Microcredit and Informal Risk Sharing: Experimental Evidence from the Village Banking Program in China*

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Abstract

This study examines the impacts of a large-scale government-led microcredit program on informal risk sharing among poor households in rural China using a randomized controlled trial. The results show that access to microcredit reduced informal borrowing for an average household in treatment villages. In particular, informal borrowing decreased substantially for program members, regardless of whether or not they had borrowed from the program. Further analyses suggest that the program alleviated households' dependence on informal borrowing to deal with consumption shocks for program members who did not borrow from the program. Meanwhile, for such households, the crowding-out effect on informal borrowing existed even during the program's announcement period. These results are consistent with the theoretical prediction that access to microcredit raises the *expected* utility of autarky relative to that derived from risk-pooling arrangements, and thus reduces implementable risk-sharing contracts.

Keywords: financial inclusion, informal financing, risk-sharing network, limited commitment

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1. Introduction

The interactions of formal and informal institutions have received considerable attention from researchers and policymakers.[1] In particular, scholars are increasingly interested in investigating the interplay between formal financial devices and informal networks.[2] While informal risk-sharing is widely observed throughout the developing world (Platteau and Abraham, 1987; Udry, 1994), it is well recognized that these arrangements are far from perfect for insuring risks (Townsend, 1994; Fafchamps and Lund, 2003). Motivated by the inadequacy of the informal arrangements, governments and commercial organizations have introduced formal financial institutions in these areas. Therefore, it is important to understand how informal networks react to such financial initiatives and what the barriers are to the efficient allocation of the informal arrangements. To this end, this study aims to examine the impacts of access to a microcredit program on informal borrowing and detect the barriers to perfect risk sharing in informal networks.

It is *ex ante* unclear whether a microcredit program will crowd in or crowd out informal borrowing. On the one hand, informal borrowing could increase if resources leak through the networks because of the altruistic considerations (Becker, 1993) or the repeated interactions associated with the intervention enhance economic cooperation (Feigenberg, Field, and Pande, 2013). On the other hand, informal borrowing may decrease if access to credit raises the capacity of self-insurance. The ambiguity of the impact requires empirical evidence, which is still quite limited at present.

This study contributes towards filling the gap by leveraging a randomized controlled trial with the phased rollout of a microcredit intervention in China to investigate its impact on households' informal borrowing. Specifically, the China's central government initiated a village banking program in 2006 to reduce poverty in rural areas. Under the program, each designated poor village received 150,000 *yuan* (US $24,000) as a revolving village credit fund. Authorized

[1] See Bau (2021) for an example of the interaction between social pension plans and family-based old age support, and see Gruber and Hungerman (2007) for another example of the interaction between public sector spending and private charitable contributions. For a more comprehensive review of the literature on the interaction between formal and informal institutions, see Cox and Fafchamps (2007).
[2] See below for details of the literature review.

randomized controlled trials (RCTs) were conducted in ten counties of five provinces to evaluate the impacts of the program. The randomization was conducted by researchers on the village level, with three rounds of surveys to collected detailed information from households and villages—a baseline survey in August 2010 just before the intervention and two follow-up surveys in July 2012 and 2014, respectively. For five out of the ten counties, the control villages turned to be treated after the second wave of the survey, whereas the treatment status remained unchanged for villages in the other five counties throughout the study period.

According to the estimates of the average intention-to-treat effect using data from the first two waves, the likelihood of taking up informal interest-free loans for households in treatment villages are, on average, significantly less than that for households in the control villages. The results are robust if we use the frequency or amount of informal interest-free loans as alternative outcome variables, or include transfers as well.

To examine the mechanisms of the crowding-out effect on informal borrowing, the study uses a simple model of risk sharing with limited commitment, which provides the following predictions:

i. *Anticipation* of access to credit in a future period when the agent expects she will be credit constrained will increase the expected utility of autarky relative to the utility derived from risk-pooling agreements, thus reducing the scope of implementable risk-sharing contracts (or informal borrowing).
ii. In such case, anticipation of access to credit will reduce the reliance on informal borrowing to insure against idiosyncratic shocks.
iii. Consequently, anticipation of access to credit will *increase* overall decision utility, although it crowds out informal borrowing.

Guided by the conceptual framework, the study then empirically examines these predictions.

In a similar vein to Miguel and Kremer (2004), the study exploits the rollout of the program to identify households that would counterfactually borrow, versus those that would not, in control villages treated at a later stage, and hence estimates the effects for borrowers versus non-borrowers. The results indicate that the crowding-out effect of the program on informal borrowing exists not only among households that had borrowed from the program but also appears among program members who had not borrowed from the program. This implies that the crowding-out effect on informal borrowing was not simply due to the substitution effect of extra credit from the program. Instead, it coincides with predictions of limited commitment models that access to—not necessarily taking up—village banking loans will reduce informal borrowing.

To investigate whether the reduction in informal borrowing as a result of access to program loans was due to decreases in dependence on informal borrowing to insure against idiosyncratic shocks, the study examines the interaction effect of access to the program and health shocks—the most common adverse shock—on informal borrowing. Consistent with prediction of the conceptual framework, the results show that the incidence of taking up interest-free loans was positively associated with medical expenses in the absence of the village banking program, whereas being in the treatment villages significantly reduced the likelihood of taking up informal interest-free loans as a response to health shocks—an effect driven mainly by program members who did not borrow from the village banks.

These results indicate that *anticipation* is a salient channel by which the program raises the value of autarky and thus reduces households' engagement in risk-sharing contracts, which offers us a smoking gun of limited commitment among risk sharing of informal networks. The study then provides several pieces of direct evidence regarding the role of anticipation. First, the data demonstrates that among the program members, most of those who did not borrow from village banks by the time of the second survey reported they had joined the program because they "did not need credit right away, but maybe later," indicating that these households had expected that they could borrow from the village bank at some point in the future. Second, using information from the third wave of the survey, we actually observe that a substantial proportion of these households turned to borrowing from the village banks after the second survey. Lastly, and most importantly, the results show that the program reduced informal borrowing even during its announcement period for program members who did not borrow from the village banks.

Alternative explanations for the reduction in informal borrowing are examined and ruled out, including decreases in the supply of informal credit and decreases in credit demand due to increases in income. Specifically, using information from answers to hypothetical questions on available credit in an emergency to isolate the credit demand from its supply, the study finds no evidence on any decrease in access to informal credit. Meanwhile, the crowding-out effects are robust by controlling for households' income, indicating that the decrease in informal borrowing is unlikely due to an income effect.

Additional analyses suggest that the crowding-out effect on taking up informal interest-free loans persists over a longer period of about four years after the intervention, and the effect seems stronger than that in the short run when we account for both interest-free loans and transfers. Although the role of informal financial networks to insure against risks worsened, an examination of the impact on subjective well-being reveals significant welfare gains for program members. This coincides with the prediction that the reduction in informal borrowing as a result of access to microcredit was driven by less desire for risk pooling and was associated with an increase in decision utility.

The study contributes to the extant literature in several aspects. First, the study is closely related to research on how microcredit programs affect informal networks. Feigenberg, Field, and Pande (2013) find that repeated social interactions in group meetings of a microcredit program can facilitate economic cooperation among group members. Banerjee et al. (2021) examine how the structure of social networks changes in response to exposure to formal credit markets, and find a decrease in links between households that are unlikely to borrow from microfinance due to global network externality. That is, a shift in incentives of one group of people to form links can have spillover effects on the incentives of people in other parts of the network to form links when the returns to social interaction depend on who else is socializing. Consistent with Banerjee et al. (2021), this study also finds that access to microcredit crowds out informal loans even among non-borrowers of the microcredit program. It complements the extant studies by showing that access to microcredit may have a *direct* effect on non-borrowers by changing their expected utility of autarky.

Second, this study builds on the literature that examines the barriers to informal risk sharing. Several theories have been proposed to explain the incomplete insurance within risk-sharing networks, such as limited commitment (Coate and Ravallion, 1993; Ligon, Thomas, and Worrall, 2000; Attanasio and Rios-Rull, 2003; Dubois, Jullien, and Magnac, 2008), imperfect information (Lim and Townsend, 1998; Attanasio and Pavoni, 2011; Karaivanov and

Townsend, 2014), and transaction costs (Jack and Suri, 2014). Whereas plenty of studies shown empirical evidence that rejects full risk-sharing (e.g., Mazzocco and Saini, 2012), many of them found results confirming that limited commitment plays a significant role as an impediment to efficient risk sharing (Foster and Rosenzweig, 2001; Ligon, Thomas, and Worrall, 2002; Fafchamps and Lund, 2003). Leveraging a randomized controlled trial with the phased rollout of access to a village banking program, the study is able to test efficient risk sharing without long panel data that was usually required in previous studies. Meanwhile, it complements the literature by highlighting the role of anticipation on testing limited commitment models, which relies on the notion that anticipating relaxation of credit constraint in the future may increases in the expected utility of autarky and thus reduces implementable risk-sharing contracts.

Third, this study also contributes to a broader literature that investigates the effect on informal risk sharing of access to development projects (Heß, Jaimovich, and Schundeln, 2021) and financial initiatives, such as cash transfers (e.g., Albarran and Attanasio, 2003), savings accounts (e.g., Dupas, Keats, and Robinson, 2019), and formal insurance (e.g., Cai, 2016).[3] The study adds to the literature by providing experimental evidence of the impact on informal risk sharing of access to microcredit interventions among only a few other studies (Feigenberg, Field, and Pande, 2013; Banerjee et al., 2021).[4]

The remainder of this paper proceeds as follows. Section 2 introduces the program, the experiment and survey design, and the data. Section 3 describes the empirical strategies and the regression specifications. Section 4 presents the main results. Section 5 outlines a brief conceptual framework to guide the mechanism analysis. Section 6 investigates the mechanisms empirically. Section 7 contains the results of longer-run impacts and welfare implications. Finally, Section 8 concludes.

2. Program and Data

[3] Earlier studies that examined the interaction between formal and informal financial devices concentrated mainly on the suppliers of credit (e.g., Bose, 1998; Jain, 1999; Guirkinger, 2008; Gine, 2011). See Karlan and Zinman (2019) for a recent example that examined the demand and competitor responses to price changes of a microlender in Mexico using experiments.
[4] The paper is also linked to experimental studies on microcredit, which generally find little evidence that microcredit reduces informal loans (see Banerjee, Karlan, and Zinman (2015) for the summary). However, the impacts on informal borrowing are not the focus of these studies.

2.1. The Village Banking Program

As in many other developing countries, formal financial institutions in China locate their branches primarily in cities, whereas many rural people—particularly those in poor areas—lack access to formal credit due to the absence of collateral and high transaction costs. To expand financial inclusion and eradicate poverty for households in these poor rural areas, China's national government initiated a village banking program in 2006 based on international experience.[5] The program was promoted as an attempt to cultivate self-managed organization to deliver sustainable credit services to households in poor villages so the inhabitants could support themselves by investing and growing their business operations and improve their lives.[6]

Each program village received start-up funds of 150,000 *yuan* (US $24,000) from the national government, which formed the initial capitalization of the village banks. These village banks are self-managed organizations supervised by the Poverty Alleviation Office of county governments. The highest authority of the village bank is the general meeting of all members of the program, and the bank's administration is in the hands of self-elected councils which create bylaws, manage the funds, and conduct bookkeeping.

According to the practical guidance proposed by officers of the national government, the village bank loans should be used for income-generating activities. The interest rate should be discussed and decided by a general meeting in accordance with the principle of covering the operation costs. It can be determined according to the prevailing market interest rate. The village bank loans do not require collateral, but should rely on guarantees to share liability for repayment. The loan period should not exceed 12 months.

To access the funds from the village bank, households should participate in the program by formally registering as members of the village bank. There is no substantive requirement to be eligible for being a program member, except that the representative of a household should

[5] First introduced by FINCA, village banking is one of the most influential microfinance methodologies in the world. It is rooted in the idea of giving small loans to a group of poor people who share joint liability for eliminating the need for collateral (the primary obstacle to borrowing from formal financial institutions). The village banking methodology had also been adopted by other governments, such as Indonesia's KUPEDES village banking program and the Thai Million Bath Village Fund program. Different from practices in many other places, village banks in China do not provide saving services.

[6] Most previous poverty alleviation programs initiated by the China's government targeted villages or counties and were managed by the local government. They proved to be unsuccessful due to neglecting households' incentives. The village banking program had the advantage of mobilizing households' participation and eliminating market failures by using local information.

be 18 years old or above, capable of working, and be a regular resident of the village. Meanwhile, program members need to submit a lump-sum membership fee of 200 *yuan* (US $32), which is discounted or waived for poor households.[7] To take up loans from the village bank, the program members need to submit an application to the administrative council for approval. The council makes a final decision based on the stated purpose, loan size, and duration. In practice, the vast majority of applications are approved as long as they follow the general guidance.[8]

The program began on a trial basis in 100 villages in 14 provinces, and then gradually scaled up across the country. By the end of 2013, the program had covered 19,397 villages in 28 provinces and the total funds distributed through the program had reached 4.5 billion *yuan* (US $723 million) according to the Yearbook of China's Poverty Alleviation and Development (2014).

2.2. Experimental Design

To evaluate the village banking program, an RCT authorized by the State Council Leading Group Office of Poverty Alleviation and Development was implemented beginning in September 2010. Specifically, the experiment covered five provinces, including one province in Eastern China (Shandong), two provinces in Central China (Henan and Hunan), and two provinces in Western China (Sichuan and Gansu). Each province recommended two counties that had not previously implemented the program to serve as study sites. Figure 1 depicts the geographic location of these provinces and counties. In each of the 10 counties, five designated poor villages were recommended by the county officials as being eligible for inclusion in the RCT.[9] The research team randomly chose three of the five eligible villages for treatment and left the other two villages serving as the control group. There was a total of 30 treatment villages and 20 control villages.[10]

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[7] The membership fee is refundable (without interest) when households withdraw from the program, except that they default on program loans.

[8] Only one household in the data reported that their application had been rejected by the administrative council. It is worth mentioning that the guidance on the terms of loans and the criteria for being eligible to participate in, and borrow from, village banks did not change during the study period.

[9] By comparing the characteristics of the study sample and those of the universe using data from the 2007 China Agriculture Census, Cai et al. (2020) shown that the chosen counties were representative of the designated poor counties in each of five provinces and the sampled villages were representative of the poor villages in each county as well.

[10] It is worth noting that these villages are relatively far from each other—an average distance of 14.5 kilometers. Given the poor transportation conditions in these areas, it is very unlikely to have spillover effects between treatment and control groups.

The treatment status of the villages remained unchanged until the second wave of the survey (i.e., July 2012). After that, 10 control villages from five counties in three provinces (i.e., Henan, Sichuan, and Gansu) started initiating the village bank program as well, whereas the treatment status of the villages in the other five counties did not change until July 2014, the time of the third wave of the survey. Figure 2 illustrates the timeline of the experiment.

With the design of the RCT, the average intention-to-treat effect (hereafter referred to as the average treatment effect) of the program can be estimated unbiasedly by comparing the outcomes of *all* sampled households in the treatment villages and those in the control villages (i.e., "A" versus "B" as demonstrated in Figure 3) by the time of the second survey.

In addition, with the phased rollout out of the program, one can identify the latent program borrowers (i.e., group "F" in Figure 3) in the control villages that were treated at a later stage using information on those who would borrow from the program when their villages later became eligible. This allows us to obtain estimates of the average treatment effect (ATE) on program borrowers by comparing the "C" and "F" groups of households. Similarly, one can estimate the ATE on program members who did not borrow from the village banks and the ATE on non-members by comparing the "D" and "G", and the "E" and "H" groups of households, respectively. I will discuss the validity of the identification in detail in Section 3.2.

Lastly, the experimental design also allows us to estimate the longer-run effect of the program about four years after the intervention by comparing the average outcomes measured in the third survey of households in villages that treated in the first round and those in villages that remained untreated by the time of the third survey.

2.3. Survey and Data

In August 2010, prior to the initiation of the village banking program, a baseline survey was conducted in both the treatment and control villages (see Figure 2). Specifically, the research team randomly selected 30 households in each village and collected detailed

Relatedly, as the program scaled up, there were, on average, two other out-of-sample villages in the same township that implemented village banks by the time of the third wave of the survey. They were usually established after the second wave of the survey. Meanwhile, they were also relatively far from the in-sample villages, with an average *minimum* distance of 10 kilometers to the control villages and 7.7 kilometers to the treatment villages. Therefore, the potential spillover effects from out-of-sample village banks could be very small, if any.

information using household and village questionnaires.[11] Particularly relevant to this study, the survey collected information on every loan taken up by the households between January 2009 and July 2010, including the timing of borrowing, the loan amounts, sources of borrowing (i.e., loans from formal financial institutions, informal interest-bearing loans, and informal interest-free loans), the reason for borrowing (i.e., consumption or production), length of the loan term, whether collateral was needed, and the type of collateral if it was required.[12] In total, 1,500 households in 50 villages were surveyed at baseline.

Afterwards, during August 2010 to the end of the year, the village banking program was initiated in the treatment villages. This included a training session for cadres from all program villages, separate information sessions to introduce program objections and rules to the villagers, and elections of organizing committees to draft charters, approve membership applications, and organize the first general meeting of all program members to elect an administrative council and a supervisory board for the village bank.

Starting in January 2011, the treatment villages gradually began to disburse loans, and by June 2011, most treatment villages had done so, except for the villages from one county.[13] In July 2012, the research team re-interviewed the households and collected information similar to the baseline survey, as well as information on the village banking program. In particular, regarding the information on loans, the second wave of the survey recorded all loans taken up between August 2010 and June 2012, including those borrowed from village banks. A total of 1,351 households in the 50 villages were successfully followed in the second survey, which consisted of the sample used in the main analysis below.

[11] To ensure random selection of household within village, the research team exploited the stratified systematic sampling method. Specifically, the investigators randomly chose one natural village (a subunit of administrative villages that exists spontaneously) from a list of all natural villages in the administrative village ranked by per capita income. Then, they randomly selected 30 households from a complete list of households in the natural village ranked by per capita income. The evidence below in Section 2.5 confirms the random sampling of households within the village.

[12] In the questionnaire, the purpose of borrowing is categorized into more disaggregated items, which are grouped into production loans (e.g., purchasing fertilizer, purchasing other input in agriculture, purchasing animal husbandry, and conducting business) and consumption loans (e.g., purchasing food, purchasing daily necessities, purchasing a car or other durable goods, paying for medical expenses, paying for expenses on weddings and funerals, paying for expenses on education, paying for expenses of building a house, and paying for expenses of giving birth to a child). Results below that separately examine the impacts on production loans and consumption loans are barely changed if we take borrowing for educational expenses as production loans.

[13] Villages in one county in Hunan Province did not deliver loans until June 2012. The financial officers in charge of distributing the program fund were busy training newly recruited staff at that time, which affected implementation of the program in that county. The late implementation in the county may tend to make the estimates *understated*. However, if anticipation serves as a salient channel, as evidenced below, the estimates may not be seriously biased. Actually, the results are quite similar if the sample in the county are excluded from the analyses.

The 10% attrition rate of households occurred mainly due to the temporary absence of all household members at the time of the second survey (e.g., visiting relatives) or the migration of entire households. It is worth mentioning that there is no significant difference in attrition between the treatment and control groups. Specifically, the attrition rates of the control and treatment groups are 9.5% and 10.2%, respectively, whereas the difference is statistically insignificant (p=0.660). Meanwhile, an examination of the balance of pre-program borrowing and the baseline household characteristics between the treatment and control groups in the analysis sample shows that the normalized differences between the treatment and control groups are all less than one-tenth and are not statistically significant. The omnibus test of joint orthogonality further confirms that the treatment and control groups are well balanced on pre-program borrowing and household characteristics in the analysis sample. See Table A1 in the online Appendix for details.[14]

In July 2014, the research team conducted the third wave of the survey. As described earlier, 10 control villages from three provinces were treated after the second wave of the survey. Therefore, one can observe which households in these villages participated in the program and took up loans from the village banks based on the third wave of the survey. These households were used as comparison groups for program participants and borrowers that were observed from the second wave of the survey in villages treated in the first round.

In addition to the household survey, the research team also collected information on villages using a village questionnaire in each wave. The information includes the type of geographic features, population, area of arable land, and public spending financed by superior governments on various projects, among others. In the two follow-up waves of the survey, the village questionnaire also included questions on the implementation of the village banks.

Lastly, besides the survey data, the study also uses the administrative data from the village banks, which includes detailed information on all loans lent by the village banks by the time of the second survey.

[14] Relatedly, Table A2 in the online Appendix compares the village-level baseline characteristics of the treatment and control groups. The results show that none of the differences between the two groups is statistically significant when examined individually, although some of the normalized differences are closed to or over one quarter and they are jointly significant at the 90 confidence level (p=0.08). To take account of the differences, I control for these village-level baseline characteristics in the regression analyses. See Section 3.1 below.

2.4. Risky Environment and Pre-Program Borrowing

Before going ahead to present the empirical strategy for identifying the program impacts on informal risk sharing, it is important for us to understand the nature of risky environment and the coping strategies of households before the intervention.

Table 1 shows that exposure to adverse shocks are quite common among the households. As reported, about two-thirds of the households suffered health shocks in 2009, which cost an average of 5,248 *yuan*, or one-third of the households' annual income. For some other events, although they could somehow be anticipated, the extraordinary amount of the expenses usually made it difficult for households to completely smooth their consumption due to credit constraints. For instance, over half of the households had one or more students, with an average educational expenditure of 3,900 *yuan* per year. Although the frequency of building a house or giving birth to a child was relatively small, the expenses were considerably greater than the other expenses. In terms of production shocks, 38 percent of the households had a poor harvest in 2009, and 8 percent experienced a loss of domestic animals with an average monetary value of 2,523 *yuan*. Taken together, 92 percent of the households suffered at least one adverse shock described above in 2009. The average monetary value was estimated at 11,778 *yuan*, or about half of the household annual income.

To examine the extent to which the shocks are idiosyncratic, column (4) reports the village-level variance for the occurrence of the shocks as a percentage of the total variance, which was measured as the R-squared of regressions of these shocks on the village dummies. The lower the value, the more idiosyncratic the shock will be. As indicated by the estimates, the idiosyncratic component accounts for 86-95 percent of the variance in adverse shocks. This is comparable to other studies (Udry, 1990; Townsend, 1995; Dercon, 2002; Morduch, 2005). For example, Morduch (2005) reports that idiosyncratic shocks contributed to 75-96 percent of the total variance of household income in south India. The results also reveal that consumption shocks are more idiosyncratic than shocks to agricultural production such as crop farming.[15]

[15] To provide additional evidence that consumption shocks are more idiosyncratic than production shocks, Figure A1 in the online Appendix depicts the month of borrowing that occurred in 2009. As shown, production loans were taken up mainly during March and April—the lean season for agriculture. In contrast, the occurrence of consumption loans was almost equally distributed across every month of the year, except for the months around the beginning of spring and fall school semesters (i.e., March and September, respectively) which may reflect an excess demand for credit to finance educational expenses.

Informal borrowing was widely observed as a main risk-coping strategy besides self-insurance (Dercon, 2002). Table 2 presents households' incidence and amount of borrowing from various sources between January 2009 and July 2010. As shown in Panel A, some 59 percent of the households took up loans during this period. The loans were mainly from informal sources. Specifically, more than half of the households took up informal interest-free loans, whereas only 13 percent borrowed from formal financial institutions and 4 percent took up informal interest-bearing loans. The results also show that consumption loans are more common than production loans, particularly among informal interest-free loans. As presented, 42 percent of households borrowed informal interest-free loans for consumption purposes, while only 16 percent borrowed for production purposes from the same source.[16] The results are consistent with prior studies that find informal borrowing from relatives and friends is mainly for consumption purposes, whereas investment is financed mainly by formal financial institutions (Fafchamps and Lund, 2003; Guerin et al., 2012).[17]

Panel B reports the median of loan amounts among households who ever took up a given type of loans between January 2009 and July 2010. As shown, despite the fact that informal interest-free loans are more common than loans from other sources, their sizes are relatively small. Specifically, the median amount is 5,000 *yuan*, which is only half the size of the loans from formal financial institutions. A separate examination of production loans and consumption loans shows similar results. Lastly, the statistics also indicate that the size of a production loan is usually smaller than a consumption loan.

Overall, the results in Table 2 suggest that informal interest-free loans are more common than other types of loans, but are used mainly for smoothing consumption rather than investment. Moreover, the results indicate that extant formal financial institutions play a limited role in meeting financial demand for household production in the poor rural areas surveyed in the sample. That is exactly the reason why the China's government initiated the village banking program in these areas.

[16] See Table A3 in the online Appendix for the distribution of consumption loans categorized in more disaggregated items.

[17] One explanation of why informal borrowing is less common for investment than consumption is related to the differences between consumption and production shocks. As evidenced above, while consumption shocks are usually idiosyncratic among households from the same village, production shocks are mostly common to all households in the same village. Therefore, it is harder to insure production shocks through safety nets within the village. Meanwhile, since failed investments can undermine informal insurance arrangements, this may hinder profitable but risky investments funded by informal borrowing (Lee and Persson, 2016).

2.5. The Terms of Loans

To understand the interplay between microcredit initiatives and the informal borrowing, it is necessary to be aware of the differences between the characteristics of program loans and loans from various sources on the existing credit market. Table 3 conducts such a comparison in columns (1) to (4). To check whether the sampling of households within a village is random in the survey, the table also compares the statistics of program loans based on the household survey data (column 4) and the administrative data from the village banks (column 5).[18]

As shown in column (4), the average size of program loans is 3,795 *yuan*, which is considerably small compared to loans from other sources. This may not be surprising, given that most village banks had set a maximum loan size of 5,000 *yuan*. The average annual nominal interest rate of the program loans is 9.8 percent. It is the same as the interest charged by the formal financial institutions, indicating that the village banks indeed followed the guidance of the program in practice. The average length of time to maturity of the program loans is eight months, which is slightly shorter than that of the formal loans or the informal interest-free loans. None of the program loans require any physical collateral, but they do usually rely on social collateral such as guarantees, most of which are in the form of joint liability in a group lending.[19]

To test the hypothesis of the random sampling of households in the villages, we compare the statistics for the terms of program loans calculated based on the household survey data to those based on the administrative data which include loans received by all clients of the village banks over the same period as recorded in the household survey. As shown, the statistics reported in column (5) are very similar to those reported in column (4). The results confirm the representativeness of the surveyed households in the treatment villages.

3. Empirical Strategy

[18] The statistics of terms of loans from other sources reported in columns (1) to (3) are also based on the household survey data. To account for the possibility that the program might affect the terms of loans from formal financial institutions and informal sources, I focus only on loans taken up by households in the control villages.

[19] To examine the geographic variance of the loan terms, the table also reports the standard deviations of loan characteristics within and across villages in parentheses and brackets, respectively. As expected, the terms of the program loans vary much less within the same village than those that vary across villages. In contrast, for the terms of loans from other sources, the within-village variations are about the same as, or sometimes even larger than, the across-village variations.

3.1. Average Treatment Effect

To estimate the average treatment effect of the program on households' informal borrowing, I employ the following regression equation:

$$y_{ij,2} = \alpha + \beta T_j + \lambda y_{ij,1} + \Gamma V_j + \varepsilon_{ij}, \tag{1}$$

where i denotes a household and j denotes a village; $y_{ij,2}$ is the incidence of borrowing—for the main outcome of interest, it measures whether the household borrowed any informal interest-free loans between August 2010 and June 2012 covered by the second survey; T_j is an indicator of village treatment status in the first round. The baseline outcome ($y_{ij,1}$) and a set of village-level characteristics (V_j) are controlled to account for possible baseline differences and to improve the precision of the estimation, where the latter includes the type of geographic features, population, area of arable land, and public spending financed by superior governments on various projects.[20] The error term ε_{ij} is clustered at the level of randomization unit (i.e., village) in accordance with Abadie et al. (2017). For the main results, I also report p-values based on cluster bootstrap-t procedures, which are robust to distortions in the inference for a small number of clusters (Cameron, Gelbach, and Miller, 2008).[21]

The parameter of interest, β, indicates the average treatment effect of the program on the incidence of borrowing for *all* households in the treatment villages. It can be estimated unbiasedly as long as the assignment of treatment among villages is random. In the main text, I report estimation results of Equation (1) using the ordinary least squares (OLS) method, although the estimates are robust when I use nonlinear estimation method. See Section B2 in the online appendix for details.

Besides the extensive margin of borrowing, I also examine the program impacts on two intensive margins of borrowing, i.e., the frequency and amount of borrowing.[22] For a robustness check, I incorporate private transfers as well as informal borrowing when measuring

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[20] According to McKenzie (2012), a specification like Equation (1) is more efficient than the difference-in-differences or the simple difference in post-treatment estimations. Section B1 in the online appendix shows that the results are robust to the inclusion of the control variables.

[21] The number of clusters in the estimation sample of the study vary between 40 and 50, which may not be large enough. Yet they are larger than the conventional rule of thumb that Cameron, Gelbach, and Miller (2008) considered as few clusters (i.e., five to thirty).

[22] Whereas the estimates of the impact on the extensive margin of informal borrowing provide lower bounds for the impact of the program on informal risk sharing (i.e., exist or enter the informal arrangements), the estimates of the impact on the frequency and amount of borrowing also include the impact on the inframarginal changes in informal risk sharing.

the informal risk-sharing arrangements. Meanwhile, I replace the outcome with the variable that was measured in the third wave of the survey to investigate the longer-run effect.

3.2. Treatment Effect on (Non-)borrowers

To further examine the average treatment effect on households that borrowed from the program as well as the average treatment effect on those program members who were non-borrowers and those who were non-members, I follow Miguel and Kremer (2004) and identify the latent borrowers (non-borrowers or non-members) in the control group taking advantage of the phased rollout of the village banking program.

Specifically, $y_{ij,2}^{1}$ is defined as the outcome of interest measured in the second wave of the survey for household i in village j if it had access to the village banking program, and $y_{ij,2}^{0}$ is the counterfactual outcome if the household had not have access to the program. The average treatment effect on program borrowers (ATE-borrowers) can then be written as $ATE_B = \mathrm{E}\left(y_{ij,2}^{1} | T_j = 1, B_{ij,2} = 1\right) - \mathrm{E}\left(y_{ij,2}^{0} | T_j = 1, B_{ij,2} = 1\right)$, where T_j is the same as that defined earlier, and $B_{ij,2}$ takes on a value of one if the household borrowed from the program by the time of the second survey, and zero otherwise.

By randomization taking place across villages, the potential outcome of borrowers in the treatment villages in the absence of the program shall be the same as the outcome of "latent borrowers" in the control villages, i.e., the households that would borrow from the program if their villages were treated in the first round. However, $B_{ij,2}$ is unobservable for households in the control villages. To address the challenge, in the spirit of Miguel and Kremer (2004), the program borrowers (observed in the third wave of the survey) in the control villages that were later treated in the second round are used as the comparison group. This gives $\widehat{ATE_B} = \mathrm{E}\left(y_{ij,2}^{1} | T_j = 1, B_{ij,2} = 1\right) - \mathrm{E}\left(y_{ij,2}^{0} | T_j = 0, B_{ij,3} = 1\right)$, where $B_{ij,3}$ is an indicator of whether households i borrowed from the program as observed in the third wave of the survey. $\widehat{ATE_B}$ can be estimated from Equation (1) by regressing the outcome of interest ($y_{ij,2}$) on the treatment dummy and other control variables with the sample of program borrowers (observed in the second wave of the survey) in the villages that were treated in the first round and the sample of program borrowers (observed in the third wave of the survey) in the control villages that turned to be treated later.

Similarly, the average treatment effect on program members who did not borrow from the village banks (ATE-members and non-borrowers) and the average treatment effect on non-members (ATE-non-members) can be estimated by the difference between the mean outcomes for the non-borrowers among program members (or non-members) in the treatment group and the mean for the "latent non-borrowers" (or "latent non-members") in the control group that were treated after the second wave of the survey.[23]

To assess the potential bias of the estimator, whether borrowing from the program is perceived as being determined by the underlying latent utility (i.e., $B^*_{ij,t}$) and a threshold value of the cost (i.e., c) to take up village bank loans. Specifically, $B_{ij,t} = 1$ if and only if $B^*_{ij,t} > c$.[24] The latent variable $B^*_{ij,t}$ is assumed to be a function of the previous informal borrowing $y_{ij,t-1}$, other characteristics of the previous period $X_{ij,t-1}$, and a random error term $e_{ij,t}$ drawing from the same distribution for every i and t. That is, $B^*_{ij,t} = f(y_{ij,t-1}, X_{ij,t-1}) + e_{ij,t}$. Then the estimation bias of the above strategy can be written as:

$$\widehat{ATE_B} - ATE_B = \mathrm{E}\big(\varepsilon_{ij,2} | T_j = 1, B_{ij,2} = 1\big) - \mathrm{E}\big(\varepsilon_{ij,2} | T_j = 0, B_{ij,3} = 1\big)$$

$$= \mathrm{E}\big(\varepsilon_{ij,2} | T_j = 1, e_{ij,2} > c - f(y_{ij,1}, X_{ij,1})\big) - \mathrm{E}\big(\varepsilon_{ij,2} | T_j = 0, e_{ij,3} > c - f(y_{ij,2}, X_{ij,2})\big). \quad (2)$$

If $\partial f(\cdot)/\partial y_{ij,t-1} = 0$, the above term will be zero. That is, $\widehat{ATE_B}$ will be an unbiased estimator for ATE_B. Turning to the data, in fact, the estimated coefficient of receiving informal interest-free loans in prior period in a probit regression on borrowing from (or participating in) the village bank is not significantly different from zero.[25]

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[23] Alternatively, we can estimate the effects from a specification with interaction terms of treatment and indicators of the three types of household by pooling the sample of all households in the control villages that were treated later and the sample of households in villages that were treated in the first round. Section C in the online Appendix describes this alternative estimation strategy in detail and shows that the results presented in the text are largely robust to this alternative estimation strategy.

[24] The cost is assumed to be constant over time. This corresponds to the fact that the criteria for being eligible to participate in, and borrow from, the village banks and the official guidance on the terms of loans were generally followed in practice and remained unchanged during the phased rollout of the program.

[25] The estimated marginal effect of taking up informal interest-free loans on the likelihood of participating in, and borrowing from, the village bank are 0.051 and 0.053, respectively. Neither of the estimates is statistically different from 0 on the significance level of 10%. See Table D1 in the online Appendix for details. Similarly, Section D in the online Appendix demonstrates the potential estimation bias for the program impacts on informal interest-free borrowing for the non-borrowers among program members and the non-members.

To further assess the validity of the identification assumption, I examine the balance on the outcome variables observed at the baseline (i.e., borrowing from a particular source) between program borrowers in villages that were treated in the first round and their counterparts in the control villages that were treated later. Similarly, the exercise can be conducted for program members who did not borrow from the village banks and the households that were not program members. Table D2 in the online Appendix reports the results. As shown in Panel A, the baseline incidences of borrowing are quite similar between households in the treatment and control groups for each comparison, and the differences are statistically insignificant in almost all cases except for the incidence of taking up of informal interest-bearing loans for program members who did not borrow from the village banks. Panels B and C show that the number of loans and the amount of loans at baseline are balanced as well between each type of household in the treatment group and their counterparts in the control group. The results of the joint test further suggest that we cannot reject the null hypotheses that for each type of household the treatment and control groups are balanced in terms of baseline borrowing outcomes. Additional examination on balance of baseline household characteristics in Table D3 in the online Appendix suggest that the characteristics that significantly predicting participating in or borrowing from the program (i.e., age of household head, area of household plotting land, and farm labor ratio in household) are also balanced for most cases.

Last but not least, I show further pieces of evidence on the identification assumption in the analyses below by estimating impacts on some placebo outcomes. This is based on the hypothesis that, if the estimated impacts are driven by potential difference in unobservable determinants of participating in, and borrowing from, the program between the treatment and control groups, we should observe similar impacts in these placebo tests. However, as shown below, the hypothesis is rejected.

Overall, these results strongly suggest that the counterparts identified from information on latent participation and borrowing status of households in the control villages that were treated later are reasonable comparison groups, and the potential bias of the estimation strategy (if any) is likely to be small.

3.3. Heterogeneity in Responses of Informal Borrowing to Shocks

To detect whether the impacts of the program on informal borrowing actually reflect its impacts on risk sharing, I follow Udry (1994) and Fafchamps and Lund (2003) to investigate whether informal borrowing serves to share risk[26] and whether the responses of informal borrowing to adverse shocks are different depending on the treatment status of the village with the following regression equation:

$$y_{ij,2} = \alpha + \delta S_{ij,2} + \gamma S_{ij,2} \times T_j + \beta T_j + \lambda y_{ij,1} + \Gamma V_j + \varepsilon_{ij}, \tag{3}$$

where $S_{ij,2}$ is the log of one plus the monetary value of adverse shocks to household i in village j during the year prior to the second survey, and the other notations are the same as those defined earlier. If adverse shocks are insured by informal networks, then the propensity to take up informal interest-free loans would increase with adverse shocks (i.e., $\delta > 0$). The presence of the village banking program may either enhance or weaken the role of informal borrowing for insuring against adverse shocks, which could be tested by the estimator of the coefficient of the interaction term, γ. If the village banking program substitutes informal borrowing to cope with adverse shocks, we expect $\gamma < 0$. Otherwise, we may expect $\gamma > 0$ if the program complements informal borrowing.

4. Main Results

4.1. The Crowding-Out Effect on Informal Borrowing

This section begins by investigating the ATE of the village banking program on the borrowing behavior of households with Equation (1). The results are presented in Table 4. Panel A reports the estimates of the impacts on the incidence of borrowing for any purpose, whereas Panels B and C further examine the impacts by the stated reason for borrowing.

As shown in column (1) of Panel A, on average, 26 percent of households in the treatment villages had borrowed from the village banks by the time of the second survey. The results in columns (2) to (4) suggest that access to the program crowded out borrowing from other sources to some extent. In particular, column (4) indicates that households' likelihood of taking up interest-free loans declined, on average, by eight percentage points as a result of being in the

[26] An alternative test for risk sharing is to examine how adverse shocks (e.g., illness) affect individual consumption conditional on the aggregate income of the risk sharing pool. See Dercon and Krishnan (2000) and Gertler and Gruber (2002) for examples.

treatment group. The point estimate is statistically significant at the 5% level in accordance with the standard t-test or is significant at the 10% level according to the wild bootstrap method. In terms of magnitude, the estimated impact is about 16% of the average value in the control group at baseline. The last column of Panel A shows that the incidence of overall borrowing from any source for households in the treatment group increased, on average, by six percentage points compared to that of the control group, although the estimate is only marginally significant at the level of 10%. To summarize, we see a significant crowding-out effect of the program on taking up informal interest-free loans for an average household in the treatment village.

Panels B and C present the impacts of the program on the incidence of borrowing for production and consumption purposes, respectively.[27] Column (1) reports that 21 percent of households in the treatment group received village bank loans for production purposes, whereas only five percent of them borrowed from village banks for consumption purposes. This is not surprising since the program was designed to stimulate income-generating activities. Column (4) shows that, on average, the percentage of households in the treatment villages that took up informal interest-free loans for consumption purposes was significantly lower than that in the control villages (Panel C). In contrast, there was barely no effect on taking up loans from the same source for production purposes (Panel B). The results imply that the significant reduction in the likelihood of taking up informal interest-free loans observed in Panel A is due mainly to the negative impact of the program on informal borrowing to smooth consumption. The last column shows that for an average household in the treatment villages the overall likelihood of taking up production loans increased by 19 percentage points as a result of the program, a substantial expansion of access to production credit compared to the baseline mean. Meanwhile, access to the program significantly reduced the propensity to take up consumption loans by nine percentage points, an effect driven primarily by the impact on taking up informal interest-free loans. Hereafter, I focus on the impacts on informal interest-free loans. For purpose of illustration, I also report the results of taking up informal interest-free loans for consumption or production separately.

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[27] Admittedly, money is fungible across uses, and the respondents may sometimes misreport their real reason for borrowing. However, we will see below in Section 6.2 that consumption borrowing indeed responds to medical expenses, whereas production borrowing does not.

4.2. Robustness Checks

To check the robustness of the main results, Panel A of Table A4 in the online Appendix explores the ATE on two intensive margins (i.e., the frequency and amount) of informal borrowing. Consistent with the estimated impacts on the extensive margin, the results on both the frequency and amount of borrowing indicate significant crowding-out effects on informal interest-free borrowing. Specifically, the results show that, on average, the number of interest-free loans taken up from informal sources decreased by 0.16, and the amount decreased by 1,431 *yuan* as a result of the program. Meanwhile, examination of the impact on the intensive margins further confirms that the effects were driven mainly by borrowing for consumption purposes.

To address the concern that risk may be insured not only by informal loans but also by private transfers circulated in social networks and it is difficult to distinguish between the two instruments, Panel A of Table A5 in the online Appendix examines the ATE of the program on the aggregate of informal interest-free loans and private transfers, where the latter includes gifts received from the extended family and broad social networks with other relatives and friends. Column (1) reports the impacts on the likelihood of a household taking up any informal interest-free loans or receiving any private transfers, while column (2) reports the impacts on the sum of monetary values of informal interest-free loans and gifts received from social networks. As shown, households in the treatment villages received, on average, a significantly smaller amount of loans and transfers by 2,019 *yuan* than households in the control villages, although the incidence of receiving any interest-free loans or transfers from social networks did not differ between households in the treatment and control villages. The crowding-out effect on the total amount of informal interest-free loans and transfers should partially alleviate the concern about focusing only on informal interest-free loans.[28]

Section B in the online appendix report results on additional robustness checks by using alternative regression specification and estimation method. The results are qualitatively unchanged.

[28] See the mechanism analyses below for more robustness examination of using the aggregate of informal interest-free loans and transfers as the outcome variable.

To sum up, the results of the ATE indicate a significant crowding-out effect of the microcredit program on informal borrowing, begging further examination on the potential mechanisms.

5. Conceptual Framework

This section provides a simple conceptual framework of risk sharing with limited commitment by following the work of Ligon, Thomas, and Worrall (2002) and Albarran and Attanasio (2003) among others to generate predictions for guiding the empirical analysis.

Let us consider the case of agents ($i = 1, 2, \cdots, \mathrm{n}$) in the Arrow-Debreu's world. Each agent lives for infinite periods and receives an endowment e^i in each period that determined by the state of nature s_t (e.g., the aggregate shock and the idiosyncratic shocks). s_t follows a Markov process featured by a transition probability matrix Γ. The history up to time t is denoted as $s^t = (s_1, \cdots, s_t)$.

Assuming agent can borrow from formal financial institutions up to a limit $b^i(s_t)$, thus she can re-allocate the resources across period subject to the life-time resource constraint

$$w_i \colon \sum_{k=0}^{\infty} c^i(s^{t+k}) \leq \mathrm{E}\left\{\sum_{k=0}^{\infty} e^i(s_{t+k}) \,|s_t\right\},$$

and the borrow constraints of every period

$$\omega_i \psi_{ik} \colon c^i(s^{t+k}) - \mathrm{E}\left[e^i(s_{t+k})\middle|s_t\right] \leq \mathrm{E}\left[b^i(s_{t+k})\middle|s_t\right], \mathrm{k} = 0,1, \cdots.$$

For simplicity, the interest rate is assumed to be 1.

In autarky, each agent maximizes her expected discounted utility

$$u\left[c^i(s^t)\right] + \mathrm{E}\left\{\sum_{k=1}^{\infty} \beta^k u\left[c^i(s^{t+k})\right]|s_t\right\}$$

subject to the above constraint conditions, where β is the utility discount factor. The first-order conditions imply $\frac{\beta^k(1+\psi_{i0})}{1+\psi_{ik}} = \frac{u'[c^i(s^t)]}{E\{u'[c^i(s^{t+k})]|s_t\}}$. Consequently, the value of autarky is equal to

$$\Omega^i(s_t) = u\left[\tilde{c}^i(s^t)\right] + \mathrm{E}\left\{\sum_{k=1}^{\infty} \beta^k u\left[\tilde{c}^i(s^{t+k})\right]|s_t\right\},$$

where $\tilde{c}^i(s^{t+k})$ satisfies the moment conditions. Accordingly, we have following lemmas:[29]

Lemma 1: *In a period when the borrow constraint of an agent is binding, her value of autarky*

[29] See Section E in the online Appendix for the proof on the lemma, proposition, and corollary listed in the text.

will be lower than the optimum when there is no borrow constraint.

Lemma 2: *Raising the upper limit the agent can borrow in a future period when she expects she will be credit constrained will raise the value of autarky utility she derived from constrained optimum.*

When there are mutual insurance arrangements through transfers or informal loans, a contract between the agents i and j, $\kappa^{ij}(s^t)$, specifies the net transfers agent i received from the other agent j given the history (a negative value indicating a transfer in the opposite direction). The aggregate transfers received by agent i is denoted as $\kappa^i(s^{t+k}) = \sum_{j=1,j\neq i}^{n} \kappa^{ij}(s^{t+k})$. Therefore, the resource constraint of agent i in period $t+k$ is $c^i(s^{t+k}) - \mathrm{E}\left[e^i(s_{t+k})\middle|s_t\right] \leq \mathrm{E}\left[b^i(s_{t+k})\middle|s_t\right] + \kappa^i(s^{t+k})$.

It is important to notice that the life-time consumption of an agent does not necessarily equal to her life-time resources in this case, because mutual insurance does not guarantee the life-time net transfer equal to 0. However, for the whole economy the resource constraint must be binding in the life time, whereas in each period the aggregate excess consumption should not be greater than the aggregated borrow limit. Specifically, we have

$$w\colon \sum_{i=1}^{n} \sum_{k=0}^{\infty} c^i(s^{t+k}) \leq \mathrm{E}\left\{\sum_{i=1}^{n} \sum_{k=0}^{\infty} \left[e^i(s_{t+k})\middle|s_t\right]\right\},$$

$$w\Psi_k\colon \sum_{i=1}^{n} c^i(s^{t+k}) - \mathrm{E}\left\{\sum_{i=1}^{n} \left[e^i(s_{t+k})\middle|s_t\right]\right\} \leq \mathrm{E}\left\{\sum_{i=1}^{n} \left[b^i(s_{t+k})\middle|s_t\right]\right\},$$

$$\mathrm{k} = 0,1,\cdots.$$

The Pareto efficient contracts can be characterized by solving the social planner's problem: $\max \sum_{i=1}^{n} \lambda_i U^i(s^t)$ subject to the aggregate resource constraint of entire life time and the aggregated borrowing constrain in every period, where λ_i is the Pareto weight the social planner assigned to agent i. In full risk-sharing, the first-order conditions of the social planner's problem imply

$$\frac{\lambda_i}{\lambda_j} = \frac{E\{u'[c^j(s^{t+k})]|s_t\}}{E\{u'[c^i(s^{t+k})]|s_t\}}, \mathrm{k} = 0,1,\cdots$$

$$\frac{\beta^k(1+\Psi_0)}{1+\Psi_k} = \frac{u'[c^i(s^t)]}{E\{u'[c^i(s^{t+k})]|s_t\}}, \mathrm{k} = 0,1,\cdots; \mathrm{i} = 1,\cdots,\mathrm{n}.$$

Consumption can be smoothed across both agent and period. Within the period, the expected marginal utility are equalized across agent according to their Pareto weights. The ratio of the expected marginal utility of consumption in two given periods is fixed for every agent, which is determined by the utility discount factor and the shadow price of the aggraded credit available in the two periods. Consequently, we have following lemmas:

Lemma 3: *The full risk-sharing arrangements depend solely on the total borrowing limit of all agents in each period regardless their distribution across agent.*

Lemma 4: *Raising the total borrowing limit in a period when the whole economy is credit constrained will increase the transfers (or informal loans) between agents in that period; whereas an expectation of an increase in total borrowing limit in a future period will not affect consumption and hence transfers in the current period if the whole economy is currently credit constrained.*

If agents are able to deviate from the contract due to lack of enforceability, then they will revert to the autarkic equilibrium according to the standard theory of subgame perfect equilibrium. The constrained efficient allocation can be characterized by solving the same social planner's problem as above augmented with the following participation constraints of every agent:

$$\lambda_i \phi_i : U^i(s^t) \geq \Omega^i(s_t)$$

That is, to make sure a contract is implementable, the expected discounted utility the agent achieve from risk-sharing arrangements should be at least as great as the expected discounted utility in autarky. The first-order conditions yield the following moment conditions:

$$\frac{\lambda_i(1+\phi_i)}{\lambda_j(1+\phi_j)} = \frac{E\{u'[c^j(s^{t+k})]|s_t\}}{E\{u'[c^i(s^{t+k})]|s_t\}}, \mathrm{k} = 0,1,\cdots.$$

As indicated by Lemma 2, raising borrowing limit in any period of life when the agent expects she will be credit constrained will increase the value of autarky. This will make the participation constraints of these agents tighter. Therefore, we have following proposition:

Proposition: *Anticipation of access to credit in a future period when the agent expects she will be credit constrained will increase the expected utility of autarky relative to the utility derived from risk-sharing agreements, thus reducing the scope of implementable risk-sharing contracts (or informal borrowing).*

The proposition also implies following two corollaries:

Corollary 1: *Anticipation of access to credit in a future period when the agent expects she will be credit constrained will reduce the reliance on informal borrowing to insure against idiosyncratic shocks.*

Corollary 2: *Anticipation of access to credit in a future period when the agent expects she will be credit constrained will increase overall decision utility, although it crowds out informal borrowing.*

The model can be easily extended to a case with investment. Conceptually, given that informal mutual insurance arrangements play a limited role in financing investment or insuring against community-wide shocks (as evidenced above), agents would have to save for investment.[30] Anticipating that they will be able to borrow for production in the future, agents thus rely less on internal finance for investment. This relaxes agents' liquidity and improves their ability to smooth consumption by self-insuring, which increases the expected utility they can achieve in autarky relative to the expected utility of being involved in risk-sharing agreements. As a consequence, the scope of implementable risk-sharing contracts becomes narrower and the agents rely less on mutual insurance arrangements for risk sharing.

6. Tests on the Mechanisms

This section empirically examines the predictions derived by the conceptual framework above. For this purpose, I examines the effect on three types of household according to their participation and borrowing status in the program by the time of the second survey: (1) households that borrowed from village banks, (2) households that participated in the program but did not borrow from the village banks, and (3) households that did not participate in the program. By doing so, I assume the program participants who did not borrow from the village banks anticipated that they could borrow from the village banks when they needed to at some point in the future.

Table A6 in the online Appendix provides some descriptive evidence on the assumption.

[30] This is in line with the "internal finance theory of growth" for small firms (Carpenter and Petersen, 2002). That is, most small firms finance their growth almost entirely on internal finance.

Specifically, in the second wave of the survey, the program members were asked, “Why did you join the village banking program?” Panel A of Table A6 describes the reasons according to households’ status for borrowing from the village banks. As shown, about half of the participants had borrowed from the village banks by the time of the second survey. Among the non-borrowers, the most common reason for joining the village banking program was “Do not need credit right away, but maybe later” (49%). On the other hand, among the borrowers of the program, the most common reason for joining the program was “Need credit right away” (57%).

To examine whether some of those non-borrowers among program members actually borrowed later, I examine their borrowing behavior from the village banks using information from the third wave of the survey. Panel B of Table A6 describes the transition of membership and borrowing status among households in villages that were treated in the first round. As shown, a substantial share (21 percent) of the program members who had not borrowed from the village banks by the time of the second survey did in fact do so after the second survey. In contrast, only eight percent of non-member households began to join the program and borrow from the village banks after the second survey.

These statistics indicate that many non-borrowers among the program members indeed considered joining the program as an opportunity to borrow in the future, and the majority of borrowers in the program were already credit-constrained when they joined the program, whereas most households that did not participate in the program might be those were not credit-constrained in any period.[31]

According to the predictions derived from the conceptual model, in the scenario of full risk-sharing, access to microcredit may crowd-in informal borrowing. However, if there is limited commitment, access to microcredit may crowd-out informal borrowing. In particular, the limited commitment models predict that, for non-borrowers, anticipation of being able to borrow in the future when they expects they might be credit constrained will raise their value of autarky relative to the utility derived from risk-sharing arrangements, thus crowd out their informal borrowing.

The following analysis starts by presenting evidence on the crowding-out effect of having

[31] See Section 6.3 for additional evidence.

access to credit among non-borrowers to show that the reduction in informal borrowing was not simply due to the substitution effect. Then, it investigates how access to credit affects reliance on informal borrowing to insure against health shocks to verify that the reduction in informal borrowing indeed reflected a decrease in informal risk sharing. This is followed by some other direct evidence on the role of anticipation in explaining the crowding-out effect. Finally, the alternative explanations are examined.

6.1. Treatment Effect on Borrowers and Non-borrowers

To understand the crowding-out effect on informal interest-free borrowing, Table 5 examines the effect on three types of household by employing the strategy presented earlier, which uses the information on latent participants and borrowers of the village banks. As demonstrated in column (1), for all program members—regardless of whether or not they had borrowed from the village banks, access to microcredit significantly reduced their likelihood of receiving informal interest-free loans, whereas for households that were not program members the effect was economically and statistically insignificant.

The results have two implications. First, the negative impacts on informal interest-free borrowing are not simply due to the substitution effect of the injection of extra credit from the program, because the crowding-out effect appears even among program members that had not yet borrowed from the village banks.[32] Second, the difference in the estimated impacts between program members and non-members implies that access to credit may affect the incentives to become involved in informal financial networks only when it alters the value of autarky. As evidenced above, households that did not participate in the program were more likely to be those that were not credit constrained. Thus, their expected utility of autarky was presumably not affected even they were in the treatment villages.

Columns (2) and (3) of Table 5 examine the impacts on production and consumption loans, respectively. The results on consumption loans further confirm that the program reduced interest-free borrowing from informal sources among all program members, irrespective of whether they had actually borrowed from the program or not. In contrast, there was no

[32] This echoes the finding of Banerjee et al. (2021) which show that financial networks for both borrowers and non-borrowers are shrinking as a result of exposure to microfinance initiatives in India.

economically or statistically significant impact on production loans for any of the three types of household, except that the program slightly crowded out some informal interest-free borrowing for production purposes among those who borrowed from the village banks. The insignificant impact on production loans among program members who did not borrow from the village banks is reassuring. This implies the crowding-out effect on taking up informal interest-free loans (for consumption purposes) among the non-borrowers is unlikely to have been driven by possible heterogeneity in selection of non-borrowing between the treatment and control groups; otherwise we will observe similar impacts on informal interest-free loans for production purposes.

For robustness checks, Panels B through D of Table A4 in the online Appendix examine the treatment effects on the intensive margins of informal borrowing for the three types of household. The results are generally in line with the estimated impacts on the extensive margin. The only exception is that the program also significantly reduced the amount of informal interest-free loans among non-member households as shown in Panel D, although the magnitude of the effect was much smaller than that of the program members. The results in Panels B through D of Table A5 in the online Appendix confirm that the program crowded out informal loans and transfers mainly among program members for both the intensive margin and extensive margin, while the impacts were less salient among households that were not program members.[33]

In sum, the results above indicate that the program crowded out informal borrowing not only among households that borrowed from the village banks but also those program members who had not borrowed from the village banks. This is consistent with the model's prediction that access to—not necessarily taking up—village banking loans would reduce informal borrowing due to the increase in the expected utility of autarky.

6.2. Responses of Informal Borrowing to Health Shocks

To examine how the village banking program affected the role of informal financial arrangements in insuring against risks, I investigate the responses of informal borrowing to

[33] Additional checks in Section B in the online appendix suggest that the basic results are also robust to regression specification without controlling for the other variables or using nonlinear estimation method.

health shocks—the most common adverse shock to households in the sample—and the interaction effect of access to microcredit using Equation (3).

Table 6 reports the regression results on the incidence of taking up informal interest-free loans for any purpose. As shown in column (1), the likelihood of taking up informal interest-free loans increased significantly with the log of medical expenses, indicating that borrowing from informal financial networks was an important strategy for households to mediate health shocks. The negative coefficient of the interaction term of treatment and medical expenses in column (2) indicates that households in the treatment village responded less to health shocks by informal interest-free borrowing. The results in columns (3), (5), and (7) confirm that, for all the three types of household, their borrowing from informal financial networks responded positively to health shocks in the absence of the program. Columns (4) and (6) show that access to the program reduced the pass-through of medical expenses to informal interest-free borrowing for program members, though the interaction effect was only significant for non-borrowers (for borrowers it was of similar magnitude but less precise). In contrast, the results in column (8) show that the interaction effect was neither statistically nor economically different from 0 for households that did not participate in the program.

These results are consistent with the prediction of the conceptual model above. First, in the absence of a village banking program, households do rely on informal networks to cope with health emergencies when they arise. Second, being a member of a village banking program grants access to microloans, which reduces the reliance on informal borrowing to insure against health risks. Given that the non-borrowers among the program members had not yet borrowed from the program by the time of the second survey, they had to rely more on self-financing to mediate any health shocks. This is likely due to less desire for building up savings for investment as a result of having the option to borrow from the village banks, which relaxed the liquidity for consumption smoothing. Lastly, the differences in the interaction effect between program members and non-members indicate that households relied less on mutual insurance to smooth consumption shocks only when their expected utility of autarky was influenced by access to the village banking program.

Table A7 in the online Appendix reports the estimated impacts on the incidence of informal borrowing by stated purpose. As expected, Panel A indicates that the results are quite similar by using the incidence of taking up informal interest-free loans for consumption purposes as the dependent variable. In contrast, Panel B (as placebo tests) shows that, overall, the incidence of taking up informal interest-free loans for production purposes does not respond to medical expenses. Meanwhile, heterogeneity in the responses of the non-borrowers in the treatment and control groups is not observed for production loans. This implies that the significant reduction in the pass-through of medical expenses to informal interest-free loans among non-borrowers in the treatment group compared to those in the control group may not have been driven primarily by a potential difference, if any, in the determination of (not) borrowing from the program between the treatment and control groups.

Taken together, the results imply that the crowding-out effect on informal borrowing found earlier does reflect a decrease in informal risk sharing as a result of the program. In addition, the results highlight that anticipation of access to credit can be an important channel through which financial initiatives may increase the value of expected utility of autarky, and thus reduce the scope of implementable risk-sharing arrangements.

6.3. Announcement Effect

This section provides additional evidence on the role of anticipation in explaining the crowding-out effect of the program on informal interest-free borrowing by distinguishing the announcement effect from the disbursement effect. To do so, I decomposed the post-policy window into three periods.

As described earlier, the program was initiated in September 2010, and the village banks started to disburse loans at different times between January and July of 2011. After that, all the program villages had begun to lend loans, except those in a single aforementioned county. Therefore, we can divide the loans recorded in the second survey into three periods according to their disbursement times, namely, the announcement period (from August to December of 2010), the transition period (from January to July of 2011), and the disbursement period (from August 2011 to June 2012). Table 7 examines the effects of the program on informal interest-free borrowing during the three periods.

The results show that even during the announcement period, the program significantly reduced informal interest-free borrowing among member households that had not borrowed from village banks by the time of the second survey. This result indicates that anticipating an option to borrow from a village bank in the future indeed alters the incentive of these households to become involved in informal risk-sharing arrangements.[34] For the program borrowers, however, the negative effect on informal interest-free borrowing is observed only during the transition and disbursement periods, and no announcement effect is evident. This coincides with the conjecture that these households were more seriously constrained by liquidity and were farther from being able to self-finance than the non-borrowers among the program members. Similar to the results in Table 5, we do not observe any significant impact on the probability of taking up informal interest-free loans for non-member households in each period.

Overall, the results imply that access to microcredit may crowd out informal interest-free borrowing by altering people's anticipation, which raises the expected utility of autarky. Since the announcement effect exists only among program members who did not borrow from the program, it implies that anticipation of access to the program can have a *direct* effect on informal borrowing for these households by raising their expected utility of autarky in addition to a possible spillover effect through global network externality (Banerjee et al., 2021).[35]

6.4. Alternative Explanations

The observed borrowing can be considered as an outcome determined by both sides of credit supply and demand. Therefore, the reduction in informal interest-free borrowing can be caused by a decrease in the supply of credit or (and) a decrease in the demand for credit. This section examines alternative mechanisms that might explain the negative effect on taking up informal interest-free loans, including a decrease in available credit and a reduction in the demand for credit due to an increase in households' income.

Supply of Credit—To examine the program impacts on the supply of credit, I use the following hypothetical questions. Specifically, for informal borrowing, the respondents were asked: "If you need money urgently, can you borrow from anyone else?" and "If you can, how

[34] In a similar vein, Agarwal and Qian (2014) show that households' consumption and debt responded to a fiscal stimulus program in Singapore even during its announcement period.

[35] Related to the anticipation effect, one may worry that households in control villages could also have expected the roll-out of the program as it was scaled up. If this was the case, it would lead to *underestimation* for the crowding-out effect. However, it is worth mentioning that, in practice, the control villages were kept uninformed of the program intentionally. Meanwhile, they were relatively far from any village banks established over the study period, as indicated in footnote 10.

many persons can you borrow from, and how much can you borrow?" Similarly, for formal borrowing, the respondents were asked, "If you need money urgently, can you borrow from Rural Credit Cooperatives or other formal financial institutions?" and "If you can, how much can you borrow?" Different from observed actual borrowing, these hypothetical questions reveal the potential credit households are able to access if an emergency arises, which reveals respondents' ability to borrow and arguably reflects only the supply of credit.

Table 8 reports the estimates of the program's impact on available credit in an emergency of the three types of household using the hypothetical questions. As shown, no significant change occurs in the ability to borrow from informal networks or formal financial institutions for households that borrowed from the program. Program members who did not borrow from the village banks even showed an increased capacity to take out informal loans, in terms of both extensive (i.e., the probability of borrowing) and intensive margins (i.e., the number of persons they could borrow from and the amount of loans they could take up in times of need). Similarly, their capacity to borrow from formal financial institutions also increased. These results may reveal an increase in the social capital of these households as a result of sharing joint liability with program borrowers in group lending of the village banks. Finally, no significant change appears in available credit among non-member households.

Overall, these results suggest there is no evidence of any significant reduction in the capacity to borrow from either formal or informal sources among households in the treatment villages. If anything, we observe an increase in available credit for program members who did not borrow from the program.[36] Therefore, the negative effect on informal interest-free borrowing should not be due to a decrease in the supply of credit, but instead a decrease in households' demand for credit.

Income Effect—To examine whether the decrease in informal interest-free borrowing is because of the increase in household income that is evident in Cai, Park, and Wang (2020), I further control for the change in household income in Equation (1) and report the results in columns (1) to (3) in Table A8 in the online Appendix. As expected, the incidence of informal interest-free borrowing decreases as household income increases. However, the estimate of the

[36] Taking account of possible attenuation bias arising from reporting bias, the positive and significant impacts on available credit for non-borrowers of program members are actually reassuring.

program impact conditional on change in income is similar to the benchmark results in Table 5 for every type of household. To further account for the concern of endogeneity in the variable of change in household income, we use the predicted value of change in household income by regressing it on the village treatment dummy, the baseline village and household characteristics, and their interaction terms with the treatment dummy. As shown in columns (4) to (6), for each type of household, the estimates of the treatment effect are rather robust. These results imply that the decrease in demand for credit is unlikely due mainly to the income effect. Meanwhile, the above evidence that the crowding-out effect existed even during the announcement period also indicates that change in income may not be the primary reason of the decrease in informal interest-free borrowing.

7. Extensions

7.1. Longer-Run Impacts

This section examines the longer-run impacts on informal interest-free borrowing by comparing the outcomes of households in the villages that were treated in the first round and those in villages that remained untreated by the time of the third survey (i.e., about four years after the initiation of the program). Table 9 presents the results on the ATE over this longer run. For comparison, the table also reports the short-run impacts using the same sample.

The results indicate that the negative effect on informal interest-free borrowing persists over a longer time. Specifically, the incidence of taking up informal interest-free loans decreased on average by seven percentage points between the first and second survey as a result of the program, whereas it decreased by ten percentage points between the second and third survey. Consistent with the observed short-run effect, the reduction in informal interest-free borrowing in the longer run was driven mainly by the borrowing for consumption purposes. The results in Table A9 in the online Appendix further confirm that the village banking program reduces the frequency and amount of informal interest-free borrowing in the longer run as well.

Table A10 in the online Appendix examines the longer-run effect on the aggregation of informal borrowing and the private transfers received. Consistent with the estimates reported in Table A5, the results in Panel A of Table A10 show that in the short run, the program reduces (although statistically insignificant) the total amount of informal interest-free loans and transfers received, whereas the effect on the incidence of taking up informal interest-free loans or receiving private transfers is close to 0. In contrast, in the longer run, households are significantly less likely to take up informal interest-free loans or receive private transfers and obtain a much smaller amount of informal interest-free loans and transfers as a result of the program. In sum, the village banking program reduces informal borrowing and transfers persistently in the longer run, and the effects are stronger than they are in the short run for both the intensive and extensive margins.

7.2. Impacts on Well-Being

To assess the welfare implications for access to the program, following prior studies (e.g., Karlan and Zinman, 2010), I further investigate the impacts on households' well-being by using information on self-assessment of the standard of living and life satisfaction. Whereas the standard of living captures households' material well-being, life satisfaction is used as a measure of subjective well-being, which has been shown to be closely related to the decision utility (Clark, Frijters, and Shields, 2008). To address the concern about potential reporting bias in self-assessment of the standard of living, I also examine the effect of the program on households' standard of living using the assessments of village officials. Table 10 reports the results by replacing the outcome variable in Equation (1) with the measures of material well-being and life satisfaction.

As shown in columns (1) and (2), whereas self–assessments of material well-being increase for all three types of household in the treatment villages, the results based on the assessments of village officials suggest that material well-being does improve mainly for households that borrowed from the village banks.

The results in column (3) indicate that life satisfaction increases significantly among all program members as a result of being in the treatment village, regardless of their status of borrowing from village banks. In contrast, the effect of the program on life satisfaction is not significantly different from 0 for the non-members. The positive program impacts on the subjective well-being (or decision utility) for program members, particularly non-borrowers, is consistent with the prediction of the conceptual model that the decrease in their informal interest-free borrowing as a result of access to the program arises from their diminished *desire* for mutual insurance because of an increase in the expected value of autarky. It also coincides with the finding of Kaboski and Townsend (2011) that “non-borrowers also benefit from the increased potential liquidity from the relaxed borrowing constraint in the future.”

8. Conclusion

This study leverages a randomized controlled trial with a phased rollout of a village banking program in China to examine the impact of microcredit on informal risk-sharing arrangements in the village economy. It finds that access to the program reduces households’ borrowing from informal financial networks. The rollout of the program allows us to identify those who would counterfactually borrow from (or participate in) the program versus those who would not, and hence separately estimate the treatment effects for program borrowers, non-borrowers among program members, and non-members. The results show that access to the program crowds out informal interest-free borrowing among borrowers of the program, as well as program members who did not borrow from the village banks. An examination of the interaction effect of access to the program and health shocks on informal interest-free borrowing indicates that the program reduces reliance on informal borrowing to insure against health shocks among all program members, regardless of whether or not they have borrowed from the village banks.

These results are consistent with the predictions from models of mutual insurance with limited commitment. That is, anticipation of access to extra credit in the future raises the expected utility of autarky relative to the utility derived from the risk-pooling arrangements, and thus reduces the scope of implementable risk-sharing contracts. The prediction is further confirmed by the result that the program crowds out informal borrowing even during its announcement period. Additional analyses suggest that the crowding-out effect among program members persists over a longer period of time. Although the program crowds out informal borrowing for program members, it does improve the overall welfare of these households as revealed by the positive impact on their subjective well-being. This is in line with the prediction that the reduction in informal borrowing stems from a diminished desire for mutual insurance, which leads to an increase in decision utility.

The findings of the study have several implications for policy and future research. First, the study shows that a microcredit program may affect not only the households that take up loans from the program, but also program members who have not borrowed from the program. Therefore, the effect of a microcredit intervention should be examined beyond the average treatment effect or the treatment effect on its recipients. Second, the initiation of a formal institution could have a crowding-out effect on the informal institution by changing the anticipation of the targeted population. For this reason, policy interventions need to take account of the potential announcement effect before disclosing information on the intervention to the public. Lastly, policy designers and scholars who evaluate the practice of microcredit in the developing world should pay attention to the background of their study site, particularly the potential interaction of microcredit with informal financial networks.

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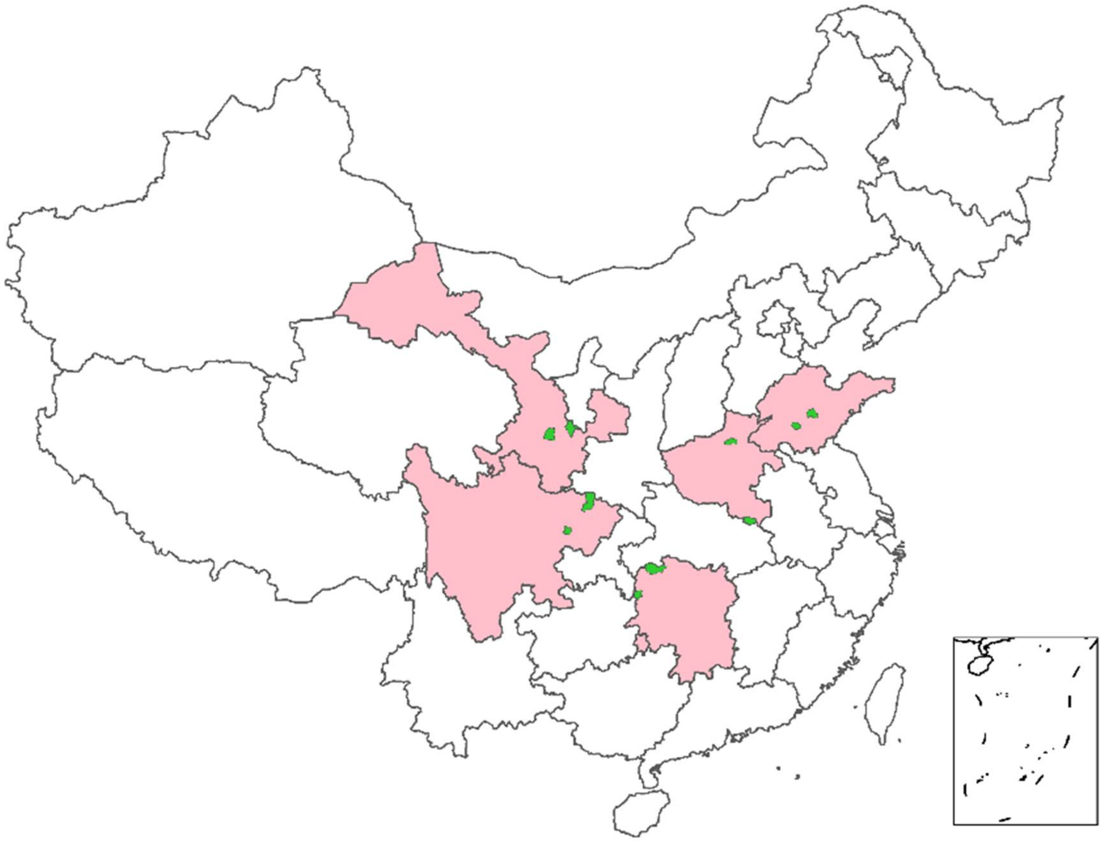
Figures and Tables

Figure 1. Survey Sampling



Notes: This figure illustrates the sampling conducted in five provinces (Gansu and Sichuan in Western China; Henan and Hunan in Middle China; and Shandong in Eastern China) and ten counties (two counties in each province). In each selected county, three villages are randomly chosen as the treatment group, and two serve as the control group (not shown).

Figure 2. Timeline of the Survey and Intervention

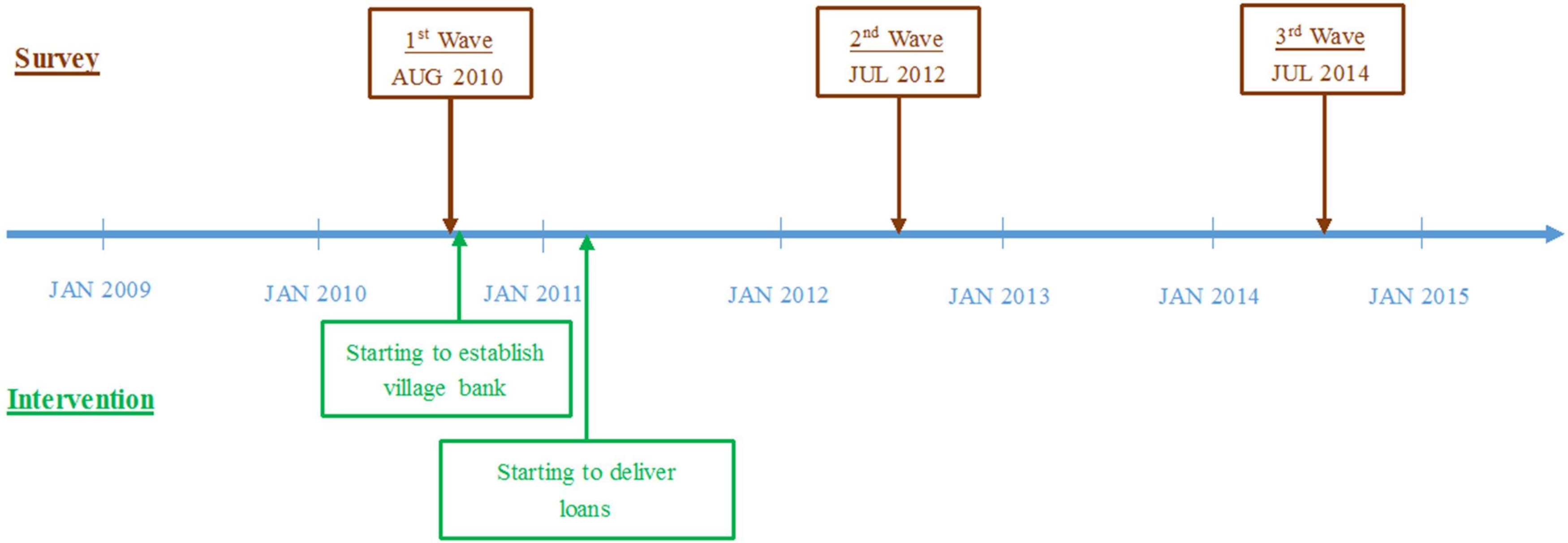
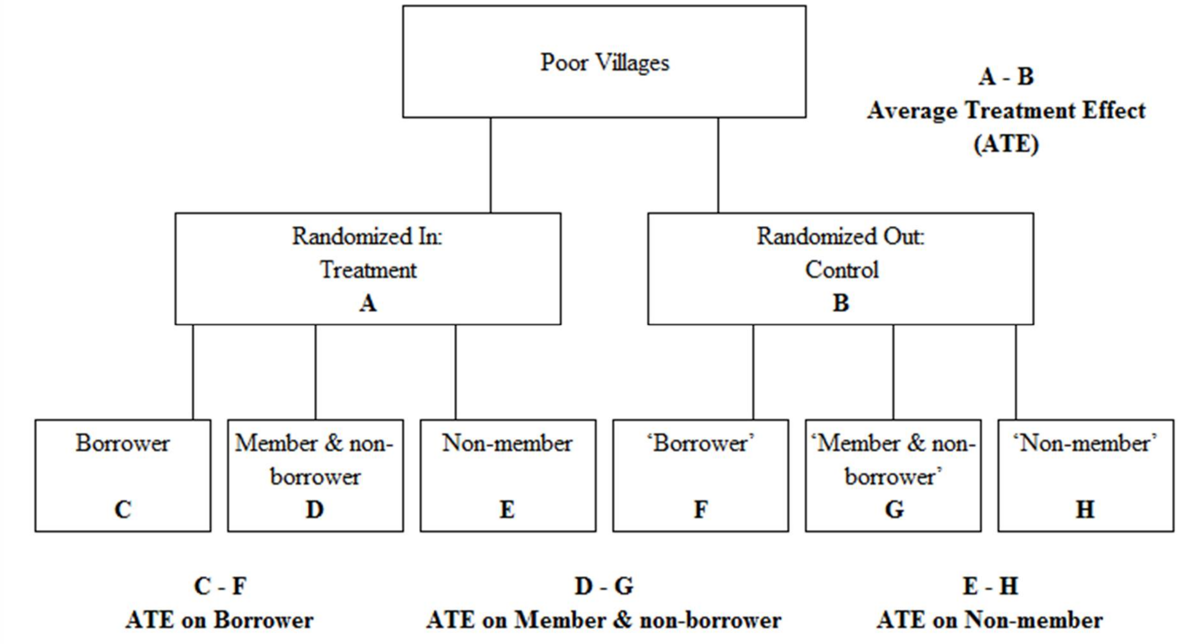


Figure 3. Framework of the Empirical Strategy



Notes: The notion '' indicate the latent counterparts in the control villages that identified from the information of participating in and borrowing from the village banks among households in villages those treated later as the program phased rollout.

Table 1. Summary Statistics on Shocks to Households

	Share of households (%)	Average expenses or monetary values (*yuan*)	Median of the proportion to household income (%)	Village-level variance of occurrence as percentage of total variance
	(1)	(2)	(3)	(4)
Health shocks	66.0	5,248	33.1	12.1
Student at school	52.9	3,900	19.6	9.9
Build a house	6.4	75,652	442.6	5.6
Give birth to a child	6.0	N.A.	N.A.	5.3
Poor harvest	37.7	N.A.	N.A.	14.2
Domestic animal died, lost, or stolen	8.4	2,523	9.7	6.4
Any type of shocks	91.7	11,778	48.4	6.7

Notes: The table describes the shocks that occurred in 2009 among the households. The number of observations is 1,351. Column (1) reports the percentage of households that suffered a corresponding shock. Column (2) reports the average expenses or monetary values of the corresponding shocks conditional on the occurrence of the shocks. Column (3) reports the median of the proportion to household annual income in 2009. Column (4) reports the village-level variance of occurrence as a percentage of the total variance, which is calculated by the R-squared of a regression on village dummies. N.A. indicates "Not available."

Table 2. Pre-Program Borrowing by Source and Purpose

	Loan source			
	Any	Formal	Informal (interest-bearing)	Informal (interest-free)
	(1)	(2)	(3)	(4)
Panel A: Incidence				
All loans	0.59	0.13	0.04	0.51
Production loans	0.22	0.05	0.02	0.16
Consumption loans	0.47	0.08	0.02	0.42
Panel B: Amount (Conditional on borrowing)				
All loans	5,000	10,000	10,000	5,000
Production loans	2,000	5,000	8,500	2,000
Consumption loans	5,000	10,000	10,000	5,000

Notes: The number of observations is 1,351. The loans are characterized by sources, including loans taken from formal financial institutions, informal interest-bearing loans, and informal interest-free loans. The loans are also categorized into production and consumption loans according to the reported reasons for borrowing. Panel A reports the proportion of households that took out any loans between January 2009 and July 2010. Panel B reports the median of the loan amount among all households that took out corresponding loans.

Table 3. Summary Statistics on Loan Terms

Loan terms	Formal loans	Informal loans (interest-bearing)	Informal loans (interest-free)	Program loans	
				Survey data	Administrative data
	(1)	(2)	(3)	(4)	(5)
Amount	26,929	26,230	9,593	3,795	3,867
	(32,166)	(44,135)	(16,986)	(1,079)	(1,309)
	[34,753]	[38,423]	[25,665]	[4,093]	[10,348]
Annual interest rate	9.84	17.20	0.00	9.84	10.13
	(5.44)	(4.80)	(0.00)	(0.85)	(0.35)
	[4.88]	[30.07]	[0.00]	[4.98]	[14.72]
Loan-term length	13.35	8.27	10.00	8.13	9.06
	(7.64)	(6.79)	(4.08)	(2.90)	(1.92)
	[6.08]	[5.95]	[4.28]	[5.47]	[19.72]
Collateralized (yes=1)	0.15	0.04	0.00	0	N.A.
	(0.33)	(0.00)	(0.05)	0	N.A.
	[0.41]	[0.35]	[0.10]	0	N.A.
Guarantee (yes=1)	0.33	0.30	0.02	0.76	N.A.
	(0.43)	(0.32)	(0.15)	(0.38)	N.A.
	[0.56]	[0.65]	[0.23]	[0.71]	N.A.
Observations	48	23	335	235	2,094

Notes: The table describes the terms of loans taken from August 2010 to June 2012. The observation unit is the loan taken by the households. Columns (1) to (3) report the mean and standard deviation of the main characteristics of the formal loans, informal interest-bearing loans, and informal interest-free loans taken by households in the control villages, respectively. Column (4) reports the mean and standard deviation for program loans taken by the surveyed households in the treatment villages. Column (5) reports the statistics for program loans taken by all households in the treatment villages using administrative data from the village banks. N.A. indicates "Not available." Standard deviations within villages are reported in parentheses, and standard deviations across villages are reported in brackets.

Table 4. Average Treatment Effect on the Incidence of Borrowing

Borrowing measured in the second wave of the survey	Any loans borrowed from				
	Village bank	Formal	Informal (interest-bearing)	Informal (interest-free)	Any
	(1)	(2)	(3)	(4)	(5)
Panel A : All loans					
ATE	0.26***	-0.03	-0.01	-0.08**	0.06
	(0.04)	(0.02)	(0.01)	(0.03)	(0.04)
p -values using wild bootstrap	0.000	0.178	0.450	0.068	0.110
Mean at baseline	0.00	0.13	0.04	0.51	0.59
Observations	1,351	1,351	1,351	1,351	1,351
Panel B : Production loans					
ATE	0.21***	-0.01	0.00	0.01	0.19***
	(0.04)	(0.01)	(0.01)	(0.01)	(0.04)
p -values using wild bootstrap	0.000	0.356	0.670	0.256	0.000
Mean at baseline	0.00	0.05	0.02	0.16	0.22
Observations	1,351	1,351	1,351	1,351	1,351
Panel C : Consumption loans					
ATE	0.05***	-0.02	-0.01*	-0.09**	-0.09**
	(0.01)	(0.01)	(0.01)	(0.04)	(0.04)
p -values using wild bootstrap	0.000	0.302	0.092	0.036	0.076
Mean at baseline	0.00	0.08	0.02	0.42	0.47
Observations	1,351	1,351	1,351	1,351	1,351

Notes: All the regressions control for village characteristics measured at baseline, including type of geographic features, population, area of arable land, public spending financed by superior governments on various projects, and the lagged dependent variable. Standard errors in parentheses are clustered by village. The *p*-values of the wild cluster are computed using the wild bootstrap method (Cameron, Gelbach, and Miller, 2008) with 1,000 replications. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 5. Impact on the Incidence of Informal Interest-Free Borrowing

Borrowing measured in the second wave of the survey	All loans (1)	Production loans (2)	Consumption loans (3)
Panel A: ATE-borrower	-0.28***	-0.10	-0.26**
	(0.10)	(0.08)	(0.11)
p -values using wild bootstrap	0.030	0.284	0.048
Mean at baseline	0.60	0.21	0.49
Observations	243	243	243
Panel B: ATE-member & non-borrower	-0.23***	0.04	-0.27***
	(0.05)	(0.03)	(0.06)
p -values using wild bootstrap	0.002	0.264	0.002
Mean at baseline	0.49	0.14	0.39
Observations	291	291	291
Panel C: ATE-non-member	-0.06	0.00	-0.05
	(0.06)	(0.02)	(0.04)
p -values using wild bootstrap	0.410	0.960	0.356
Mean at baseline	0.49	0.15	0.40
Observations	532	532	532

Notes: The table reports the estimated effect on the incidence of taking up informal interest-free loans for households that borrowed from the program (Panel A), those that participated in the program but did not borrow from it (Panel B), and those that did not participate in the program (Panel C) by the time of the second wave of the survey using information on participating in the program and borrowing from the program for households in the control villages that were treated after the second survey. All regressions also control for village characteristics measured at baseline, including type of geographic features, population, area of arable land, public spending financed by superior governments on various projects, and the lagged dependent variable. Standard errors in parentheses are clustered by village. The *p*-values of the wild cluster are computed using the wild bootstrap method (Cameron, Gelbach, and Miller, 2008) with 1,000 replications. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 6. Heterogeneity in Responses of Informal Interest-Free Borrowing to Health Shocks

Borrowing measured in the second wave of the survey	Taking up any informal interest-free loans (dummy)							
	All households		Borrower		Member & Non-borrower		Non-member	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Medical expense (in log)	0.04***	0.05***	0.04***	0.13	0.03***	0.13***	0.03***	0.04*
	(0.01)	(0.01)	(0.01)	(0.08)	(0.01)	(0.03)	(0.01)	(0.02)
Treat × Medical expense (in log)		-0.02		-0.10		-0.11***		-0.01
		(0.01)		(0.08)		(0.03)		(0.02)
Observations	1,351	1,351	243	243	291	291	532	532

Notes: The dependent variable is the incidence of taking up informal interest-free loans for any purpose. The medical expense is measured as a logarithm (plus one). All regressions also control for treatment dummy, village characteristics measured at baseline, including type of geographic features, population, area of arable land, public spending financed by superior governments on various projects, and the lagged dependent variable. Standard errors in parentheses are clustered by village. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 7. Impact on the Incidence of Informal Interest-Free Borrowing by Time Period

Borrowing measured in the second wave of the survey	Announcement period (2010.8 - 2010.12)			Transition period (2011.1 - 2011.7)			Disbursement period (2011.8 - 2012.6)		
	All loans	Production loans	Consumption loans	All loans	Production loans	Consumption loans	All loans	Production loans	Consumption loans
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: ATE-borrower	0.04	-0.00	0.04	-0.27**	0.01	-0.29**	-0.23***	-0.16*	-0.15*
	(0.07)	(0.01)	(0.08)	(0.12)	(0.03)	(0.13)	(0.08)	(0.09)	(0.08)
Observations	243	243	243	243	243	243	243	243	243
Panel B: ATE-member & non-borrower	-0.08**	0.01	-0.08**	-0.14***	0.01	-0.16***	-0.11	0.04	-0.14*
	(0.04)	(0.00)	(0.04)	(0.04)	(0.02)	(0.05)	(0.08)	(0.02)	(0.08)
Observations	291	291	291	291	291	291	291	291	291
Panel C: ATE-non-member	-0.03	-0.00	-0.02	-0.04	-0.01	-0.00	-0.05	-0.01	-0.03
	(0.03)	(0.01)	(0.02)	(0.03)	(0.01)	(0.03)	(0.04)	(0.02)	(0.03)
Observations	532	532	532	532	532	532	532	532	532

Notes: The table reports the estimated effect on the incidence of informal interest-free borrowing using information on participation in the program and borrowing from the program for households in the control villages that were treated after the second wave of the survey. All regressions also control for village characteristics measured at baseline, including type of geographic features, population, area of arable land, public spending financed by superior governments on various projects, and the lagged dependent variable. Standard errors in parentheses are clustered by village. *** p<0.01, ** p<0.05, * p<0.1.

Table 8. Impact on Available Credit in an Emergency

Outcome measured in the second wave of the survey	Can you obtain informal loans if you need them?	How many persons can you borrow loans from if you need them?	How much informal loans can you borrow if you need them?	Can you obtain formal loans if you need them?	How much formal loans can you borrow if you need them?
	(1)	(2)	(3)	(4)	(5)
Panel A: ATE-borrower	0.08	-0.17	291	-0.02	-1,061
	(0.15)	(1.71)	(6,114)	(0.12)	(4,451)
Observations	243	242	243	243	241
Panel B: ATE-member & non-borrower	0.17*	2.83*	4,083**	0.18***	4,655***
	(0.09)	(1.56)	(1,771)	(0.06)	(1,256)
Observations	285	280	285	286	282
Panel C: ATE-non-member	0.00	-0.66	4,685	0.02	2,858
	(0.04)	(0.70)	(3,084)	(0.06)	(2,588)
Observations	515	509	513	515	508

Notes: The dependent variable in column (1) is a dummy that equals one if the household can obtain informal loans in times of need, whereas the dependent variable in columns (2) and (3) measures the number of persons from whom households can take up loans and the amount of informal loans households can take up if they need them, respectively. The dependent variables in columns (4) and (5) measure the household's capacity to borrow from formal financial institutions in a similar manner. All regressions also control for village characteristics measured at baseline, including type of geographic features, population, area of arable land, public spending financed by superior governments on various projects, and the lagged dependent variable. Standard errors in parentheses are clustered by village. *** p<0.01, ** p<0.05, * p<0.1.

Table 9. Longer-Run Impact on the Incidence of Informal Interest-Free Borrowing

Outcome variables	Incidence of borrowing		
	All loans	Production loans	Consumption loans
	(1)	(2)	(3)
Panel A: Measured in the second wave			
ATE	-0.07	0.02	-0.10*
	(0.05)	(0.02)	(0.05)
Mean at baseline	0.50	0.17	0.41
Observations	962	962	962
Panel B: Measured in the third wave			
ATE	-0.10***	-0.01	-0.09***
	(0.04)	(0.02)	(0.03)
Mean at baseline	0.50	0.17	0.41
Observations	962	962	962

Notes: The table reports the estimated longer-run effect on the incidence of informal interest-free borrowing by using a sample of households in villages that were treated in the first round and those in villages that remained untreated by the time of the third survey. All regressions also control for village characteristics measured at baseline, including type of geographic features, population, area of arable land, public spending financed by superior governments on various projects, and the dependent variable measured in the baseline survey. Standard errors in parentheses are clustered by village. *** p<0.01, ** p<0.05, * p<0.1.

Table 10. Impact on Well-Being

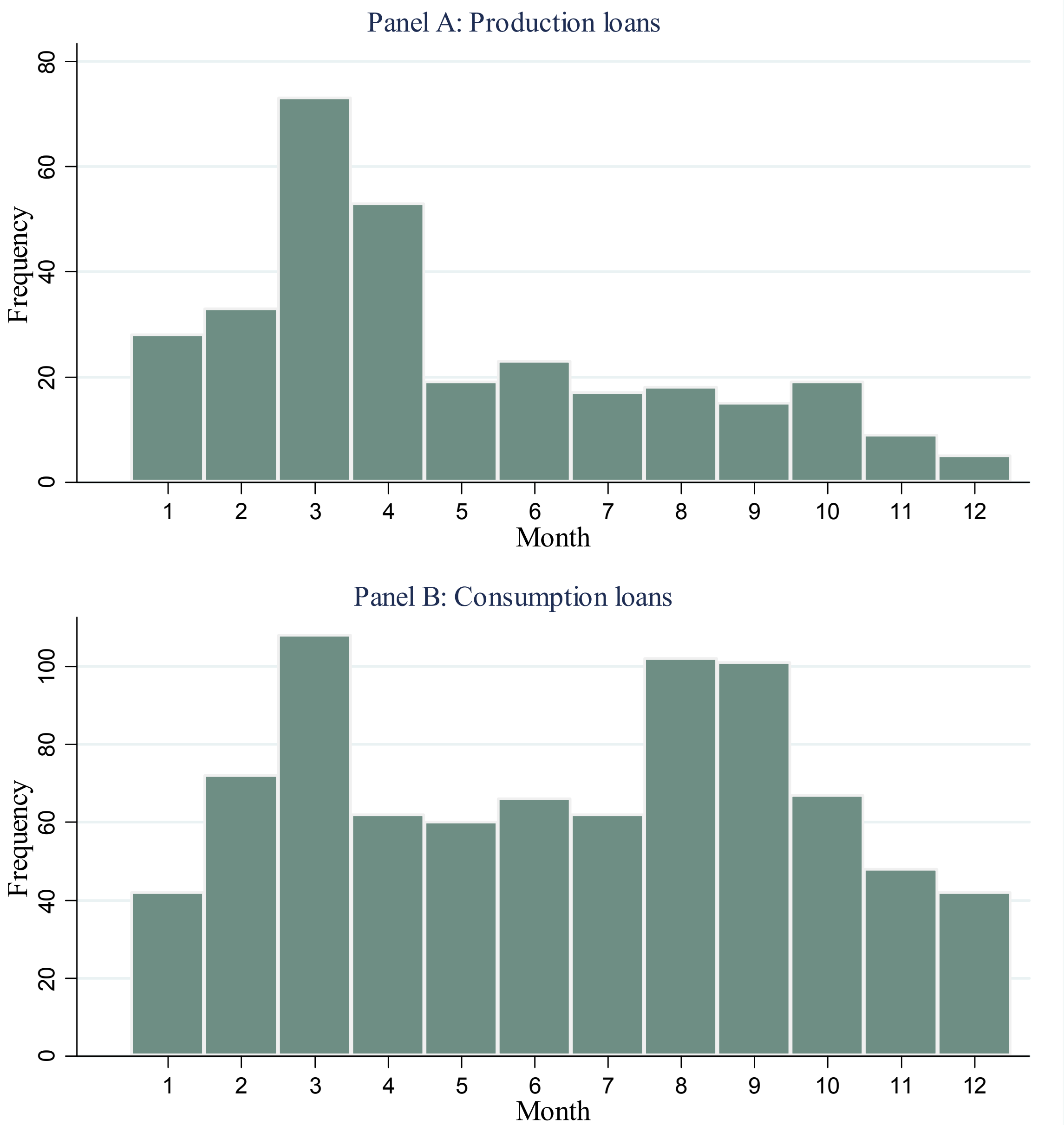
Outcome measured in the second wave of the survey (normalized)	Standard of living: Self-assessment	Standard of living: Assessment of village officials	Satisfaction with life
	(1)	(2)	(3)
Panel A			
ATE-borrower	0.77***	0.35**	0.35*
	(0.23)	(0.13)	(0.20)
Observations	242	232	242
Panel B			
ATE-member & non-borrower	0.69***	0.05	0.54***
	(0.09)	(0.06)	(0.10)
Observations	289	274	289
Panel C			
ATE-non-member	0.21**	0.06**	0.12
	(0.09)	(0.02)	(0.09)
Observations	527	506	527

Notes: The table reports the estimated effect on the subjective assessment of standard of living (including self-assessment and assessment of village officials) and life satisfaction by using information on participation in the program and borrowing from the program for households in the control villages that were treated after the second wave of the survey. The measure of self-assessment of standard of living is based on the question "Compared to two years ago, how does the standard of living of your household change? Answers: 1-worse off, 2-about the same, 3-better off." The measure of village officials' assessment is based on the question "How about the standard of living of this household in the year of 2011? Answers: 0-very poor, 1-poor, 3-average, 5-rich, 7-very rich." The measure of satisfaction with life is based on the question "Are you satisfied with your current life? Answers: 1-dissatisfied, 2-just so so, 3-satisfied." All the outcome variables are normalized by subtracting the mean and dividing by the standard deviation in the control group. The regressions also control for village characteristics measured at baseline, including type of geographic features, population, area of arable land, and public spending financed by superior governments on various projects. Regressions in column (2) further control for village officials' assessments of the standard of living of households in the years prior to the program which is measured as the average of answers to the retrospective question "How about the standard of living of this household every year from 2006 to 2010?" Standard errors in parentheses are clustered by village. *** p<0.01, ** p<0.05, * p<0.1.

Online Appendices

A. Supplementary Figures and Tables

Figure A1. Time of Borrowing



Notes: The figure illustrates the number of production and consumption loans by month in 2009 in Panels A and B, respectively.

Table A1. Balance Tests on Baseline Household Characteristics

Baseline characteristics	Control mean	Control S.D.	Mean difference	Normalized difference	t-test (T=C)
	(1)	(2)	(3)	(4)	(5)
Panel A: Borrowing outcomes (# hh in control village=542, # hh in treatment village=809)					
Receiving formal loans	0.12	0.32	0.02	0.05	0.388
Receiving informal interest-bearing loans	0.04	0.19	0.00	0.02	0.810
Receiving informal interest-free loans	0.51	0.50	0.01	0.01	0.873
Receiving loans from any sources	0.57	0.50	0.03	0.04	0.585
Average standardized difference (p-value)					0.558
Number of formal loans	0.14	0.43	0.01	0.02	0.646
Number of informal interest-bearing loans	0.04	0.22	0.02	0.06	0.324
Number of informal interest-free loans	0.89	1.26	0.08	0.04	0.640
Number of loans from any sources	1.07	1.39	0.12	0.06	0.508
Average standardized difference (p-value)					0.356
Amount of formal loans	2633	10787	-424	-0.03	0.536
Amount of informal interest-bearing loans	981	7649	-196	-0.02	0.743
Amount of informal interest-free loans	5965	13324	-775	-0.04	0.471
Amount of loans from any sources	9579	21615	-1395	-0.05	0.433
Average standardized difference (p-value)					0.442
Panel B: Household characteristics (# hh in control village=542, # hh in treatment village=809)					
Age of household head	52.09	11.18	0.81	0.05	0.541
Years of schooling of household head	5.40	3.80	-0.45	-0.09	0.275
Log household assets	9.82	1.30	-0.06	-0.04	0.756
Log household expenditure	9.66	0.69	-0.04	-0.05	0.621
Log amount of loans can borrow in emergency	6.89	3.64	-0.33	-0.06	0.287
Household plotting land (unit: *mu*)	5.75	7.83	0.29	0.03	0.846
Female ratio in household	0.47	0.15	0.00	0.01	0.690
Farm labor ratio in household	0.82	0.20	0.00	0.01	0.940
Household size	4.33	1.53	-0.14	-0.06	0.490
Average standardized difference (p-value)					0.532

Notes: The table reports the results of balance tests on the baseline characteristics between households in the treatment villages and those in the control villages. The sample includes the households that were successfully followed in the second survey. Columns (1) and (2) report the mean and standard deviation in the control group at baseline, respectively. Column (3) reports the difference in means between the treatment and control groups. Column (4) reports the normalized difference computed as the difference in means between the treatment and control groups divided by the square root of the sum of the variances in both groups. All monetary values are in 2009 CNY. 1 *mu* is equal to 667 square meters. The *p*-values in the last column are estimated by regressing the variable on a dummy of program villages, with standard errors clustered by village.

Table A2. Balance Tests on Baseline Village Characteristics

Baseline characteristics	Control mean	Control S.D.	Mean difference	Normalized difference	t-test (T=C)
	(1)	(2)	(3)	(4)	(5)
Village characteristics (# control village=20, # treatment village=30)					
Geographic features					
Mountainous (yes=1)	0.60	0.50	-0.03	-0.05	0.819
Hilly (yes=1)	0.30	0.47	0.03	0.05	0.808
Plain (yes=1)	0.10	0.31	0.00	0.00	1.000
Population	971.15	488.12	188.18	0.24	0.229
Area of arable land (unit: *mu*)	1396.07	1605.22	651.13	0.24	0.238
Public spending financed by superior government (thousand *yuan*)					
Connection (telephone, audio, cable TV)	1.98	6.89	-1.24	-0.17	0.432
Energy (electricity, gas, etc.)	4.39	8.81	-1.12	-0.11	0.616
Drinking water	5.31	14.35	-1.66	-0.10	0.645
Irrigation and water conservancy	0.50	2.24	10.07	0.26	0.155
Land improvement	0.98	3.26	4.62	0.20	0.282
Environment improvement	0.00	0.00	2.48	0.19	0.295
Education	12.45	41.66	-11.65	-0.28	0.215
Hospital and clean toilet	1.32	2.73	0.23	0.06	0.777
Others	0.42	1.77	0.38	0.14	0.501
Average standardized difference (p-value)					0.080

Notes: The table reports the results of balance tests on the baseline characteristics between the treatment and control villages. The observation unit is village. Columns (1) and (2) report the mean and standard deviation in the control group at baseline, respectively. Column (3) reports the difference in means between the treatment and control groups. Column (4) reports the normalized difference computed as the difference in means between the treatment and control groups divided by the square root of the sum of the variances in both groups. All monetary values are in 2009 CNY. 1 *mu* is equal to 667 square meters. The *p*-values in the last column are estimated by regressing the variable on a dummy of program villages.

Table A3. Summary Statistics on Consumption Loans

Loan purpose	Frequency	Percent	Share of households
	(1)	(2)	(3)
Medical expenses	442	43.8	20.7
Educational expenses	195	19.3	9.5
Build or buy a house	174	17.3	8.3
Ceremonies	122	12.1	7.0
Buy food or other daily expenses	49	4.9	3.4
Buy a car or durable goods	16	1.6	1.0
Give birth to a child	10	1.0	0.7
All consumption loans	1008	100	44.3

Notes: Column (1) reports the frequency of consumption loans taken up by the 1,351 households in 2009 by the stated purpose, while Column (2) reports their percentage share in the total number of consumption loans. The last column reports the share of households that took up loans for the corresponding purposes in 2009.

Table A4. Impact on the Number and Amount of Informal Interest-Free Borrowing

Borrowing measured in the second wave of the survey	Number of loans			Amount of loans		
	All loans	Production loans	Consumption loans	All loans	Production loans	Consumption loans
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: ATE	-0.16**	0.01	-0.17**	-1,431*	-297	-1,062
	(0.07)	(0.02)	(0.07)	(805)	(248)	(717)
Mean at baseline	0.94	0.24	0.70	5,500	1,103	4,328
Observations	1,351	1,351	1,351	1,351	1,351	1,351
Panel B: ATE-borrower	-0.60**	-0.20	-0.40	-7,907*	-1,818	-5,536*
	(0.27)	(0.12)	(0.28)	(4,135)	(1,215)	(3,229)
Mean at baseline	1.27	0.35	0.91	6,288	1,469	4,634
Observations	243	243	243	243	243	243
Panel C: ATE-member & non-borrower	-0.42***	0.07	-0.45***	-4,576*	584	-4,732*
	(0.13)	(0.06)	(0.14)	(2,403)	(532)	(2,478)
Mean at baseline	0.82	0.19	0.63	4,938	853	4,085
Observations	291	291	291	291	291	291
Panel D: ATE-non-member	-0.12	-0.02	-0.09	-2,306***	-729**	-1,228
	(0.09)	(0.03)	(0.07)	(829)	(353)	(938)
Mean at baseline	0.84	0.21	0.62	5,622	1,241	4,291
Observations	532	532	532	532	532	532

Notes: The table reports the estimated effect on the number and amount of informal interest-free loans by using information on participation in the program and borrowing from the program for households in the control villages that were treated after the second wave of the survey. All regressions also control for village characteristics measured at baseline, including type of geographic features, population, area of arable land, public spending financed by superior governments on various projects, and the lagged dependent variable. Standard errors in parentheses are clustered by village. *** p<0.01, ** p<0.05, * p<0.1.

Table A5. Impact on Informal Interest-Free Loans and Received Transfers

Outcome measured in the second wave of the survey	Borrowing any interest-free loans or receiving any private transfers	Total amount of informal interest-free loans and private transfers received
	(1)	(2)
Panel A: ATE	-0.00	-2,019*
	(0.05)	(1,036)
Mean at baseline	0.76	6,914
Observations	1,351	1,351
Panel B: ATE-borrower	-0.29***	-7,800**
	(0.10)	(3,738)
Mean at baseline	0.82	7,649
Observations	243	243
Panel C: ATE-member & non-borrower	-0.14**	-4,387
	(0.07)	(2,610)
Mean at baseline	0.74	6,443
Observations	291	291
Panel D: ATE-non-member	0.03	-1,796*
	(0.07)	(909)
Mean at baseline	0.72	6,656
Observations	532	532

Notes: The table reports the estimated effect on informal interest-free loans and received transfers by using information on participation in the program and borrowing from the program for households in the control villages that were treated after the second wave of the survey. Column (1) examines the effect on the likelihood of borrowing any interest-free loans or receiving any private transfers, where the transfer refers to those received from extended family networks, friends, and relatives. Column (2) examines the effect on the total amount of informal interest-free loans and private transfers received. All regressions also control for village characteristics measured at baseline, including type of geographic features, population, area of arable land, public spending financed by superior governments on various projects, and the lagged dependent variable. Standard errors in parentheses are clustered by village. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A6. Evidence on Anticipation of Borrowing from the Village Banks

Panel A: Reasons for joining the program

Reasons for joining the program	Borrowing from the village banks		Total
	Yes	No	
Need credit right away	125	35	160
Do not need credit right away, but maybe later	32	116	148
Follow the others	5	32	37
Village officers encouraged to do so	14	28	42
Mutual help	25	16	41
Others	18	11	29
Total	219	238	457

Panel B: Transition of membership and borrowing status (row %)

Status measured in the second wave of the survey	Status measured in the third wave of the survey			Total
	Borrower	Member & non-borrower	Non-member	
Borrower	71.0	13.0	16.0	100
Member & non-borrower	21.4	49.1	29.6	100
Non-member	8.1	9.1	82.7	100
Total	29.4	22.3	48.3	100

Notes: Panel A describes the distribution of reasons for joining the program by status of borrowing from village banks among households joining the program. Data is from the second wave of the survey. Panel B reports the row percentage by membership and borrowing status as measured in the second and third waves of the survey among households in villages that were treated in the first round. Data is from the second and third waves of the survey.

Table A7. Heterogeneity in Responses of Informal Interest-Free Borrowing by Stated Purpose to Health Shocks

Borrowing measured in the second wave of the survey	Taking up any informal interest-free loans (dummy)							
	All households		Borrower		Member & Non-borrower		Non-member	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Consumption loans								
Medical expense (in log)	0.04***	0.05***	0.03***	0.11	0.04***	0.14***	0.03***	0.04***
	(0.01)	(0.01)	(0.01)	(0.09)	(0.01)	(0.03)	(0.01)	(0.01)
Treat × Medical expense (in log)		-0.02**		-0.09		-0.12***		-0.01
		(0.01)		(0.09)		(0.04)		(0.02)
Observations	1,351	1,351	243	243	291	291	532	532
Panel B: Production loans								
Medical expense (in log)	0.00	0.00	0.01	0.05***	0.00	-0.01	0.00	-0.01
	(0.00)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Treat × Medical expense (in log)		0.00		-0.05**		0.02		0.01
		(0.01)		(0.02)		(0.01)		(0.02)
Observations	1,351	1,351	243	243	291	291	532	532

Notes: The dependent variable is the incidence of taking up informal interest-free loans for consumption and production in Panels A and B, respectively. The medical expense is measured as a logarithm (plus one). All regressions also control for treatment dummy, village characteristics measured at baseline, including type of geographic features, population, area of arable land, public spending financed by superior governments on various projects, and the lagged dependent variable. Standard errors in parentheses are clustered by village. *** p<0.01, ** p<0.05, * p<0.1.

Table A8. Impact on the Incidence of Informal Interest-Free Borrowing Conditional on Change in Income

Borrowing in the second wave	Treat change in income as exogenous			Treat change in income as endogenous		
	All loans	Production loans	Consumption loans	All loans	Production loans	Consumption loans
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
ATE-borrower	-0.27**	-0.08	-0.27**	-0.28***	-0.10	-0.26**
	(0.10)	(0.08)	(0.11)	(0.10)	(0.08)	(0.11)
Change in income/100,000	-0.06	-0.10***	0.02	-0.01	-0.08	0.07
	(0.05)	(0.03)	(0.05)	(0.10)	(0.06)	(0.09)
Observations	243	243	243	243	243	243
Panel B						
ATE-member & non-borrower	-0.20***	0.03	-0.22***	-0.21***	0.04	-0.25***
	(0.06)	(0.03)	(0.06)	(0.06)	(0.04)	(0.06)
Change in income/100,000	-0.27**	0.11*	-0.40***	-0.18	0.09	-0.24
	(0.13)	(0.06)	(0.12)	(0.19)	(0.08)	(0.19)
Observations	291	291	291	291	291	291
Panel C						
ATE-non-member	-0.07	0.00	-0.06	-0.06	0.00	-0.05
	(0.05)	(0.02)	(0.04)	(0.06)	(0.02)	(0.04)
Change in income/100,000	-0.14*	-0.01	-0.13*	-0.05	-0.01	-0.04
	(0.07)	(0.03)	(0.07)	(0.14)	(0.03)	(0.15)
Observations	532	532	532	532	532	532

Notes: Columns (1) to (3) report the estimated effect on the incidence of informal interest-free loans by further controlling for change in household income based on the regressions in Table 5. Columns (4) to (6) use the predicted value of change in household income from the regression of change in income on the dummy of village treatment status, the baseline village characteristics (including type of geographic features, population, area of arable land, and public spending financed by superior governments on various projects), the baseline household characteristics (including age of household head, years of schooling of household head, log household assets, log household expenditure, log amount of loans can borrow in emergency, household plotting land, female ratio, farm labor ratio, household size, and household income), and their interaction terms with the dummy of village treatment status. Standard errors in parentheses are clustered by village. *** p<0.01, ** p<0.05, * p<0.1.

Table A9. Longer-Run Impact on the Number and Amount of Informal Interest-Free Borrowing

Outcome variables	Number of loans			Amount of loans		
	All loans	Production loans	Consumption loans	All loans	Production loans	Consumption loans
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Measured in the second wave						
ATE	-0.20*	0.04	-0.24**	-397	188	-606
	(0.12)	(0.03)	(0.11)	(998)	(269)	(991)
Mean at baseline	0.93	0.25	0.67	4,955	1,027	3,832
Observations	962	962	962	962	962	962
Panel B: Measured in the third wave						
ATE	-0.18***	-0.00	-0.18**	-2,960*	152	-3,145**
	(0.07)	(0.02)	(0.07)	(1,496)	(560)	(1,365)
Mean at baseline	0.93	0.25	0.67	4,955	1,027	3,832
Observations	962	962	962	962	962	962

Notes: The table reports the estimated longer-run effect on the number and amount of informal interest-free borrowing by using a sample of households in villages that were treated in the first round and those in villages that remained untreated by the time of the third survey. All regressions also control for village characteristics measured at baseline, including type of geographic features, population, area of arable land, public spending financed by superior governments on various projects, and the dependent variable measured in the baseline survey. Standard errors in parentheses are clustered by village. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A10. Longer-Run Impact on Informal Interest-Free Loans and Received Transfers

Outcome variables	Borrowing any interest-free loans or receiving any private transfers	Total amount of informal interest-free loans and private transfers received
	(1)	(2)
Panel A: Measured in the second wave		
ATE	0.01	-2,235
	(0.06)	(1,837)
Mean at baseline	0.76	6,349
Observations	962	962
Panel B: Measured in the third wave		
ATE	-0.08***	-4,823**
	(0.02)	(2,165)
Mean at baseline	0.76	6,349
Observations	962	962

Notes: The table reports the estimated longer-run effect on informal interest-free loans and transfers by using samples of households in villages that were treated in the first round and those in villages that remained untreated by the time of the third survey. Column (1) examines the effect on the incidence of borrowing any interest-free loans or receiving any private transfers, where the transfer refers to those received from extended family networks, friends, and relatives. Column (2) examines the effect on the total amount of informal interest-free loans and private transfers received. All regressions also control for village characteristics measured at baseline, including type of geographic features, population, area of arable land, public spending financed by superior governments on various projects, and the dependent variable measured in the baseline survey. Standard errors in parentheses are clustered by village. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

B. Additional Robustness Checks

B.1. Robustness Check on Regression Specification

This section examines the robustness of the main results using two different regression specifications. One with no control variables, and one only controlling for the lagged dependent variable.

Columns (1) to (3) in Table B1 in this section report the simple post-treatment difference in the incidence of taking up informal interest-free loans between the treatment and control groups. The estimates suggest that the proportion of households in the treatment group that taken up informal interest-free loans was five percentage points smaller than in the control group (see Panel A), although it is not statistically significant. The difference was driven mainly by consumption loans. Specifically, the likelihood of taking up informal interest-free loans for consumption purpose for households in the treatment group declined by seven percentage points compared to that of the control group, which is statistically significant at the 10% level. In contrast, the likelihood of taking up informal interest-free loans for production purpose was not economically or statistically different between the treatment and control groups.

Panels B through D report the results on the comparison for the three types of household, respectively. As shown, the proportion of program borrowers in the treatment group that taken up informal interest-free loans was 21 percentage points lower than that of their counterparts in the control group; for program members who did not borrow from village banks, the difference was 15 percentage points and significantly different from 0. In contrast, the difference for non-members was much smaller and statistically insignificant.

Columns (4) to (6) in Table B1 examine the robustness by further controlling for the lagged dependent variable. As shown, the results are quite similar to those in columns (1) to (3). This is in line with the results that the treatment and control groups are balanced with respect to baseline outcomes as shown in Table A1.

Overall, the estimates based on the simple post-treatment comparison or the specification only controlling for the lagged dependent variable are largely consistent with the benchmark results reported in the main text.

B.2. Estimation using Nonlinear Models

In the main text, we report the OLS estimates of the program's impact on the incidence of borrowing by assuming a linear probability model as Equation (1). This section examines the robustness of the main results using the Probit model:

$$y_{ij,2} = \begin{cases} 1 & \text{if } y^*_{ij,2} = \alpha + \beta T_j + \lambda y_{ij,1} + \Gamma V_j + \varepsilon_{ij} > 0 \\ 0 & \text{otherwise} \end{cases}, \tag{B1}$$

where $y^*_{ij,2}$ is a latent continuous variable and ε_{ij} follows a normal distribution. The average marginal effect (or the average treatment effect) of the program on the incidence of borrowing is given by the formula:

$$\frac{1}{N}\sum_{i=1}^{N}[\phi(\alpha + \beta + \lambda y_{ij,1} + \Gamma V_j) - \phi(\alpha + \lambda y_{ij,1} + \Gamma V_j)],$$

which can be estimated from a Probit regression using the maximum-likelihood estimation method.

Table B2 in this section reports the results on the average treatment effects of the program on the incidence of taking up loans from various sources with Probit regressions. As shown, both the point estimates and the statistical significance are quite similar to the benchmark results that use OLS estimation method (see Table 4 in the main text). For instance, the Pobit estimates suggest the incidence of taking up interest-free loans for households in the treatment group declined, on average, by eight percentage points compared to that of the control group, and the estimate is statistically significant at the 5% level. They are the same as the OLS estimates reported in Table 4.

Table B3 presents the Probit estimates of the program's impacts on the incidence of taking up informal interest-free loans for the three types of household. Again, the Probit estimates are largely consistent with the estimates reported in Table 5 in the main text that use OLS estimation method.

Similarly, in the main text, we investigated the average treatment effect of the program on the frequency and amount of informal borrowing using OLS regressions. For robustness checks, Table B4 in this section examines the results based on the censored regressions. Specifically, consider a model of the latent variable $y^*_{ij,2}$:

$$y^*_{ij,2} = \alpha + \beta T_j + \lambda y_{ij,1} + \Gamma V_j + \varepsilon_{ij}, \tag{B2}$$

where $\varepsilon_{ij} \sim N(0, \sigma^2)$. $y^*_{ij,2}$ is only partially observable. That is,

$$y_{ij,2} = \begin{cases} y^*_{ij,2} & \text{if } y^*_{ij,2} > 0 \\ 0 & \text{if } y^*_{ij,2} \leq 0 \end{cases}. \tag{B3}$$

Given the Tobit model specified in Equations (B2) and (B3), the unconditional expectation of y (i.e., $\mathrm{E}[y|X]$) and the conditional expectation of y (i.e., $\mathrm{E}[y|y > 0, X]$) can be written as $\Phi\left(\frac{\gamma X}{\sigma}\right) * [\gamma X + \sigma\lambda(\gamma X)]$ and $\gamma X + \sigma\lambda(\gamma X)$, respectively, where X is a vector of all the explanatory variables in Equation (B2) and $\lambda(\cdot)$ is the hazard function.

The parameter β indicates the treatment effect of the program on the expectation on the latent variable y^* (i.e., $\mathrm{E}[y^*|X]$). However, we are interested in the program's impact on the unconditional and conditional expectation of y (i.e., $\mathrm{E}[y|X]$ and $\mathrm{E}[y|y > 0, X]$). According to the Tobit model above, the average unconditional treatment effect of the program on the frequency (or amount) of informal borrowing can be estimated by the formula:

$$\frac{1}{N}\sum_{i=1}^{N}[\Phi\left(\frac{\gamma X}{\sigma}\right) * [\gamma X + \sigma\lambda(\gamma X)]|_{T_i=1} - \Phi\left(\frac{\gamma X}{\sigma}\right) * [\gamma X + \sigma\lambda(\gamma X)]|_{T_i=0}],$$

and the average conditional treatment effect of the program on the frequency (or amount) of informal borrowing can be estimated by the formula:

$$\frac{1}{N}\sum_{i=1}^{N}[[\gamma X + \sigma\lambda(\gamma X)]|_{T_i=1} - [\gamma X + \sigma\lambda(\gamma X)]|_{T_i=0}].$$

The former estimator measures the average treatment effect of the program on the overall changes in the frequency or amount of informal borrowing (including changes in both extensive and intensive margins), whereas the latter estimator measures the average treatment effect of the program on the inframarginal changes in informal borrowing.

Table B4 in this section reports the estimates of the average unconditional treatment effects on the frequency and amount of informal borrowing. As shown, on average, the number of interest-free loans taken up from informal sources decreased by 0.17, and the amount decreased by 1,642 *yuan* as a result of the program. They are quite similar to the estimates based on the OLS estimation method (i.e., 0.16 and 1,431 *yuan*, see Table A4). Overall, estimates of the average treatment effect on the frequency and amount of informal borrowing are robust to estimation method using Tobit regressions.

Table B5 examines the average treatment effect of the program on the inframarginal changes in informal borrowing, i.e., the average conditional treatment effect. The results in Panel A suggest that the program significantly crowded out the frequency and amount of informal interest-free loans conditional on taking up the loans. The effects were driven mainly by borrowing for consumption purpose, whereas the effects were neither economically nor statistically different from 0 for production loans. Panels B through D examines the effects for the three types of household. Consistent with the benchmark estimates, the results suggest that the crowding-out effect on informal borrowing exists for both borrowers and non-borrowers among the program members. However, it is worth noting that for non-members the program also significantly crowded out their frequency and amount of informal borrowing in terms of inframarginal changes, although the impacts are much less than that of the program members.

Overall, the results suggest that the program not only crowded out informal borrowing on the extensive margin (i.e., existing informal risk-sharing arrangements), but also reduced the frequency and amount of informal interest-free loans in terms of inframarginal changes.

Table B1. Impact on the Incidence of Informal Interest-Free Borrowing

Borrowing measured in the second wave of the survey	Simple difference in post-treatment			Only control the lagged dependent variable		
	All loans	Production loans	Consumption loans	All loans	Production loans	Consumption loans
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: ATE	-0.05	0.01	-0.07*	-0.06	0.01	-0.07*
	(0.04)	(0.02)	(0.04)	(0.04)	(0.02)	(0.04)
Mean at baseline	0.51	0.16	0.42	0.51	0.16	0.42
Observations	1,351	1,351	1,351	1,351	1,351	1,351
Panel B: ATE-borrower	-0.21	-0.08	-0.18	-0.22	-0.08	-0.20
	(0.15)	(0.07)	(0.12)	(0.15)	(0.07)	(0.12)
Mean at baseline	0.60	0.21	0.49	0.60	0.21	0.49
Observations	243	243	243	243	243	243
Panel C: ATE-member & non-borrower	-0.15**	0.06**	-0.20***	-0.15**	0.06**	-0.19**
	(0.07)	(0.03)	(0.07)	(0.07)	(0.03)	(0.07)
Mean at baseline	0.49	0.14	0.39	0.49	0.14	0.39
Observations	291	291	291	291	291	291
Panel D: ATE-non-member	-0.05	0.00	-0.04	-0.05	0.00	-0.04
	(0.05)	(0.02)	(0.04)	(0.05)	(0.02)	(0.04)
Mean at baseline	0.49	0.15	0.40	0.49	0.15	0.40
Observations	532	532	532	532	532	532

Notes: The table reports the estimated effect on the incidence of taking up informal interest-free loans by using information on participating in the program and borrowing from the program for households in the control villages that were treated after the second survey. Columns (1) to (3) report estimates on the simple post-treatment difference in the incidence of taking up informal interest-free loans between the treatment and control groups, whereas columns (4) to (6) further control for the lagged dependent variable. Standard errors in parentheses are clustered by village. *** p<0.01, ** p<0.05, * p<0.1.

Table B2. Average Treatment Effect on the Incidence of Borrowing using Probit Regression

Borrowing measured in the second wave of the survey	Any loans borrowed from				
	Village bank	Formal	Informal (interest-bearing)	Informal (interest-free)	Any
	(1)	(2)	(3)	(4)	(5)
Panel A: All loans					
ATE	N.A.	-0.03	-0.01	-0.08**	0.06*
		(0.02)	(0.01)	(0.03)	(0.04)
Mean at baseline	0.00	0.13	0.04	0.51	0.59
Observations	1,351	1,351	1,351	1,351	1,351
Panel B: Production loans					
ATE	N.A.	-0.01	0.01	0.02	0.18***
		(0.01)	(0.01)	(0.01)	(0.03)
Mean at baseline	0.00	0.05	0.02	0.16	0.22
Observations	1,351	1,351	963	1,351	1,351
Panel C: Consumption loans					
ATE	N.A.	-0.02	-0.03***	-0.10***	-0.09**
		(0.01)	(0.01)	(0.03)	(0.04)
Mean at baseline	0.00	0.08	0.02	0.42	0.47
Observations	1,351	1,351	1,351	1,351	1,351

Notes: The table reports the estimates of the average treatment effect of the program on the likelihood of taking up loans from various sources using probit regressions. All the regressions control for village characteristics measured at baseline, including type of geographic features, population, area of arable land, public spending financed by superior governments on various projects, and the lagged dependent variable. The estimated program's impacts on the incidence of taking up village bank loans are not available (i.e., N.A.), because the dummy of treatment status predicts failure perfectly. For the regression on informal interest-bearing loans for production purpose (Panel B, column (3)), 388 observations are dropped because the control variables predict their outcome variable perfectly. Standard errors in parentheses are clustered by village. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table B3. Impact on the Incidence of Informal Interest-Free Borrowing using Probit Regression

Borrowing measured in the second wave of the survey	All loans	Production loans	Consumption loans
	(1)	(2)	(3)
Panel A: ATE-borrower	-0.28***	0.01	-0.25**
	(0.11)	(0.05)	(0.11)
Mean at baseline	0.60	0.21	0.49
Observations	243	211	243
Panel B: ATE-member & non-borrower	-0.23***	0.06**	-0.27***
	(0.06)	(0.03)	(0.07)
Mean at baseline	0.49	0.14	0.39
Observations	291	291	291
Panel C: ATE-non-member	-0.07	-0.01	-0.07
	(0.05)	(0.02)	(0.04)
Mean at baseline	0.49	0.15	0.40
Observations	532	532	532

Notes: The table reports the probit estimates of the program's effect on the incidence of taking up informal interest-free loans for households that borrowed from the program (Panel A), those that participated in the program but did not borrow from it (Panel B), and those that did not participate in the program (Panel C) by the time of the second wave of the survey using information on participating in the program and borrowing from the program for households in the control villages that were treated after the second survey. All regressions also control for village characteristics measured at baseline, including type of geographic features, population, area of arable land, public spending financed by superior governments on various projects, and the lagged dependent variable. For the regression in column (2) of Panel A, 32 observations are dropped because the control variables predict their outcome variable perfectly. Standard errors in parentheses are clustered by village. *** p<0.01, ** p<0.05, * p<0.1.

Table B4. Impact on the Number and Amount of Informal Interest-Free Borrowing using Tobit Regression—Average Unconditional Treatment Effect

Borrowing measured in the second wave of the survey	Number of loans			Amount of loans		
	All loans	Production loans	Consumption loans	All loans	Production loans	Consumption loans
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: ATE	-0.17**	0.02	-0.19***	-1,642**	111	-1,801***
	(0.07)	(0.02)	(0.07)	(696)	(148)	(677)
Mean at baseline	0.94	0.24	0.70	5,500	1,103	4,328
Observations	1,351	1,351	1,351	1,351	1,351	1,351
Panel B: ATE-borrower	-0.55**	0.01	-0.46*	-6,420**	-1	-6,008*
	(0.27)	(0.06)	(0.28)	(3,025)	(382)	(3,165)
Mean at baseline	1.27	0.35	0.91	6,288	1,469	4,634
Observations	243	243	243	243	243	243
Panel C: ATE-member & non-borrower	-0.54***	0.09*	-0.59***	-5,564***	675	-6,439***
	(0.17)	(0.05)	(0.19)	(1,980)	(435)	(2,183)
Mean at baseline	0.82	0.19	0.63	4,938	853	4,085
Observations	291	291	291	291	291	291
Panel D: ATE-non-member	-0.17*	-0.01	-0.13*	-2,458**	-250	-1,723**
	(0.10)	(0.03)	(0.08)	(966)	(324)	(830)
Mean at baseline	0.84	0.21	0.62	5,622	1,241	4,291
Observations	532	532	532	532	532	532

Notes: The table reports the average unconditional treatment effects on the number and amount of informal interest-free loans from Tobit regressions using information on participation in the program and borrowing from the program for households in the control villages that were treated after the second wave of the survey. All regressions also control for village characteristics measured at baseline, including type of geographic features, population, area of arable land, public spending financed by superior governments on various projects, and the lagged dependent variable. Standard errors in parentheses are clustered by village. *** p<0.01, ** p<0.05, * p<0.1.

Table B5. Impact on the Number and Amount of Informal Interest-Free Borrowing using Tobit Regression—Average Conditional Treatment Effect

Borrowing measured in the second wave of the survey	Number of loans			Amount of loans		
	All loans	Production loans	Consumption loans	All loans	Production loans	Consumption loans
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: ATE	-0.14**	0.04	-0.16***	-1,388**	231	-1,637*
	(0.06)	(0.03)	(0.06)	(587)	(309)	(613)
Mean at baseline	0.94	0.24	0.70	5,500	1,103	4,328
Observations	1,351	1,351	1,351	1,351	1,351	1,351
Panel B: ATE-borrower	-0.42**	0.02	-0.38*	-5,147**	-2	-5,189**
	(0.20)	(0.09)	(0.21)	(2,277)	(591)	(2,482)
Mean at baseline	1.27	0.35	0.91	6,288	1,469	4,634
Observations	243	243	243	243	243	243
Panel C: ATE-member & non-borrower	-0.43***	0.23***	-0.49***	-4,546***	1,820***	-5,538***
	(0.13)	(0.05)	(0.15)	(1,560)	(431)	(1,793)
Mean at baseline	0.82	0.19	0.63	4,938	853	4,085
Observations	291	291	291	291	291	291
Panel D: ATE-non-member	-0.13*	-0.03	-0.11*	-1,981**	-486	-1,483**
	(0.08)	(0.05)	(0.06)	(789)	(617)	(725)
Mean at baseline	0.84	0.21	0.62	5,622	1,241	4,291
Observations	532	532	532	532	532	532

Notes: The table reports the average conditional treatment effects on the number and amount of informal interest-free loans from Tobit regressions using information on participation in the program and borrowing from the program for households in the control villages that were treated after the second wave of the survey. All regressions also control for village characteristics measured at baseline, including type of geographic features, population, area of arable land, public spending financed by superior governments on various projects, and the lagged dependent variable. Standard errors in parentheses are clustered by village. *** p<0.01, ** p<0.05, * p<0.1.

C. Alternative Strategy for Estimating Treatment Effect on (Non-)borrowers

This section checks the robustness of the results on mechanisms analyses reported in the main text by using a different estimation strategy. Specifically, we can estimate the average treatment effect on the three types of household (i.e., program borrowers, program members who did not borrow from the village bank, and households that were not program members) using the following equation:

$$y_{ij,2} = \alpha + \beta_1 T_j \times B_{ij} + \beta_2 T_j \times Nb_{ij} + \beta_3 T_j \times Nm_{ij} \\ + \gamma_1 B_{ij} + \gamma_2 Nb_{ij} + \lambda y_{ij,1} + \Gamma V_j + \varepsilon_{ij}, \quad \text{(C1)}$$

where i and j represent household and village, respectively; $y_{ij,2}$ is the outcome of interest measured in the second wave of the survey; T_j is an indicator of village j being treated in the first round; B_{ij} is an indicator of program borrower (observed by the time of the second survey for households in villages treated in the first round, or observed by the time of the third survey for households in villages treated in the second round); similarly, Nb_{ij} and Nm_{ij} are dummies indicating program members who did not borrow from the village bank and those who were not program members, respectively; $y_{ij,1}$ is the baseline outcome; V_j is a vector of village baseline characteristics, including the type of geographic features, population, area of arable land, and public spending financed by superior governments on various projects; ε_{ij} is the error term which is clustered at the level of village.

If the expectation of ε_{ij} is the same for the three types of households in villages treated in the first round and their counterparts in villages treated in the second round, respectively, the parameter β_1 denotes the average treatment effect on program borrowers (ATE-borrowers), β_2 indicates the average treatment effect on those program members who did not borrow from the program (ATE-members and non-borrowers), and β_3 corresponds to the average treatment effect on households that were not program members (ATE-non-members). These parameters can be estimated by pooling the sample of all households in the control villages that were treated later and the sample of all households in villages that were treated in the first round.

Table C1 in this section investigates the program impacts on the likelihood of taking up informal interest-free loans by estimating the above equation. The results indicate that the incidence of informal interest-free borrowing of program borrowers in the villages treated in the first round is 23 percentage points lower than their counterparts in the control villages that were treated later, and the estimated coefficient is significantly different from 0. Meanwhile, the estimates also show a crowding-out effect of a 17-percentage point reduction in informal interest-free borrowing among program members who did not borrow from the village banks, and the effect is statistically significant at the level of 5%. In contrast, the point estimate on the average treatment effect of the program on non-members is much smaller (i.e., -0.07) and statistically is not significantly different from 0. These estimates are largely consistent with the results in Table 5 presented in the text.

In a similar vein, we can examine the heterogeneity in the responses of informal interest-free borrowing to health shocks across household type using the following estimation equation:

$$y_{ij,2} = \alpha + \delta_1 S_{ij,2} \times B_{ij} + \delta_2 S_{ij,2} \times Nb_{ij} + \delta_3 S_{ij,2} \times Nm_{ij}$$
$$+\gamma_1 S_{ij,2} \times B_{ij} \times T_j + \gamma_2 S_{ij,2} \times Nb_{ij} \times T_j + \gamma_3 S_{ij,2} \times Nm_{ij} \times T_j$$
$$+\beta_1 T_j \times B_{ij} + \beta_2 T_j \times Nb_{ij} + \beta_3 T_j \times Nm_{ij} + \alpha_1 B_{ij} + \alpha_2 Nb_{ij} + \lambda y_{ij,1} + \Gamma V_j + \varepsilon_{ij}, \quad \text{(C2)}$$

where $S_{ij,2}$ is the log of one plus medical expenses of household i in village j during the year prior to the second survey, and the other notations are the same as those defined above. The parameters δ_1, δ_2, and δ_3 indicate the responses in the incidence of taking up informal interest-free loans to medical expenses in the absence of the program for the latent counterparts of program borrowers, program members who did not borrow from village banks, and households that were not program members, respectively. The parameter γ_1 (or γ_2, γ_3) denotes the estimates of the differences in the responses of informal interest-free borrowing to health shocks between program borrowers (or program members who did not borrow from the village banks, or households that were not program members) in villages treated in the first round and their counterparts in villages treated in the second round.

Table C2 in this section reports the estimation results of Equation (C2). As shown in column (1), in the absence of the village banking program, all three types of household responded positively in terms of informal interest-free borrowing to health shocks. Column (2) indicates that the responses in informal interest-free borrowing among program members (irrespective of whether they borrowed from the program or not) in villages treated in the first round were much smaller than their counterparts in villages treated in the second round. In contrast, there is essentially no difference in the responses between non-members in the treatment group and their counterparts in the control group. The estimates are quite similar to those reported in Table 6 in the text, confirming that the crowding-out effect of the program on informal risk sharing exists among program members who did not borrow from the village bank.

Table C3 examines the impacts of the program on informal interest-free borrowing of the three types of household by time period. Consistent with Table 7 in the text, the results show a significant crowding-out effect on informal borrowing among program members who did not borrow from the village bank even during the program's announcement period. The rest of the estimates are also similar to those in Table 7.

Table C4 investigates the average treatment effects on available credit of the three types of household in case of emergency. As shown, there was no significant reduction in available credit from either informal or formal sources. If anything, the results indicate significant increases in the amount of informal loans and formal loans available to households that were program members but did not borrow from the village bank. Again, these estimates are largely similar to those reported in Table 8 in the text, corroborating that the reduction in informal interest-free borrowing should not be driven by a decrease in available credit; rather, it is likely to be caused mainly by a reduction in credit demand.

Lastly, Table C5 in the section analyzes the program's welfare implications for different households. In line with the findings in Table 10 in the main text, the results indicate significant improvement in material well-being among program borrowers according to the assessment of the village officials, although evidence based on self-assessment might suggest that the material well-being of all three types of household in the treatment group was higher than their counterparts in the control group. However, when we look at the impact on life satisfaction (a proxy of decision utility), the results indicate significant welfare gain among program members who did not borrow from the village banks. This coincides with the theoretical prediction that household's decision utility increases when they choose to rely less on mutual insurance as a result of anticipating access to credit from the program.

Overall, the results from analyses of using the alternative strategy for estimating the effects on different types of household are largely similar to those reported in the main text.

Table C1. Impact on the Incidence of Informal Interest-Free Borrowing

Borrowing measured in the second wave of the survey	All loans	Production loans	Consumption loans
	(1)	(2)	(3)
Treat × borrower	-0.23*	-0.07	-0.22*
	(0.13)	(0.07)	(0.11)
Treat × member & non-borrower	-0.17**	0.06**	-0.22***
	(0.07)	(0.02)	(0.08)
Treat × non-member	-0.07	0.00	-0.06
	(0.06)	(0.02)	(0.04)
Observations	1,065	1,065	1,065

Notes: The table reports the estimated average treatment effect on the incidence of taking up informal interest-free loans for program borrowers, program members who did not borrow from the village bank, and households that were not program members, using information on participating in the program and borrowing from the program for households in the control villages that were treated after the second survey. The sample consists of all households in villages that were treated in the first or second round. All regressions also control for the indicators of household type (program borrowers or program members who did not borrow from the village banks), the village characteristics measured at baseline (including type of geographic features, population, area of arable land, public spending financed by superior governments on various projects), and the lagged dependent variable. Standard errors in parentheses are clustered by village. *** p<0.01, ** p<0.05, * p<0.1.

Table C2. Heterogeneity in Responses of Informal Interest-Free Borrowing to Health Shocks

Borrowing measured in the second wave of the survey	Taking up any informal interest-free loans (dummy)	
	(1)	(2)
Medical expense (log)×borrower	0.04***	0.16*
	(0.01)	(0.08)
Medical expense (log)×member & non-borrower	0.03***	0.14***
	(0.01)	(0.02)
Medical expense (log)×non-member	0.04***	0.04*
	(0.01)	(0.02)
Treat×Medical expense (log)×borrower		-0.12
		(0.08)
Treat×Medical expense (log)×member & non-borrower		-0.12***
		(0.02)
Treat×Medical expense (log)×non-member		-0.00
		(0.02)
Observations	1,065	1,065

Notes: The sample consists of all households in villages that were treated in the first or second round. The dependent variable is the incidence of taking up informal interest-free loans for any purpose. The medical expense is measured as a logarithm (plus one). All regressions also control for the dummies of the three types of household, the interaction terms of the treatment dummy and the indicator of household type, village characteristics measured at baseline (including type of geographic features, population, area of arable land, public spending financed by superior governments on various projects), and the lagged dependent variable. Standard errors in parentheses are clustered by village. *** p<0.01, ** p<0.05, * p<0.1.

Table C3. Impact on the Incidence of Informal Interest-Free Borrowing by Time Period

Borrowing measured in the second wave of the survey	Announcement period (2010.8 - 2010.12)			Transition period (2011.1 - 2011.7)			Disbursement period (2011.8 - 2012.6)		
	All loans	Production loans	Consumption loans	All loans	Production loans	Consumption loans	All loans	Production loans	Consumption loans
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treat × borrower	-0.00	0.01	-0.01	-0.27**	-0.02	-0.26**	-0.15	-0.10	-0.10
	(0.05)	(0.01)	(0.05)	(0.12)	(0.05)	(0.12)	(0.09)	(0.07)	(0.08)
Treat × member & non-borrower	-0.07*	0.00	-0.07*	-0.09**	0.02	-0.11**	-0.10	0.04**	-0.13
	(0.04)	(0.01)	(0.03)	(0.04)	(0.02)	(0.05)	(0.09)	(0.02)	(0.09)
Treat × non-member	-0.03	-0.00	-0.02	-0.04	0.00	-0.02	-0.04	-0.01	-0.03
	(0.02)	(0.01)	(0.02)	(0.03)	(0.01)	(0.03)	(0.04)	(0.02)	(0.04)
Observations	1,065	1,065	1,065	1,065	1,065	1,065	1,065	1,065	1,065

Notes: The table reports the estimated effect on the incidence of informal interest-free borrowing using information on participation in the program and borrowing from the program for households in the control villages that were treated after the second wave of the survey. The sample consists of all households in villages that were treated in the first or second round. All regressions also control for the indicators of household type (program borrowers or program members who did not borrow from the village banks), the village characteristics measured at baseline (including type of geographic features, population, area of arable land, public spending financed by superior governments on various projects), and the lagged dependent variable. Standard errors in parentheses are clustered by village. *** p<0.01, ** p<0.05, * p<0.1.

Table C4. Impact on Available Credit in an Emergency

Outcome measured in the second wave of the survey	Can you obtain informal loans if you need them?	How many persons can you borrow loans from if you need them?	How much informal loans can you borrow if you need them?	Can you obtain formal loans if you need them?	How much formal loans can you borrow if you need them?
	(1)	(2)	(3)	(4)	(5)
Treat × borrower	0.03	1.16	-3,293	0.02	-509
	(0.14)	(1.40)	(6,433)	(0.10)	(2,898)
Treat × member & non-borrower	0.10	1.78	3,977**	0.22***	3,535**
	(0.10)	(1.43)	(1,567)	(0.05)	(1,559)
Treat × non-member	0.02	-0.89	4,304	0.02	1,209
	(0.04)	(0.73)	(2,883)	(0.05)	(2,083)
Observations	1,042	1,030	1,040	1,043	1,030

Notes: The sample consists of all households in villages that were treated in the first or second round. The dependent variable in column (1) is a dummy that equals one if the household can obtain informal loans in times of need, whereas the dependent variable in columns (2) and (3) measures the number of persons from whom households can take up loans and the amount of informal loans households can take up if they need them, respectively. The dependent variables in columns (4) and (5) measure the household's capacity to borrow from formal financial institutions in a similar manner. All regressions also control for the indicators of household type (program borrowers or program members who did not borrow from the village banks), the village characteristics measured at baseline (including type of geographic features, population, area of arable land, public spending financed by superior governments on various projects), and the lagged dependent variable. Standard errors in parentheses are clustered by village. *** p<0.01, ** p<0.05, * p<0.1.

Table C5. Impact on Well-Being

Outcome measured in the second wave of the survey (normalized)	Standard of living: Self-assessment	Standard of living: Assessment of village officials	Satisfaction with life
	(1)	(2)	(3)
Treat×borrower	0.91***	0.31***	0.24
	(0.26)	(0.11)	(0.22)
Treat×member & non-borrower	0.56***	0.01	0.43***
	(0.11)	(0.05)	(0.14)
Treat×non-member	0.20**	0.04	0.11
	(0.09)	(0.03)	(0.09)
Observations	1,057	1,011	1,057

Notes: The table reports the estimated effect on the subjective assessment of standard of living (including self-assessment and assessment of village officials) and life satisfaction by using information on participation in the program and borrowing from the program for households in the control villages that were treated after the second wave of the survey. The sample consists of all households in villages that were treated in the first or second round. The measure of self-assessment of standard of living is based on the question "Compared to two years ago, how does the standard of living of your household change? Answers: 1-worse off, 2-about the same, 3-better off." The measure of village officials' assessment is based on the question "How about the standard of living of this household in the year of 2011? Answers: 0-very poor, 1-poor, 3-average, 5-rich, 7-very rich." The measure of satisfaction with life is based on the question "Are you satisfied with your current life? Answers: 1-dissatisfied, 2-just so so, 3-satisfied." All the outcome variables are normalized by subtracting the mean and dividing by the standard deviation in the control group. The regressions also control for the indicators of household type (program borrowers or program members who did not borrow from the village banks), and the village characteristics measured at baseline (including type of geographic features, population, area of arable land, and public spending financed by superior governments on various projects). Regressions in column (2) further control for village officials' assessments of the standard of living of households in the years prior to the program, which is measured as the average of answers to the retrospective question "How about the standard of living of this household in every year from 2006 to 2010?" Standard errors in parentheses are clustered by village. *** p<0.01, ** p<0.05, * p<0.1.

D. Identifying the Treatment Effect on Non-borrowers and Non-members

The average treatment effect on program members who did not borrow from the village banks by the time of the second survey (ATE-members and non-borrowers) is $ATE_{Nb} = \mathrm{E}\left(y_{ij,2}^{1}|T_j = 1, Nb_{ij,2} = 1\right) - \mathrm{E}\left(y_{ij,2}^{0}|T_j = 1, Nb_{ij,2} = 1\right)$, where $Nb_{ij,2}$ takes on a value of one if the household participated in the program but did not borrow from the village bank by the time of the second survey, and zero otherwise. This can be estimated by $\widehat{ATE_{Nb}} = \mathrm{E}\left(y_{ij,2}^{1}|T_j = 1, Nb_{ij,2} = 1\right) - \mathrm{E}\left(y_{ij,2}^{0}|T_j = 0, Nb_{ij,3} = 1\right)$, where $Nb_{ij,3}$ is an indicator of whether households i participated in the program but did not borrow from the village bank, as observed in the third wave of the survey. By formulating the program participation as determined by an indicator function $\mathbb{1}(g\left(y_{ij,1}, X_{ij,1}\right) + \zeta_{ij,2} > c')$, where $g(\cdot)$ denotes utility derived from program participation, c' is the participation cost, and $\zeta_{ij,2}$ is a random error term, the estimation bias can be written as:

$$\widehat{ATE_{Nb}} - ATE_{Nb} = \mathrm{E}\left(\varepsilon_{ij,2}|T_j = 1, Nb_{ij,2} = 1\right) - \mathrm{E}\left(\varepsilon_{ij,2}|T_j = 0, Nb_{ij,3} = 1\right)$$

$$= \mathrm{E}\left(\varepsilon_{ij,2}|T_j = 1, e_{ij,2} < c - f\left(y_{ij,1}, X_{ij,1}\right), \zeta_{ij,2} > c' - g(y_{ij,1}, X_{ij,1})\right)$$

$$-\mathrm{E}\left(\varepsilon_{ij,2}|T_j = 0, e_{ij,3} < c - f\left(y_{ij,2}, X_{ij,2}\right), \zeta_{ij,3} > c' - g(y_{ij,2}, X_{ij,2})\right). \quad \text{(D1)}$$

Similarly, the average treatment effects on households that did not participate in the program can be written as $ATE_{Nm} = \mathrm{E}\left(y_{ij,2}^{1}|T_j = 1, Nm_{ij,2} = 1\right) - \mathrm{E}\left(y_{ij,2}^{0}|T_j = 1, Nm_{ij,2} = 1\right)$, where $Nm_{ij,2}$ is a dummy indicating that the household did not participate in the program by the time of the second survey. This can be estimated by comparing the average outcome of non-members (observed in the second wave of the survey) in villages treated in the first round and the mean outcome of non-members (observed in the third wave of the survey) in villages treated in the second round, namely, $\widehat{ATE_{Nm}} = \mathrm{E}\left(y_{ij,2}^{1}|T_j = 1, Nm_{ij,2} = 1\right) - \mathrm{E}\left(y_{ij,2}^{0}|T_j = 0, Nm_{ij,3} = 1\right)$. The estimation bias can be formulized as:

$$\widehat{ATE_{Nm}} - ATE_{Nm} = \mathrm{E}\left(\varepsilon_{ij,2}|T_j = 1, Nm_{ij,2} = 1\right) - \mathrm{E}\left(\varepsilon_{ij,2}|T_j = 0, Nm_{ij,3} = 1\right) =$$

$$\mathrm{E}\left(\varepsilon_{ij,2}|T_j = 1, \zeta_{ij,2} < c' - g(y_{ij,1}, X_{ij,1})\right) - \mathrm{E}\left(\varepsilon_{ij,2}|T_j = 0, \zeta_{ij,3} < c' - g(y_{ij,2}, X_{ij,2})\right). \quad \text{(D2)}$$

As demonstrated in Equations (D1) and (D2), $\widehat{ATE_{Nb}}$ will be an unbiased estimator for ATE_{Nb} if $\partial f(\cdot)/\partial y_{ij,t-1} = 0$ and $\partial g(\cdot)/\partial y_{ij,t-1} = 0$, and, similarly, the estimation bias of ATE_{Nm} will be 0 if $\partial g(\cdot)/\partial y_{ij,t-1} = 0$.

To test the above hypotheses, Table D1 in the section reports the estimation results of probit regressions on participating in, and borrowing from, the program as predicted by the households' characteristics, by pooling the sample of households in villages that were treated in the first round and the sample of households in the control villages that were treated later. The outcome variables are measured using the second wave data for treatment group and the third wave data for the control group, whereas the covariates are measured using data from prior wave, respectively. As shown, households with greater expenditure were more likely to participate in the program. This coincides with the fact that most households joined the program to relax credit constraint (see Table A6 in this Appendix). The propensity to borrow from the program correlates with the age of the household head in an inversed U-shape, whereas it increases with household's area of plotting land and share of the labor force working on a farm. These results are consistent with the conjecture that households with more investment opportunity are more inclined to borrow from the program. In addition, the results in columns (2) and (4) indicate that the estimated coefficients of taking up informal interest-free loans in the probit regressions of participating in, and borrowing from, the village bank are 0.18 and 0.22, respectively (implying a marginal effect of 0.051 and 0.053, respectively). However, neither of the estimates is statistically different from 0 on the significance level of 10%. The results suggest that we cannot reject the null hypotheses that participating in, and borrowing from, the village bank are uncorrelated with prior status of taking up informal interest-free loans. Therefore, the estimation bias, if any, is likely to be small.

Table D1. Probit Regressions on Participating in and Borrowing from the Program

Dependent variable	Participating in the program		Borrowing from the program	
	(1)	(2)	(3)	(4)
Covariates measured in prior wave				
Age of household head	0.03	0.03	0.07*	0.07*
	(0.03)	(0.03)	(0.04)	(0.04)
(Age of household head/10)^2	-0.03	-0.03	-0.07**	-0.07*
	(0.03)	(0.03)	(0.04)	(0.04)
Years of schooling of household head	0.01	0.02	0.02	0.02
	(0.02)	(0.02)	(0.02)	(0.02)
Log of household assets	0.05	0.06	0.03	0.04
	(0.05)	(0.05)	(0.06)	(0.06)
Log of household expenditure	0.14*	0.12	0.12	0.10
	(0.07)	(0.08)	(0.10)	(0.10)
Log of amount of money that can be borrowed in a emergency	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Household plotting land	0.01	0.01	0.02	0.02*
	(0.01)	(0.01)	(0.01)	(0.01)
Female ratio in household	-0.09	-0.05	-0.22	-0.18
	(0.35)	(0.35)	(0.30)	(0.31)
Farm labor ratio in household	0.16	0.15	0.73***	0.70***
	(0.20)	(0.20)	(0.27)	(0.26)
Household size	0.01	0.01	0.03	0.02
	(0.03)	(0.03)	(0.03)	(0.03)
Taking up informal interest-free loans		0.18		0.22
		(0.11)		(0.14)
Constant	-3.53***	-3.56***	-5.47***	-5.47***
	(1.11)	(1.10)	(1.43)	(1.45)
Observations	1,065	1,065	1,065	1,065

Notes: The table reports estimation results of probit regressions on participating in, and borrowing from, the program, by pooling the sample of households in villages that were treated in the first round and the sample of all households in the control villages that were treated later. The outcome variables are measured using the second wave data for treatment group and the third wave data for the control group, whereas the covariates are measured using data from prior wave, respectively. The area of plotting land are measured in mu, where 1 *mu* equals 667 square meters. Farm labor refers to the labor force that worked on farming activities for more than half a year in 2009. All the regressions also control for the county dummies. Standard errors in parentheses are clustered by village. *** p<0.01, ** p<0.05, * p<0.1.

Table D2. Balance Tests on Baseline Borrowing Outcomes for the Three Types of Household

Baseline characteristics	Program borrowers			Program member & non-borrowers			Program non-members		
	Control mean	Control S.D.	Normalized difference	Control mean	Control S.D.	Normalized difference	Control mean	Control S.D.	Normalized difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Access to loans									
Receiving formal loans	0.16	0.37	0.11	0.16	0.37	-0.05	0.11	0.31	-0.03
Receiving informal interest-bearing loans	0.11	0.32	-0.12	0.00	0.00	0.20 **	0.05	0.23	-0.05
Receiving informal interest-free loans	0.53	0.51	0.11	0.52	0.50	-0.05	0.50	0.50	-0.02
Receiving loans from any sources	0.58	0.51	0.18	0.56	0.50	0.00	0.57	0.50	-0.01
Panel B: Number of loans									
Number of formal loans	0.16	0.37	0.14	0.20	0.53	-0.08	0.14	0.48	-0.06
Number of informal interest-bearing loans	0.11	0.32	0.00	0.00	0.00	0.19 **	0.06	0.29	-0.05
Number of informal interest-free loans	0.84	1.01	0.23	0.80	1.12	0.02	0.83	1.24	0.01
Number of loans from any sources	1.11	1.20	0.25	1.00	1.39	0.02	1.04	1.41	-0.02
Panel C: Amount of loans									
Amount of formal loans	6316	17388	-0.13	1900	6316	-0.04	1727	6718	0.00
Amount of informal interest-bearing loans	6526	19549	-0.26 *	0	0	0.15 *	1620	10708	-0.09
Amount of informal interest-free loans	13921	22361	-0.33	6548	15326	-0.10	6235	13911	-0.05
Amount of loans from any sources	26763	43480	-0.34 **	8448	20321	-0.05	9582	20197	-0.08
Average standardized difference (p-value)	0.837			0.957			0.431		

Notes: The normalized difference computed as the difference in means between the treatment and control groups divided by the square root of the sum of the variances in both groups. *** p<0.01, ** p<0.05, * p<0.1, indicating the significant level of test on the hypothesis that the difference equals 0. The last row reports the p-values of test on the null hypothesis that the average standardized difference in all variables is equal to 0. The standardized difference of each variable is measured as the difference between treatment and control groups divided by the standard deviation in the control group or that in the treatment group if the standard deviation in the control group is 0.

Table D3. Balance Tests on Baseline Household Characteristics for the Three Types of Household

Baseline characteristics	Program borrowers			Program member & non-borrowers			Program non-members		
	Control mean	Control S.D.	Normalized difference	Control mean	Control S.D.	Normalized difference	Control mean	Control S.D.	Normalized difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Household characteristics									
Age of household head	52.74	9.74	0.04	55.30	9.83	-0.17	48.53	11.13	0.27 ***
Years of shooling of household head	8.58	3.11	-0.74 ***	4.54	3.62	0.08	5.83	3.98	-0.18
Log household assets	10.18	1.20	-0.46 ***	9.83	1.12	-0.03	10.10	1.13	-0.08
Log household expenditure	10.21	0.65	-0.62 ***	10.00	0.62	-0.47 **	9.77	0.58	-0.16
Log amount of loans can borrow in emergency	5.06	4.54	0.25	6.71	3.78	-0.02	6.69	3.87	-0.03
Household plotting land (unit: *mu*)	11.32	23.76	-0.27 *	7.09	8.28	-0.14	9.26	7.76	-0.19
Female ratio in household	0.44	0.20	0.12	0.47	0.13	0.05	0.46	0.15	0.01
Farm labor ratio in household	0.83	0.17	0.17	0.82	0.16	-0.05	0.84	0.19	-0.12
Household size	4.05	0.97	0.02	4.76	1.52	-0.29 ***	4.65	1.51	-0.15 *

Notes: The normalized difference computed as the difference in means between the treatment and control groups divided by the square root of the sum of the variances in both groups. *** $p<0.01$, ** $p<0.05$, * $p<0.1$, indicating the significant level of test on the hypothesis that the difference equals 0. All monetary values are in 2009 CNY. The area of plotting land are measured in *mu*, where 1 *mu* equals 667 square meters. Farm labor refers to the labor force that worked on farming activities for more than half a year in 2009.

E. Proof of Lemma, Proposition, and Corollary

E.1. Proof of Lemma 1

Proof: This follows directly from the notion that the maximized utility of a constrained optimum is lower than the maximized utility of unconstrained optimum. *Q.E.D.*

E.2. Proof of Lemma 2

Proof: Similar to the proof of Lemma 1, this follows directly from the notion that the maximized utility of a constrained optimum is lower than the maximized utility of unconstrained optimum. *Q.E.D.*

E.3. Proof of Lemma 3

Proof: The Pareto efficient contracts (or the full risk-sharing arrangements) can be characterized by solving the social planner's problem: $\max \sum_{i=1}^{n} \lambda_i U^i(s^t)$ subject to the aggregate resource constraint of entire life time

$$w\colon \sum_{i=1}^{n} \sum_{k=0}^{\infty} c^i(s^{t+k}) \leq \mathrm{E}\left\{\sum_{i=1}^{n} \sum_{k=0}^{\infty} \left[e^i(s_{t+k}) \middle| s_t\right]\right\},$$

and the aggregated borrowing constrain in every period

$$w\Psi_k\colon \sum_{i=1}^{n} c^i(s^{t+k}) - \mathrm{E}\left\{\sum_{i=1}^{n} \left[e^i(s_{t+k}) \middle| s_t\right]\right\} \leq \mathrm{E}\left\{\sum_{i=1}^{n} \left[b^i(s_{t+k}) \middle| s_t\right]\right\},$$

$$\mathrm{k} = 0,1,\cdots.$$

As shown, the aggregated borrowing constrain only depends on the total borrowing limit of all agents. It does not depend on the distribution of borrowing limit across agent. Therefore, the full risk-sharing arrangements depend solely on the total borrowing limit of all agents in each period regardless their distribution across agent. *Q.E.D.*

E.4. Proof of Lemma 4

Proof: In full risk-sharing, the first-order conditions of the social planner's problem imply

$$\frac{\lambda_i}{\lambda_j} = \frac{E\{u'[c^j(s^{t+k})]|s_t\}}{E\{u'[c^i(s^{t+k})]|s_t\}}, \mathrm{k} = 0,1,\cdots.$$

That is, within each period, the expected marginal utility are equalized across agent according to their Pareto weights. If the total borrowing limit (i.e., $\mathrm{E}\{\sum_{i=1}^{n}[b^{i}(s_{t+k})|s_{t}]\}$) increases in a period when the whole economy is credit constrained, the additional credit will be allocated to agents to finance their consumption according to above first-order conditions. Given the optimum allocation of additional credit may not be exactly identical to the distribution of increases in borrowing limit across agents, transfers (or informal loans) will be realized to make sure the Pareto efficient contracts satisfy the first-order conditions above, implying increases in the transfers (or informal loans) between agents in the period.

However, if the credit constraint of the whole economy is currently binding, the full risk-sharing arrangements in current period will not be affected even though the total borrowing limit in a future period will be relaxed. Therefore, the transfers (or informal loans) in current period will not be affected as well. *Q.E.D.*

E.5. Proof of Proposition

Proof: From Lemma 2, we have that raising borrowing limit in any period of life when the agent expects she will be credit constrained will increase her expected discounted utility of autarky. According to the participation constraint condition

$$\lambda_{i}\phi_{i}\colon U^{i}(s^{t}) \geq \Omega^{i}(s_{t}),$$

it implies her participation constraint is tighter. Since only those risk-sharing contracts satisfying the participation constraint conditions are sustainable under limit commitment, it will reduce the scope of such contracts (or informal borrowing). *Q.E.D.*

E.6. Proof of Corollary 1

Proof: The aggregate transfers (or informal borrowing) received by agent i is $\kappa^{i}(s^{t+k})$, which depend on the state of nature s_{t} including the idiosyncratic shocks of her own and the others. From the Proposition, we have that raising borrowing limit in any period of life when the agent expects she will be credit constrained will reduce the implementable contracts. Therefore, in such case, response of transfers (or informal borrowing) with respect to idiosyncratic shocks will also reduce. *Q.E.D.*

E.7. Proof of Corollary 2

Proof: The corollary follows directly from the notion that the agent chooses to deviate from (some of) the risk-sharing arrangements because she can achieve higher expected utility from autarky than she derives from these risk-sharing arrangements. *Q.E.D.*