Targeting credit through community members

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Abstract

Delegating the allocation of public resources to community members is an increasingly popular form of delivering public resources in developing countries. However, this approach is associated with the tradeoff between improved information about potential beneficiaries and favoritism towards local elites, which could be strengthened in the context of credit. Unlike targeting cash transfers to the poor, the optimal targeting of credit is a more complex problem involving issues of productivity, repayment, and market responses: This paper analyzes this problem using a large-scale lending program, the Thai Million Baht Credit Fund, which decentralizes the allocation of loans to an elected group of community members, and provides three main results. First, exploiting a long and detailed panel, I recover pre-program structural estimates of household productivity and find that resources from the program were not allocated to high-productivity, poor households, which is inconsistent with poverty and productive efficiency as targeting criteria. Second, using socioeconomic networks data, I show that actual targeting is strongly driven by connections to village elites and is related to lower program profitability, which suggests favoritism as a reason for mistargeting. Finally, I exploit quasi-experimental variation in the rollout of the program and uncover evidence that, in general equilibrium, informal credit markets compensate for targeting distortions by redirecting credit towards unconnected households, albeit at higher interest rates than those provided by the program. The results highlight the limitations of community-driven approaches to program delivery and the role of markets in attenuating potential targeting errors.

Keywords: credit, social networks, targeting.

JEL: D14, G21, O12, O16, O17, L14, Z13

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1 Introduction

Community-driven development approaches to delivering public resources have gained increasing attention from academics and policy makers around the world. In developing countries, a number of social programs such as public works or cash transfer programs rely on community members for their implementation or monitoring.[1] One of the foundations of this approach is the idea that community members, as opposed to traditional policy makers, have better information to identify local needs. In the context of credit, delegating the allocation of resources to community members may lead to more accurate identification of potential borrowers and may fulfill the promise that was only partially materialized by traditional microfinance: providing affordable credit to poor, high-productivity households.[2]

One important class of community-based policies to expand access to credit is that of government infusions of resources into villages for the establishment of local credit funds which are managed by elected groups of community members.[3] The economic rationales for this approach include the reduction of intermediation and administrative costs as well as the benefit from information available to community members, which is costly to obtain by policy makers. On the other hand, community members may engage in favoritism towards politically connected households (Bardhan and Mookherjee, 2005). This tension is particularly salient in cases in which community members disperse public funds based on criteria that are hard to observe (unlike poverty targeting) and subject to moral hazard, as is the case with credit markets. Thus, whether the allocation of resources is consistent with poverty, productive efficiency or favoritism as targeting criteria is an empirical question. While the ability of community members to identify profitable households has been documented (Hussam et al., 2017), little is known regarding the effective use of this information when community members themselves are in charge of the allocation of public credit. Moreover, although the use of pre-program data has been essential for the empirical analysis of community-based approaches to delivering resources to the needy (Alatas et al., 2012), other studies analyzing how local leaders allocate

[1] See for example Mansuri and Rao (2004) for a review in the case of community-based approaches to infrastructure projects. Community based targeting of cash transfers has been studied by Alatas et al. (2012), and participatory rankings among community members have been used in graduation programs (Banerjee et al., 2015), and other programs that involved the delivery of cash transfers to the ultra poor (Bandiera et al., 2017).

[2] Uptake of credit in recent microcredit interventions has been low, due to, among other reasons, high interest rates and the difficulty of identifying high-productivity borrowers (Banerjee et al., 2015; Crépon et al., 2015; Banerjee et al., 2015). Reviews from either a policy or an academic perspective regarding the challenges of microfinance are provided by World Bank (2008); Armendáriz de Aghion and Murdoch (2004); Banerjee and Duflo (2010); Morduch (1999); Karlan and Morduch (2010).

[3] Broadly, community-based credit approaches consist of fostering local credit funds to be managed by community members. A clear example is the Million Baht program in Thailand (Kaboski and Townsend, 2012) and the Integrated Rural Development Program in India (Bardhan and Mookherjee, 2006b). While self-funded village credit groups are a growing research topic in the literature (see Deininger (2013); Greaney et al. (2016); Ksoll et al. (2016); Karlan et al. (2017), among others), there are other types of government-funded programs with a community based approach around the world such as the Andhra Pradesh Rural Poverty Reduction Project in India, and the Rural Financial Institutions Programme in Uttar Pradesh.

productive resources are based on post-program measures of productivity which are likely to be affected by the program itself (Bardhan and Mookherjee, 2006b; Basurto et al., 2017). In addition, previous studies have focused only on understanding how community members allocate resources but have ignored the role of markets in reallocating resources, which may attenuate potential targeting errors.

This paper empirically assesses these issues in the context of one of the largest community-based credit programs, the Thai Million Baht Village Fund (MBVF). Between 2001 and 2002, the government donated resources to over 90% of rural villages for the creation of local credit funds, which represented, on average, a 25% increase in the available funds for credit in each village. These funds were fully managed by elected village committees made up of community members, who decided who obtained credit and under what loan conditions.[4] This paper reports results from three empirical exercises: first, using a long panel, I structurally estimate a household production function and use the estimated factor elasticities to recover *pre-program* estimates of household total factor productivity.[5] I combine these estimates with baseline per-capita consumption data to test: *(i)* whether village committee members delivered credit to poor, high-productivity households, and *(ii)* whether offering credit to villagers based on alternative targeting criteria (i.e., means-testing and a baseline repayment probabilities) would have delivered credit to poor, high-productivity households. Second, I combine detailed data on pre-program socioeconomic networks with data about loan characteristics and repayment to test for favoritism towards households with connections to the local elite. Third, I use quasi-experimental variation in the rollout of the program to test for within-village general equilibrium responses in credit markets, which could lead to program spillovers to households with limited access to credit from the program.

First, I find that the program does not target poor, high-productivity households and that, in terms of poverty and productive efficiency, the program is outperformed by alternative targeting criteria. In practice, the allocation of loans was regressive and productively inefficient: the distribution of baseline per-capita consumption corresponding to program beneficiaries first-order stochastically dominated that of non-beneficiaries. Moreover, only 40% of high-productivity households (top 25% of the productivity distribution) borrowed from the program, and , on average, program borrowers had lower baseline productivity than non-borrowers. This allocation was not consistent neither with concerns regarding equity nor repay-

[4]The importance of this program and the fundamental tradeoffs in the allocation of productive resources have been of interest in the literature, but there are both unanswered questions and methodological limitations to existing studies. Kaboski and Townsend (2012) and Kaboski and Townsend (2011) have documented the effects of the MBVF on several household outcomes and the cost-effectiveness of the program. Breza et al. (2017) analyze whether baseline productivity explains heterogeneity in the effects of the program on investment and income growth but do not explore the mechanisms behind the allocation of resources from the program. Thus, what the program's *de facto* targeting criterion was–poverty reduction, productive efficiency, or favoritism–is yet unknown.

[5]Concretely, I exploit data on households' financial statements, in particular balance sheets, to measure capital as the value of the stock of total fixed assets for each household. The financial accounts data was compiled by Samphantharak and Townsend (2010).

ment. By comparing program borrowers to households that would have been eligible under an alternative targeting criterion based on baseline wealth rankings (i.e., means testing), I find that, on average, the means-testing criterion would have targeted the poorest households without sacrificing productivity. Furthermore, by comparing program borrowers to households that would have been eligible under an alternative targeting criterion based on baseline repayment probabilities (i.e., repayment score), I find that 38% of households who received credit from the program would have been ineligible under the repayment-score criterion. On average, these households were 12% less productive than households who did not borrowed from the program but exhibited high repayment probabilities. Reallocating program resources across these groups would have led to an average productivity gain of 4.5%, at no cost in terms of baseline per-capita consumption.

Second, while neither poverty targeting nor productive efficiency were the relevant allocation criteria, subsidized credit was disproportionately allocated to households with socioeconomic connections to the local elite. Combining socioeconomic networks data and data on baseline membership in the village council (the highest political authority in each village), I classify households as connected with the elite if they *i)* are members of the village council, *ii)* are first-order kin of the local elite, or *iii)* had direct pre-program socioeconomic ties to the local elite. I find that connected households are 20 percentage points more likely to obtain credit from the program than unconnected households. Connected households were not poorer or more productive than unconnected households, and yet they obtained more credit. Moreover, connected households already had access to institutional credit before the program and had similar baseline delinquency rates. While the correlation between program participation and connection to local elites falls by 45% after controlling for total number of connections in the village, demographic characteristics, business orientation, and credit history, connected households were still 10 percentage points more likely to obtain credit from the program. Thus, the slanted allocation towards connected households was only partially explained by improvements in information regarding borrower characteristics.

I find evidence of favoritism towards connected households with implications for program profitability. Connected households were favored with low initial interest rates leading to *ex post* lower internal rates of return for the program. A cross-section sample of loans corresponding to 344 households who borrowed both from the program and privately funded local credit groups allows me to compare loan performance across different lenders for the same household and control for unobserved borrower characteristics.[6] I test for favoritism by analyzing whether connected households obtain more favorable loan conditions in the case of program loans compared to loans from private credit groups and comparing these differences to those

[6]These groups constitute quasi-formal sources of credit. They include production credit groups and women's, groups among others. See Kaboski and Townsend (2005) for an in-depth assessment of these type of lenders.

for unconnected households. The results show that program loans to connected households were granted at lower initial interest rates (1.5 percentage points). These differences compromised the profitability of the program: the *ex post* internal rate of return on program loans to connected households is 2 percentage points lower than the return on privately funded loans (on average 7%). These results are driven by differences for connected households, as there were no detectable differences for unconnected households.

Third, while committee members favored connected households and the program might not have directly reached unconnected households, the program indirectly benefited unconnected households by increasing the supply of overall credit available in the village. Aggregate borrowing increased by 24% in the sample villages within a year from the rollout of the program. Using high-frequency data, I exploit cross-village variation in the monthly rollout of the program to identify the short-term effects of the program on credit use for unconnected households. While connected households benefited directly from the program, unconnected households obtained loans from other lenders in the system. Event-study estimates for unconnected households show that borrowing from informal lenders increased by 30%; this result was mostly driven by loans from relatives. I also find suggestive evidence of an increase in formal borrowing for unconnected households, albeit at higher interest rates than those from the program. There was also re-lending: the probability of lending to other households increased by 2 percentage points in the case of connected households. Overall, spillovers mildly offset the difference in program borrowing between connected and unconnected households: back of the envelop calculations suggest that these effects only account for one-third of program-borrowing gap between connected and unconnected households.

This paper makes three main contributions to the literature studying community-based approaches to distributing public resources. First, it highlights the limitations of these approaches to distribute productive resources when attributes of program beneficiaries are not easily observable by most community members. Unlike the context of poverty targeting, in the context of credit, the relevant targeting criteria may only be observable by direct economic interactions, strengthening the tension between information and favoritism. Alatas et al. (2012) provide evidence that households with connections to local elites are not more likely to receive cash transfers when resources are allocated by community members relative to a proxy-means-testing targeting criterion. The results from this paper show that this pattern may not hold in the case of credit and are consistent with evidence of favoritism in financial markets in the context of banks and firms (Khwaja and Mian, 2005; Haselmann et al., 2017). In addition, while Hussam et al. (2017) show that community members can identify productive households in India, this paper shows that accurate use of information may depend on social connections. In practice, both lack of information about unconnected households and favoritism can impose higher program-participation costs to households without the relevant

connections, with consequences for poverty targeting, productive efficiency, and program sustainability. These losses should be considered whenever policy makers choose among alternative approaches to program delivery.

The second contribution to the targeting literature is methodological. The use of pre-program data has been central to the assessment of community-based approaches to allocate cash transfers to the needy (Alatas et al., 2012). However, studies evaluating the productive efficiency of community-based allocations rely on contemporary or post-program measures of productivity (Bardhan and Mookherjee, 2006b; Basurto et al., 2017). This paper improves previous empirical assessments by exploiting a long panel dataset to recover *pre-program* structural estimates of household productivity, which are unlikely to be affected by the program. In terms of results, using self-reported data collected after the implementation of a fertilizer subsidy program in Malawi, Basurto et al. (2017) provide evidence of a tradeoff between targeting the poor and targeting high-return households. Using post-program structural estimates of baseline household productivity, I show that such a tradeoff was not relevant in the more general case of credit.

Third, by studying a context in which active credit markets interact with the implementation of a large-scale program, this paper examines the targeting problem both from a partial and general equilibrium perspective. The literature has generally focused only on the targeting or screening process. This paper expands the analysis beyond the program and tests the consequences of the *de facto* targeting criterion on village credit markets. By providing novel evidence on the role of informal credit markets in attenuating targeting errors, this paper contributes to the literature documenting general equilibrium effects and spillovers from large-scale programs (Angelucci and De Giorgi, 2009; Muralidharan et al., 2017; Kaboski and Townsend, 2012). In particular, the results show that economic connections and political economy factors can affect not only the distribution of public resources in the village economy, but also the redistribution of these resources through markets (Kinnan and Townsend, 2012; Acemoglu, 2010). More broadly, the results suggest that a complete understanding of targeting problems should involve an analysis of how resources are redistributed across agents.

The results from this paper also build on the literature studying the introduction of micro-credit products in developing countries. A core concern in the development economics literature is that of delivering affordable credit to poor, high-productivity households to enable them to escape poverty traps (Banerjee and Duflo, 2010; Morduch, 1999). While the literature has mostly focused on studying the effects of the introduction of credit products on several household outcomes,[7] an empirical assessment of the productive efficiency of

[7] Banerjee et al. (2015) provide a review of six randomized controlled trials studying the introduction of microcredit products in a varied of contexts. In particular, Banerjee et al. (2015) and Crépon et al. (2015) document low uptake rates in contexts in which credit was not directly offered to entrepreneurs. Deininger (2013) analyzes the impacts of access to credit on members of self-help

the allocation of credit in large-scale programs has not yet been provided. My results show that even with low intermediation and administrative costs, credit from the MBVF program did not reach poor, productive households. A comparison of these results with those from studies analyzing selection into credit highlights the importance of different screening mechanisms in credit markets. For instance, Beaman et al. (2014) show that high-return households select into credit in a context in which the screening mechanism is price.[8] This paper documents a less efficient result in a context in which the *de facto* screening mechanisms are social connections with local elites.

2 The village financial system and the Village Fund program

2.1 The village financial system

The context of this study corresponds to Thai villages, an environment in which most households own land (80%) and obtain over one-third of their revenues from agricultural activities (see Appendix table BXV). While most households obtain revenues from cultivation activities, the average household obtains revenues from 4 different economic activities: most households also obtain revenues from wage labor (78%), fishing and shrimping (40%) and off-farm family businesses (30%). To finance their economic activities, households borrow either from institutional lenders, informal lenders or relatives. Among institutional sources of credit there are formal lenders, mainly the state-owned Bank of Agriculture and Agricultural Cooperatives (BAAC), and quasi-formal lenders such as savings and credit groups and cooperatives.[9] In terms of the quantity of loans, half come from informal sources, while formal and quasi-formal sources of credit provide over 70% of the total loan amount in the village financial system.[10] On average, households hold more than one loan and around one-third of the households hold informal loans (see Appendix Table BXVI), which have higher interest rates than formal or quasi-formal loans (see Table 1).

2.2 The Million Baht Village Fund program

The Million Baht Village Fund (MBVF) program consisted of an initial transfer of THB 1 million (USD 22,500 in 1999 values), from the Government of Thailand to rural and peri-urban villages.[11] The aim of the

groups. Kaboski and Townsend (2012) also provide an assessment in the context of the MBVF program.

[8] They do so in the context of a micro-credit program in Mali, managed by an NGO with no government intervention at all.

[9] Quasi-formal institutions include organizations that have a set of procedures for recording their operations, but do not have a physical location. Examples of these are production credit groups (PCGs), women's groups and other village credit groups. See Kaboski and Townsend (2005) for a detailed description of these quasi-formal organizations in the Thai context.

[10] The top panel in Figure CXI illustrates the structure of the portfolio of loans associated with the villages in the study sample, both in terms of the number of loans and the amount of credit provided before the program was implemented.

[11] Average loan size is approximately USD 450 which represents roughly 25% of a households's yearly income.

program was to stimulate the village economies by expanding access to credit; program funds were used as seed capital for the creation of revolving credit funds in 95% of all villages in Thailand. [12] Moreover, the program increased the aggregate gross lending portfolio by 24% during the first year of its implementation in the sample villages, and modified significantly the composition of the portfolio of loans in each village (See and Appendix Figure CXI). The program offered loans at an average interest rate of 7% per year, which was the lowest rate in the market at that time: The average interest rate for other institutional loans was 11% per year (see Table 1). The program represents an unexpected event in that it was announced following a change in government and rapidly reached borrowers: As of the second year of implementation, the program had provided individual liability loans to 62% of households in the study sample.

The MBVF program differs from formal lenders in its management, relying on community members to manage credit funds. While there are other local savings and credit groups in which community members manage funds, they differ from the MBVF program in the way that they are funded: The MBVF is mostly subsidized, and local credit and saving groups are self-funded.[13] In each village, the MBVF program is managed by a village fund committee (VFC),a group of 10-12 elected community members that is responsible for evaluating loan applications and monitoring loans.[14] Committee members generally met once or twice a year to review loan applications. While the program was governed by a set of regulatory guidelines, committee members had full discretion to approve or deny applications and set loan amounts, terms, and the initial interest rate.[15] Although the Central Government provided villages with incentives for sustainable management and sanctions in case of mismanagement, there were no direct incentives for committee members.

2.3 Local elites and the MBVF program

Each Thai village is governed by a village head and a group of advisors who make up the village council; they are hereinafter referred to as the "local elite". The Village Council members are elected by villagers,

[12] A detailed discussion of the application process that villages were required to follow to get access to the funds and the way in which those funds were delivered is provided by Kaboski and Townsend (2012), Boonperm et al. (2013), Menkhoff and Rungruxsirivorn (2011) and Haughton et al. (2014).I do not address that process here as all of the villages in the sample participated in the program.

[13] In order to borrow, households were require to purchase a share of the fund, at a very low costs. However, the funds themselves come from a one-time transfer by the Government.

[14] The members of the Village Fund Committee were elected for a 2 year term in a transparent setting and received a small compensation for their services (Menkhoff and Rungruxsirivorn, 2011), however Haughton et al. (2014) documents that most of the members continued in the position for several years.

[15] The most important of these regulations were that loans could not exceed THB 20,000, a positive interest rate had to be imposed on all loans, the initial loan term could not exceed one year, and collateral could not be required, although households had to have one or two cosigners.

appointed by district authorities, and usually serve in office until retirement.[16] The Village Council represents the main link between community members and higher-level authorities. For instance, village council members attend district meetings, collect resources from villagers for religious celebrations or public works, and oversee resolution of disputes between villagers (Moerman, 1969; Mabry, 1979). In the study sample, Village Council members are richer, have larger extensions of land, and are more likely to have off-farm family businesses (see Appendix Table BXVII).

The village fund committee was *de jure* an independent entity, but it is possible that the local elite, had enough *de facto* authority to influence committee decisions. Although the election of village fund committee members is intended to induce accountability in the allocation of loans, committee members may have incentives to favor their political supporters or households with connections to the local elite. For instance, when elections could not take place, the committee members were appointed by the village Head.[17] The local elites could indirectly influence committee members through their economic or family connections: On average, 46% of households in the sample report transacting with village council members during the two years preceding the program and 13% of sample households are direct relatives of elite members (see Appendix Table BXVIII). In addition, relatives of the local elite could end up in charge of the funds even in transparent elections. [18] Moreover, households with business connections to local elites could use their privileged position to influence loan allocation decisions or to obtain preferential treatment. In such a context, the potential gains in information from decentralizing the allocation of resources to community members could be undermined by rent-seeking behavior (Bardhan and Mookherjee, 2005).

3 The Village Fund committee as a social planner

The central aim of this paper is to evaluate the allocation of resources by community members. The program's stated objective was to establish credit funds in order to expand access to institutional credit and promote career development and income generation (Government of Thailand, 2004), which suggest that poverty, productivity and repayment were important dimensions to be considered. For instance, access to institutional credit was particular low among the poor[19], the government claimed publicly that resources

[16] This was the case during the study period. However, a reform in 2011 established 5 year terms, but allowed Village Heads to run for reelection.

[17] Haughton et al. (2014) document that 15% of village fund committee members were appointed directly by either the Village Head or the Village Council

[18] (Cruz et al., 2017) document that individuals who belong to more central families are more likely to be elected for office in the Philippines

[19] Per-capita consumption was 16% lower for households without access to institutional credit at baseline (See Panel A from Appendix Table BXX.

were allocated to productive activities (Phongpaichit and Baker, 2004), and the sustainability of the village funds relies heavily on repayment. However, there were no explicit guidelines regarding the target population. Thus, theoretical analysis of the optimal targeting rules will provide insights to understand the different sources that affect the allocation of credit by community members.

In this section, I sketch a simple theoretical framework characterizing the optimal allocation of public resources and apply this framework to the context of the MBVF program. Drawing on the notion that the village fund committee allocates loans in order to maximize a village welfare function as if the committee was a benevolent social planner, the theoretical framework sketched in this section expands the work of Bardhan and Mookherjee (2006b) by allowing villagers to differ in terms of repayment. The insights from the theoretical framework imply that evaluating the allocation of credit involves considering whether the resources were provided to poor, high-productivity households.

The general problem of community members in charge of allocating public resources is represented in (1). Community members choose the allocation of resources $\mathbf{b} = \{b_i^*\}_{i=1}^{i=N_v}$ that maximizes the weighted sum of utilities corresponding to their fellow villagers (N_v) subject to feasibility, sustainability and other constraints imposed by the central government ($F(\mathbf{b})$):

$$\max_{\{b_1,\ldots,b_{N_v}\}} \sum_{i=1}^{i=N_v} \psi_i V(b_i)$$

$$\text{s.t.}$$

$$F(\mathbf{b}) \leq 0 \tag{1}$$

Political favoritism, social norms, and preferences may determine the weights associated to each village member (ψ_i), which I assume are exogenous to the allocation problem. V_i denotes a household i indirect utility function which is increasing and concave in b_i–i.e., the value function from the corresponding household optimization problem–. Consider the problem of MBVF committee. For the sake of simplicity, suppose that households repay their loans with an exogenous probability q_i which is known to the committee, and that loans are provided at a government-imposed interest rate r. In this case, community members solve the problem in (1) facing a sustainability constraint of the form: $F(\mathbf{b}) = \sum_{i=1}^{i=N_v} b_i - \sum_{i=1}^{i=N_v} q_i(1+r)b_i$. The first order conditions imply:[20]

[20]More generally, the optimal allocation of resources implies that the ratios between the marginal weighted utility of obtaining

$$\hat{\psi}_i \frac{\partial V_i}{\partial b_i} = \hat{\psi}_j \frac{\partial V_j}{\partial b_j} \tag{2}$$

$$\hat{\psi}_i = \frac{\psi_i}{1 - q_i(1+r)}$$

$$\forall i, j \tag{3}$$

where $\tilde{\psi}_i$ denotes the effective weight after incorporating the potential loss from providing a loan to a given household (i). In words, MBVF committee members will allocate resources such that the weighted marginal utilities from receiving extra-liquidity are equal across all villagers. Note that while committee members will punish households with a low probability of repayment, they may still deliver credit to risky households if their personal weights ψ_i are high enough for a particular households–i.e., a relative–. If the marginal utility of an extra unit of liquidity $\frac{\partial V_i}{\partial b_i}$ is diminishing with respect to b_i, then equation (2) implies that, conditional on the effective weights, it is optimal for MBVF committee members to provide resources to households who would benefit the most out of the program–i.e., high $\frac{\partial V_i}{\partial b_i}$–.

The identity of these households depends on the economic context in which households make their optimal decisions regarding consumption and input use. For instance, in a context of complete markets, optimal input choice should not depend on household characteristics (i.e., wealth) as households behave as unconstrained profit maximizer firms. In that context well functioning credit markets will deliver resources to all profitable projects, and the marginal utility from a program loan should not be a function of poverty. However, in contexts of incomplete credit markets, input use will be a function of household's characteristics, and the marginal utility of a household from obtaining a loan from the program will depend on the type of frictions that characterize rural credit markets.

For ease of exposition I discuss two frictions in credit markets: borrowing constraints and high borrowing interest rates which would make self-financing a more attractive option for households even in absence of borrowing limits.[21] In the case of borrowing constraints, a loan from the program will relax these con-

public resources and the marginal costs of satisfying allocation constraints are equal across all villagers.

$$\frac{\psi_i \frac{\partial V_i}{\partial b_i}}{\frac{\partial F}{\partial b_i}} = \frac{\psi_j \frac{\partial V_j}{\partial b_j}}{\frac{\partial F}{\partial b_j}}$$

$$\forall i, j$$

[21]Several models could generate such a friction. For instance, the existence of intermediation costs or information rents would create a gap between the interest rates obtained by deposits and the borrowing interest rates, making self-financing a cheaper option

straints by providing access to more liquidity. In the second case, because the program offered credit at the lowest interest rate in the village, obtaining a loan for the program would lead to a reduction of the interest rate at which unconstrained households borrow. The following two propositions characterize the household marginal utility derived from a program loan in both cases.

Proposition 1: *If households face borrowing constraints, the marginal utility of relaxing this constraint is decreasing in initial wealth. Moreover, the marginal utility of relaxing a household's liquidity constraint is an increasing function of household productivity if the distortion in the optimal choice of inputs is large. Proof:* See Appendix section F.0.1.

Intuitively, as richer households can substitute credit with initial wealth, conditional on productivity, their optimal choice of inputs will be less likely to be distorted by the presence of liquidity constraints and the shadow price of relaxing such a constraint will be smaller; this substitution may not be possible for poor households. In the case of productivity, as liquidity-constrained households cannot obtain funds to finance their optimal inputs choice, the marginal product of inputs will exceed the costs of financing inputs. This distortion will be higher for high-productivity households. As poor, high-productivity households are more likely to face binding liquidity constraints and experience higher distortions in their optimal choice of inputs, their marginal utility from a program loan will be higher.

Proposition 2:*If households do not face binding borrowing constraints but face high borrowing interest rates, the marginal utility from a reduction in the interest rate is a decreasing function of initial wealth and an increasing function of household productivity Proof:* See Appendix section F.0.2.

Intuitively, conditional on productivity, households with low initial wealth will borrow more and would benefit from a decrease in the interest rate. In contrast, as optimal input choice is increasing in household productivity, conditional on initial wealth, more productive households will demand more inputs, will borrow more and hence will benefit the most out of a decrease in the interest rate.

Propositions 1 and 2 and the first order conditions from the VF committee's problem (2) imply that if the probability of repayment is constant across households, and committee members weight all households equally, it is optimal to deliver more resources to poor, high-productivity households. In practice, any deviations from such behavior should be explained either by differences in repayment probabilities q_i, differences in committee member's preferences for a particular household ψ_i or the inclusion of further restrictions to the committee member's problem. In the case of the MBVF program, targeting non-poor, low-productivity households would be justified if these households had high repayment probability. However, if this was not the case, then targeting non-poor, low-productivity households should be explained by committee members

than borrowing.

preferences weighting other household characteristics unrelated to poverty, productivity or repayment such as political connections or differences in the cost of obtaining information.

Motivated by the implications of the previous theoretical framework, this paper reports results from three empirical exercises analyzing the allocation of loans from the program: First, I test whether village committee members delivered credit to poor, high-productivity households. Second, I compare the relative performance of the actual allocation in terms of poverty targeting and productive efficiency with benchmark counterfactual allocation criteria: means testing and repayment score. The former will test the empirical relevance of a trade-off between poverty and productivity, while the latter will test the extent to which there is a trade-ff between targeting high-repayment probability and high-productivity households. Third, I analyze the extent to which socioeconomic connections with local leaders relate to deviations from the optimal target population, and the extent to which these deviations are explained by information or favoritism.

4 Data and measurement

This study uses data from 172 waves of the Townsend-Thai Monthly Survey (Townsend, 2014). Starting in September 1998, the survey covers two years prior to and 12 years after the program's implementation. The survey follows a sample of 709 households from randomly selected villages corresponding to four provinces in Central and Northeast Thailand.[22] The dataset provides detailed information regarding transactions among households, the portfolio of loans held by each household, input use, and household financial statements.

While Kaboski and Townsend (2012) and Kaboski and Townsend (2011) used the Annual Townsend-Thai dataset to exploit cross-village variation in order to study the effects of the program on household outcomes, the monthly version of the survey is optimal to analyze how resources were distributed within a village. The monthly panel provides detailed information regarding socioeconomic interactions and loan repayment which is not available in the yearly survey. While the annual survey covers a high number of villages, it includes a small number of households in each village. In contrast, the monthly survey includes on average 44 households per village which allows for within-village analysis.

Out of 709 households who were interviewed in the first wave of the survey, 509 households were interviewed in the subsequent 171 waves, and, on average, 670 households are interviewed in each wave. As most of the analysis of this paper concerns comparisons of pre-program characteristics corresponding to the first 40 waves of the survey, I focus on the unbalanced panel of 671 households for whom data regarding

[22] Provinces: Chachoengsao, Lop Buri, Buri Ram, and Si Sa Ket.

baseline interactions were available and present robustness checks using the balanced sample for results that are obtained using variation over time (see Appendix Section E.1).

4.1 Measuring poverty

I approximate poverty using the average baseline per-capita consumption corresponding to the year preceding the program. I focus on per-capita consumption rather than wealth to capture the short-term component of poverty.

4.2 Measuring pre-program productivity

To assess productive efficiency, I focus on household total factor productivity as the main variable of interest. I exploit a panel data set to estimate the parameters from a production function which I use to recover pre-program estimates of household total factor productivity. I estimate a production function corresponding to household aggregate value-added by implementing the two-stage approach proposed by Olley and Pakes (1996); Levinsohn and Petrin (2003) and Ackerberg et al. (2015), using intermediate inputs as the proxy variable. I approximate output using total revenues from all household economic activities which include agriculture, livestock farming, fishing and shrimping, off-farm family businesses and wage work outside the household. Capital is measured as the value of the stock of household fixed assets which include land, value of livestock, real-state, appliances and agricultural equipment. Labor is measured as total hours per year of labor provided by household members (on average 85% of total labor) and workers outside the household. Intermediate inputs are measured as the value of inputs purchased outside the household which were used in revenue-generating activities.[23] I also provide robustness checks using productivity estimates from a gross-revenue function estimated by GMM following a dynamic-panel approach.

The choice of the empirical approach implies a series of assumptions which are discussed in the following paragraphs. First, because there is heterogeneity in the sources of income in the households in the data and because most households have several sources of income,[24] I aggregate revenues and input use all household's economic activities. This decision comes at a cost of interpretation of the elasticities, since a production function is specific to one particular process. [25] As the goal of this paper is not to compare

[23]These inputs include fertilizer, seeds, hired labor from other households, feed for cattle, and other tools required for non-farm family businesses.

[24]A behavior typical of rural environments in which household manage risk by diversifying their sources of income (Alderman and Paxson, 1994). Panel C from Appendix Table BXV shows that on average a household obtains revenues from 4 different sources: typically cultivation, labor provision, livestock and off-farm family businesses.

[25]This problem is typically assessed in firm-level analysis by estimating production functions by industries. However the concept of "industry" is not applicable in the context in which households have several sources of income and sort in and out a particular type of business. For instance, Nyshadham (2014) documents that households transition in and out of off-farm businesses fairly

elasticities across sectors but to quantify variations in output conditional on input use, the analysis in this paper focuses on productivity measures from all household activities.

Second, as there is heterogeneity in household economic activities and in the intermediate inputs contributing to the generation of revenues, I estimate a value-added production function. [26] However, a value-added approach assumes that households can't produce any output without intermediate inputs–i.e., the underlying production function is Leontief on intermediate inputs (Ackerberg et al., 2015; Gandhi et al., 2016)–; which is a strong assumption in the context of subsistence agriculture but a weak assumption when households have several sources of income such as off-farm business.[27]

Third, I choose a choice-based approach (Ackerberg et al., 2015) to recover productivity estimates over a dynamic-panel approach (i.e., Anderson and Hsiao (1982)). While both rely on assumptions regarding the timing of capital and labor choices, they differ in the assumptions regarding the dynamics of unobserved productivity and the way in which households accommodate productivity shocks. The former does not impose a functional form in the dynamics of unobserved productivity but the latter imposes linearity (productivity follows a first-order autoregressive process). However, the former uses intermediate inputs to proxy for changes in unobserved productivity under the assumption that households can freely adjust intermediate inputs. This assumption will be violated if there are adjustment frictions. In the context of the sample villages, while there might be borrowing constraints, households hold large amounts of inventories which may allow them to adjust intermediate inputs to productivity shocks.[28] More formally, Section 4.2.2 provides results from a graphical test for this assumption proposed by Levinsohn and Petrin (2003), and from a test for rigidities in input adjustment suggested by Shenoy (2017).

4.2.1 Identification of the production function

In this section I describe the main behavioral assumptions needed to identify a value-added production function, and defer a detailed discussion of these assumptions, estimation details and specification checks

often in the Thai villages of this sample. In the data, all households have at least two sources of revenues.

[26] There are other reasons for the choice of a value-added function approach as opposed to a revenue function. The first reason is to minimize the chances of double accounting in cases in which a households uses the output of one activity as intermediate input for another–i.e, using agricultural output as feed for livestock–. The second reason follows from the discussions on Ackerberg et al. (2015), and more generally in Gandhi et al. (2016), regarding the lack of identification of the elasticities corresponding to intermediate inputs in gross revenue functions in choice-based methods such as the one used in this paper.

[27] In a nutshell, this assumption means a household can't produce crops without fertilizer, which may not be true. However, adoption of fertilizer and seeds is quite high in the data. This assumption is also weak when we think of households having several sources of income.

[28] See Samphantharak and Townsend (2010) for a detailed description of household financial choices in context of incomplete credit and insurance markets in these villages. In fact, ongoing work by Kinnan et al. (2017) find that less central households in the village socioeconomic network have higher levels of inventory to accommodate production in contexts of idiosyncratic shocks.

to Appendix sections D.0.1 and D.1.6.[29] Formally, the goal is to recover pre-program estimates for ω_{it}: productivity shocks, observed by the households but unobserved by the researcher. Let y_{it} denote total value added in logs [30], k_{it} denote log capital, l_{it} denote log labor, and ϵ_{it} denote unforeseen exogenous shocks to production. The log value-added production function is:

$$y_{it} = \beta_0 + \beta_l l_{it} + \beta_k k_{it} + \omega_{it} + \epsilon_{it} \tag{4}$$

The empirical challenge is to consistently estimate the parameters from equation (4) in a context in which households choose labor and capital in response to productivity shocks (ω). Levinsohn and Petrin (2003) and Ackerberg et al. (2015) provide a solution by using variation from a proxy variable (m_{it}) that monotonically responds to productivity shocks to control for variation in productivity, conditional on labor and capital choices.[31] I use the value of inputs to proxy for variation in productivity. Hence, the main identification assumption is that households flexibly adjust their demand for intermediate inputs in order to accommodate productivity shocks in a strict monotonic way ($m_{it} = f_t(\omega_{it}; l_{it}, k_{it})$), , conditional on capital and labor choices. Strict monotonicity allows me to model variation in productivity shocks as a function of intermediate inputs ($\omega_{it} = f_t^{-1}(m_{it}; l_{it}, k_{it})$) and use this function to control for variation in productivity.[32]

Four other assumptions are necessary to recover total factor productivity estimates. First, I assume that, conditional on village-specific shocks, ω_{it} follows a first-order Markov process. Second, I assume that the stock of capital is a predetermined with respect to productivity shocks–i.e., it is a function only of investment and the stock of capital in the previous period ($k_t = k(i_{it-1}, k_{it-1})$)–. This is operationalized by measuring capital as the stock of fixed assets at the beginning of each calender year. Third, I allow labor choices to be flexibly adjusted in response to contemporary productivity shocks, but assume that labor decisions are not correlated with future shocks to productivity. Finally, as physical measures of output and intermediate inputs are not available, I include village-year fixed effects, and assume that input and output prices are common for households in the same village in a given year. [33]

[29] Appendix section D.0.1 describes the theoretical model consistent with the empirical estimates, the moment conditions required for estimation, and describes the estimation procedure. Appendix Section D.1.6 provides a test for over-identifying restrictions and discusses other alternative specifications.

[30] Value added is computed by subtracting the value of purchased inputs from the gross revenues generated by a household in a given time period.

[31] Most firm-level studies either use investment as the proxy variable (Olley and Pakes, 1996), or intermediate inputs, such as electricity, as proxy variables (Levinsohn and Petrin, 2003).

[32] This motivates the first stage of the estimation approach. However, as discussed by Ackerberg et al. (2015) and Gandhi et al. (2016), none of the elasticity estimates are identified from equation (4). See Appendix section D.1.1 for a discussion of the moment conditions required for the identification and estimation of the elasticities β_l,β_k.

[33] Accounting for the influence of prices requires incorporating a demand-system to the estimation framework and exploit variation in aggregate demand which is not available in this context (De Loecker, 2011).

Following the estimation process detailed in Appendix Section D.1.3. Appendix table DXXI presents estimates for the elasticities of labor and capital corresponding to equation (4). Column (3) presents results for my preferred specification which uses 13 years of panel data to compute production function elasticities which are then used to compute pre-program households productivity and provides evidence of constant returns to scale. Column (4) reports elasticities obtained by instrumenting pre-determined capital with its first lag to account for potential measurement error. The results are robust to using only data corresponding to pre-program periods (1999-2001) and a balanced panel of non-attriter households for the estimation (Columns (5) and (6)).[34] Finally, using an overidentified version of the model (see Column(7)), I find that it is not possible to reject the null that the model's structural restrictions hold.[35] Finally, Appendix table DXXII shows that results are fairly robust to alternative measurements of capital, labor, and revenues and to estimating different production functions for households whose primary source of revenues are related to agriculture (see Panel B).

4.2.2 Validation and discussion of the main identifying assumption

The main assumption of this approach is that, conditional on capital and labor, there is a strict monotonic relation between intermediate inputs and productivity. Appendix Figure DXII provides a graphical examination of this assumption by plotting the productivity estimates as a function of the value of purchased inputs, after partialling out the variation from capital, labor, and village-year shocks (Levinsohn and Petrin, 2003). I find evidence of a strict monotonic relation between productivity and the proxy variable. Table DXXIII reports results from the test for adjustment rigidities suggested by Shenoy (2017) and shows that there is no evidence of rigidities in the adjustment of intermediate inputs. [36] An alternative way of relaxing this assumption is to estimate a value-added function using a dynamic-panel approach (Anderson and Hsiao, 1982) through GMM after "ρ-differencing" equation (4). Columns(8) and (9) from Appendix table DXXI reports elasticities from this approach which are similar to the benchmark estimations obtained following a choice-based model. Columns(10)-(11) relax the value-added assumption and report factor elasticities from a gross-revenue function estimated through GMM following a dynamic approach. Identification in this case

[34]Production function elasticities using only pre-program data are very similar, almost identical. I base my conclusions on pre-program productivity measures using elasticities corresponding to 13 years which are more conservative than results using only pre-program data.

[35]Note that although an overidentified system would deliver more precisely estimated coefficients, the fact that I only observe two years of baseline data limits the estimation of TFP precisely for the baseline years, which are the main input for the analysis in this study. More importantly, consistency of these estimates depends on the correct specification of the variance-covariance matrix.

[36]To implement the test, I first regress value added on a flexible third-order polynomial of current choices of capital, labor and intermediate goods and compute the residuals. Second, I test whether flexible polynomials of lags for capital, labor and intermediate inputs have explanatory power on the residuals from the first regression. Rejecting the null of no explanatory power of lagged inputs will be supportive of rigidities in the market for inputs.

comes at the cost of assuming that there are rigidities in the adjustment of intermediate inputs which allow the econometrician to use input choices in previous periods as instruments for current inputs (see Appendix section D.1.5 for details). As no approach is perfect, I report results from estimates of total factor productivity following the dynamic panel approach for all the comparisons in this paper. I also report results using direct measures of financial profitability following Samphantharak and Townsend (2010), such as the asset-turnover ratio and profitability margins per unit of revenue.

4.3 Measuring repayment behavior

I track the full stream of disbursements and payments associated to each loan reported in the survey, until a loan is fully paid or defaulted on, and use these data to construct four indicators of loan performance: First, I count the number of times a borrower failed to make a payment and construct delinquency rates for each loan. Second, I compute an indicator of whether the loan experienced any delinquent payment. Third, I identify whether a loan was repaid in a longer period than its original term. Fourth, I measure returns to the lender using the *ex post* internal rate of return on each loan in order to have a common measure of loan profitability that accounts for loan size and changes in the loan payment schedule. Although default is observed, there is little variation on this as default rates are mostly zero in the data (See Table 1).

I complement these indicators with information regarding the loans' initial characteristics such as size, term, the need for collateral, or a cosigner. Initial interest rates were self-reported and are converted to yearly values by multiplying them by 12 or 52, in the case of monthly and weekly rates, respectively. A summary of loan characteristics by type of lender is presented in Table 1. To recover baseline delinquency rates for each household and avoid sample selection, I take the average over all the loans that were obtained before the program, including loans from informal lenders.[37]

4.4 Measuring connections with local elites

The dataset contains information regarding different types of socioeconomic interactions between households in the village.[38] To prevent potential effects of the program on network formation, I use only pre-program interactions to identify connections. With the aim of capturing several dimensions of social interactions, I use information on all types of transactions among community members.[39] Thus, a household is

[37]Use of institutional credit was not universal and would limit the ability to use pre-program information for households without access to institutional credit.

[38]The transactions can be roughly categorized in seven groups: output sales/purchases, asset purchases/relinquishments, transfers (gifts), borrowing/lending, paid labor provision/demand, unpaid labor exchange, and other inputs, which include materials purchases/sales as well as advising and mentorship.

[39]Summary statistics by interaction type are provided in Appendix Table BXIX

defined as connected with the local political elites if any of its members reports either being a member of the village council, or a first-degree kin of a council member, or having engaged in at least one interaction, of any type, with any village council member during the baseline periods.[40]

There are two limitations to these connections measures: First, by using the extensive margin of transactions to define connections, it is possible that a household is identified as connected because of one isolated interaction. Since the relative salience of each interaction cannot be identified nor valued, when pertinent I provide robustness checks using an alternative definition of connectedness based on Principal Component analysis across the different types of transactions. Second, since only village council members in the sample can be identified, as opposed to all village council members, there is a potential downward bias in measuring connections with elites. Thus, the results based on comparisons between connected and unconnected households represent lower bounds of the true differences. However, this bias should not be strong as village council members represent only 10% of the households and at least one committee member is observed in each village in the sample.[41]

5 Targeting analysis

In this section I analyze first if the program was successful at reaching poor,productive households, and then I test: *i)* whether there was a tension between targeting the poor and delivering credit to high-productivity households and *ii)* whether allocating resources based on a repayment-score would have led to a more equitable or productively efficient allocation.

While the program currently operates in several villages, I focus on the first two years of the program for two reasons.[42] First, I compare baseline characteristics between program beneficiaries and non-beneficiaries, and to the extent that consumption and productivity responded to the program or significantly varied over time, baseline characteristics are more representative of the context around the rollout of the program. Second, modifications were made to the program years after its rollout, such as changes in the orientation of the funds to community improvement projects, sanctions for poorly managed funds, and rewards for successful ones.

[40] While other measures–such as geodesic distance (shortest path)–might provide a better approximation of the distance between a household (node) and the elites in the network, these measures are subject to potentially high biases arising from the sampled nature of the transaction data. As noted by Chandrasekhar and Lewis (2017) there is non-classical measurement error when connections are computed using only a sample of the nodes in a network and the associated bias gets more complicated to tackle when network statistics that involve indirect connections are employed (e.g the path length to the closest elite member).

[41] Appendix Table BXVII shows demographic characteristics by type of connection with the elites. Appendix Table BXVIII, complements this information by presenting summary statistics of baseline connections with local elites.

[42] I choose two years in order to capture households that may not have needed credit during the first year but obtained credit during the second year.

5.1 Comparisons of program beneficiaries and non-beneficiaries

I find that the program did not target resources neither following a poverty targeting nor a productive efficiency criterion. Figure 1 depicts the cumulative distribution function of per-capita consumption and value-added total factor productivity for program beneficiaries and non-beneficiaries. Loans from the program were allocated to richer households; the distribution of per-capita consumption for program beneficiaries first-order stochastically dominates that of non-beneficiaries. Regarding productivity, the program on average targeted households from the middle of the distribution of total factor productivity and was less likely to target high-productivity households: less than half of high-productivity households (i.e., top 25% of the distribution of productivity) obtained loans from the program.

Table 2 quantifies the extent to which the program misdirected resources in relation to both the poverty targeting and the productive efficiency. Panel A shows that on average, the program targeted wealthier households and the differences arise at the bottom of the distribution of per-capita consumption; the 10th percentile is 22% higher for households who had access to credit from the program. In terms of productivity, the 75th percentile of the distribution of total factor productivity is 15% lower for program beneficiaries than for non-beneficiaries. This pattern is similar in the case of complementary measures of productivity and is particularly stronger in the case of the alternative gross-revenue productivity estimates obtained by the alternative dynamic panel approach (see bottom panel).

5.2 Poverty targeting , productive efficiency and repayment

Basurto et al. (2017) highlight the importance of distinguishing between poverty targeting and poverty reduction, which may arise in a context in which the poor may not necessarily be the most productive. To test the salience of this tradeoff, I evaluate the the allocation achieved by community members in relation to the allocation that would have been observed had loans been offered according to a pro-poor criterion– i.e., means testing (MT). This criterion aims to capture the allocation that would have been observed if the Village Fund committee placed a high weight on delivering resources to the poor.

Similarly, it could be the case that committee members faced a tradeoff between targeting poor, high-productivity households and households with a high expected repayment rate. To test the importance of this tradeoff, I compare the the allocation achieved by community members to the allocation that would have been observed had loans been offered according to a repayment score based on predicted baseline probability of missing a due payment for institutional loans. While an allocation based only on a scoring model may not fully reflect the choices that would be made by a traditional MFI credit officer, it is still policy relevant

as it captures information regarding *ex ante* risk which might be costly to the lender (Schreiner, 2000) and is informative regarding the decisions that would have been made by a risk-averse lender.

In order to identify households who would have been targeted by a means-testing criterion, I compute the average stock of per-capita gross assets over pre-program periods and construct within-community wealth rankings.[43] Using these rankings, the households with the lowest positions are classified as the MT target group and are selected into this group until reaching the uptake rates of the MBVF during the initial two years of the program (avg. 62%). I follow a similar approach using percentile rankings of predicted delinquency rate giving priority to households with low predicted delinquency rate.[44] This process classifies households into four groups: households that would have been targeted by both the program and the respective alternative criterion, households that would have been excluded from both allocations, households that were reached by the MBVF program but would have been excluded by the alternative criterion, and households that were excluded from the VF program but would have been targeted by the alternative criterion (see Appendix Table AVIII).

Means testing would have targeted a different set of people: over 40% of households targeted by the program would have been excluded by the MT criterion. While these households are by construction richer, they are on average more likely to be low-productivity households (bottom 25% of the productivity distribution). Figure 2 plots the probability of obtaining credit from the program under the observed allocation and the means-testing criterion as a function of percentiles of per-capita consumption and total factor productivity and shows that, in terms of productivity, means-testing does a better job than the program at excluding low-productivity households, but the program is slightly more likely to include high productivity households than a MT criterion. Thus, on average, there doesn't seem to be a cost in terms of productive efficiency from reallocating credit to the poor. Table 3 compares means and quantiles of per-capita consumption and productivity between households who would have been targeted by the program but would not have been eligible under the MT criterion and households who would have been targeted by the MT criterion but were not program beneficiaries.[45] Overall, the results show that MT outperforms the program under all metrics.

[43]Gross assets data is obtained from the households' balance sheets compiled by Samphantharak and Townsend (2010). Gross assets include non-land fixed assets (i.e., household assets, cultivation and family business assets), livestock and land value.

[44]To recover baseline credit scores related to loans from institutional lenders, I use a subset of households with pre-program access to institutional credit (i.e., credit from formal or quasi-formal lenders) to estimate a model of baseline delinquency rate for institutional loans as a function of household demographic and productive characteristics. I then use the coefficients of that model to generate predicted delinquency rates for all households in the sample and construct percentile credit-score rankings in each village assigning a higher credit score to households with low predicted delinquency. The household characteristics include household head age, gender and years of education, total land holdings and shares of total revenues by source. All continuous variables are grouped by quartiles and are interacted with household head gender in the model. The model also includes village fixed effects and overall explains over one-quarter of the variation in the probability of exhibiting delinquent payments in the baseline period.

[45]See Appendix Figure AVII for an illustration. A characterization of targeted households is presented in Table 5 (Columns (6)-(7)).

Contrary to the program, a means-testing criterion would have offered credit to the ultra poor and simultaneously would have excluded households belonging to the bottom 25% of the distribution of total factor productivity.

Over a third of households who obtained credit from the program would have been excluded by a repayment-score targeting criterion. These households were more likely to be low productivity households (bottom 25%) though also less likely to belong to the top 25% of the distribution of per-capita income. Relative to the program, a repayment-score criterion would have offered credit to a higher share of poor households, a lower share of households in the middle of the per-capita consumption distribution and a higher share of households from the top of the distribution. On average, reallocating resources to high-repayment probability would not be related to a cost in terms of equity with respect to the program. In terms of productivity, targeting credit following a repayment-score criterion would have delivered credit to the households with the highest productivity. This differences in terms of efficiency are sizeable: households who obtained credit from the program but would have been ineligible by a repayment-score criterion were on average 12% less productive than households who did not obtain credit from the program but would have been offered credit by the alternative criterion. The results are driven by differences in the top of the productivity distribution (see Table 4). Again, the same pattern is observed across different proxies for productivity. Overall, reallocating resources from program borrowers with low repayment probability to high-repayment probability households who did not obtain program credit would yield an average increase in productivity of 4.5%.[46]

5.3 Discussion

I find that resources from the program were not optimally allocated neither with respect to poverty targeting, nor with respect to productive efficiency. Moreover, the allocation of resources is not consistent with an allocation that would have targeted households with the highest repayment probability measured by a scoring model. Thus, the allocation achieved by the elected Village Fund committee is unlikely to have been motivated by concerns regarding equity, productive efficiency or risk. These results contrast sharply with experimental evidence from a NGO-led credit program in Mali in which low-return households self-selected out from credit (Beaman et al., 2014). The main explanation is the screening mechanism used by each program. The program in Mali had zero government intervention allowing price to be the main relevant screening mechanism. Section 6 show how the relevant screening mechanism in the Thai case was related

[46]This result is obtained by simply dividing the productivity gap from reallocating resources (12%) and scaling it down by the share of program borrowers who would be ineligible under the repayment criterion (0.38).

to political connections.

In this paper, I study a government-funded program managed by elected community members who have full discretion in the application process and in defining loan conditions. The theoretical framework discussed in Section 3 suggest that failure to provide credit to poor, high-productivity households might be related to Village Fund committee members weighting their fellow villagers based on different criteria. A compelling hypothesis is that committee members weighted more households with socioeconomic connections to local leaders. However, there are other factors that could influence the way in which Village Fund committee members weight each household such as externalities of financing a particular project or simply lack of demand for credit; Section 6 directly examines the role of connections with local authorities in the allocation of resources and discuses alternative compelling explanations while Section 7 discusses concerns regarding the demand from credit for households with lower chances of obtaining credit exploiting variation in the supply of credit in the village financial system induced by the program's rollout.

6 Access to credit from the program, connections with local elites, and favoritism

A central concern related to efforts to decentralize the allocation and management of public resources to community members relates to perverse incentives that may lead to favoritism or resource capture. However, the appeal of decentralized approaches to policy members relies on the idea that social connections may transmit information regarding program beneficiaries which might be costly to obtain by traditional policy makers. In this section I first show that households with connections to local elites are more likely to obtain credit from the program. Second, I discuss the extent to which this relation is related to information and/or favoritism.

Figure 3 depicts raw averages of the probability of obtaining a loan from the program for elite members, connected households and disconnected households. Resources from the program were disproportionately allocated to households with connections with the elites. This pattern is not explained by differences in baseline repayment history (see Appendix table AIX). In terms of poverty targeting or productive efficiency, connected households were neither poorer nor more productive. Panel A from Appendix table AX shows that while on average connected households are similar to unconnected households in terms of per-capita consumption, among the poorest households, connected households are better off: The 10th percentile in the distribution of per-capita consumption is 12% larger for connected households. Panel B shows that connected households were on average as productive as unconnected households; however the 75th percentile

of total factor productivity is 17% lower for connected households. This pattern is even stronger across other measures of productivity (see panels C-E), and is precisely observed in the regions of the per-capita consumption and productivity distributions where program beneficiaries differed from non-beneficiaries (see table 2).

To understand the extent to which village fund committee members use connections to proxy for desirable borrower characteristics, Table 5 shows regressions of the probability of obtaining a loan from the program during the first two years of its implementation on connections with the elites controlling for the number of links each household has in the socioeconomic network (degree), a set of baseline demographic characteristics, productive characteristics, credit history, and village fixed effects.[47] Column (1) shows that connected households are 18 percentage points more likely to obtain credit from the program and that these correlation is reduced to 10 percentage points after controlling for relevant household characteristics (see Column (3)), baseline access to credit (Column (4)) and productivity (Column (5)). Column (6) decomposes connections with local leaders by type of connection–i.e., council membership, connection through transactions, or being a first-degree relative–and shows that the correlation is driven by council membership and direct transactions with council members. These results suggests that connections carry important information and are encouraging as community-based approaches to targeting are suppose to exploit information available to community members. However, the results suggest that improvements in information do not fully explain the disproportionate allocation of resources towards connected households; even after controlling for relevant borrower characteristics, connected households are still 10 percentage points more likely to obtain credit from the program.[48] One alternative explanation for this allocation, which could potentially be consequential for the program's sustainability, is favoritism. If that were true, connected households should obtain better loan terms, leading to lower returns for the lender.

[47]The baseline delinquency rate is computed as the number of times a household fails to make a loan payment as a share of all payments due for loans obtained before the introduction of the program. Although only 60% of households ever reported holding a loan from formal or quasi-formal sources in the baseline periods, most households reported holding loans from either informal lenders. I use information regarding the history of payments of each reported loan, regardless the source, to compute delinquency rates and avoid dropping observations from households that, despite not obtaining institutional credit, have credit experience from informal loans.

[48]Note that the R-squared from column (6) is considerably lower than that of columns (7)-(9), suggesting that the control variables capture important information explaining the probability of obtaining credit under different allocation criterion. This pattern suggests that household characteristics could be good predictors of uptake of credit from pre-existing sources nut they are not as good in the case of program, which suggests that selection in unobservable characteristics is even more important in the MBVF case.

6.1 Favoritism towards connected households

In order to test for favoritism accounting for unobserved borrower characteristics, I use a sub-sample of 344 households who have ever borrowed from both the program and other local credit sources. I compare differences in initial interest rates and *ex post* returns to the lender for loans obtained from the program with respect to loans from local credit groups for connected households to similar differences for unconnected households, controlling for borrower and lender fixed effects.[49]

Comparison local credit groups include production credit groups (PCGs), women's groups, and other village organizations that provide credit. These credit groups and the MBVF program are managed in similar ways: The allocation of credit is decided by community members. However, they differ the way they are funded: The MBVF program is fully funded by the government while local credit groups rely on contributions from group members.[50][51] The similarities and differences across these sources of credit allow me to focus on two sources of variation: variation in borrower's connection status, which captures the potential political influence; and variation in the origin of the funds, which captures the ability of borrowers to take advantage of their connections.

I focus on initial loan characteristics such as interest rates, term, and size. As repayment frequencies vary across loans, I focus on loan outcomes from a cross section of loans that reached maturity, and were obtained after the implementation of the program. As the recovery rate of loans from the program is 99% in the sample,[52] I measure loan performance as the probability of a delinquent payment and the delinquency rate of the loan. Since differences in loan characteristics may affect repayment, and loan size may reduce administrative costs and interest rates, the main outcome of interest is the *ex post* internal rate of return.

In order to test for favoritism I use the following specification:

$$Y_{kijt} = \alpha_i + \theta_j + \beta \text{Connected}_i \times \text{MBVF}_j + \delta_{vt} + \epsilon_{kijt} \tag{5}$$

The unit of observation is a loan k obtained by household i from lender j in year t. Y_{kijt} denotes the loan outcome or loan characteristic of interest. α_i and θ_j denote households and lender fixed effects. While the analysis is in principle cross sectional, I control for village-specific time-varying shocks by including village-year fixed effects (δ_{vt}). $Connected_i$ and $MBVF_j$ are indicators of whether a borrower

[49] Such an approach is common in the literature in the context of credit and political connections Khwaja and Mian (2005), testing across monitoring models Shaban (1987), and the study of the role of comparative advantages and taste-based discrimination in agricultural tasks Foster and Rosenzweig (1996).

[50] These sources of credit have been shown to be helpful in promoting asset growth, consumption smoothing, and occupational mobility through the provision of cash credit to community members in the context of Thailand (Kaboski and Townsend, 2005).

[51] See Table 1 for comparative summary statistics for different sources of credit.

[52] The recovery rate is 96% for local credit groups (see Table 1).

has pre-program connections with the elites and whether the loan was obtained from the MBVF program. The parameter of interest is β which measures relative performance of loans to connected households from the MBVR program, under the assumption that there were no unobserved shocks differentially affecting program loans corresponding to connected households. This concern is partially assessed by including borrower fixed effects, but this assumption would be violated if, for example, the program modified repayment behavior specific to a type of lender (MBVF or local credit groups) and a type of borrower (connected or unconnected). [53]

Columns (1) to (4) from Table 6 present means of loan characteristics and outcomes by type of borrower and lender. Column (8) presents estimates of β corresponding to the specification in equation (5), and shows that loans from connected households are relatively larger (22%) and cheaper: The initial interest rate for program loans to connected households is 1.5 percentage points lower than that for loans from local credit groups to connected households, while unconnected households borrow at the same rate regardless of the source. To understand whether better loan conditions relate to favoritism or actually reflect lower risk, Column (8) in Panel B shows that while connected households were less likely to have had a delinquent payment on loans obtained through the program, this difference did not compensate for the preferential interest rates, as delinquency is very low for both sources of credit.[54] As a result, there is a 2-percentage-points decrease in the *ex post* internal rate of return to the lender for MBVF loans to connected households, which accounts for 25% of the average *ex post* internal rate of return for loans from self-funded local credit groups. Note that all the differences arise from differences in loan outcomes for connected households; Columns (5) and (6) show differences in loan outcomes by type of lender for connected and unconnected households, respectively. No significant differences, other than loan size, are detected for unconnected households.

6.2 Discussion

The results in this section support the hypothesis of favoritism towards connected households in the context of the program in the form of cheaper credit, which is associated with foregone returns to the program. However, the results do not imply that the repayment rate for the program was poor or that it was not profitable, overall. The results do suggest that program loans could have gone to better hands and that the program could have grown faster. For instance, this behavior may explain why, despite its high repayment rate, the

[53] I provide supporting evidence for this assumption in Appendix Table AXIII. Columns (4)-(6) test for differential short-run effects of the program on borrowing from credit groups using the rollout of the program; there are no significant effects and the point estimates are not economically meaningful. An explanation of the empirical approach used to obtain these results is deferred to Section 7.

[54] See Table 1 for statistics for the financial system.

program's lending portfolio was not able to grow at the same pace as the Thai economy (Haughton et al., 2014).

Although the evidence in this section is consistent with the notion of costly favoritism, there are other compelling reasons why connected households obtained more credit from the program. First, village fund committee members may have tried to increase employment or stimulate the market for inputs. Appendix table AXI shows no differences in capital-to-labor ratios between connected and unconnected households. Second, elected committee members may have different preferences which not necessarily follow a poverty targeting, productive efficiency or risk minimizing criteria. I argue that such a large difference in program participation will be harder to reconcile with alternative preferences other than taste-based discrimination. Third, unobserved application costs may differentially affect unconnected households. While the program relaxed the need for collateral, borrowers were still required to obtain two cosigners and finding a reliable cosigner might be costlier for unconnected households, yet this potential explanation is supportive of the main implication of the results in this section: a community based approach to allocating credit imposes higher program participation costs to unconnected households.

The evidence in this section is meaningful to the extent that unconnected households needed extra-liquidity and selection into the program is mostly explained by supply side constraints. Section 7 provides evidence supporting this assumption inspired in the following idea: To the extent that the program favored connected households, other actors in the financial system should be willing to serve unconnected households who want credit. The following section provides evidence of how credit markets reacted to an expansion of credit in the village economy that targeted connected households.

7 Program spillovers to unconnected households

The results from the previous section show that the program favored households with connections with the elite and might not have directly reached unconnected households. However, as favoritism is costly, other lenders in the market should be willing to provide loans to disfavored, unconnected households. In this section, I test the empirical relevance of this argument by analyzing whether the supply shock generated by the program indirectly increased credit use by unconnected households. Because institutional lenders are likely to face adjustment costs, I focus the analysis on informal markets which might be flexible enough to quickly respond to the increased in credit supply induced by the program.[55] Analyzing program spillovers is

[55]The literature has documented the importance of informal markets in providing resources to households that may not have direct access to formal credit (Kinnan and Townsend, 2012), or were not eligible for social programs (Angelucci and De Giorgi, 2009).

important for two reasons. First, it allows for analyzing the extent to which the resulting program allocation was driven by unconnected households self-excluding from the program or actually being disfavored by program committee members. Second, testing for program spillovers is informative about the role of markets in offsetting targeting errors and about the extent to which resources to improve targeting of social programs may be a first order concern for policy members.

7.1 Empirical strategy

The program represented a sudden increase in total lending in the village economy. Figure AVIII shows that there was a spike in aggregate lending within the first two months of the release of the funds from the program which lead to further increase. Following the introduction of low interest loans from the program, aggregate borrowing increased by 24% in the sample villages within a year from the rollout of the program. To determine if there were short-term reactions in credit markets, I exploit monthly variation in the differential rollout of the program across villages: The resources were released in June 2001 in the first village in the study sample, and the rollout continued until February 2002 for the last village in the dataset (nine months). As this variation is relevant over a short period of time, I restrict the analysis to the 18 months just before and after the program was introduced in each village, and hence the results are only informative of the short-run impacts of the release of the program. Identification of the treatment effects from the rollout of the program is achieved under the assumption that, conditional on household time-invariant characteristics, the rollout of the program was not related to unobserved shocks that determined household decisions to obtain credit. A main concern in this context is the potential coincidence of the program's rollout with different periods in the agricultural cycle. Section 7.3 and Appendix Section E develop a framework to directly test for this threat to identification and discuss other methodological issues.

In order to examine the presence of pre-program trends and the dynamics of the effect of the program, I compute flexible difference-in-differences estimates of the rollout of the program on credit using the following empirical specification (6):

$$Y_{ivt} = \alpha_i + \delta_t + \sum_{j=-18, j\neq -1}^{j=18} \beta_j \mathbb{I}[\tau_{vt} = j] + \epsilon_{ivt} \tag{6}$$

where Y denotes total borrowing by household i, in village v, at month t. τ_{vt} denotes time to treatment for each village in a given month. Household fixed effects are denoted by α_i, and δ_t denotes a set of calendar

months and year indicators.[56] The coefficients of interest are $\{\beta_j\}_{j=-18}^{18}$, which capture the difference between total average borrowing by households in period $\tau_{vt} = j$ relative to the month preceding release of the funds ($\tau_{vt} = -1$) compared to the difference in total borrowing by households in villages where funds were not released by that month. A causal interpretation of these parameters relies on the assumption of parallel pre-treatment trends between the villages ($\beta_j = 0, \forall j < 0$) and the absence of post-program, village-specific shocks that may affect borrowing decisions. To approximate the average treatment effect of the rollout of the program over the period of analysis, I also estimate:

$$Y_{ivt} = \alpha_i + \delta_t + \beta Post_{vt} + \epsilon_{ivt} \tag{7}$$

In this equation, $Post_{vt}$ is an indicator that takes the value of 1 in the months following the rollout of the program in each village. The parameter of interest in equation (7) is β, which captures the average differences in credit uptake before and after the release of program funds across households from villages that experienced the release of the funds in different periods.

A comment regarding inference should be made: I use mainly variation across 16 villages in the timing of the rollout of the program to identify intention-to-treat effects of the program, and the scarce number of villages poses a threat both to power and accurate inference. I present standard errors clustered at the household level to account for serial correlation. To account for within village correlation of error terms in the context of a small number of clusters, the regression tables report p-values from the wild bootstrap-t procedure suggested by Cameron and Miller (2015) imposing the null hypothesis of no effect. However, this approach tends to have low power and lead to conservative inference.[57]

7.2 Results

I find that unconnected households indirectly benefited from the program through informal local credit markets, mostly from relatives. Figure 4 presents flexible difference-in-difference estimates corresponding to equation (6) and shows that there was an increase in borrowing from informal lenders by unconnected households, and no effect for connected households. Figure 5 shows that the effect is mostly driven by loans from relatives, usually at null interest rates–average interest rate is 9%, but median interest rate is 0–(see

[56] Note that as time to treatment is strongly correlated with survey wave, inclusion of monthly dummies could lead to multicollinearity and failure to identify any meaningful parameter and inability to test for parallel trends. By using calendar month and year fixed effects it is possible to construct a survey-wave-specific intercept and weaken the correlation with the 'time-to-treatment' variable. Future versions of the paper will implement the methods suggested by Borusyak and Jaravel (aper) for this type of problems to test for pre-program trends more formally.

[57] As discussed by Cameron and Miller (2015) most available corrections for small number of clusters lead to appropriate acceptance rates, but they have reduced power. This is a concern in this paper as the number of cross-section observations is small.

Table 1).[58] Table 7 presents average treatment effects by connections with the elites and shows that the program lead to a 30% increase in informal debt in the case of unconnected households.

The results reported in the previous paragraph are consistent with evidence of re-lending. Appendix table AXIV shows that the probability of lending to other households increased by 2 percentage points in the case of connected households (12% of pre-program average), as a result of the rollout of the program. Event-study estimates show that there was a surge in total lending for connected households within two months of the rollout of the program (see Appendix figure AIX), yet these effects are imprecisely estimated.

Moreover, kinship networks were not the only margin of adjustment. Appendix Figure 6 shows that unconnected households borrowed more from BAAC, the state-owned bank, in some periods following the rollout of the program. These results constitute only suggestive evidence of spillovers as the average effect is not significant, though economically meaningful (See Appendix Table AXIII).

Overall, the program had little effect on total borrowing for connected households. Appendix Table AXII and Appendix figure AX present estimates of the impact of the rollout of the program on the probability of holding any loan and total debt from any source, by connections with the elites. The figure shows that the program barely increased access to credit for connected households despite providing them with over twice as much resources than connected households. This finding is not surprising as connected households had higher access to credit even before the program.

Despite spillovers and general equilibrium effects driving increases in non-program borrowing for unconnected households, the magnitudes are not big enough to fully compensate the differences in total borrowing form the program. Back-of-the-envelop calculations suggest that the effects on non-program borrowing for unconnected households only account from one-third of the differences in borrowing from the program between connected and unconnected households.[59] The result shows that unconnected households needed liquidity and suggest that the allocation of loans from the program was not explained by self-exclusion. Unconnected households were less likely to obtained credit from the program and when they did, they obtained less money, suggesting that other lenders in the system were helpful in reallocating the resources towards disfavored households.

[58] However, interlinked transactions in the kinship network may make up for zero interest rates on loans.

[59] This result is obtained from adding the effect on borrowing from relatives (THB 416) and from BAAC (1,018) for unconnected households and dividing it by the difference between the effect of the rollout of the program on program borrowing for connected households (THB 7,092) and unconnected households (THB 2,583)

7.3 Threats to identification, robustness, and attrition

The assumption that the rollout of the program was exogenous with respect to credit decisions is central to the identification strategy in the preceding section. While the flexible difference-in-difference estimates show that there were no differential pre-program trends, it is not clear that there were no post-program, village-specific shocks that may have affected credit decisions. Although monthly fixed effects control for seasonality, it could be the case that the funds were differentially released in periods in which higher activity in the credit market was expected. For instance, villages with earlier implementation benefited from the program during planting season, but villages with delayed implementation received the funds at the end of harvest season. If households decided to finance operations in a particular season, the estimates from the preceding difference-in-difference approach could also capture the effect of the agricultural cycle on credit. In Appendix Section E, I discuss in detail a placebo exercise designed to test if the results were driven by village-specific, seasonal patterns confounded with the rollout of the program. Concretely, I use observations corresponding to survey waves up to a year before the program was implemented in the first village ($\tau_{v,t} \in [-36, -6)$), and normalize τ, the variable representing time-to-treatment, to be between -12 and 17 (the base category is -1), such that the calendar months in which the funds were actually released coincide with the ones in the placebo exercise. I then estimate equation (6) in this sample and compare the results from the placebo sample to those reported in this paper. There are no significant effects in the placebo exercise.

Regarding attrition, I provide replications of the main difference-in-difference results presented in this paper for the 509 households that were interviewed in all 172 rounds of the survey. Results are not sensitive to attrition (See Appendix Section E). Regarding potential noise in the measure of connections, I replicate the main tables of the paper using an index of connection with the elites, the computation of which is based on the first principal component of the correlation matrix of connection with elites through all possible socioeconomic interactions. All results hold under both approaches (see Appendix section E.2).

8 Concluding remarks and discussion

Community-based approaches to targeting public resources are increasingly popular in the policy world. Despite that, little is known regarding the performance of these approaches in market-driven environments such as credit. This paper brings together two central debates in development economics: the delivery of public resources through local democratic organizations and the provision of affordable credit to poor, high-productivity households. The results in this paper highlight the limitations of a subsidized community-based

credit program to deliver credit to poor, high productivity households. Consistent with the traditional concern of resource capture in the literature that studies the decentralization of public programs to community members (Bardhan and Mookherjee, 2005), resources from the program were disproportionately allocated to households with baseline business connections with local elites.

These results are partially explained by the role of information. After controlling for demographic and productive characteristics as well as credit history, the correlation between connections and program participation reduces, yet it is still strong. This result suggests that the cost of obtaining relevant borrower information was higher for unconnected households and has important policy implications in contexts in which attributes for beneficiaries are hard to observe. The extent to which community-based targeting approaches lead to better targeting will depend on how connected are potential beneficiaries. Concretely, if poor,high-productivity households are socioeconomically isolated, even in the absence of rent-seeking behavior they may be less likely to be targeted. This result complements evidence showing how village network characteristics explain heterogeneity in targeting errors from a community-based cash transfer program(Alatas et al., 2012).

This paper also documents evidence of favoritism in a context of transparent elections of village fund committee members and speaks to the debate regarding the delivery of public resources through local democratic organizations. While the expectation was that transparent elections would ensure accountability, the results in this paper suggests that elections politicized the allocation of resources. The results are consistent with the theoretical prediction that decentralization may lead to regressive allocations when policies are financed through government grants instead of user contributions (Bardhan and Mookherjee, 2006a), as is the case of the MBVF program, and with cross-village studies documenting favoritism and clientelism (Asher and Novosad, 2017; Anderson et al., 2015). Overall, the results suggest that differences in connections to the local elite across households capture different in costs of accessing to public resources. These costs are related to information transmission but also to favoritism and are consequential in terms of equity, productive efficiency and program sustainability.

The results contrasts sharply with evidence in the context of community-based targeting of cash-transfer programs (Alatas et al., 2012) but is consistent with evidence of favoritism towards politically connected firms and credit from state-owned banks (Khwaja and Mian, 2005). The intuition for this result is that, as opposed to targeting cash transfers, the allocation of credit not only involves information regarding poverty but also productivity and repayment. Information regarding poverty is more likely to be objective and common knowledge to the community as a whole. Community members may use observable characteristics that describe a poor household and may not need to interact directly in order to figure out who is poor. In

contrast, information regarding productivity and repayment requires direct economic interactions and thus may increase the incentives for moral hazard behavior.

A first order concern is that of how to effectively use the information available to community members and simultaneously prevent rent-seeking behavior in community-based approaches. One way could be by fostering self-funded credit groups, as opposed to creating village funds with subsidized resources. This is already a popular policy approach backed with encouraging evidence of its effects both on household productive behavior (Kaboski and Townsend, 2005; Deininger, 2013) and in relieving households from high-interest money lenders (Hoffmann et al., 2017). Research testing whether there are social barriers preventing poor, high-productivity households from participating in these groups would shed light regarding the effectiveness of this approach to alleviate poverty. Moreover a more careful comparison of the mechanisms driving selection into credit across different policy-relevant implementation approaches –i.e., CBT, self-help groups and traditional microfinance– would provide insights towards future policy directions. An alternative way is to provide monetary incentives for accurate information (Hussam et al., 2017), however the implementation of these incentives may require bureaucracy which is precisely what CBT approaches are trying to avoid.

This study also speaks to the importance of understanding the interactions of public policy efforts with markets, and political economy factors in a general equilibrium framework. In particular, this paper contributes with novel evidence showing that credit markets may offset potential targeting errors. While evidence of spillovers from large scale programs towards mistargeted households may suggest that targeting should not be a first order concern as markets may deliver resources to the intended destination, the relevant question is the price mistargeted households have to pay in order to benefit from public resources. This study finds that other lenders in the village financial system and kinship networks are important in indirectly delivering results to households lacking of connections with local leaders. While the former involved higher interest rates than those from loans from the program, the latter may imply interlinked transactions which may be costly for either the borrower or the lender. These costs may ultimately determine if targeting should be a first or second order issue in public policy.

Finally, this paper provides evidence that aids in interpreting the results from the impact evaluation of the MBVF program. First, Kaboski and Townsend (2012) find increases in consumption and income growth with no effect on investment. Ongoing work by Breza et al. (2017) document heterogeneous effects of credit from the MBVF on investment, driven by heterogeneity in productivity. My results provide a bridge between these studies by showing that credit was inefficiently allocated and documenting the mechanisms leading to that allocation. Second, other studies analyzing whether the program reached poor households

suggest that resources were directed towards the poor, based on inter-village comparisons (Haughton et al., 2014; Menkhoff and Rungruxsirivorn, 2011). By using socioeconomic networks data, the results from this paper suggest that cross-village comparisons hide substantial asymmetries in access to resources from the program, which only a detailed intra-village analysis is able to capture.

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9 Figures

9.1 Targeting analysis

Interpretation: Poor and high-productivity households were excluded from the program.

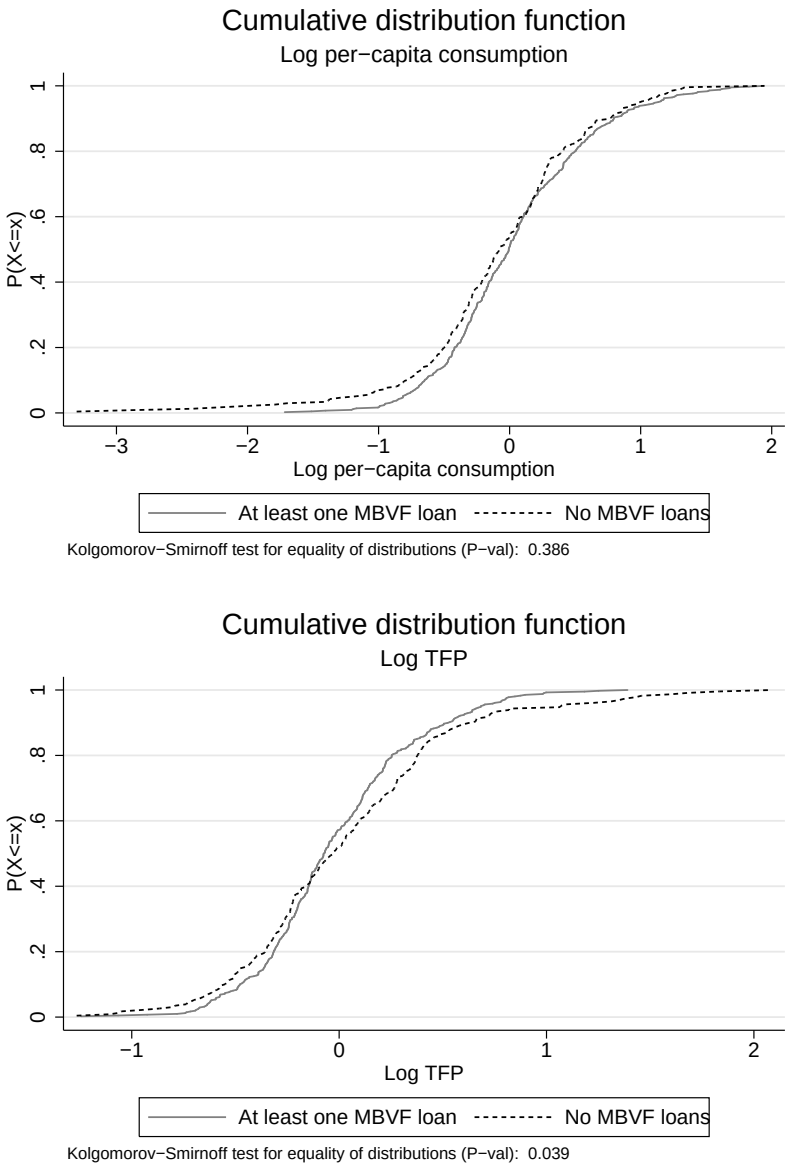

Figure 1: Cumulative distribution function of baseline log per-capita consumption and productivity

Note: The top figure plots the cumulative distribution function (CDF) of log per-capita consumption, measured at baseline, for households with access to credit from the program (62%) and households who didn't obtain credit from the program (38%) during the first two years of its implementation. The bottom figure plots a similar comparison for the CDF of log total factor productivity. Both variables are centered with respect to the village mean in order to perform within village comparisons. Per-capita consumption is measured as the total per-capita expenditure in consumption goods for the 12 months preceding the introduction of the program. Baseline total factor productivity is estimated using capital and labor elasticities corresponding to a value-added production function estimated as in Ackerberg et al. (2015).

Interpretation: The program allocated credit differently with respect to a means-testing allocation and an allocation based on repayment scores.

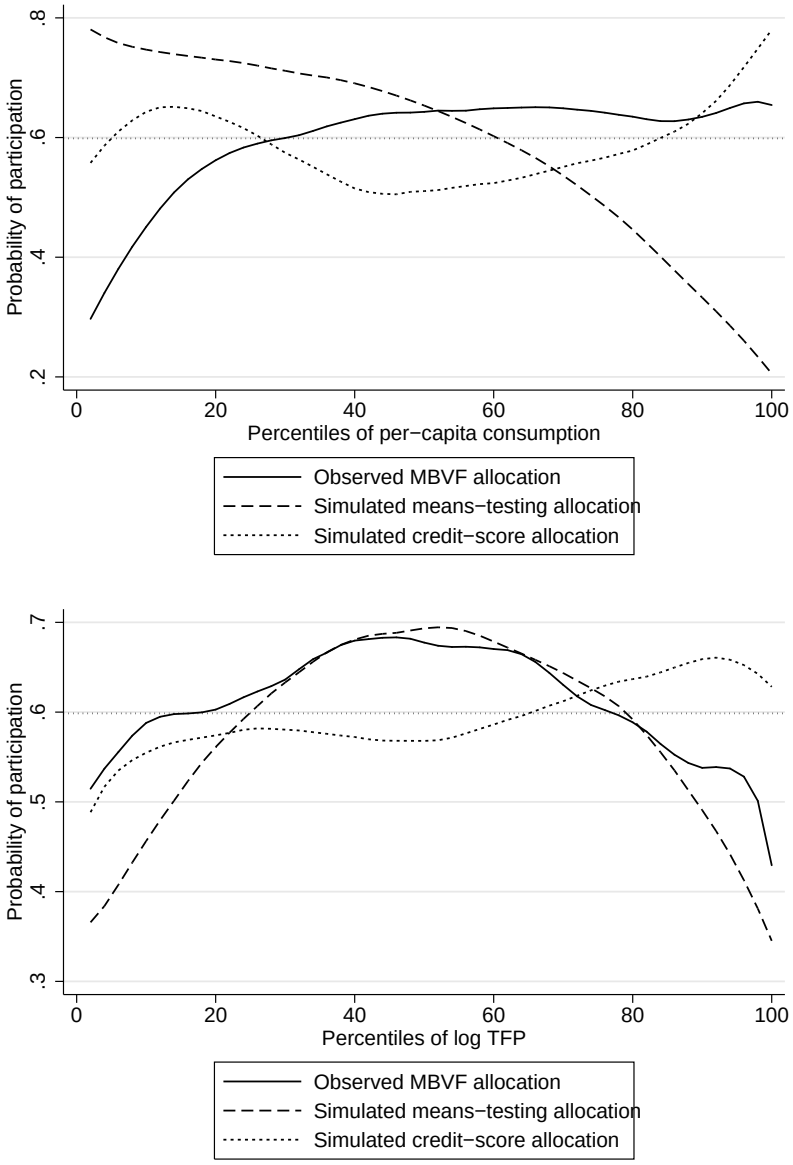

Figure 2: Participation in the program under alternative allocation criteria

Note: The figure depicts the probability of holding a loan from the the Village Fund program during the first two years of the program, the probability of being offered a loan under a means-testing criterion–i.e., wealth rankings, and the probability of being offered a loan based on repayment scores–i.e., predicted delinquency rate–(y-axis), as functions of percentiles of baseline per-capita consumption and total factor productivity (x-axis). The figures are computed using a double-residual, second-order local polynomial regression to account for village fixed effects. First, I obtain residuals from a regression of each variable on village fixed effects (i.e., Access to credit from the program, being targeted by the means-testing criterion, log per-capita consumption, and log TFP). Second, I use a second-order local polynomial regression of the residuals for the probability of being targeted by the program on percentiles of residuals of log per-capita consumption (top panel) and log TFP (bottom panel). I replicate this procedure for the probability of being targeted by a means-testing criterion, and the probability of being targeted based on baseline credit scores. Per-capita consumption is measured as the total per-capita expenditure on consumption goods during the 12 months preceding the introduction of the program. Baseline total factor productivity is estimated using capital and labor elasticities corresponding to a value-added production function estimated as in Ackerberg et al. (2015).

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9.2 Access to credit from the program, connections with local elites and favoritism

Interpretation: Village council members and households with socioeconomic ties to them had disproportionate access to credit from the program.

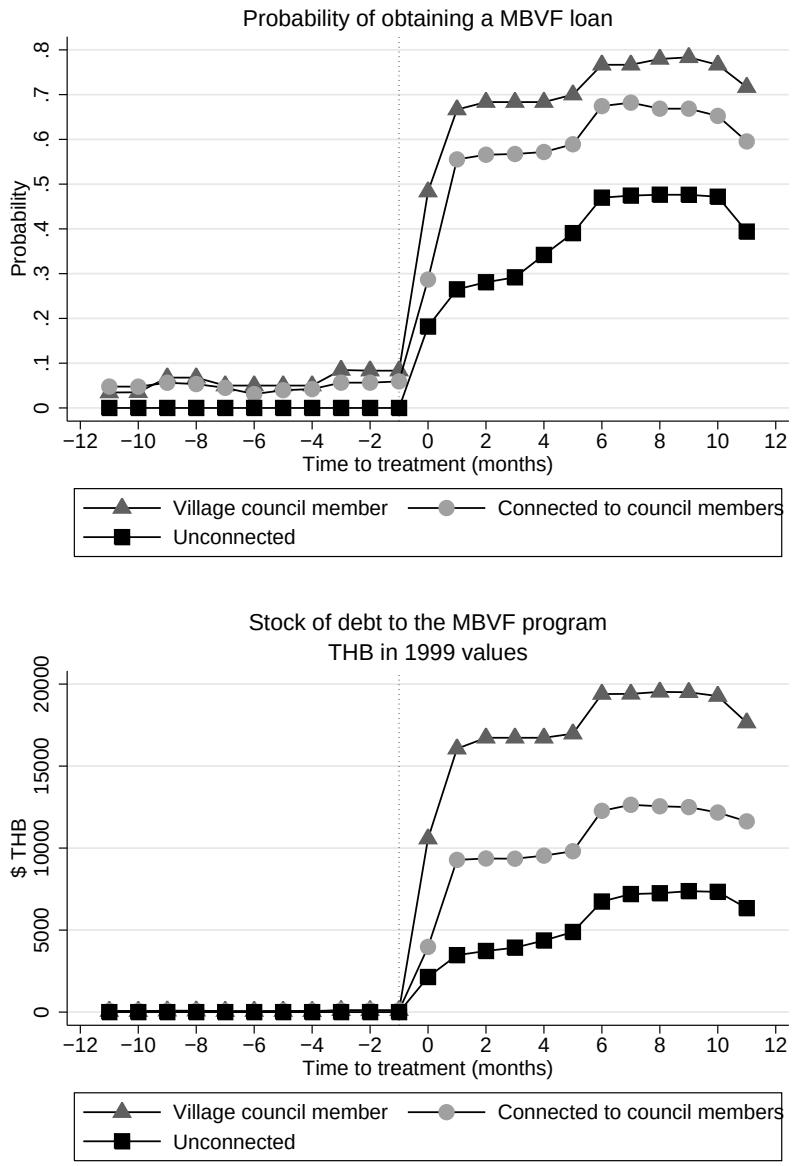

Figure 3: Access to credit from the MBVF program

Note: The figure depicts the probability of holding an outstanding loan from the the Village Fund program ((top panel) and the average gross stock of debt from the program (bottom panel) for the 12 months preceding and following the implementation of the program. Each symbol denotes the mean for each category in a given month. The dotted line denotes the period preceding the release of the program's funds $\tau_{v,t} = -1$. Village council member: .households in which at least one member is either the village head or on the village council during pre-program periods. Connected to council members: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected: households without any direct connection with members of the village council.

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9.3 Spillovers to unconnected households

Interpretation: Unconnected households indirectly benefited from the rollout of the program by obtaining loans from informal lenders.

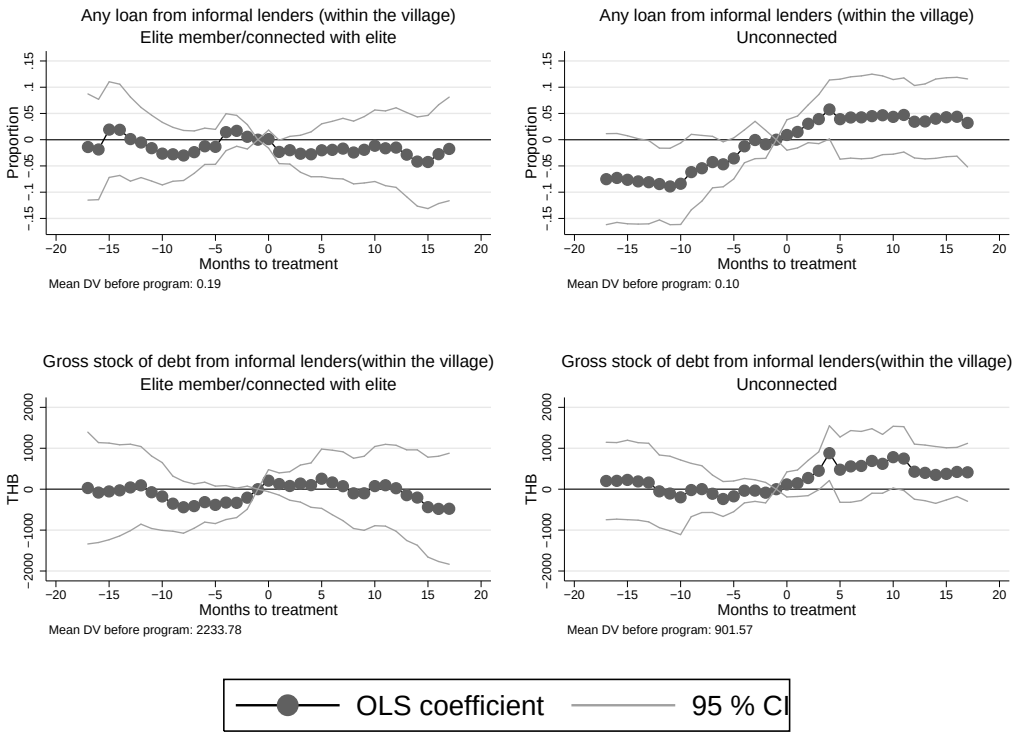


Figure 4: Short-term effects of the program on credit from local informal lenders

Note: The figure depicts OLS point estimates from a flexible difference-in-difference model following equation 6. Each dependent variable was regressed on household fixed effects, calendar month and year fixed effects, an a set of indicators that denote time to treatment. Each dot represents the coefficient associated with each of these indicators. The base category corresponds to the period preceding the first month of operation of the fund: $\tau_{vt} = -1$. Confidence intervals are constructed using standard errors clustered at the household level, to account for serial correlation. Informal lenders include both relative and non-relative personal lenders.

Interpretation: Spillovers to unconnected households were mostly driven by credit from relatives.

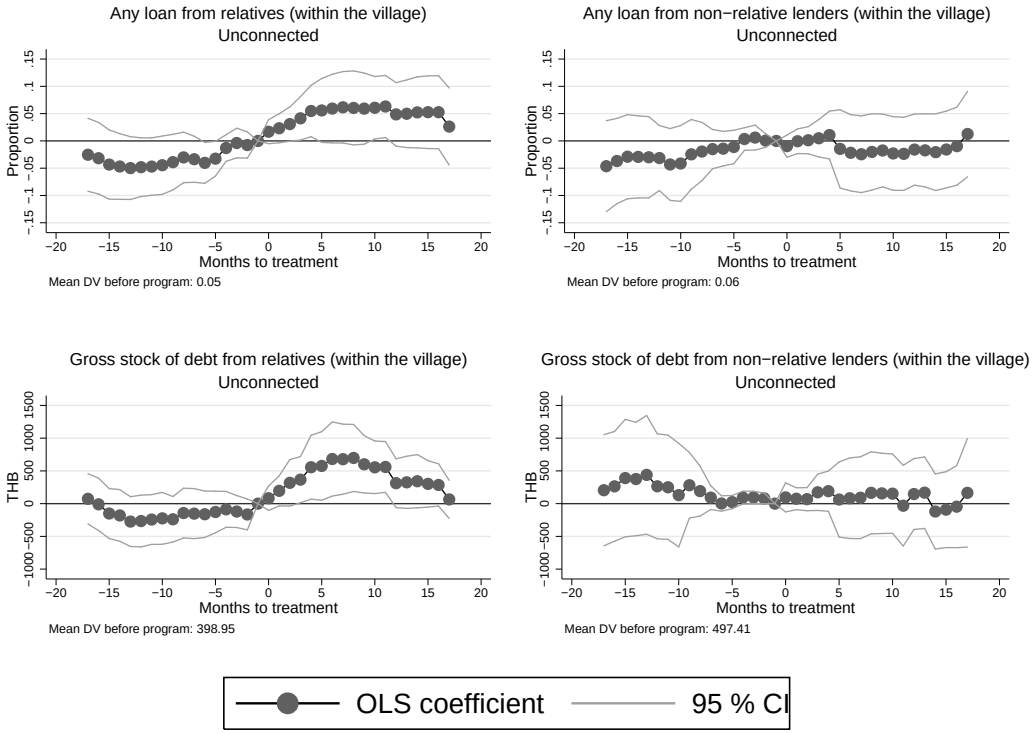


Figure 5: Short-term effects of the program on credit from relatives for unconnected households

Note: The figure depicts OLS point estimates from a flexible difference-in-difference model following equation 6. The left-hand panel presents estimates for loans from relatives, while the right-hand panel shows estimates for loans from local non-relative lenders. Each dependent variable was regressed on household fixed effects, calendar month and year fixed effects, and a set of indicators that denote time to treatment. Each dot represents the coefficient associated with each of these indicators. The base category corresponds to the period preceding the first month of operation of the fund: $\tau_{vt} = -1$. Confidence intervals are constructed using standard errors clustered at the household level, to account for serial correlation. The estimating sample includes only unconnected households.

Interpretation: Unconnected households also obtained more formal credit (from BAAC).

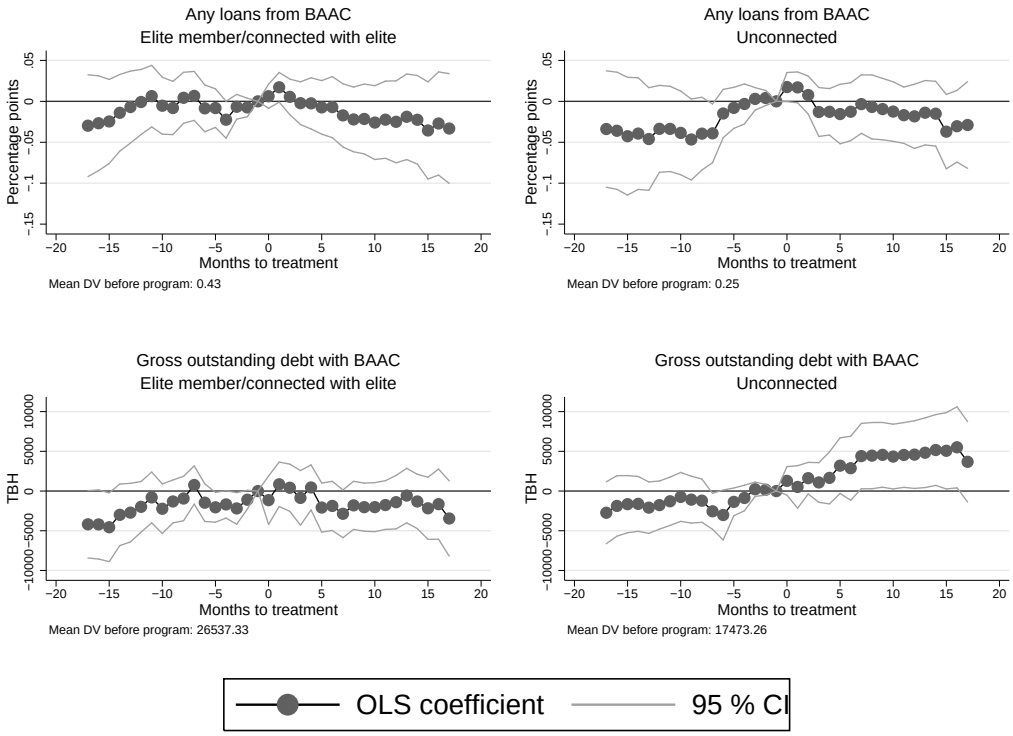


Figure 6: Short-term effects of the program on non-program institutional credit (BAAC)

Note: The figure depicts OLS point estimates from a flexible difference-in-difference model following equation 6. The top panel reports coefficients for the probability of holding any outstanding loan from the Bank of Agriculture and Agricultural Cooperatives (BAAC). The bottom panel presents results for the stock of outstanding debt with BAAC. Each dependent variable was regressed on household fixed effects, calendar month and year fixed effects, and a set of indicators that denote time to treatment. Each dot represents the coefficient associated with each of these indicators. The base category corresponds to the period preceding the first month of operation of the fund: $\tau_{vt} = -1$. Confidence intervals are constructed using standard errors clustered at the household level, to account for serial correlation.

10 Tables

10.1 Loan characteristics and performance

Table 1: Summary of loan characteristics, formal/quasi-formal lending, MBVF program, and informal lending

Panel A: Formal and Quasi-formal lenders

	Formal				Quasi-formal				Village Fund			
	N	Mean	Median	SD	N	Mean	Median	SD	N	Mean	Median	SD
Loan Characteristics												
Loan size ($ TBH-1999 values)	2876	44671.48	30000	40142.85	4634	14616.16	4000	30685.55	6373	16129.47	16500	8262.43
Required collateral	2876	0.18	0	0.39	4634	0.06	0	0.23	6373	0	0	0.02
Required cosigner	2876	0.7	1	0.46	4634	0.56	1	0.5	6373	0.93	1	0.25
Group loan	2876	0.67	1	0.47	4634	0	0	0.04	6373	0	0	0.02
Initial loan term	2831	15.56	13	14.51	4321	14.22	13	10.86	6344	11.36	13	3.87
Loan Performance												
Observed loan term	2876	16.31	13	14.45	4634	14.92	13	12.9	6373	11.94	13	4.13
Differences (observed-initial term)	2831	0.81	0	13.78	4321	1.16	0	8.05	6344	0.61	0	2.08
Percentage of months with missed payments	2303	0.03	0	0.13	4086	0.01	0	0.08	5880	0.01	0	0.06
Recovery rate	2876	0.99	1	0.09	4634	0.96	1	0.15	6373	1	1	0.02
Failure to repay full amount	2876	0.03	0	0.16	4634	0.1	0	0.3	6373	0	0	0.05
Interest rate and returns to lender												
Expected interest rate (initial, annual)	2788	12%	7%	207%	4295	10%	5%	42%	6344	7%	6%	37%
Effective interest rate (annual)	2876	7%	6%	16%	4634	7%	5%	20%	6373	6%	6%	8%
Internal rate of return (annual)	2656	7%	7%	12%	4545	7%	5%	40%	6372	6%	6%	8%

Panel B: Informal lenders

	Relatives				Non-relatives			
	N	Mean	Median	SD	N	Mean	Median	SD
Loan Characteristics								
Loan size ($ TBH-1999 values)	1108	13802.96	6000	21883.38	2407	21536.85	5000	42919.93
Required collateral	1108	0.04	0	0.19	2407	0.06	0	0.24
Required cosigner	1108	0	0	0.04	2407	0.01	0	0.12
Group loan	1108	0	0	0	2407	0	0	0.02
Initial loan term	473	6.98	5	7.36	1578	6.08	3	6.94
Loan Performance								
Observed loan term	1108	13.9	7	19.43	2406	8.95	5	13
Differences (observed-initial term)	473	2.72	0	12.8	1578	1.34	0	8.09
Percentage of months with missed payments	886	0.01	0	0.07	2043	0.01	0	0.07
Recovery rate	1108	0.98	1	0.1	2407	0.98	1	0.14
Failure to repay full amount	1108	0.04	0	0.2	2407	0.06	0	0.23
Interest rate and returns to lender								
Expected interest rate (initial, annual)	470	14%	0%	25%	1571	22%	12%	31%
Effective interest rate (annual)	1108	12%	0%	34%	2406	19%	12%	33%
Internal rate of return (annual)	1036	14%	0%	57%	2283	26%	13%	69%

Note: The table presents summary statistics for a sample of all loans that have reached maturity in the dataset and were obtained from January 1999 to December 2012. Loans that reached maturity include loans that were fully repaid and defaulted loans. Statistics are presented by type of lender for comparison. Panel A presents summary statistics for loans from formal and quasi-formal sources and MBVF program loans. Almost all formal loans (98%) are obtained from the Bank for Agriculture and Agricultural Cooperatives (BAAC). Quasi-formal lenders include production credit groups, cooperatives, womens's group and other loans from village organizations that keep records of their operations but do not have a physical location. Panel B presents summary statistics for loans from non-relative personal lenders (either inside or outside the village) and relatives (either inside or outside the village). Interest rates are nominal. Initial interest rates are self reported and converted to annual values by multiplying them by 12 or 52, in the case of monthly and weekly rates, respectively. Effective interest rate is computed by dividing the cumulative payments over the life of the loan by the principal minus one, and dividing this ratio by the loan's term (in years). Internal rates of return are computed using the entire payment stream over the life of the loan.

10.2 Targeting Analysis

Interpretation: Program beneficiaries were richer and less productive.

Table 2: Differences in baseline poverty and productivity characteristics for program beneficiaries and non-beneficiaries

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log per-capita consumption (N=660)						
	Mean	Percentiles				
		0.1	0.25	0.5	0.75	0.9
Any MBVF loan	0.135**	0.218*	0.116**	0.108*	0.057	0.012
	(0.056)	(0.124)	(0.049)	(0.057)	(0.068)	(0.094)
Panel B: Total factor productivity (logs) (N=637)						
	Mean	Percentiles				
		0.1	0.25	0.5	0.75	0.9
Any MBVF loan	-0.052	0.099**	0.050	-0.029	-0.152***	-0.083
	(0.044)	(0.048)	(0.034)	(0.048)	(0.042)	(0.068)
Panel C: Asset turnover (log revenues/assets) (N=666)						
	Mean	Percentiles				
		0.1	0.25	0.5	0.75	0.9
Any MBVF loan	0.182	0.645***	0.287	0.027	-0.105	-0.081
	(0.122)	(0.179)	(0.176)	(0.115)	(0.137)	(0.128)
Panel D: Profitability margin (profits/revenues) (N=674)						
	Mean	Percentiles				
		0.1	0.25	0.5	0.75	0.9
Any MBVF loan	-0.049**	-0.076	-0.044	-0.055***	-0.032***	-0.009
	(0.021)	(0.046)	(0.028)	(0.015)	(0.007)	(0.006)
Panel E: Total factor productivity (logs)-only baseline data (N=637)						
	Mean	Percentiles				
		0.1	0.25	0.5	0.75	0.9
Any MBVF loan	-0.001	0.117**	0.046	-0.005	-0.070*	-0.036
	(0.036)	(0.051)	(0.032)	(0.033)	(0.037)	(0.067)
Panel F: Total factor productivity (logs)-Dynamic Panel (N=629)						
	Mean	Percentiles				
		0.1	0.25	0.5	0.75	0.9
Any MBVF loan	-0.110*	0.141**	0.025	-0.093*	-0.150	-0.277***
	(0.065)	(0.069)	(0.045)	(0.052)	(0.092)	(0.101)

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents within-village comparisons of program beneficiaries and non-beneficiaries. Column (1) presents coefficients corresponding to regressions of baseline characteristics on an indicator of whether a household obtained a loan from the program during the first two years following the program implementation in each village, and village fixed effects. Columns (2)-(6) present results for equivalent quantile regressions. The bandwidth used for the estimation of quantile regressions was selected using Hall-Sheather's method. Robust standard errors are presented in parentheses. Panel A reports results for baseline per-capita consumption (in logs). Baseline per-capita consumption is measured as total expenditures during the 12 months preceding the implementation of the program. Panel B reports results for baseline log total factor productivity estimates recovered using capital and labor elasticities corresponding to a value-added production function estimated as in Ackerberg et al. (2015). Panel C presents results for baseline asset turnover ratio (in logs) computed as the average ratio of total revenues over a calendar year divided by the average stock of fixed assets in each household, over the two calendar years preceding the program's rollout (1999-2000). Panel D presents estimates for baseline profitability margins measured as

the average ratio of net revenues (net of costs of purchased inputs outside the household) to gross revenues in a given year. Panel E presents results for baseline log total factor productivity estimates recovered using capital, labor elasticities corresponding to a model estimated using only pre-program data. Panel F presents results for productivity computed using a dynamic panel estimation approach corresponding to a gross-revenue function.

Interpretation: Means-testing (MT) criterion, would offer credit to poorer and more productive households with respect to the program.

Table 3: Differences in per-capita consumption and productivity for households targeted either by the program or the MT criterion

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log per-capita consumption (N=311)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Targeted by the program and excluded by MT	0.499***	0.477***	0.377***	0.411***	0.484***	0.490***
	(0.065)	(0.156)	(0.044)	(0.074)	(0.057)	(0.094)
Panel B: Total factor productivity (logs) (N=309)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Targeted by the program and excluded by MT	-0.037	-0.108*	-0.089**	-0.039	-0.024	-0.012
	(0.055)	(0.065)	(0.038)	(0.065)	(0.068)	(0.094)
Panel C: Asset turnover (log revenues/assets) (N=327)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Targeted by the program and excluded by MT	-0.941***	-0.597*	-0.785***	-1.161***	-1.074***	-1.193***
	(0.186)	(0.323)	(0.166)	(0.121)	(0.146)	(0.245)
Panel D: Profitability margin (profits/revenues) (N=329)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Targeted by the program and excluded by MT	-0.146***	-0.226***	-0.166***	-0.160***	-0.083***	-0.010
	(0.039)	(0.041)	(0.045)	(0.024)	(0.026)	(0.014)
Panel E: Total factor productivity (logs)-Revenue function dynamic panel (N=305)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Targeted by the program and excluded by MT	-0.233**	-0.085	-0.075	-0.207***	-0.367***	-0.411***
	(0.091)	(0.085)	(0.053)	(0.060)	(0.119)	(0.079)

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents within village comparisons between households who obtained loans from the program during the first two years of its implementation but would have been excluded according to a means-testing criterion and households who were excluded from the program but would have been offered a loan by a means-testing criterion. Column(1) presents coefficients corresponding to regressions of baseline characteristics on an indicator of whether a household was reached by the Village Fund, but would have been excluded by a MT criterion, and village fixed effects. The omitted category (comparison group) is comprised of the households who would have been included by MT but were excluded from the program. Columns (2)-(6) present results for equivalent quantile regressions. The bandwidth used for the estimation was selected using Hall-Sheather's method. Robust standard errors are presented in parentheses. Panel A reports results for baseline per-capita consumption (in logs). Baseline per-capita consumption is measured as total expenditures during the 12 months preceding the implementation of the program. Panel B reports results for baseline log total factor productivity estimates recovered using capital and labor elasticities corresponding to a value-added production function estimated as in Ackerberg et al. (2015). Panel C presents results for baseline asset turnover ratio (in logs) computed as the average ratio of total revenues over a calendar year divided by the average stock of fixed assets in each household, over the two calendar years preceding the program's rollout (1999-2000). Panel D presents estimates for baseline profitability margins measured as the average ratio of net revenues (net of costs of purchased inputs outside the household) to gross revenues in a given year. Panel E presents results for baseline log total factor productivity estimates recovered using capital, labor, and intermediate inputs elasticities corresponding to a gross-revenue function estimated using a dynamic panel estimation approach.

Interpretation: A counterfactual allocation based on a predicted repayment probabilities would offer credit to more productive households with respect to the program.

Table 4: Differences in per-capita consumption and productivity for households targeted either by the program or the credit-score criterion

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log per-capita consumption (N=273)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Targeted by the program and excluded by a credit score criterion	0.052	0.317	0.092	-0.083	-0.191**	-0.341**
	(0.103)	(0.212)	(0.076)	(0.071)	(0.086)	(0.142)
Panel B: Total factor productivity (logs) (N=276)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Targeted by the program and excluded by a credit score criterion	-0.116*	0.070*	0.022	-0.138*	-0.172***	-0.152***
	(0.060)	(0.039)	(0.043)	(0.075)	(0.037)	(0.050)
Panel C: Asset turnover (log revenues/assets) (N=285)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Targeted by the program and excluded by a credit score criterion	-0.021	0.127	-0.039	-0.230	-0.300*	-0.318***
	(0.176)	(0.285)	(0.193)	(0.155)	(0.153)	(0.093)
Panel D: Profitability margin (profits/revenues) (N=290)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Targeted by the program and excluded by a credit score criterion	0.016	0.024	-0.039	-0.004	-0.013*	-0.009**
	(0.030)	(0.081)	(0.036)	(0.023)	(0.006)	(0.004)
Panel E: Total factor productivity (logs)- Revenue function dynamic panel (N=305)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Targeted by the program and excluded by a credit score criterion	-0.164	0.202**	0.038	-0.117	-0.229*	-0.522***
	(0.100)	(0.093)	(0.090)	(0.092)	(0.137)	(0.133)

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents within village comparisons between households who obtained loans from the program during the first two years of its implementation but would have been excluded according to a credit score (CS) criterion and households who were excluded from the program but would have been offered a loan by a CS criterion. Column(1) presents coefficients corresponding to regressions of baseline characteristics on an indicator of whether a household was reached by the Village Fund, but would have been excluded by a CS criterion, and village fixed effects. The omitted category (comparison group) is comprised of the households who would have been included by CS but were excluded from the program. Columns (2)-(6) present results for equivalent quantile regressions. The bandwidth used for the estimation was selected using Hall-Sheather's method. Robust standard errors are presented in parentheses. Panel A reports results for baseline per-capita consumption (in logs). Baseline per-capita consumption is measured as total expenditures during the 12 months preceding the implementation of the program. Panel B reports results for baseline log total factor productivity estimates recovered using capital and labor elasticities corresponding to a value-added production function estimated as in Ackerberg et al. (2015). Panel C presents results for baseline asset turnover ratio (in logs) computed as the average ratio of total revenues over a calendar year divided by the average stock of fixed assets in each household, over the two calendar years preceding the program's rollout (1999-2000). Panel D presents estimates for baseline profitability margins measured as the average ratio of net revenues (net of costs of purchased inputs outside the household) to gross revenues in a given year. Panel E presents results for baseline log total factor productivity estimates recovered using capital, labor, and intermediate inputs elasticities corresponding to a gross-revenue function estimated using a dynamic panel estimation approach.

10.3 Access to credit from the program, connections with local elites, and favoritism

Interpretation: Connections with local elites are strong predictors of access to credit from the program even after controlling for desirable borrower characteristics.

Table 5: Connections with local elites, access to MBVF credit, and borrower characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	**DV: Household obtained at least one loan from the MBVF**						**Any institutional loan (baseline)**	**Means testing**	**Credit score**
Relationship with village council members									
Connected through socioeconomic interactions	0.185***	0.141***	0.111**	0.097**	0.092*		0.085**	0.074	-0.009
	(0.043)	(0.046)	(0.048)	(0.048)	(0.049)		(0.038)	(0.047)	(0.045)
Village council member						0.164**			
						(0.070)			
Directly connected to a council member (interactions)						0.079			
						(0.050)			
First-degree relative of council member						0.061			
						(0.057)			
Network centrality									
Degree (# of links)		0.010***	0.005	0.003	0.004	0.003	0.010***	-0.015***	-0.000
		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Household demographic characteristics									
Number of males (15-64)			-0.054	-0.059*	-0.065*	-0.064*	0.026	-0.000	0.001
			(0.034)	(0.034)	(0.034)	(0.035)	(0.029)	(0.034)	(0.033)
Number of females (15-64)			-0.053	-0.051	-0.077**	-0.079**	-0.019	0.035	0.054
			(0.037)	(0.036)	(0.037)	(0.038)	(0.028)	(0.038)	(0.039)
Number of household members			0.046***	0.043**	0.045**	0.045**	0.017	0.004	-0.016
			(0.017)	(0.017)	(0.018)	(0.018)	(0.016)	(0.018)	(0.019)
Average years of schooling			0.023*	0.015	0.019	0.018	0.042***	-0.090***	0.059***
			(0.013)	(0.013)	(0.014)	(0.014)	(0.011)	(0.012)	(0.011)
Household head's age			-0.002	-0.001	-0.003	-0.003	-0.005*	-0.003	0.000
			(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
Average age			-0.001	-0.001	0.001	0.001	0.003	-0.000	-0.002
			(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Household head is a male			0.037	0.036	0.038	0.027	0.023	0.031	0.167***
			(0.046)	(0.045)	(0.047)	(0.048)	(0.036)	(0.046)	(0.048)
Sources of revenue (share of total)									
Wage labor			0.265**	0.210	0.164	0.151	0.262**	0.238	0.073
			(0.128)	(0.128)	(0.142)	(0.143)	(0.118)	(0.148)	(0.147)
Family business			0.258**	0.225*	0.209	0.196	0.217*	0.005	0.038
			(0.130)	(0.130)	(0.142)	(0.142)	(0.114)	(0.149)	(0.148)
Fishing/shrimping			0.427*	0.356	0.428*	0.410	0.503**	-0.222	0.496*
			(0.244)	(0.244)	(0.253)	(0.251)	(0.196)	(0.240)	(0.260)
Livestock			0.252*	0.176	0.174	0.177	0.431***	0.106	0.446***
			(0.142)	(0.142)	(0.155)	(0.155)	(0.130)	(0.164)	(0.159)
Agriculture			0.442***	0.333*	0.258	0.254	0.549***	-0.168	-0.068
			(0.167)	(0.171)	(0.185)	(0.186)	(0.151)	(0.191)	(0.186)
Credit history									
Avg. baseline delinquency			-1.008***	-1.040***	-1.009***	-1.001***	0.206	0.096	
			(0.293)	(0.304)	(0.317)	(0.312)	(0.251)	(0.211)	
Avg. baseline income volatility			0.038*	0.029	0.039	0.037	0.052***	-0.062**	
			(0.022)	(0.022)	(0.024)	(0.024)	(0.020)	(0.026)	
Pre-program access to institutional credit				0.182***	0.152**	0.146**		0.016	
				(0.058)	(0.060)	(0.060)		(0.058)	
Household productivity									
Estimated household total factor productivity					-0.088*	-0.084*	-0.092**	0.095*	0.030
					(0.049)	(0.049)	(0.038)	(0.049)	(0.047)
Observations	649	649	616	616	587	587	587	587	608
Adjusted R-squared	0.11	0.12	0.14	0.16	0.15	0.15	0.39	0.23	0.19
Within R-squared	0.03	0.04	0.07	0.09	0.08	0.08	0.17	0.16	0.10

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents within-village comparisons of the probability of obtaining a MBVF loan between connected households and unconnected households under several specifications (Columns (1) to (3)). Column(1) presents OLS coefficients from cross-section regressions of an indicator of whether a household obtained a loan from the program within two years of its implementation, controlling for village fixed effects. Column(2) controls for degree centrality in the socioeconomic network. Column(3) includes baseline household characteristics and Column (4) controls for baseline access to credit and Column(5) includes estimated productivity. Column (6) replicates the approach in Column (3) breaking down connections with the elite by type of connection. Columns(7) to (9) replicate the estimations for the probability of having held any institutional loan before the program (Column (7)), the probability of being targeted by the means-testing criterion (Column (8)), and the probability of being targeted by the credit-score criterion (Column (9)). Baseline access to institutional credit is an indicator of whether a household had any loan from either formal lenders or quasi-formal lenders. The delinquency rate is computed as the share of loans in which a household held any delinquent payments, and is computed based on repay information regarding loans from all type of lenders, including loans from relatives and informal lenders. Robust standard errors are reported in parentheses. Income volatility: log of the coefficient of variation of monthly income computed over all the survey waves preceding the program. Connected to council members: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected households: households without any direct connection with members of the village council.

Interpretation: Connected households are favored with lower interest rates, leading to lower returns for the lender.

Table 6: Differences in loan outcomes and characteristics by connections with the elites and by lender

Panel A: Loan characteristics	Means				Difference (MBVF-CG)		Difference-in-differences		
	Connected (N=231)		Unconnected (N=83)		Connected (N=231)	Unconnected (N=83)	All borrowers N=344		
	MBVF	Local credit groups (CG)	MBVF	Local credit groups (CG)	(1)-(2)	(3)-(4)	(5)-(6)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Initial interest rate (annual)	0.054	0.078	0.059	0.067	-0.0212***	-0.0065	-0.0150***	-0.0124***	-0.0120***
					(0.003)	(0.004)	(0.005)	(0.004)	(0.004)
Term (months)	11	12	11	13	-0.2714	-0.9951*	0.7482	0.8483	0.7822
					(0.242)	(0.539)	(0.625)	(0.596)	(0.604)
Loan size (TBH-1999 prices)	15175	4029	11659	3992	11,168***	8,550***	2,579***	2,179***	
					(375.973)	(706.135)	(750.099)	(739.294)	

Panel B: Loan outcomes	Means				Difference (MBVF-CG)		Difference-in-differences		
	Connected (N=231)		Unconnected (N=83)		Connected (N=231)	Unconnected (N=83)	All borrowers N=344		
	MBVF	Local credit groups (CG)	MBVF	Local credit groups (CG)	(1)-(2)	(3)-(4)	(3)-(6)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Any delinquent payment	0.008	0.017	0.003	0.000	-0.0117**	0.0040	-0.0157***	-0.0095*	-0.0095*
					(0.005)	(0.003)	(0.005)	(0.005)	(0.005)
Delinquent payments as a share of due payments	0.006	0.010	0.002	0.000	-0.0063**	0.0020	-0.0082**	-0.0049	-0.0048
					(0.003)	(0.001)	(0.003)	(0.003)	(0.003)
Any loan extension	0.470	0.400	0.372	0.336	0.0206	0.0239	-0.0034	-0.0233	-0.0211
					(0.022)	(0.038)	(0.044)	(0.042)	(0.041)
Ex post internal rate of return (annual)	0.060	0.077	0.068	0.059	-0.0183***	0.0081	-0.0263***	-0.0243***	-0.0236***
					(0.004)	(0.007)	(0.008)	(0.007)	(0.007)
Borrower fixed effect					YES	YES	YES	YES	YES
Lender fixed effect					NO	NO	NO	YES	YES
Village -year trends					NO	NO	NO	YES	YES
Weights for loan size					NO	NO	NO	NO	YES
Observations					5,193	1497	6,690	6,690	6,690

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: Columns (1)-(2) present raw means for connected household loans obtained from the MBVF program (1) and other local credit groups (2). Columns (3)-(4) present raw means for unconnected household loans obtained from the MBVF program (3) and other local credit groups (4). Columns (5) and (6) present differences in loan outcomes and characteristics across lenders for connected and unconnected households, respectively. Both differences control for borrower fixed effects. Columns (7)-(9) present difference-in-differences estimates under several specifications (First difference: Lender. Second difference: Connection status). Each coefficient captures the difference in differences in attributes of loans obtained by connected households from the program compared to loans from local credit groups, and similar differences for unconnected households. Column (7) presents estimates that only control for borrower and lender fixed effects. Column (8) includes a full set of village-year dummies. Column (9) replicates the estimates presented in Column (8) weighting each observation by loan size. Standard errors are clustered at the household level to account for correlation in loan outcomes corresponding to a single borrower. The sample corresponds to loans obtained after the rollout of the program by a set of 344 households who borrowed from both sources of credit at some point. Local credit groups include production credit groups, women's groups, and other loans from local non-bank institutions. Connected to council members: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected households: households without any direct connection with members of the village council.

10.4 Spillovers to unconnected households

Interpretation: Positive short-term effects of the program on credit from relatives for unconnected households.

Table 7: Difference-in-differences estimates of the short-run effect of the program on credit from local informal lenders

Panel A: Any loan from informal lenders

	Connected			Unconnected		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Any informal	Relatives	Non-relatives	Any informal	Relatives	Non-relatives
$Post_{vt}$	-0.007	-0.004	-0.005	0.047**	0.051***	0.002
	(0.014)	(0.011)	(0.012)	(0.022)	(0.019)	(0.012)
	[0.796]	[0.664]	[0.824]	[0.168]	[0.020]	[0.936]
Observations	13,212	13,212	13,212	6,948	6,948	6,948
R-squared	0.665	0.667	0.637	0.575	0.539	0.601
Baseline DV mean	0.150	0.0680	0.111	0.0815	0.0507	0.0498
Clusters	367	367	367	193	193	193

Panel B: Gross stock of debt with informal lenders

	Connected			Unconnected		
	(1)	(2)	(3)	(4)	(5)	(6)
	Any informal	Relatives	Non-relatives	Any informal	Relatives	Non-relatives
$Post_{vt}$	336.651*	124.218	240.688*	655.017***	555.634***	108.889
	(195.463)	(111.706)	(141.390)	(230.061)	(204.585)	(110.267)
	[0.116]	[0.196]	[0.172]	[0.120]	[0.008]	[0.816]
Observations	13,212	13,116	13,075	6,948	6,868	6,948
R-squared	0.672	0.669	0.702	0.607	0.604	0.597
Baseline DV mean	1540	554.8	998.5	865.2	398.1	472.3
Clusters	367	367	366	193	193	193

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents difference-in-difference estimates of the short-run effect of the rollout of the program on borrowing from informal lenders, by connectedness with the local elites. Informal lenders include personal money lenders and relatives in the village. The reported coefficients correspond to OLS regressions of the respective dependent variables on whether the resources from the program were released in village v in month t, controlling for household fixed effects and calendar month and year fixed effects (see equation (7)). Estimations were performed using all the available observations for the 18 months before and after the rollout of the program in each village. Panel A reports results for probability of holding a loan and Panel B shows results for the gross stock of debt (winsorizing the top 1% of observations). Standard errors, presented in parentheses, are clustered at the household level to allow for flexible serial correlation. P-values that account for potential within village correlation are presented in brackets; they are computed using a wild bootstrap t-procedure to account for a reduced number of clusters (16) as in Cameron and Miller (2015). Connected: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected: households without any direct connection with members of the village council.

A Supporting Evidence

Table AVIII: Distribution of targeted households by alternative criteria

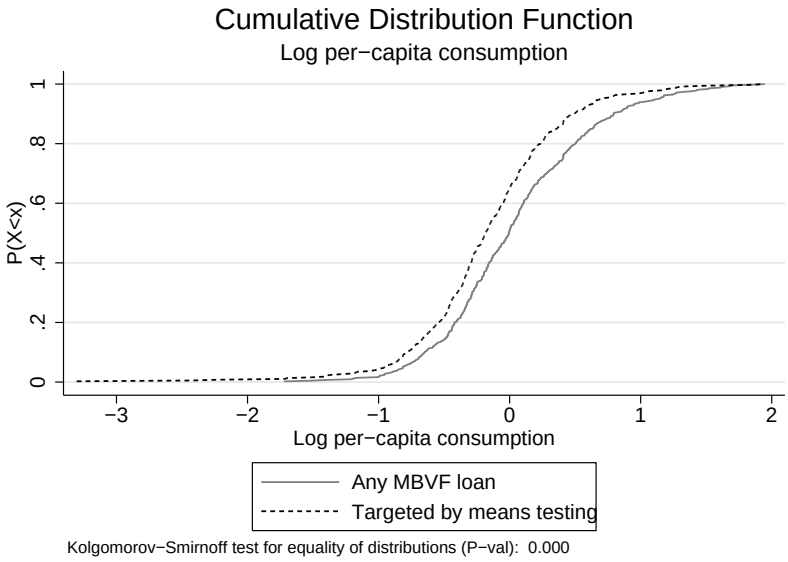
Panel A: Distribution of households under alternative allocation criteria

	Means testing	Credit score	Random assignment
Included in alternative only	24.08	21.97	19.48
Included in MBVF only	25.21	23.1	21.17
Included in both allocations	34.65	36.76	40.95
Excluded from both allocations	16.06	18.17	18.4

Panel B: Share of program beneficiaries which would have been excluded from the benchmark criteria

	Means testing	Credit score	Random assignment
Share	0.42	0.39	0.34

Note: The table presents the distribution of households across different targeting criteria. Each column represents an alternative targeting criteria–means testing, credit score, and random assignment. The first row in Panel A presents the share of households which would have been targeted by only the alternative targeting criterion but did not obtain credit from the program. The second row presents the share of households that obtained a loan from the program but would not have been eligible for a loan under the alternative criterion. The third row presents the share of households that obtained loans from the MBVF and would have also be eligible by the alternative criterion. The fourth row presents the share of households which would have been ineligible by the alternative criterion and did not borrow from the program. The reference period corresponds to the first two years following the implementation of the program. Panel B presents the share of program beneficiaries who would have been ineligible by alternative targeting criteria.



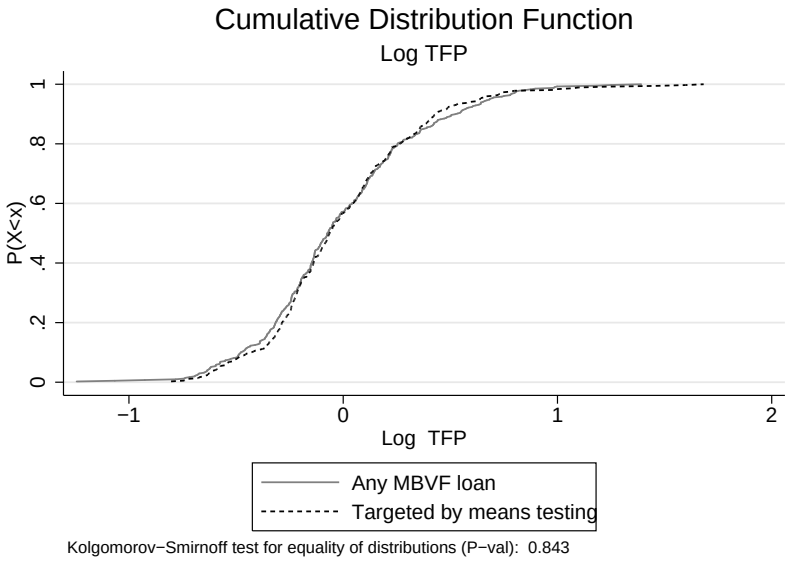


Figure AVII: Cumulative distribution functions of baseline log per-capita consumption and productivity for households targeted by different criteria

Note: The top panel shows the cumulative density functions (CDF) of per-capita consumption (in logs) , measured at baseline, for households served by the program, and the baseline distribution of log per-capita consumption for households who would have been reached under the alternative criterion (MT). The bottom panel shows CDFs of value-added total factor productivity, measured at baseline, for households served by the program and for households who would have been reached under the alternative criterion. Both variables are centered with respect to the village mean in order to perform within-village comparisons. Per-capita consumption is measured as the total per-capita expenditure on consumption goods during the 12 months preceding the implementation of the program. Baseline total factor productivity is estimated using capital and labor elasticities corresponding to a value-added production function estimated as in Ackerberg et al. (2015).

Interpretation: Connected households had higher access to credit before the program, higher income volatility and had higher chances of ever defaulting.

Table AIX: Connections with the elites and baseline borrower characteristics

VARIABLES	(1) Access to institutional credit	(2) Avg. delinquency rate	(3) Ever missed a payment	(4) Income volatility
Connected	0.155***	-0.003	0.133***	0.308***
	(0.036)	(0.007)	(0.040)	(0.084)
Constant	0.478***	0.027***	0.264***	0.591***
	(0.028)	(0.006)	(0.031)	(0.068)
Observations	649	616	616	649
R-squared	0.325	0.052	0.169	0.122

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents within-village comparisons of baseline characteristics across elite members or households directly connected with local elites and unconnected households. The table presents OLS coefficients from cross-section regressions of each baseline characteristic (columns) on an indicator that captures whether the household includes a village council member, a first-degree kin of council members or a member with pre-program socioeconomic interactions with village council member (Connected), after controlling for village fixed effects. Access to institutional credit is an indicator of whether a household held any loan from either formal lenders or quasi-formal lenders. The delinquency rate is computed as the share of loans for which the household had made any delinquent payments and is computed using repayment information for loans from all lender types, including loans from relatives and informal lenders. Income volatility: log of the coefficient of variation of monthly income computed over all the survey waves preceding the program. Robust standard errors are reported in parentheses.

Interpretation: Households with connections with local elites are better off among the poor, and less productive among high-productivity households

Table AX: Connections with local elites and indicators of poverty and productivity

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log per-capita consumption (N=660)						
	Mean	Percentiles				
		0.1	0.25	0.5	0.75	0.9
Connected	0.058	0.124*	0.005	0.057	0.152**	-0.029
	(0.057)	(0.067)	(0.055)	(0.050)	(0.070)	(0.076)
Panel B: Total factor productivity (logs) (N=637)						
	Mean	Percentiles				
		0.1	0.25	0.5	0.75	0.9
Connected	0.003	0.114**	0.084**	0.032	-0.102*	-0.173***
	(0.047)	(0.050)	(0.042)	(0.047)	(0.056)	(0.051)
Panel C: Asset turnover (log revenues/assets) (N=680)						
	Mean	Percentiles				
		0.1	0.25	0.5	0.75	0.9
Connected	0.007	0.021***	0.018**	0.022	0.008	-0.007
	(0.040)	(0.005)	(0.008)	(0.015)	(0.052)	(0.151)
Panel D: Profitability margin (profits/revenues) (N=684)						
	Mean	Percentiles				
		0.1	0.25	0.5	0.75	0.9
Connected	-0.045	-0.111	-0.032	-0.100***	-0.044***	-0.021**
	(0.033)	(0.068)	(0.034)	(0.019)	(0.010)	(0.010)
Panel E: Total factor productivity (logs) -Revenue function dynamic panel (N=637)						
	Mean	Percentiles				
		0.1	0.25	0.5	0.75	0.9
Connected	-0.244***	-0.002	-0.077	-0.194***	-0.238***	-0.317***
	(0.072)	(0.073)	(0.059)	(0.058)	(0.080)	(0.120)

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: Column (1) presents coefficients corresponding to a regression of baseline characteristics on an indicator of won an indicator that captures whether the household includes a village council member, a first-degree kin of council members or a member with pre-program socioeconomic interactions with village council member (Connected). Columns (2)-(6) present results for equivalent quantile regressions. The bandwidth used for the estimation of quantile regressions was selected using Hall-Sheather's method. Robust standard errors are presented in parentheses. Panel A reports results for baseline per-capita consumption (in logs). Baseline per-capita consumption is measured as total expenditures during the 12 months preceding the implementation of the program. Panel B reports results for baseline log total factor productivity estimates recovered using capital and labor elasticities corresponding to a value-added production function estimated as in Ackerberg et al. (2015). Panel C presents results for baseline asset turnover ratio (in logs) computed as the average ratio of total revenues over a calendar year divided by the average stock of fixed assets in each household, over the two calendar years preceding the program's rollout (1999-2000). Panel D presents estimates for baseline profitability margins measured as the average ratio of net revenues (net of costs of purchased inputs outside the household) to gross revenues in a given year. Panel E presents results for baseline log total factor productivity estimates recovered using

capital, labor, and intermediate inputs elasticities corresponding to a gross-revenue function estimated using a dynamic panel estimator.

Table AXI: Correlates of capital to labor ratios with baseline connections with the elites

VARIABLES	(1) Capital/total labor	(2) Capital/total labor	(3) Capital/total household labor	(4) Capital/total household labor	(5) Capital/total paid labor	(6) Capital/total paid labor	(7) Capital/intermediate inputs	(8) Capital/intermediate inputs
Connected	-5,756.6	-4,336.9	-15,248.6	-11,583.5	8,662.0	19,961.1	-3,339.9	-2,975.3
	(3,804.648)	(2,868.427)	(11,243.225)	(8,523.766)	(30,842.346)	(32,260.138)	(2,432.885)	(2,405.957)
Constant	6,693.1*	-20,265.8	16,106.6	-51,985.2	45,453.2**	-16,843.6	4,415.5**	-425.5
	(3,707.781)	(13,311.410)	(10,589.058)	(39,507.624)	(21,931.358)	(117,465.289)	(2,161.469)	(3,574.069)
Observations	634	633	629	628	458	457	617	616
R-squared	0.032	0.089	0.027	0.073	0.074	0.089	0.077	0.087
Village FE	YES	YES	YES	YES	YES	YES	YES	YES
Demographic controls	NO	YES	NO	YES	NO	YES	NO	YES

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents within-village differences in means of baseline fixed capital to labor ratios and fixed capital to purchased input use. Capital is measured in 1999 TBH, labor is measured in hours per year and spending in intermediate inputs is measured in 1999 Baht. Demographic control characteristics include average household age and education, household head's gender and age, household size, and the number of males and females of working age in the household.

Interpretation: The program induced a positive supply shock of credit in the village financial system.

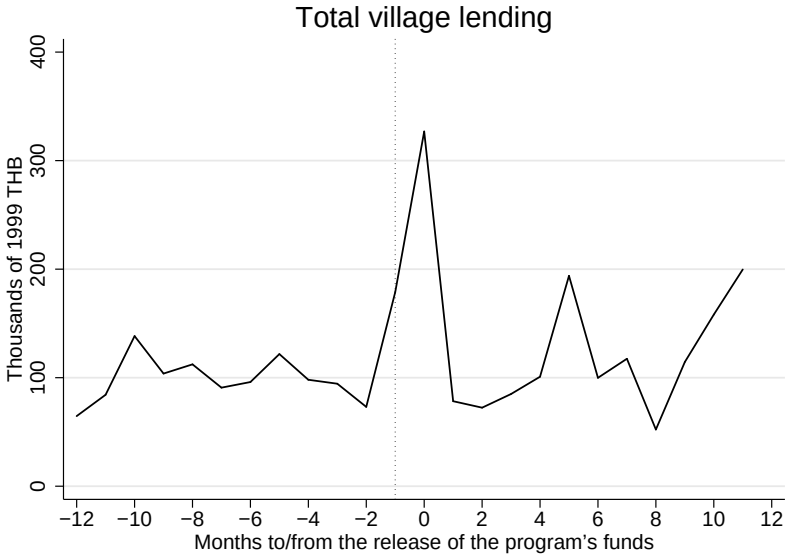


Figure AVIII: Average village lending

Note: The top panel shows depicts village means for total lending in the months around the program rollout. The dotted line denotes the month preceding the release of the program's funds.

Table AXII: Effects of the rollout of the program on program and total borrowing by connections with the elites

Panel A: Effects on credit from the program

	Any credit from MBVF			Gross debt from MBVF		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	All	Connected	Unconnected	All	Connected	Unconnected
$Post_{vt}$	0.328***	0.384***	0.233***	5,529.391***	7,092.555***	2,538.676***
	(0.019)	(0.026)	(0.029)	(373.757)	(527.504)	(409.526)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.008]
Observations	23,228	14,830	8,398	23,155	14,779	8,376
R-squared	0.613	0.632	0.564	0.590	0.619	0.523
Clusters (# households)	671	430	241	671	430	241

Panel B: Effects on total credit

	Any credit			Total Gross outstanding debt		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	All	Connected	Unconnected	All	Connected	Unconnected
$Post_{vt}$	0.074***	0.070***	0.086***	3,264.857*	4,689.658*	601.350
	(0.013)	(0.014)	(0.024)	(1,965.708)	(2,642.890)	(2,863.731)
	[0.000]	[0.004]	[0.008]	[0.120]	[0.124]	[0.856]
Observations	23,228	14,830	8,398	23,128	14,795	8,333
R-squared	0.661	0.628	0.660	0.866	0.825	0.910
Baseline DV mean	0.665	0.747	0.521	60747	59840	62356
Clusters (# households)	671	430	241	671	430	241

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents difference-in-difference estimates of the short-run effect of the rollout of the program on total borrowing, by connectedness with the local elite. The reported coefficients correspond to OLS regressions of the respective dependent variables on whether the resources from the program were released in village v in month t, controlling for household fixed effects and calendar month and year fixed effects (see equation (7)). Estimations were performed using all the available observations for the 18 months before and after the rollout of the program in each village. Panel A reports results for the effect of the rollout of the program on the program's uptake and Panel B shows results for total borrowing (winsorizing the top 1% of observations). Standard errors, presented in parentheses, are clustered at the household level to allow for flexible serial correlation. P-values that account for potential within village correlation are presented in brackets; they are computed using a wild bootstrap t-procedure to account for a reduced number of clusters (16) as in Cameron and Miller (2015). Connected: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected: households without any direct connection with members of the village council.

Table AXIII: Difference-in-differences estimates of the short-run effect of the program on credit from non-program institutional lenders

Panel A: Effects on any credit from non-program institutional lenders

	BAAC			Local credit groups		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	All	Connected	Unconnected	All	Connected	Unconnected
$Post_{vt}$	0.015*	0.014	0.020	0.015	0.024	0.010
	(0.008)	(0.010)	(0.013)	(0.013)	(0.017)	(0.018)
	[0.092]	[0.184]	[0.120]	[0.412]	[0.352]	[0.668]
Observations	23,228	14,830	8,398	23,228	14,830	8,398
R-squared	0.842	0.830	0.852	0.666	0.661	0.643
Baseline DV mean	0.366	0.434	0.247	0.255	0.313	0.152
Clusters (# households)	671	430	241	671	430	241

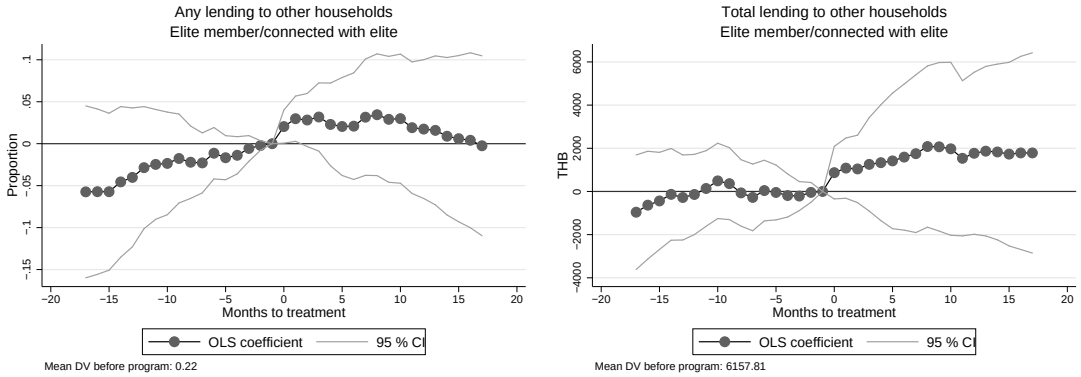
Panel B: Effects on total credit from non-program instutional lenders

	BAAC			local credit groups		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	All	Connected	Unconnected	All	Connected	Unconnected
$Post_{vt}$	602.755	552.604	1,018.859	114.126	97.648	350.761
	(1,074.413)	(1,728.284)	(1,069.061)	(660.160)	(852.427)	(1,075.008)
	[0.392]	[0.620]	[0.324]	[0.544]	[0.544]	[0.632]
Observations	23,095	14,747	8,348	23,106	14,747	8,359
R-squared	0.876	0.857	0.914	0.773	0.796	0.720
Baseline DV mean	23369	26650	17565	6890	8409	4204
Clusters (# households)	670	430	240	671	430	241

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents difference-in-difference estimates of the short-run effect of the rollout of the program on borrowing from non-program institutional lenders, by connectedness with the local elite. The reported coefficients correspond to OLS regressions of the respective dependent variables on whether the resources from the program were released in village v in month t, controlling for household fixed effects and calendar month and year fixed effects (see equation (7)). Estimations were performed using all the available observations for the 18 months before and after the rollout of the program in each village. Standard errors, presented in parentheses, are clustered at the household level to allow for flexible serial correlation. P-values that account for potential within village correlation are presented in brackets; they are computed using a wild bootstrap t-procedure to account for a reduced number of clusters (16) as in Cameron and Miller (2015). Connected: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected: households without any direct connection with members of the village council.

Figure AIX: Short-term effects of the program on lending to other households (connected households)



Note: The figure depicts OLS point estimates from a flexible difference-in-difference model following equation 6. The left panel presents estimates for the probability of lending to other households, and the right panel presents estimates for total lending to other households. Each dependent variable was regressed on household fixed effects, calendar month and year fixed effects, and a set of indicators that denote time to treatment. Each dot represents the coefficient associated with each of these indicators. The base category corresponds to the period preceding the first month of operation of the fund: $\tau_{vt} = -1$. Confidence intervals are constructed using standard errors clustered at the household level, to account for serial correlation. The estimation sample includes only households with baseline connections with the local elites.

Table AXIV: Difference-in-differences estimates of the short-run effect of the program on lending to other households

	Connected		**Unconnected**	
	(1)	(2)	(3)	(4)
VARIABLES	Any lending	Total lending	Any lending	Total lending
Post	0.033**	730.314	0.011	398.918
	(0.014)	(917.580)	(0.016)	(431.449)
	[0.016]	[0.548]	[0.424]	[0.432]
Observations	13,212	13,097	6,948	6,879
R-squared	0.783	0.862	0.783	0.675
Baseline DV mean	0.207	6148	0.140	2798
Clusters (# households)	367	365	193	193

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents difference-in-difference estimates of the short-run effect of the rollout of the program on lending to other households, by connectedness with the local elite. The reported coefficients correspond to OLS regressions of the respective dependent variables on whether the resources from the program were released in village v in month t, controlling for household fixed effects and calendar month and year fixed effects (see equation (7)). Estimations were performed using all the available observations for the 18 months before and after the rollout of the program in each village. Standard errors, presented in parentheses, are clustered at the household level to allow for flexible serial correlation. P-values that account for potential within village correlation are presented in brackets; they are computed using a wild bootstrap t-procedure to account for a reduced number of clusters (16) as in Cameron and Miller (2015). Connected: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected: households without any direct connection with members of the village council.

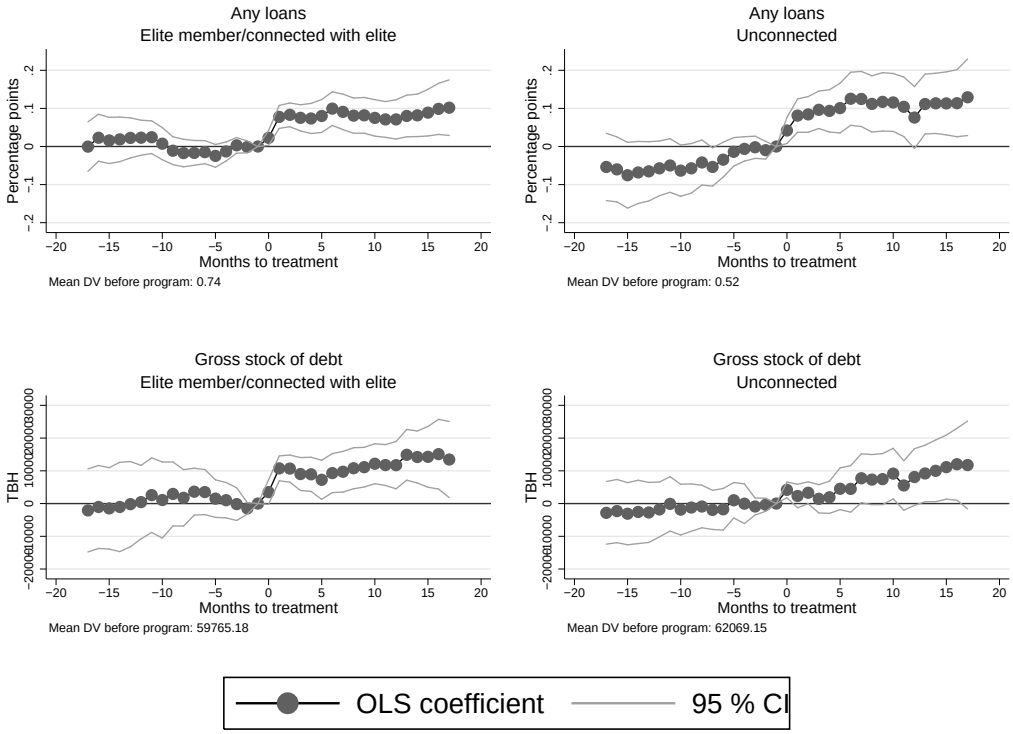


Figure AX: Short-term effects of the program on total borrowing

Note: The figure depicts OLS point estimates from a flexible difference-in-difference model following equation 6. The top panel reports coefficients for the probability of holding any outstanding loan from any source (both institutional and informal). The bottom panel presents results for the stock of outstanding debt. Results for connected households are shown in the left-hand panels while results for unconnected households are shown in the right-hand panels. Each dependent variable was regressed on household fixed effects, calendar month and year fixed effects, and a set of indicators that denote time to treatment. Each dot represents the coefficient associated with each of these indicators. The base category corresponds to the period preceding the first month of operation of the fund: $\tau_{vt} = -1$. Confidence intervals are constructed using standard errors clustered at the household level, to account for serial correlation.

B Appendix Tables

Table BXV: Summary statistics for baseline characteristics(1999-2000)

Summary statistics N=675

Panel A: Demographic characteristics				
Variable	**Mean**	**Std. Dev.**	**Min**	**Max**
Household size	4.09	1.78	1.00	14.75
Males	1.94	1.11	0.00	8.00
Females	2.14	1.15	0.00	6.75
Mean hh age	35.59	13.78	12.15	89.88
Head of household is male	0.76	0.43	0.00	1.00
Mean hh years of schooling	4.27	2.39	0.00	16.00
Panel B: Land and wealth				
Variable	**Mean**	**Std. Dev.**	**Min**	**Max**
Landless	0.22	0.42	0.00	1.00
Land in hectares	21.46	32.66	0.00	320.00
Land value/Assets	0.48	0.34	0.00	0.99
Total household assets	1826612	6393885	3463	143000000
Panel C: Revenues				
Variable	**Mean**	**Std. Dev.**	**Min**	**Max**
Total household revenues	224866	630660	0	11900000
Cultivation (share)	0.34	0.35	0	1
Livestock (share)	0.08	0.21	0	1
Fishing-Shrimping (share)	0.06	0.18	0	1
Off-farm business (share)	0.11	0.26	0	1
Wage labor (share)	0.32	0.36	0	1
Other (share)	0.09	0.18	0	1
Cultivation (any)	0.74	0.44	0	1
Livestock (any)	0.65	0.48	0	1
Fishing-Shrimping (any)	0.41	0.49	0	1
Off-farm business (any)	0.31	0.46	0	1
Wage labor (any)	0.78	0.42	0	1
Other (any)	0.84	0.37	0	1
Number of sources of revenue	3.73	1.30	0	6
Panel D: Per-capita annual income and consumption (1999 TBH)				
Variable	**Mean**	**Std. Dev.**	**Min**	**Max**
Per-capita income	21306	90105	0	2030435
Per-capita consumption	15060	13271	0	193597

Note: The table presents summary statistics for demographic and productive characteristics corresponding to the two years preceding the rollout of the MBVF program for the households in the Townsend-Thai Monthly Survey.

Table BXVI: Summary statistics for credit adoption by type of lender

Panel A: Full sample (N=643)					
Variable	**Mean**	**Median**	**Std. Dev.**	**Min**	**Max**
Any loans (any source)	0.67	1	0.47	0	1
Any formal/quasi-formal loans	0.58	1	0.49	0	1
Any informal loans	0.31	0	0.46	0	1
Number of loans (total)	1.76	1	2.14	0	18
Number of loans (formal+quasi-formal)	1.12	1	1.32	0	8
Number of loans (informal)	0.64	0	1.39	0	14
Gross stock of debt (total)	60747	20000	120655	0	1015000
Gross stock of debt (formal+quasi-formal)	50235	9500	110795	0	890000
Gross stock of debt (informal)	8076	0	21900	0	200000
Panel B: Village council members (elites) (N=60)					
Variable	**Mean**	**Median**	**Std. Dev.**	**Min**	**Max**
Any loans (any source)	0.85	1	0.36	0	1
Any formal/quasi-formal loans	0.82	1	0.38	0	1
Any informal loans	0.33	0	0.47	0	1
Number of loans (total)	2.82	2	2.66	0	17
Number of loans (formal+quasi-formal)	2.11	2	1.75	0	8
Number of loans (informal)	0.70	0	1.47	0	11
Gross stock of debt (total)	81791	39625	116003	0	762000
Gross stock of debt (formal+quasi-formal)	72502	30900	114488	0	762000
Gross stock of debt (informal)	9289	0	22731	0	172000
Panel C: Households with baseline connections with the elites (N=352)					
Variable	**Mean**	**Median**	**Std. Dev.**	**Min**	**Max**
Any loans (any source)	0.73	1	0.44	0	1
Any formal/quasi-formal loans	0.66	1	0.47	0	1
Any informal loans	0.34	0	0.47	0	1
Number of loans (total)	2.03	1	2.26	0	18
Number of loans (formal+quasi-formal)	1.29	1	1.32	0	8
Number of loans (informal)	0.74	0	1.54	0	14
Gross stock of debt (total)	56200	22000	104156	0	795400
Gross stock of debt (formal+quasi-formal)	46085	15000	96334	0	795400
Gross stock of debt (informal)	8477	0	21734	0	200000
Panel D: Households without baseline connections with the elites (N=231)					
Variable	**Mean**	**Median**	**Std. Dev.**	**Min**	**Max**
Any loans (any source)	0.52	1	0.50	0	1
Any formal/quasi-formal loans	0.39	0	0.49	0	1
Any informal loans	0.25	0	0.43	0	1
Number of loans (total)	1.09	1	1.53	0	10
Number of loans (formal+quasi-formal)	0.61	0	0.93	0	6
Number of loans (informal)	0.49	0	1.09	0	8
Gross stock of debt (total)	62356	2780	142614	0	1015000
Gross stock of debt (formal+quasi-formal)	50936	0	128394	0	890000
Gross stock of debt (informal)	7160	0	21908	0	200000

Note: The table presents summary statistics for the probability of holding a loan, the number of outstanding loans, and gross stock of debt in a given month, by type of lender. Formal loans include loans from the Bank of Agriculture and Agricultural Cooperatives or commercial banks. Quasi-formal loans include loans from cooperatives, production credit groups (PCGs), village funds and other village organizations. Informal loans include loans both from personal lenders and relatives inside or outside of the village. Connected: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected: households without any direct connection with members of the village council.

Table BXVII: Demographic characteristics by membership in the Village council

	Village council members (Elites) (N=60)		Directly connected with elites (N=352)		Unconnected (N=231)	
	Mean	**Std. Dev.**	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
Females	2.12	1.17	2.21	1.20	2.16	1.34
Males	2.43	1.25	1.95	1.11	1.78	1.13
Females 15 to 64	1.37	0.76	1.35	0.77	1.31	0.86
Males 15 to 64	1.52	0.81	1.15	0.77	1.11	0.92
Average years of schooling (household)	5.32	1.79	4.51	1.75	4.76	2.39
Average age (household)	34.53	12.67	36.18	13.82	39.23	15.40
Head of household is male	0.93	0.25	0.76	0.43	0.71	0.45
Owns an off-farm business	0.97	0.18	0.84	0.37	0.57	0.50
Land (in rai)	226	286	137	169	122	233
Per-capita wealth (TBH in 1999 values)	908636	3675508	384790	701427	595293	1608951

Note: The table presents summary statistics for baseline demographic characteristics by relationship with members of the village council. Connected: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected: households without any direct connection with members of the village council.

Table BXVIII: Baseline socioeconomic and kinship relationships with village council members

Variable	**Obs**	**Mean**	**Median**	**Std. Dev.**	**Min**	**Max**
Village council member (elites)	643	0.09	0.00	0.29	0	1
Directly transacts with elites	643	0.55	1.00	0.50	0	1
Degree with the elites	643	1.32	1.00	1.55	0	8
Geodesic distance to elites (excludes singletons)	631	1.30	1.00	0.72	0	4
Closeness to the elite	643	0.48	0.50	0.20	0	1
First degree relative with the elites	643	0.13	0.00	0.34	0	1

Table BXIX: Summary statistics for connections with the elite by socioeconomic interaction type

Type of transaction	**Obs**	**Mean**	**Std. Dev.**	**Min**	**Max**
Assets purchase	583	0.06	0.24	0	1
Assets sale	583	0.05	0.23	0	1
Contribution/Transfer	583	0.02	0.13	0	1
Gift reception	583	0.05	0.22	0	1
Lending	583	0.05	0.22	0	1
Borrowing	583	0.08	0.27	0	1
Paid employee	583	0.25	0.43	0	1
Employer	583	0.11	0.32	0	1
Provides unpaid labor	583	0.22	0.42	0	1
Receives unpaid labor	583	0.21	0.41	0	1
Input sale	583	0.10	0.30	0	1
Input reception	583	0.30	0.46	0	1
Output sale	583	0.13	0.34	0	1
Output purchase	583	0.19	0.39	0	1

Note: Input sale and reception include physical inputs as well as mentoring and advising. Socioeconomic interactions are based on data corresponding to the periods preceding the rollout of the program. Calculations exclude village council members.

Table BXX: Differences in baseline poverty and productivity characteristics by baseline access to credit and alternative targeting criteria

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log per-capita consumption (N=660)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
High Baseline access to institutional credit	0.161***	0.143**	0.060	0.082	0.195***	0.164
	(0.060)	(0.064)	(0.057)	(0.061)	(0.057)	(0.116)
Panel B: Total factor productivity (logs) (N=637)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
High Baseline access to institutional credit	-0.067	-0.051*	-0.027	0.007	-0.087*	-0.147**
	(0.044)	(0.030)	(0.034)	(0.047)	(0.051)	(0.068)
Panel C: Log per-capita consumption (N=660)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Offered credit under means-testing criterion	-0.417***	-0.253***	-0.309***	-0.428***	-0.500***	-0.577***
	(0.051)	(0.067)	(0.054)	(0.050)	(0.060)	(0.075)
Panel D: Total factor productivity (logs) (N=637)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Offered credit under means-testing criterion	-0.017	0.182***	0.139***	0.017	-0.131***	-0.178***
	(0.039)	(0.029)	(0.031)	(0.049)	(0.047)	(0.066)
Panel E: Log per-capita consumption (N=660)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Offered credit based on credit score	0.096*	0.080	0.072	0.126***	0.204***	0.177**
	(0.053)	(0.062)	(0.051)	(0.049)	(0.058)	(0.082)
Panel F: Total factor productivity (logs) (N=637)						
	Mean			Percentiles		
		0.1	0.25	0.5	0.75	0.9
Offered credit based on credit score	0.069*	0.020	0.059*	0.074*	0.119***	0.077
	(0.038)	(0.034)	(0.033)	(0.041)	(0.043)	(0.064)

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: Column (1) presents coefficients corresponding to a regression of baseline characteristics on an indicator of whether a household obtained institutional credit during the baseline periods (Panels A and B), an indicator of whether a household would have been offered credit under a counterfactual means-testing criterion(Panels C and D), and an indicator of whether a household would have been offered credit under a counterfactual allocation based on predicted credit scores (Panels E and F). Columns (2)-(6) present results for equivalent quantile regressions. The bandwidth use for the estimation of quantile regressions was selected using Hall-Sheather's method. Robust standard errors are presented in parentheses.

C Appendix Figures

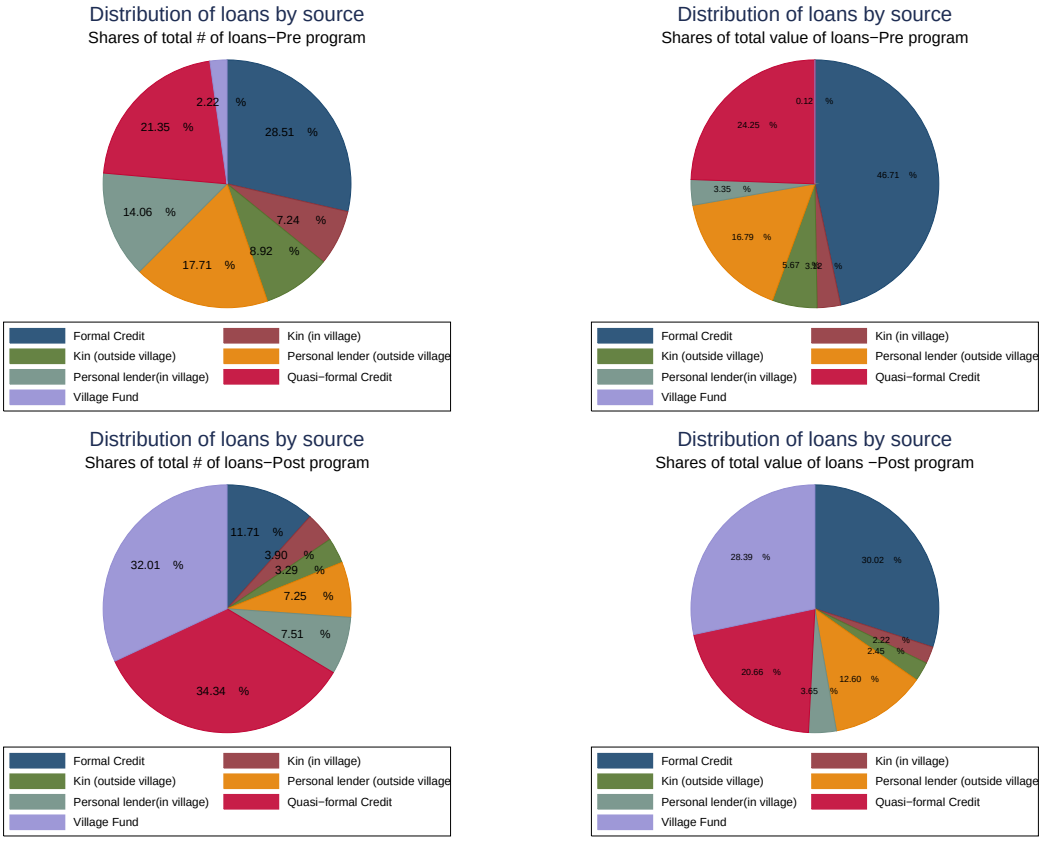


Figure CXI: Loan portfolio in the village economy before and after the program

Note: The top panel illustrates the distribution of loans by source (number and value of loans) for loans started between 1999 and 2000 (baseline periods). The bottom panel replicates the results for the two years following the rollout of the program. Formal loans include loans from the Bank for Agriculture and Agricultural Cooperatives (BAAC) and commercial banks. Quasi-formal loans include agricultural cooperatives and production credit groups (PCGs).

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D Appendix: Productivity

D.0.1 Estimating value-added productivity

This section provides a detailed explanation of the estimation of total factor productivity from a value-added production function, following the approach proposed by Ackerberg et al. (2015). Value added (VA) is computed as total revenues R net of the value of the intermediate inputs M used to generate them over a calendar year. Assuming that households choose the amount of labor L and capital K to be used in order to generate value added, it is possible to represent the log value-added production function as follows (variables in lower case denote logs):

$$y_{it} = \beta_0 + \beta_l l_{it} + \beta_k k_{it} + \omega_{it} + \epsilon_{it} \tag{Dviii}$$

This expression is consistent with a Cobb-Douglas value-added production function, or a production function which is Leontief in intermediate inputs and Cobb-Douglas in capital and labor. This specification allows for the existence of two different shocks to production: shocks to productivity that are observed or forecasted by each household (ω_{it}) but not observed by the researcher, and shocks to production that are unobserved by both the household and the researcher(ϵ_{it}). As profit-maximizing households allocate capital and labor such that the marginal product of each factor equals the factor's price. This behavior leads to the main empirical challenge in the estimation of a the production function: Capital and labor are chosen based on the observed productivity shocks ω, which means that an OLS regression of log value added on log labor and log capital would be biased. Following the insights discussed in Olley and Pakes (1996) and Levinsohn and Petrin (2003), Ackerberg et al. (2015) propose a two-stage approach to recover consistent estimates of β_l and β_k as well as predicted values for the productivity shocks.

D.1 Identification assumptions

The identification of the parameters from equation (Dviii) is achieved through assumptions corresponding to the information available to each household when deciding on the use of labor and capital, the process through which productivity evolves over time, and the extent to which input decisions can be adjusted in response to productivity shocks. This section intuitively describes these assumptions and refers the reader to Ackerberg et al. (2015) for more formal statements of these assumptions.

The first assumption is related to the information available to households at each point in time. The

estimation approach assumes that during period t, households are aware of current productivity shocks as well as past productivity shocks; however, future shocks to productivity are not known by the households. Denote each household's information set at t as I_{it}. Since households do not expect or observe current transitory shocks to production ϵ_{it}, this assumption implies that the shocks to production are orthogonal to productivity shocks:

$$\mathbf{E}[\epsilon_{it}|I_{it}] = 0 \tag{Dix}$$

The second assumption is related to the ability of households to use information to predict shocks to productivity and the persistence of these shocks. This paper assumes that productivity evolves according to a first-order Markov process, which is known to households:

$$\begin{aligned} \omega_{it} &= g(\omega_{it-1}) + \zeta_{it} \\ \mathbf{E}[\zeta_{it}|I_{it-1}] &= 0 \end{aligned} \tag{Dx}$$

This assumption, while restrictive in terms of the dynamics of productivity, is weaker than assumptions that would be made in an OLS approach or fixed-effects model or a dynamic panel approach (see for example Anderson and Hsiao (1982)). In the context of this study, it allows productivity at baseline to be a good predictor of productivity in the periods following the implementation of the program, and hence to be a relevant margin for evaluating the targeting performance of the program. The third assumption is related to the law of motion for the stock of capital (k_{it}). In particular, the assumption is that capital in the current period k_t is a function of the stock of capital and investment in the previous period k_{t-1}, i_{t-1}:

$$k_t = k(i_t, k_{t-1}) \tag{Dxi}$$

This assumption means that capital is fixed in the sense that households would experience high costs to adjust their choices of capital in response to current productivity shocks. A further assumption is that labor decisions are made in any time period up to period t. Thus, labor is allowed to adjust with respect to current productivity shocks. In this sense, labor is a free input in this model. While this assumption implies that lagged values of l could be used as instruments for current values of labor, the fact that capital is pre-determined is not enough to recover consistent estimates of β_l and β_k, as investment might be a

function of observed productivity and hence k_t may be correlated with ω_t given that there is persistence in the productivity shocks. Thus, variation in productivity still needs to be controlled for. The final two assumptions allow the researcher to control for variation in productivity by imposing some structure on the way intermediate inputs relate to productivity. The key assumptions in this approach are that conditional on their labor and optimal capital decisions, as well as the observed shocks to productivity, in each period households demand intermediate inputs according to a monotonically increasing function of ω_{it}, conditional on labor and capital choices:

$$\begin{aligned} m_{it} &= f_t(k_{it}, l_{it}, \omega_{it}) \\ w_{it} &= f_t^{-1}(m_{it}, k_{it}, l_{it}) \end{aligned} \tag{Dxii}$$

This assumption allows for inversion of f and use of the conditional variation in m to control for the variation in productivity shocks that are not observed by the researcher; that is, it allows ω to be written as a function of the intermediate input m, capital k, and labor l. This assumption rules out models in which there are adjustment costs to intermediate inputs, or models in which there are shortages in the supply of these inputs. While restrictive, the latter assumption has the advantage that it is testable as discussed in Levinsohn and Petrin (2003).

D.1.1 Moment conditions

Using the assumptions in (Dix) to (Dxiii), it is possible to derive the two moment conditions that will allow the identification of β_l and β_k.

$$\mathbf{E}[\epsilon_{it}|I_t] = \mathbf{E}[y_{it} - \Phi_t(m_{it}, k_{it}, l_{it})|I_{it}] = 0 \tag{Dxiii}$$

$$\mathbf{E}[\zeta_{it} + \epsilon_{it}|I_{it-1}] =$$

$$\mathbf{E}[y_{it} - \beta_0 - \beta_l l_{it} - \beta_k k_{it} - g(\Phi_{t-1}(m_{it-1}, k_{it-1}, l_{it-1}) - \beta_l l_{it-1} - \beta_k k_{t-1})|I_{it-1}] = 0 \tag{Dxiv}$$

with $\Phi_t = \beta_0 + \beta_l l_{it} + \beta_k k_{it} + f_t^{-1}(m_{it,l_{it},k_{it}})$ The first moment condition results from plugging in (Dxiii) into (Dviii), and combining it with (Dix). The Second moment condition exploits the assumption that productivity evolves according to a first-order Markov process as in (Dxi). Note that none of the structural parameters can be identified only from the first equation, however it is possible to use these moment conditions to identify $\hat{\Phi}_t$, and plug in $\hat{\Phi}_{t-1}(m_{it-1}, l_{it-1}, k_{it-1})$ into the second equation (Dxiv).

The resulting set of moment conditions after this process is:

$$\mathbf{E}\left[(y_{it} - \beta_0 - \beta_l l_{it} - \beta_k k_{it} - g(\Phi_{t-1}) - \beta_l l_{it-1} - \beta_k k_{t-1})) \otimes \begin{pmatrix} 1 \\ l_{it-1} \\ k_{it} \\ \hat{\Phi}_{it-1} \end{pmatrix}\right] = 0 \tag{Dxv}$$

The behavioral assumptions made in this sections are represented in this set of moment conditions. First, as capital is pre-determined, k_t is a function of investment at $t-1$ and thus $k_{it} \in I_{it-1}$. This means that capital is chosen prior to observing innovations in the productivity process ζ_{it}. However, this approach does not restrict the adjustment of labor and it is perfectly possible that a household will adjust labor given the innovations ζ_{it}. The only restriction in terms of the adjustment of labor decisions is that households cannot forecast ζ_{it}, and thus their past labor decisions are orthogonal with respect to current innovations to productivity. Finally, note that there is no extra variation coming from the intermediate input m in the latter set of moment conditions; the relevant variation was already used to recover $\hat{\Phi}_t$ from (Dxiii). This last observation prevents identification of a elasticity parameter for m, and hence the identification of a revenue function without assuming that the underlying technology is Leontief in intermediate inputs. [60]

D.1.2 Estimation and variable definition

The estimation approach to recovering β_l and β_k follows the simplification detailed in Appendix A.4 in Ackerberg et al. (2015). This process reduces the system in (Dxv) to:

$$\mathbf{E}\left[\hat{\zeta_{it}} \otimes \begin{pmatrix} l_{t-1} \\ k_t \end{pmatrix}\right] = 0 \tag{Dxvi}$$

with $\hat{\zeta_{it}} = (\hat{\Phi}_t - \beta_l l_{it} - \beta_k k_{it}) - h(\hat{\Phi}_{t-1} - \beta_l l_{it-1} - \beta_k k_{it-1})$. h is an arbitrary function. The estimation is performed through the generalized method of moments (GMM) using k_{it} and l_{it-1} as instruments. To operationalize this process, value added y is computed as the total revenues, over a calendar year t, net of the value of the inputs purchased outside the household that were used to generate revenue during the period (m_{it}). The proxy variable is the total value of inputs, purchased outside the household, that were used for generating revenues (m_{it}). These inputs include fertilizer and seeds for agriculture, tools for fishing, transportation

[60] Gandhi et al. (2016) discuss this issue extensively and develop an alternative approach which in principle allows to estimate a revenue function and relax these assumptions.

spending, appliances to be used in off-farm family businesses, and labor from outside the household. Labor is measured as the total hours per year of labor employed in households's revenue-generating activities. On average 85% is provided by household members; this includes hours spent on agriculture, fishing, caring for cattle, working at the off-farm family business, and working for wages outside the household. Capital is measured as the value of the total stock of fixed assets, and to be consistent with the assumptions regarding the timing of the inputs, it is measured in January of each calendar year. Section D.1.6 discusses robustness checks against alternative measures of labor and capital.

D.1.3 Estimation procedure

D.1.4 Value added function estimation

The elasticities from the household value added function and the estimates for productivity are recovered following the process detailed below.

1. Using the 14 years of data, the first-stage regression corresponding to the sample analog of (Dxiii) is estimated. The function f_t^{-1} that maps productivity ω_{it} into the demand for intermediate inputs is approximated using a third-order polynomial on m, k, and l. To allow f to vary with changes in the price of final output and inputs over time and across villages–but which are common to households within a village–village-year fixed effects (δ_{vt}) are included in the first stage:

$$y_{it} = \sum_{h=0}^{h=3} \sum_{j=0}^{j=3} \sum_{n=0}^{n=3} \phi_{hjn} m_{it}^h l_{it}^j k_{it}^n + \delta_{vt} + e_{it}$$

2. $\hat{\Phi}_t$ is computed as $\hat{\Phi}_t = \sum_{h=0}^{h=3} \sum_{j=0}^{j=3} \sum_{n=0}^{n=3} \phi_{hjn} m_{it}^h l_{it}^j k_{it}^n + \delta_{vt}$.

3. Using candidate values for β_k and β_l, obtained from an OLS regression, $\hat{\omega_{it}}$ is computed as:

$$\hat{\omega_{it}} = \hat{\Phi}_t - \beta_l l_{it} - \beta_k k_{it}$$

4. Since productivity is assumed to follow a first-order Markov process, the following equation is estimated:

$$\hat{\omega_{it}}(\beta_l, \beta_k) = \sum_{n=1}^{n=3} \psi_n \hat{\omega}_{it-1}(\beta_l, \beta_k) + \delta_{vt} + \tilde{\zeta}_i$$

5. The resulting residuals $\hat{\zeta}_i(\beta_l, \beta_k)$ are used to construct the sample analog of (Dxvi), and $\hat{\beta}_l$ and $\hat{\beta}_k$ are estimated using GMM.

6. To account for the uncertainty in the estimation of the first stage, standard errors are computed using 500 non-parametric block bootstrap samples stratified at the village level. Additionally, p-values associated with percentile t-bootstrap tests for significance are reported in order to provide an asymptotic correction for a small sample estimation.

7. Value-added productivity is recovered using the GMM estimates $\hat{\beta}_k$ and $\hat{\beta}_l$:

$$\hat{\omega}^*_{it} = \hat{\Phi}_t - \hat{\beta}_l l_{it} - \hat{\beta}_k k_{it}$$

8. For the analysis in the paper I only focuss on estimates of productivity $\hat{\omega}^*_{it}$ corresponding to the average over the baseline years 1999-2000.

9. I also report results using only data from 1999-2001 to estimate the elasticities (baseline data only). Results are robust to this approach.

D.1.5 Revenue Function estimation

An alternative way of recovering factor elasticities and productivity is to estimate a household revenue function following a dynamic panel model by "ρ-differencing" the equation below:

$$y_{it} = \beta_0 + \beta_l l_{it} + \beta_k k_{it} + \beta_m m_{it} + \omega_{it} + \epsilon_{it} \tag{Dxvii}$$

and assuming that ω follows a first-order autoregressive process: $\omega_{it} = \rho\omega_{it-1} + \zeta_{it}$. In this case, the dependent variable y denotes total revenues for a household, and m (intermediate inputs) is also included in the revenue function.

The estimation process is detailed below:

1. First I subtract ρy_{t-1} from both sides of the equations.

$$(y_{it} - \beta_k k_{it} - \beta_l l_{it} - \beta_m m_{it}) = \rho(y_{it-1} - \beta_k k_{it-1} - \beta_l l_{it-1} - \beta_m m_{it-1}) + \zeta_{it} + \epsilon_{it} - \rho\epsilon_{it-1}$$

2. Using candidate values for β_k, β_m, and β_l obtained from an OLS regression of (Dxvii), $\hat{\omega_{it}}$ is com-

puted as:

$$\hat{\omega}_{it} = (y_{it} - \beta_k k_{it} - \beta_l l_{it} - \beta_m m_{it}) - \beta_0$$

3. Since productivity is assumed to follow a first-order autoregressive process, the following equation is estimated:

$$\hat{\omega}_{it}(\beta_l, \beta_k, \beta_m) = \rho \hat{\omega}_{it-1}(\beta_l, \beta_k, \beta_m) + \delta_{vt} + \tilde{\zeta}_i$$

where δ_{vt} include a full set of village-year fixed effects.

4. The resulting residuals $\hat{\zeta}_i(\beta_l, \beta_k, \beta_m)$ are used to construct the sample analog of:

$$\mathbf{E}\left[(\omega_{it} - \rho\omega_{it-1}) \otimes \begin{pmatrix} 1 \\ l_{it-1} \\ k_{it-1} \\ m_{it-1} \end{pmatrix}\right] = 0 \qquad \text{(Dxviii)}$$

and $\hat{\beta}_l$ and $\hat{\beta}_k$ are estimated using GMM.

5. Revenue productivity is recovered using the GMM estimates $\hat{\beta}_k$ and $\hat{\beta}_l$:

$$\hat{\omega}^*_{it} = y_{it} - \hat{\beta}_l l_{it} - \hat{\beta}_k k_{it} - \hat{\beta_m}$$

6. I use only pre-program values of $\hat{\omega}_{it}$ corresponding to an average of predicted productivity for 1999-2000.

Table DXXI: Value Added Function Estimates

Panel A: Production function estimates (Value-added Cols (1)-(9) Revenues (10)-(11))

	OLS (1)	FE (2)	ACF (all years) (3)	ACF (M.E. in k) (4)	ACF(pre-program) (5)	ACF (balanced panel) (6)	ACF (OI) (7)	DP (all years) (8)	DP (pre-program) (9)	DP (all years) (10)	DP (pre-program) (11)
Labor (log)	0.514***	0.391***	0.724**	0.698**	0.645***	0.821	0.432***	0.695	0.683	0.219	0.491
S.E.	(0.010)	(0.022)	(0.350)	(0.390)	(0.301)	(1.038)	(0.031)	(0.449)	(0.482)	(0.371)	(0.493)
P-val (Bootstrap)			{0.03}	{0.01}	{0.00}	{0.28}	{0.00}				
Capital (log)	0.232***	0.0838**	0.233***	0.253***	0.163***	0.232***	0.177***	0.247	0.174	0.0834	0.0804
S.E.	(0.008)	(0.032)	(0.129)	(0.161)	(0.134)	(0.103)	(0.017)	(0.176)	(0.188)	(0.161)	(0.211)
P-val (Bootstrap)			{0.00}	{0.00}	{0.00}	{0.00}	{0.00}				
Intermediate inputs (log)										0.508***	0.342*
S.E.										(0.119)	(0.157)
Obs.	7226	7226	6438	6438	1106	5317	6372	6372	1096	6417	1102
Returns to scale (RTS)	0.747	0.475	0.958	0.951	0.808	1.053	0.610	0.943	0.857	0.81	0.914
Chi2 (constant RTS)	446.4	184.2	0.00885	0.00838	0.201	0.00224	127.3	0.00856	0.0465	1.735	0.373
P-Val (constant RTS)	0.000	0.000	0.714	0.927	0.5306	0.808	0.000	0.926	0.829	0.188	0.541
Test for OI restrictions (Jstat)							0.365				
Test for OI restrictions (Pval)							0.856				

Panel B: Summary Statistics for baseline productivity

	OLS (1)	FE (2)	ACF (all years) (3)	ACF (M.E. in k) (4)	ACF(pre-program) (5)	ACF (balanced panel) (6)	ACF (OI) (7)	DP (all years) (8)	DP (pre-program) (9)	DP (all years) (10)	DP (pre-program) (11)
Mean	4.10	6.92	2.51	2.46	4.02	1.79	5.45	2.54	3.58	3.58	3.89
Sd	0.94	1.02	0.58	0.58	0.60	0.61	0.65	0.94	0.93	0.92	0.87

$***p < 0.01, **p < 0.05, *p < 0.1$ Based on Bootstrap-t p-values for columns (3)-(6)

Note: The table presents estimates of a production function from different approaches as well as tests for the null of constant returns to scale. All estimations control for village $\times$ year fixed effects. In columns (1) to (5), the dependent variable is Value added from all the economic activities of the household. It is computed by subtracting the value of the intermediate inputs from the total revenues for each household. Revenues correspond to agriculture, livestock-raising and fishing, paid labor and family business activities. Labor is measured in hours/year across all activities and includes work performed by household members as well as by people outside the household. Capital is the value of each household's fixed assets, measured at the beginning of each year. All variables are in logs. Column (1) presents OLS estimates, Column (2) presents fixed-effects estimates. Columns (3)-(6) report GMM estimates using all of the observations from all available periods (benchmark), correcting for potential measurement error in capital (instrumenting by the first lag of log capital), the benchmark estimation using only from pre-program periods, and only the sample of households observed during all waves of the survey, respectively. The instruments for these specifications are the first lag of labor and capital measured at the beginning of each year. Column (7) presents estimates from an overidentified model that also includes the second lag of labor and first lag of capital as instruments. Column(8)-(9) presents estimates from a dynamic panel model estimated through GMM using lagged versions of capital and labor as instruments. Columns (10) and (11) present estimates for a gross revenue function based following a dynamic panel approach. Standard errors from the two-stage procedure are presented in parentheses. P-values using the empirical distribution of the t-statistic derived from 500 bootstrap samples (percentile-t bootstrap), to allow for small sample asymptotic correction, are reported in braces.

D.1.6 Alternative specifications and discussion

To avoid imposing restrictive assumptions regarding the use of credit and the interactions of all possible sources of income that households may have, this paper uses the benchmark specification that employs total revenues over all activities and total expenditures on intermediate inputs. Table DXXII presents robustness checks of the productivity estimates associated with different definitions of labor, capital, and revenues. Column (1) replicates the benchmark estimates for comparison. Column (2) presents estimates from a model that excludes hired labor. In this case β_l only captures the contribution of labor provided by household members. While labor from household members accounts on average for 85% of total labor, and the resulting coefficients are similar with respect to the benchmark specification, excluding hired labor reduces the observations as there are some households that rely exclusively on hired labor. Column (3) excludes household assets from the computation of capital. Household assets are mainly composed of the value of the dwelling in which households live and other appliances in the household. The resulting estimates are basically identical to the benchmark specification. Finally, Column (4) reports estimates that exclude revenues and expenses related to paid labor outside the household. The resulting estimates are smaller in the case of β_l with respect to the benchmark cases. Note however that excluding revenues from wage labor reduces the available observations, as some households may rely exclusively on this source of revenue.

Table DXXII: Production function estimates under alternative specifications

Panel A: GMM Estimates of the Value-Added function

	Benchmark Specification (1)	**Excluding hired labor (2)**	**Excluding household assets (3)**	**Excluding wage earnings (4)**
Labor (log)	0.724*	0.812	0.745	0.634***
	(0.350)	(0.949)	(0.396)	(0.0497)
Capital (log)	0.233*	0.254	0.210*	0.213
	(0.110)	(0.160)	(0.0997)	(0.136)
Obs	6438	6231	6438	5592
Returns to Scale	0.958	1.066	0.955	0.846
Chi2 (Test for constant RTS)	0.00885	0.952	3.828	35.44
Pval (Test for constant RTS)	0.925	0.329	0.0504	0.000

Panel B:Value-added productivity estimates

	Benchmark Specification (1)	**Excluding hired labor (2)**	**Excluding household assets (3)**	**Excluding wage earnings (4)**
Mean	2.51	1.69	2.68	3.87
SD	0.58	0.72	0.59	0.77

Panel C: GMM Estimates of the Value-Added function

	ACF Farm (1)	**ACF No Farm (2)**	**DP Farm (3)**	**DP No Farm (4)**
Labor (log)	1.010	1.085	0.749	0.734
	(2.012)	(234.9)	(0.605)	(0.550)
Capital (log)	0.145	0.249	0.141	0.189
	(0.207)	(29.21)	(0.226)	(0.220)
Obs	3517	2921	3474	2898
Returns to Scale	1.155	1.335	0.890	0.923
Chi2 (Test for constant RTS)	0.006	0.000	0.0179	0.0103
Pval (Test for constant RTS)	0.943	0.999	0.894	0.919

Panel D:Value-added productivity

	Benchmark Specification (1)	**Excluding hired labor (2)**	**Excluding household assets (3)**	**Excluding wage earnings (4)**
Mean	1.42	-0.55	3.50	3.00
SD	0.85	0.70	0.95	0.94

Note: Panel A presents estimates of production function from different specifications using the method proposed by Ackerberg et al. (2015). All estimations control for village $\times$ year fixed effects both in the first and second stage and are estimated using GMM. The dependent variable is Value-added from all the economic activities of the household. It is computed by subtracting the value of intermediate inputs from the total revenues for each household. Revenues correspond to agriculture, livestock-raising and fishing, paid labor, and family business activities. Labor is measured in hours/year across all activities and includes work performed by household members as well as by people outside the household. Capital is the value of each household's fixed assets, measured at the beginning of each year. All variables are in logs. Column (1) replicates the benchmark specification used in the paper. Column (2) presents estimates excluding hired labor from the estimations. Column (3) presents estimates from a model that excludes households' assets from the computation of capital and Column (4) presents estimates of value-added excluding earnings and costs from labor outside the household. Bootstrap standard errors are clustered at the household level to account for serial correlation, and are presented in parentheses. Panel B provides summary statistics for productivity measures that were estimated using each specification. Panel C presents estimates for households for whom farm activities (agriculture, livestock and fishing) were on average the main sources of income Column (1) and for whom non-farm activities were the main source of income Column (2) using the approach proposed by Ackerberg et al. (2015). Columns (3)-(4) replicate this estimations using a dynamic panel approach.

D.1.7 Testing the monotonicity assumption

The main identifying assumption in this context is the existence of a demand function that maps the demand of intermediate inputs m purchased outside the household to productivity in a strictly monotonic way. The empirical implication of this assumption is that the productivity estimates should exhibit a strictly monotonic relationship to the value of the intermediate inputs, conditional on labor, capital, and village-year fixed effects. Figure DXII provides a graphical test for the strict monotonicity assumption. The y-axis plots residuals from a regression of the value of intermediate inputs m_{it} on a third-order polynomial of labor and

capital and a full set of village-year dummies. The x-axis plots residuals of a similar regression in which the dependent variable corresponds to the value-added productivity estimates. The picture depicts a clear monotonic relation among these variables, validating the main identification assumption in this approach.

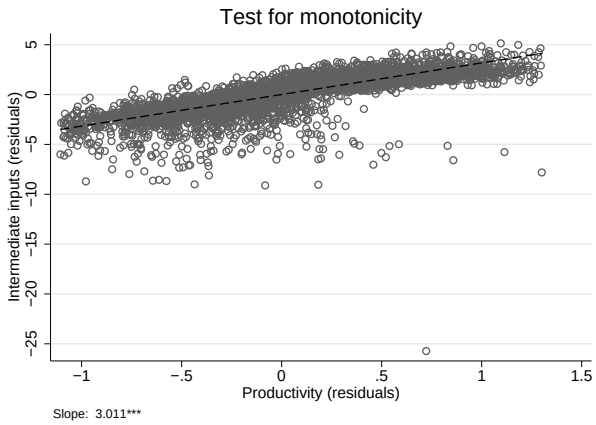


Figure DXII: Productivity and intermediate inputs

Note: The figure plots residuals of a regression of productivity on a third-degree polynomial of log labor and log capital, controlling for village-year fixed effects (x-axis) and residuals of a regression of log purchased inputs on a third-degree polynomial of log labor and log capital,controlling for village-year fixed effects (y-axis). Top and bottom 1% of observation are winsorized.

A more formal test for the validity of this assumption is provided by Shenoy (2017). The idea is that if firms were constrained with respect to the intermediate inputs or faced rigidities in the markets of intermediate inputs, production in period t should be a function of past input choices (first lags of capital, labor and intermediate inputs). I test for this using a two-stage approach. First, I regress log value-add y on a third order polynomial of current values of log capital,labor and intermediate inputs ($h(k_t, l_t, m_t)$), controlling for village-year fixed effects, and compute the residuals $\hat{\epsilon_{it}}$. Then I regress these residuals on a vector $\mathbf{r}_{t-1}$ of lagged capital, labor and intermediate inputs and test the extent to which all the elements of the vector $\kappa = \mathbf{0}$:

$$\hat{\epsilon}_i = \mathbf{r}_{t-1}\kappa + v_i \tag{Dxix}$$

If households do not face constraints in the adjustment of inputs, then variation in output should be only explained by current choices of input and $\kappa = 0$. Table DXXIII shows that the null of no constraints is not rejected under several specifications. While this validate the identification assumptions, note that this is not evidence of no credit constraints. For instance, households may hold excess on inventory simply because they don't have access to credit to finance increases in inputs when a households experiences positive

productivity shocks.

Table DXXIII: Test for frictions in intermediate inputs

Regressors ($\mathbf{r}_{t-1}$):	$k_{t-1}, l_{t-1}, m_{t-1}$	$\{k_{t-j}, l_{t-j}, m_{t-j}\}_{j=1}^{j=2}$	$\{k_{t-j}, l_{t-j}, m_{t-j}\}_{j=1}^{j=3}$	2nd order $f(k_{t-1}, l_{t-1}, m_{t-1})$	3rd order $f(k_{t-1}, l_{t-1}, m_{t-1})$
Observations	6,532	5,916	5,240	6,438	6,438
Adjusted R-squared	-0.000	-0.000	0.001	-0.000	0.003
F Stat :$\kappa = 0$	0.0759	0.554	1.250	0.383	1.104
P-val :$\kappa = 0$	0.927	0.758	0.279	0.930	0.338

Note: The table presents F statistics and P-values corresponding to the null hypothesis that $\kappa = 0$ (see equation Dxix) for several specifications. Column (1) presents results from a model including first lags of capital, labor and intermediate inputs. Columns (2) and (3) present results from specifications that include second and third lags of the variables respectively. Columns(4) and (5) report results from tests which include flexible polynomials of the first lags for capital, labor and intermediate inputs. Robust standard errors are presented in parenthesis.

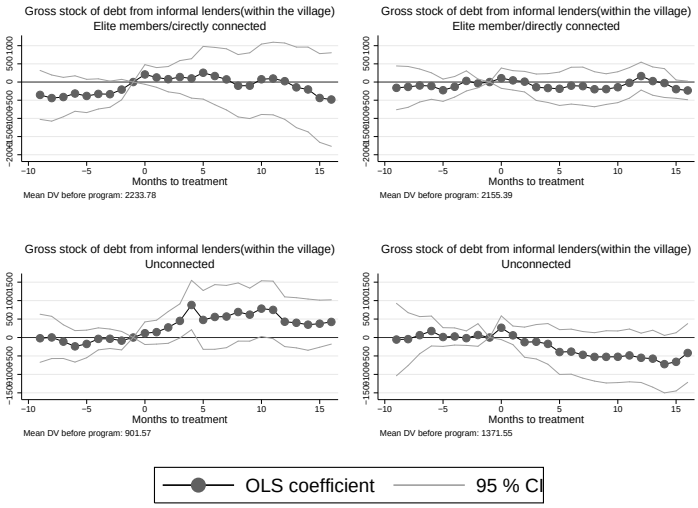
E Robustness to the agricultural cycle and placebo analysis

This section replicates the flexible difference-in-difference results of the paper in the placebo sample following the approach discussed in section 7.3. To do so, I use the two years preceding the implementation of the program. In particular I focus on a time window that excludes the data I used to compute my main estimates: $\tau_{v,t} \in [-36, -6)$. I normalize the time-to-treatment variable τ to be between -12 and 17 (centered at -1) such that the calendar months in which the funds were actually released coincide to those in the placebo exercise. For example, if the funds for a certain village were released in June ($\tau_{vt} = 0$), for that same village June would be the first month of treatment in the placebo periods $\tau_{vt}^{PLACEBO} = 0$. The placebo sample coincides with the period September 1999-February 2001.

It is worth mentioning that more placebo months are available for the villages that enter into treatment later, conversely villages that enter the treatment earlier are not observed for all the periods preceding the placebo treatment (i.e. months for which $\tau_{vt} < 0$ in the placebo sample). Appendix Figures EXIII EXIV reproduces the main figure in the paper. They plot the results from the study sample on the left-hand side, and present the placebo results on the right-hand-side. There is a pattern of pre-trends in the placebo sample which could be related to decreases in overall financial activity due to the South-East Asian financial crisis and the associate recovery, or measurement error in the first rounds of the survey. However, the flexible difference-in-difference estimates in the placebo sample look flat in most cases, and, when different from zero, move in the opposite direction of the effects reported in the original analysis suggesting that, if anything, the main estimates understate the true effects.

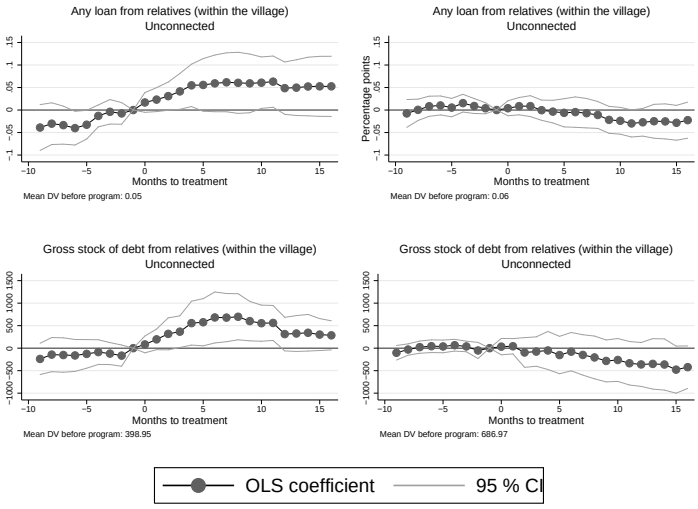
E.0.8 Placebo test for main results

Figure EXIII: Short-term effects of the VF program on credit from local informal sources



Note: The figure depicts flexible difference-in-difference estimates corresponding to equation (6). The top panel plots OLS coefficients capturing the effects of the program on borrowing from informal lenders (either personal lenders or relatives) by connected households, while the bottom panel presents estimates for total borrowing from informal lenders by unconnected households. The left-hand-side graphs present results related to the implementation of the program, while the graphs in the right-hand panel represent estimates using the placebo sample.

Figure EXIV: Short-term effects of the program on credit from relatives-unconnected households



Note: The figure depicts flexible difference-in-difference estimates corresponding to equation (6). The top panel plots OLS coefficients capturing the effects of the program on borrowing from relatives (number of outstanding loans) by unconnected households, while the bottom panel presents estimates for total borrowing from relatives by unconnected households. The left-hand-side graphs present results related to the implementation of the program, while the graphs in the right-hand panel represent estimates using the placebo sample.

E.1 Attrition

Table EXXIV: Effects of the program on total borrowing by connection with elites(excluding attriters)

Panel A: Effects on credit from the program

	Any credit from MBVF			**Gross debt from MBVF**		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	All	Connected	Unconnected	All	Connected	Unconnected
$Post_{vt}$	0.363***	0.423***	0.251***	6,320.631***	7,895.595***	2,983.796***
	(0.022)	(0.029)	(0.035)	(446.287)	(604.506)	(531.589)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.012]
Observations	18,305	12,230	6,075	18,232	12,179	6,053
R-squared	0.617	0.635	0.569	0.591	0.616	0.527
Baseline DV mean	0.0301	0.0450	0	45.57	68.19	0
Clusters (# households)	509	340	169	509	340	169

Panel B: Effects on total credit

	Any credit			**Total Gross outstanding debt**		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	All	Connected	Unconnected	All	Connected	Unconnected
$Post_{vt}$	0.073***	0.058***	0.104***	3,435.158	3,899.126	1,858.497
	(0.014)	(0.015)	(0.027)	(2,454.030)	(3,226.529)	(3,709.507)
	[0.000]	[0.000]	[0.012]	[0.232]	[0.356]	[0.612]
Observations	18,305	12,230	6,075	18,205	12,195	6,010
R-squared	0.653	0.623	0.658	0.876	0.833	0.919
Baseline DV mean	0.673	0.745	0.529	65935	61728	74462
Clusters (# households)	509	340	169	509	340	169

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents difference-in-difference estimates of the short-run effect of the rollout of the program on total borrowing, by connectedness with the local elite. The sample includes only households who are always interviewed during the 172 survey waves. The reported coefficients correspond to OLS regressions of the respective dependent variables on whether the resources from the program were released in village v in month t, controlling for household fixed effects and calendar month and year fixed effects (see equation (7)). Estimations were performed using all the available observations for the 18 months before and after the rollout of the program in each village. Panel A reports results for the effect of the rollout of the program on the program's uptake and Panel B shows results for total borrowing (winsorizing the top 1% of observations). Standard errors, presented in parentheses, are clustered at the household level to allow for flexible serial correlation. P-values that account for potential within village correlation are presented in brackets; they are computed using a wild bootstrap t-procedure to account for a reduced number of clusters (16) as in Cameron and Miller (2015). Connected: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected: households without any direct connection with members of the village council.

Table EXXV: Effects of the program on informal credit by connection with elites(excluding attriters)

Panel A: Any loan from informal lenders

	Connected			Unconnected		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Any informal	Relatives	Non-relatives	Any informal	Relatives	Non-relatives
$Post_{vt}$	-0.007	-0.000	-0.004	0.040**	0.032*	0.014
	(0.014)	(0.011)	(0.012)	(0.020)	(0.016)	(0.012)
	[0.684]	[0.916]	[0.752]	[0.188]	[0.028]	[0.800]
Observations	12,230	12,230	12,230	6,075	6,075	6,075
R-squared	0.703	0.674	0.642	0.600	0.561	0.605
Baseline DV mean	0.164	0.0650	0.111	0.0946	0.0511	0.0488
Clusters (# households)	340	340	340	169	169	169

Panel B: Gross stock of debt with informal lenders

	Connected			Unconnected		
	(1)	(2)	(3)	(4)	(5)	(6)
	Any informal	Relatives	Non-relatives	Any informal	Relatives	Non-relatives
$Post_{vt}$	624.130***	114.186	280.446*	446.749*	409.031**	176.284
	(232.407)	(79.465)	(150.523)	(231.349)	(195.825)	(127.309)
	[0.016]	[0.116]	[0.180]	[0.224]	[0.020]	[0.632]
Observations	12,115	12,168	12,096	6,004	5,995	6,075
R-squared	0.771	0.745	0.711	0.585	0.603	0.598
Baseline DV mean	1791	615.6	963.2	982.3	439	535.3
Clusters (# households)	340	340	339	169	169	169

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents difference-in-difference estimates of the short-run effect of the rollout of the program on borrowing from informal lenders, by connectedness with the local elites. The sample includes only households who are always interviewed during the 172 survey waves. Informal lenders include personal money lenders and relatives in the village. The reported coefficients correspond to OLS regressions of the respective dependent variables on whether the resources from the program were released in village v in month t, controlling for household fixed effects and calendar month and year fixed effects (see equation (7)). Estimations were performed using all the available observations for the 18 months before and after the rollout of the program in each village. Panel A reports results for the number of outstanding loans and Panel B shows results for the gross stock of debt (winsorizing the top 1% of observations). Standard errors, presented in parentheses, are clustered at the household level to allow for flexible serial correlation. P-values that account for potential within village correlation are presented in brackets; they are computed using a wild bootstrap t-procedure to account for a reduced number of clusters (16) as in Cameron and Miller (2015). Connected: households who reported having any socioeconomic interaction or direct kin relations with council members during the survey waves preceding the release of the funds from the program. Unconnected: households without any direct connection with members of the village council.

E.2 Alternative measures of connections

Table EXXVI: Effects of the program on total borrowing by connectedness score

Panel A: Effects on credit from the program

	Any credit from MBVF			Gross debt from MBVF		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	All	High	Low	All	High	Low
$Post_{vt}$	0.328***	0.389***	0.267***	3,264.857*	7,929.495***	2,851.297***
	(0.019)	(0.030)	(0.026)	(1,965.708)	(619.981)	(378.041)
	[0.000]	[0.000]	[0.000]	[0.120]	[0.000]	[0.020]
Observations	23,228	11,468	11,760	23,128	11,417	11,738
R-squared	0.613	0.641	0.570	0.866	0.636	0.527
Baseline DV mean	0.0290	0.0574	0.00151	60747	86.47	0.757
Clusters (# households)	671	331	340	671	331	340

Panel B: Effects on total credit

	Any credit			Total Gross outstanding debt		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	All	High	Low	All	High	Low
$Post_{vt}$	0.074***	0.057***	0.098***	5,529.391***	6,392.798**	987.431
	(0.013)	(0.016)	(0.020)	(373.757)	(2,685.509)	(2,734.166)
	[0.000]	[0.004]	[0.008]	[0.000]	[0.088]	[0.752]
Observations	23,228	11,468	11,760	23,155	11,433	11,695
R-squared	0.661	0.595	0.649	0.590	0.786	0.908
Baseline DV mean	0.665	0.821	0.515	42.97	60121	61356
Clusters (# households)	671	331	340	671	331	340

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents difference-in-difference estimates of the short-run effect of the rollout of the program on total borrowing, by connectedness with the local elite. The reported coefficients correspond to OLS regressions of the respective dependent variables on whether the resources from the program were released in village v in month t, controlling for household fixed effects and calendar month and year fixed effects (see equation (7)). Estimations were performed using all the available observations for the 18 months before and after the rollout of the program in each village. Panel A reports results for the effect of the rollout of the program on the program's uptake and Panel B shows results for total borrowing (winsorizing the top 1% of observations). Standard errors, presented in parentheses, are clustered at the household level to allow for flexible serial correlation. P-values that account for potential within village correlation are presented in brackets; they are computed using a wild bootstrap t-procedure to account for a reduced number of clusters (16) as in Cameron and Miller (2015). The connectedness score corresponds to an index based on the factor loadings of the first principal component related to all the different types of socioeconomic interactions with local elites. High score: households whose score is above the median. Low: households whose score is below the median.

Table EXXVII: Effects of the program on informal credit by connectedness score

Panel A: Any loan from informal lenders

	High connectedness			Low connectedness		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Any informal	Relatives	Non-relatives	Any informal	Relatives	Non-relatives
$Post_{vt}$	-0.003	-0.003	-0.007	0.025*	0.025**	0.011
	(0.019)	(0.014)	(0.016)	(0.014)	(0.012)	(0.010)
	[0.932]	[0.876]	[0.692]	[0.404]	[0.076]	[0.816]
Observations	11,468	11,468	11,468	11,760	11,760	11,760
R-squared	0.683	0.646	0.627	0.671	0.630	0.678
Baseline DV mean	0.210	0.0864	0.143	0.116	0.0555	0.0673
Clusters	331	331	331	340	340	340

Panel B: Gross stock of debt with informal lenders

	High connectedness			Low connectedness		
	(1)	(2)	(3)	(4)	(5)	(6)
	Any informal	Relatives	Non-relatives	Any informal	Relatives	Non-relatives
$Post_{vt}$	637.667**	291.817*	116.849	404.136**	220.620*	160.543*
	(266.360)	(150.283)	(166.179)	(177.592)	(128.296)	(88.772)
	[0.028]	[0.056]	[0.544]	[0.220]	[0.024]	[0.460]
Observations	11,311	11,330	11,332	11,656	11,664	11,687
R-squared	0.762	0.672	0.695	0.732	0.644	0.644
Baseline DV mean	2272	686.2	1361	1271	464.9	533.1
Clusters	331	331	330	340	340	339

$***p < 0.01, **p < 0.05, *p < 0.1$

Note: The table presents difference-in-difference estimates of the short-run effect of the rollout of the program on borrowing from informal lenders, by connectedness with the local elites. Informal lenders include personal money lenders and relatives in the village. The reported coefficients correspond to OLS regressions of the respective dependent variables on whether the resources from the program were released in village v in month t, controlling for household fixed effects and calendar month and year fixed effects (see equation (7)). Estimations were performed using all the available observations for the 18 months before and after the rollout of the program in each village. Panel A reports results for the number of outstanding loans and Panel B shows results for the gross stock of debt (winsorizing the top 1% of observations). Standard errors, presented in parentheses, are clustered at the household level to allow for flexible serial correlation. P-values that account for potential within village correlation are presented in brackets; they are computed using a wild bootstrap t-procedure to account for a reduced number of clusters (16) as in Cameron and Miller (2015). he connectedness score corresponds to an index based on the factor loadings of the first principal component related to all the different types of socioeconomic interactions with local elites. High score: households whose score is above the median. Low: households whose score is below the median.

F Appendix: Proofs of propositions 1 and 2

Consider the case of a rural household which chooses the optimal amount of inputs to be used for the family business or farm at the beginning of the year ($t = 0$) and uses the profits and other government transfers to finance consumption in the rest of the year ($t = 1$). These households may finance the only input in this economy (k_{0i}) using their initial exogenous wealth (w_i) or borrow (d_{0i}) at an interest rate of r. However, they may be liquidity constrained and only be able to borrow up to $\bar{d}$, which is exogenously determined and can be expanded by receiving loans from by the MBVF committee (b_i). Households maximize the following

simplified problem:

$$\max_{c_{1i}, k_{0i}, d_{0i}} U(c_{1i}) \tag{Fxx}$$

s.t.

$$c_{1i} + (1+r)q_i d_{0i} = A_i f(k_{0i}) \tag{Fxxi}$$

$$p_k k_{0i} \leq w_i + d_{0i} \tag{Fxxii}$$

$$d_{0i} \leq \bar{d} + b_i \tag{Fxxiii}$$

where U denotes an increasing and concave utility function of consumption in period $t = 1$ (c_{1i}), A_i denotes household total factor productivity associated to the production function $f(k_{0i})$ which is increasing and concave in k.

Assume u is a function of consumption in period t such that $u' > 0$ and $u'' < 0$. f is a production function that transforms the only input (k) into units of consumption goods and is increasing in k and concave ($f'' < 0$). Let $\lambda_1, \lambda_2, \lambda_3$ be the lagrange multipliers associated to constraints (Fxxi)-(Fxxiii), respectively. The lagrangian function associated to the optimization problem solved by household i is:

$$\mathbf{L} = u(c_{1i}) + \lambda_1(A_i f(k_{0i}) - c_{1i} - (1+r)q_i d_{0i}) + \lambda_2(w_i + d_{0i} - p_k k_{0i}) + \lambda_3(\bar{d} + b_i - d_{0i})$$

The first order conditions imply:

$$u'(c_{1i}) = \lambda_1 \tag{Fxxiv}$$

$$\frac{u'(c_{1i})}{p_k}(A_i f'(k_{0i})) = \lambda_2 \tag{Fxxv}$$

$$u'(c_{1i})\frac{1}{p_k}(A_i f'(k_{0i}) - p_k(1+r)) = \lambda_3 \tag{Fxxvi}$$

F.0.1 Proof of Proposition 1

Proposition 1. *If households face borrowing constraints, the marginal utility of relaxing this constraint is decreasing in initial wealth. Moreover, the marginal utility of relaxing a household's liquidity constraint is an increasing function of household productivity if the distortion in the optimal choice of inputs is large.*

Proof. In the context of binding liquidity constraints, each households only borrows up to $d^*_{0i} = \bar{d}$ and purchases inputs such that $k^*_{01} = \frac{w_i + \bar{d} + b_i}{p_k}$. Without loss of generality assume $q_i = 1$. Optimal consumption in this case is $c_{1i} = A_i f(\frac{w_i + \bar{d} + b_i}{p_k}) - (1+r)(\bar{d} + b_i)$. As a consequence of the envelop theorem, the marginal utility of loans from the program equals the marginal utility of relaxing the household's liquidity constraint ($\frac{\partial V}{\partial b} = \lambda_3$).

To see whether λ_3 is an increasing or decreasing function of borrowing from the program b_i, initial wealth w_i, and household productivity A_i, I obtain the respective partial derivatives of λ_3 using equation (Fxxvi).

$$\frac{\partial \lambda_{3i}}{\partial w_i} = \frac{u'' A_i f'}{p_k}\left(\frac{1}{p_k}(A_i f' - (1+r)p_k)\right) + u' \frac{A_i f''}{p_k} < 0 \tag{Fxxvii}$$

$$\frac{\partial \lambda_{3i}}{\partial A_i} = \frac{u'' f}{p_k}(A_i f' - p_k(1+r)) + u' \frac{f'}{p_k} \tag{Fxxviii}$$

$$\tag{Fxxix}$$

Equation (Fxxvii) is negative because u and f are concave, and because $A_i f' > (1+r)p_k$ when liquidity constraints are binding. The intuition is that because households are liquidity constrained, the marginal product of an extra unit of input still exceeds the costs of financing it. The sign of (Fxxviii) will depend on the curvature of the utility function and the size of the distortion in the allocation of inputs $A_i f' - p_k(1+r)$

$$\frac{f'}{f}(A_i f' - p_k(1+r)) > -\frac{u''}{u'} \tag{Fxxx}$$

Note that this condition will be satisfied depending on the concavity of the utility function. For example, this condition is trivially satisfied if household are simply profit maximizers –i.e., linear utility function–.

Equation (Fxxvii) implies that the marginal utility from borrowing from the program is decreasing in both borrowing and wealth. Equations (Fxxviii) and (2) imply that households with a higher utility derived from the program are high-productivity households. □

F.0.2 Proof of Proposition 2

Proposition 2. *If households do not face borrowing constraints but face high borrowing interest rates, the marginal utility from a reduction in the interest rate is a decreasing function of initial wealth and an increasing function of household productivity*

Proof. If the liquidity constraints are not binding, then households choose inputs based on prices, interest rates and household productivity ($k_{0i}^{**} = k(A_i, r, p_k)$). In this environment, household debt accounts for ($d_{0i}^{**} = p_k k(A_i, r, p_k) - w_i$) and the marginal utility of decreasing interest rates is: $\lambda_1 d_{0i}^{**}$ and is positive if households are net borrowers ($d_{0i}^{**} > 0$). Taking derivatives with respect to w_i and A_i:

$$\frac{\partial \lambda_1 d_{0i}^{**}}{\partial w_i} = u''(1+r)d_{0i}^{**} - u' < 0 \tag{Fxxxi}$$

$$\frac{\partial \lambda_1 d_{0i}^{**}}{\partial A_i} = u'' f d_{0i}^{**} + u' p_k \frac{\partial k^{**}}{\partial A_i} \tag{Fxxxii}$$

Equation (Fxxxi) is negative due to the concavity of u. Equation (Fxxxii) is positive if the marginal increase in utility derived from increasing inputs offsets the marginal cost in terms of utility of having to repay debt.

$$\frac{1}{f} p_k \frac{\partial k^{**}}{\partial A_i} > -u'' d_{0i}^{**}$$

This will typically be true for profit maximizing households–i.e., linear utility function–. □