villages (middle panel), and 10 control villages (lower panel). On each line, the red cross on the left-hand (right-hand) side indicates the day that the baseline (follow-up) interviews started in a village. The red dots indicate the median disbursement date for first-time loans in each village.

Figure A3 Treatment intensity and informal transfers to friends



Notes: These graphs show the probability of making (left) or receiving (right) informal transfers to and from friends for an average respondent in the group-lending (top) and individual-lending villages (bottom) as a function of the average number of XacBank loans taken by respondents in the village. The blue lines indicate the expected probability while the white lines indicate a 95 percent confidence interval.