

THE IMPACT OF VILLAGE SAVINGS AND LOANS ASSOCIATIONS ON POVERTY, VULNERABILITY, AND FINANCIAL INCLUSION*

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Abstract

This paper evaluates the impact of the introduction of village savings and loans associations (VSLA) on poverty, vulnerability, and financial inclusion outcomes in rural Peru. Using a cluster randomized control trial and relying on both survey and administrative records, we investigate the impact of VSLAs over a two year period. We find that savings groups channel expensive investments such as housing improvements and reduce households' vulnerability to idiosyncratic shocks, particularly among households in poorer districts. The treatment also induces changes in households' labor allocation choices: access to VSLAs increases female labor market participation and, in poorer areas, it fosters greater specialization in agricultural activities. Access to VSLAs also leads to a four-percentage point increase on female's access to credit, mainly driven by access to the VSLA's loans. However, the introduction of VSLAs has no impact on the likelihood to use formal financial services. On the contrary, it *discourages* access to loans from formal financial institutions and microfinance lenders among the unbanked.

KEYWORDS: saving groups, village savings and loans associations, financial inclusion, impact evaluation

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1 Introduction

Most of the rural poor around the world remain excluded from the financial system, which limits their capacity to access to savings, credit, and insurance services to invest in their productive activities or to manage idiosyncratic risk and seasonality [Collins et al., 2009]. Formal financial institutions are often absent in the most remote areas due to high screening and transaction costs. But, even when present, take-up and usage rates remain low [Karlan et al., 2014], which can be explained by high prices and lack of flexibility required to serve the rural poor [Bauchet et al., 2011], but also due to potential clients' limited understanding and biases against formal financial institutions.

The rural poor thus often satisfy their needs for financial services by relying on informal mechanisms that are frequently linked to social networks. Their demand is not only driven by exclusion from financial markets, but it also reflects preferences for attributes offered by informal services such as their convenience, easiness of use, and trustworthiness [Lee et al., 2012]. Although financial inclusion efforts tend to focus on the provision of *formal* financial services, financial markets are increasingly embracing informal services and even building upon them as they tend to be flexible, innovative, and naturally tailored to the specific needs of their clients. However, the downside of informal mechanisms may come from the high costs that arise from managing very small savings and credit portfolios, exposure to family demands, risk of losses, and theft, among others, casting doubts on their net welfare effects.

This study evaluates the effectiveness of Village Saving and Loan Associations (VSLAs), a model of provision of informal financial services that is used in 77 countries and serves over 20 million active participants worldwide.¹ One of the most attractive features of the model is that it does not rely on external capital such as the Village Banking model, but instead all funds come from group members' savings. We leverage the implementation of a clustered randomized control trial to evaluate the impact of the introduction of VSLAs on poverty, vulnerability, and financial inclusion outcomes in rural Peru. We worked with COFIDE, the Peruvian Development Bank, that has been promoting savings groups in Peruvian rural communities for fifteen years, taking advantage of the expansion they made between 2014 and 2016 in rural villages of Ayacucho, one of the poorest departments in the country².

We look at a variety of financial and social indicators coming from a survey applied two years after the beginning of the promotion of the VSLAs and credit history data obtained from one of the leading credit bureaus in Peru. Consistent with some of the previous studies on savings groups

¹See www.vsla.net. Our figures are based on access by September 25, 2020.

²COFIDE's name for their savings groups is UNICAs, for its name in spanish, Uniones de Crédito y Ahorro. The project started with COFIDE and CAF identifying the potential of the intervention, and the need to rigorously evaluate its impacts with the help of an experimental design. A long time has passed since the first meeting between Pablo Sanguinetti and Daniel Ortega (CAF), Manuel Layseca (COFIDE) and the research team, which lay out the research plan. They all played a key role in the successful completion of the project.

in Africa, we find no effects on average income or expenditures. However, we find a significant reduction of exposure to idiosyncratic shocks and increased investments in housing, variables that are directly connected to measures of multidimensional poverty [Conceição, 2019]. These effects are particularly important among households in poorer districts. Savings groups also seem to favor women’s empowerment as women in treated villages increase their labor market participation and access to credit, mainly through loans from the savings group.

However, the introduction of VSLAs has no impact on the likelihood to use formal financial services. On the contrary, it *discourages* access to loans from formal financial institutions and microfinance lenders among the unbanked. While VSLAs offer small loans out from the group’s savings, the model mainly responds to the need to save by offering a way to do it collectively. Thus, we interpret these results as an indication that access to an attractive and effective savings technology reduces the need for credit among our sample of poor rural households.

Due to the increased popularity of VSLAs around the developing world, we have seen an increase in the production of experimental studies looking at their effects on financial inclusion and the overall welfare of the members’ households, especially in Africa [Karlan et al., 2017; Ksoll et al., 2016; Beaman et al., 2014]. However, VSLAs remain rare in other parts of the big south such as Latin America. Even within the African context, results on their effectiveness are mixed. On one hand, no income or expenditure effects are found for savings groups in Ghana, Malawi, Uganda [Karlan et al., 2017], and Mali [Beaman et al., 2014]. However, Ksoll et al. [2016] report positive effects on household expenditures, meals consumed per day and number of rooms in the dwelling in northern Malawi while Karlan et al. [2017] and Beaman et al. [2014] find instead that savings groups do help households manage risk, either through consumption smoothing or food security. While Karlan et al. [2017] find positive effects on women’s empowerment, these are absent in Beaman et al. [2014]. Increased human capital investments are present in India [Baland et al., 2020], but not in Mali [Beaman et al., 2014].

This paper attempts to contribute to this literature by leveraging the expansion of the model in Latin America. Beyond the contribution in terms of the geographic focus of the study, there are two key contributions relative to previous studies. First, we focus on several dimensions that may proxy poverty status and allow some time (two years) for the treatment to play a role. Second, we focus on a previously under-explored set of outcomes related to financial inclusion. Access to credit bureau records allows us to focus on the (potentially unintended) effects of the provision of informal financial services on access and usage of formal financial services. This is an important question that should be factored into financial inclusion efforts, which tend to exclusively focus on formal financial services across the board. Our results suggests that, given the available supply of services, certain populations such as the unbanked may prefer informal over formal financial services.

The remainder of this paper is organized in five sections. Section 2 discusses the literature of savings groups or VSLAs, while Section 3 describes the PRIDER intervention and the experimental design.

Section 4 describes the estimation strategy while section 5 presents the results on take up and treatment impacts. Finally, the paper ends with a summary of the results and a discussion of their implications for policy and for the future research agenda.

2 Literature Review and Research Questions

For several decades, the expansion of microfinance services resulted in a boost to the provision of pro-poor financial services in developing countries around the world. However, studies that synthesize different experiences of this type of service in countries with high poverty rates and low access to the financial system did not find robust results neither on poverty reduction nor on effective financial inclusion levels [Banerjee et al., 2015; Crépon et al., 2015].

Formal financial institutions have proven effective in reaching the poor in urban areas [Crépon et al., 2011]. However, one of the main difficulties in the expansion and effective development of financial services targeting the rural poor is their concentration in remote areas and seasonal demands for credit that reflect their agricultural orientation [Lopez and Winkler, 2018]. Rural populations tend to demand access to lump sums to manage their cash flows, but they often struggle to find formal providers capable or willing to provide basic financial services suitable for the seasonality they face.

The rural poor tend to satisfy their needs for financial services by relying on a variety of non-institutional and decentralized informal mechanisms that have been studied in recent years [Besley et al., 1993; Beaman et al., 2014; Ksoll et al., 2016]. One of such mechanisms are savings groups or VSLAs, which involve associations of local people where members collectively save money, make loans according to local needs using common funds under specific terms and interest rates, and then redistribute financial gains between members. VSLAs are very similar to Rotating Savings and Credit Associations (ROSCAs), which have been vastly analyzed in the theoretical and empirical development literature [Besley et al., 1993]. They are formed by 6-15 individuals that commit to save a certain amount every month, and the monthly pot is then assigned to an individual member, in an order pre-defined through a lottery, until all members have received the fund once. The VSLA technology varies from the typical ROSCA by adding flexibility to their rules [Karlan et al., 2017]. First, members can save more than the minimum period amount (monthly meetings). Second, savings are allocated to members based on demand, and they pay an interest rate for the loan. Interest payments are later distributed among members at a predefined time, thus allowing for savers to have a positive return for their savings. In addition, VSLAs tend to have a solidarity fund that can be used when the group agrees that a member needs interest-free support. VSLAs, thus have a much cleaner focus on savings compared to village banks, as they set a stronger commitment on periodic deposits, and even provide a positive return for those net savers derived from the interest paid by borrowers.

Savings groups are a relatively more recent phenomenon than microfinance and the literature on

the effects of VSLAs on the finances and welfare of the poor has been concentrated in very poor regions of Africa [Anyango et al., 2007; Johnson et al., 2010; Cameron and Ananga, 2015; Burlando and Canidio, 2015; Brannen and Sheehan-Connor, 2016; Burlando and Canidio, 2017; Green, 2019]. Most of this literature emphasizes the impact that this financial scheme has on variables associated with the welfare and poverty of the beneficiaries. Most recent papers find that the effects are not large and robust enough to permanently get a household out of poverty, at least during standard study periods such as two or three years. In a large study of African countries such as Ghana, Malawi, and Uganda, Karlan et al. [2017] find that VSLAs are not sufficiently strong so as to generate significant effects on income and household expenditures in rural areas. Nevertheless, they find positive effects on non-agricultural businesses and women empowerment within the household. In contrast, Ksoll et al. [2016] is one of the few papers that finds statistically significant effects of VSLAs on household expenditures, meals consumed per day and number of rooms in the dwelling for Malawi.

Other studies have instead focused on the role of VSLAs on people's ability to become more resilient while facing multiple socioeconomic risks. Beaman et al. [2014] find that VSLAs in Mali generate positive effects on food safety, consumption smoothing, and precautionary savings. However, they did not find significant effects on income or expenditure, health, education, social capital, or women empowerment within the household. In turn, Breza and Chandrasekhar [2019] find an unintended consequence of the model when information about the progress on self-savings goals is shared with another village member: consumption is reduced in order to finance savings in the VSLAs.

A few recent papers attempt to highlight potential limitations in the performance and management of VSLAs that risk their sustainability over time. Le Polain et al. [2018], for instance, points to misperceptions and customary practices in savings groups in Congo. First, members mainly convert their savings into loans to avoid the risks of storing cash thus sacrificing the level of capital accumulation for the group. Also, the allocation of credit is not usually guided by credit performance, but rather by risk diversification and other more subjective criteria. Finally, VSLA's members often expect that accumulated paid interest should reward only active borrowers instead of passive savers. Cassidy and Fafchamps [2018] focuses instead on a relatively low heterogeneity among the members of the savings groups, mainly in terms of productive activity. This limits the possibility of intermediation between agricultural and non-agricultural households, thus increasing the relative concentration of common risks associated with the groups' main activity.

Studies about VSLAs are scarce for contexts other than African countries. Among the few papers related to self-help savings groups in the context of Latin America, Kast et al. [2018] find that efforts to encourage savings and deposits through self-help peer groups and text messaging increased savings rates and boosted saving patterns in Chile, mainly among low-income entrepreneurs and exclusively in a urban context. Our paper contributes to the literature by providing evidence about the effects of VSLAs in rural areas of Latin America. Beyond the standard measured outcomes related to poverty, consumption smoothing, and risk coping, this paper relies on unique adminis-

trative data that also allows us to contribute to the discussion on the role of VSLAs on financial inclusion and demand for formal financial services.

3 Context and Experimental Design

3.1 The Intervention

The Development Finance Corporation (COFIDE), a development bank in Peru, tries to support financial inclusion in rural areas of the country. In 2005, they created the Inclusive Program for Rural Business Development (PRIDER by its acronym in Spanish), which promotes the establishment of Village Savings and Loan Associations (VSLAs). The program aims to draw on the strength of interpersonal relationships in rural communities to foster the creation and sustained growth of these VSLAs. PRIDER has been operating in nine different regions of the country, most of them located in the Peruvian highlands.³

VSLAs, also known as savings groups or self-help groups, are associations created with the common goal of gradually accumulating savings that can be used as capital to meet the financial needs of its members. Under the PRIDER model, each savings group is constituted as a civic association made up of ten to thirty families from the same community. The group meets regularly to make savings contributions that are put together into a common fund. This fund is then used to support the economic activities and needs of their members through loans that pay an interest rate and thus provide a return on savings. Indeed, the operating rules require that all the funds collected should be completely lent out to members of the group at all times. The financial profit generated through lending is proportionally distributed at the end of each year depending on the individual level of savings.

PRIDER's central objective is to improve the living conditions of poor families in rural areas. Its main goal is to foster financial inclusion in an efficient and sustainable way, both to reduce the vulnerability of low-income households as well as to improve their income generating capacity. On top of the financial intermediation that is facilitated, their VSLA model offers additional support activities aimed at contributing to the integral development of families. For instance, they focus on restoring weak social ties and strengthening participants' self-esteem, facilitate access to agricultural inputs, offer management tools, and promote associativity through the savings groups and productive associations.

Once PRIDER identifies candidate villages to introduce the VSLA model, their officers gather the community's degree of interest and identify potential members. A VSLA is set up when officers identify at least ten people interested in becoming members. The group chooses its representatives

³PRIDER mainly operates in Lambayeque and Cajamarca but, in recent years, it has expanded its coverage into Ica, Piura, Loreto, Huanuco, Puno, San Martin, and Ayacucho.

who are then asked to attend three training sessions about savings, credit, and the management of the financial transactions of the group. The members of the newly constituted savings group choose a minimum required level of periodic savings. In the group's accounting records, individual deposits represent shares of the group's stock. Members also determine passive and active interest rates, the terms and the penalties for delayed payments, and internal rules. They designate a President, a Treasurer, and a Secretary, appointments that change periodically as a way to share knowledge within the group.

Each VSLA is self-managed. Once the group is operational, members hold monthly meetings to collect deposits, evaluate new loan requests, disburse new loans, and collect fees and interest on outstanding loans. Attendance is mandatory and absences are penalized. To retain members during a working cycle, additional penalties are imposed on those who decide to abandon the group before the end of the group's working year: the leaving member gets back the value of her share, but she does not receive the accumulated financial profit corresponding during the year.

3.2 Study Timeline

In 2014, PRIDER decided to expand its operations in Ayacucho, an impoverished department in the southern part of Peru. Their plan to boost the creation of VSLAs covered four provinces: Huamanga, Huanta, Cangallo, and Vilcashuaman. Eligible villages randomized into the treatment in these provinces were approached in three rounds between 2014 and 2016 with the goal of creating at least 120 savings groups. The first two provinces were served during 2014 and 2015 and the latter two were only contacted in 2016.

Figure 1 chronologically organizes the intervention activities (in bold) as well as the data collection activities (in italics). Between 2014 and 2016, PRIDER progressively implemented the VSLA program in the study area. Prior to each of the three recruiting phases, we collected baseline data in the control and treatment villages. Follow-up surveys were conducted on a rolling basis, two years after each round of implementation had ended.

3.3 Sample Selection and Randomization

Within the targeted provinces, PRIDER excluded villages that were extremely remote or with high risk of violence associated to local terrorist groups. Additionally, a village was eligible only if it was defined as rural, as per the 2007 Population and Housing Census, and recorded population size above 150. PRIDER pre-identified 260 villages that complied with these criteria and asked its field officers to validate their suitability for the expansion of the program.

The final sample consisted of 240 eligible villages, which were stratified by district. Within each strata, the treatment was randomized at the village level. We first randomized the treatment a

hundred times and measured the differences across control and treatment groups in terms of their pre-program characteristics at the village-level.⁴ We randomly chose one of the randomization trials among those that did not generate significant differences across groups. Table A.1 in the Appendix implements balancing test between the treatment and control groups at the village level. Figure A.1 in the Appendix shows the geographical locations of the villages included in the experiment by treatment arm and implementation round.

Tables A.2-A.3 in the Appendix provide basic descriptive statistics, as well as balancing tests of the randomization at household-level, relying on baseline survey records. Consistent with the random treatment assignment, we found few significant differences between both groups.

The average age of the sample is forty-two. Only 18% of the sample reported having at least full secondary while 82% said that the language most used at home was Quechua. Over 85% of the households report agricultural or livestock activities, although only 35% report selling at least part of their production in the market in exchange for money.⁵ On average, households in the sample spend twenty-seven hours per week working in the family farm unit.

The baseline survey also confirms low levels of access to formal financial services. Table A.4 shows that only 6% of the households in the sample had access to a formal loan in the twelve months prior to the baseline survey, while 25% report having access to informal loans. Likewise, 53% of the interviewees report that they frequently want to save more than their partners, while 33% report that they must frequently save their money separately so that it is not allocated to other expenses. Only 5% of the respondents declare to save money outside the household.

3.4 Data and Measurement

To conduct the baseline survey, we randomly chose ten households per village. This number was adjusted downwards in smaller villages. The sampling procedure in each village depended on the geographical extension and spatial distribution of dwellings. Since most of the villages did not have an updated cartography, the surveyor began the process in the town square of each village. She choose a block of houses on the edge of the town square and picked one as a starting point. The first house with willingness to respond becomes the first observation in that village. The team then skips three houses and moves clockwise until they find a second household willing to be surveyed. Once two houses were effectively surveyed in a block, the surveyor moves to the block on the opposed diagonal. This process continues until the limits of the village are reached. The team then returns to the town square to repeat the same process but going in the opposite direction. At the household level, the respondent is chosen among the head of the household and his/her partner so as to reach

⁴Using census data, we construct variables such as the existence of a health center, a secondary school, or any school of any educational level near the location, total population, percentage of households with adequate drainage service, percentage of households with electric lighting provided through a public network, and literacy rate.

⁵The difference is dedicated to self-consumption and bartering.

a given sex and age quota at the village level.⁶

The total number of households surveyed during the three rounds of baseline surveys is 2,369, with 1,169 in the treatment group and 1,200 in the control group. Table A.5 in the Appendix reports the number of records by survey round. The baseline survey covers topics such as characteristics of the respondent, characteristics of the household, employment and time use, non-agricultural business activities, agricultural and forestry activities, savings and credit history, business attitudes, level of trust and social networks, household vulnerability, and financial knowledge. This questionnaire also gathers basic information about other household members such as relationship with respondent, age, gender, and education level.

Two years after each baseline survey round was completed, we revisited the 240 villages in the experimental sample and retraced the sample of households originally surveyed. The follow up questionnaire was similar to the baseline instrument and targeted the same respondent who was initially interviewed.⁷ The final sample in the follow up survey consists of 1827 households (see Table A.5).

The average attrition level between surveys was modest: on average, less than a quarter of the households initially sampled were not reached two years after the baseline. However, the household re-contact rate in the treatment group was higher in treatment villages (81%) than at control ones (74%). Column 1 in Table A.6 confirms that attrition at the household level is differential by treatment status, even after controlling for other important criteria which are orthogonal to the treatment assignment. These differences in re-contact rates vanish when we focus on the last two rounds (see column 2 of Table A.6). At the individual level, attrition rates are not differential by treatment status neither in the full sample nor in the samples limited to rounds 2 and 3. To take into account potential biases due to differential attrition, all estimation results include dummies for each survey round in the set of controls.

A second source of data are credit bureau administrative records. We are able to match both our respondents as well as their partners through their national identification numbers. These data includes individual-monthly level information on outstanding debt at both regulated and unregulated formal financial institutions. Even though the latter are not really informal lenders, they tend to relax the minimum requirements to get a loan at the cost of higher interest rates when compared to banks [Campion et al., 2010]. For each loan, these data records pending loan balances, the type of loan, and its status. We set the months that are considered pre and post intervention

⁶The quota was established ex-ante so as to replicate the global distributions of sex and age in each village.

⁷Surveyors received a list including the full names of people surveyed at baseline, as well as those of their partner, and contact information (address, reference, telephone number of the person to be surveyed, and a person who could help to contact her). Upon reaching each village, surveyors contacted a key informant, either a local authority or someone from the community, to obtain detailed information about the location of the households to be surveyed. Once they identified a household's location, the surveyor explained the terms of confidentiality and read the informed consent to the respondent. Upon agreement, the surveyor proceeded to apply the questionnaire. The protocol considered up to three contact attempts. If no respondent was reached, the household was removed from the sample.

based on the date in which the first VSLA was created in the village. Loans with regulated institutions come from the Superintendency of Banks (SBS) during the period August 2012-April 2020. These records provide outstanding debt levels with banks and other supervised microfinance institutions. Loans obtained from unregulated financial institutions such as cooperatives and NGOs were obtained from two private credit bureaus that operate in Peru. Data for household heads comes from Sentinel and covers the periods August 2014 through August 2019. Partners' data instead comes from Equifax, covering the months between March 2015 and March 2020.

A third source of data are the transactional records of the VSLAs' operations between October 2014 and October 2018. These data provide information on savings and loans balances at the individual-monthly level. All variables measured in soles, both from credit bureau and VSLAs transactional records, are transformed to August 2014 dollars.

Aggregate records at the group level from PRIDER indicate that, by October 2018, 171 VSLA had been created in the targeted provinces and 159 of them were still working. These savings groups comprised 2,261 members, with 65% of female participation. On average, each savings group started out with a financial capital of 98.7 dollars, which increased by 5135% after four years of operation (see panel (a) in Figure 2). When a savings group is set up, the share of members who request a loan is low. This share increases over time and, on average, reaches 0.75 by the second year of operation (see panel (b) in Figure 2) as the group requires a larger share of borrowers to keep the resources lent at all times. As the savings deposited into the group increase, so does the average loan per member (see panels (c) and (d) in Figure 2).

4 Estimation Strategy

The sample of interest to evaluate the effectiveness of the program consists of all households with records in the follow-up survey, as they have data on the outcome variables two years after exposure to the intervention. To measure the effect of the savings groups, we estimate the intention to treat (ITT) from an OLS regression:

$$Y_{ij} = \alpha + \beta T_j + \gamma X_{ij} + \delta_j + \varepsilon_{ij} \quad (1)$$

where Y_{ij} is the outcome variable for household i located in village j . T_j is a dummy variable that equals one when the household belongs to a village selected to be supported by PRIDER to form a savings group. X_{ij} is a matrix that includes individual as well and village level characteristics, including the value of the outcome variable at baseline and dummies for each survey round. Implementation of an analysis of covariance (ANCOVA) to estimate the treatment effects leads to large improvements in power compared to a difference-in-difference specification [McKenzie, 2012]. The regression model includes fixed effects at the district level denoted by δ_j , while ε_{ij} is the error term.

We assume that the errors are independent between communities, but not within a community and thus implement the Huber-White correction at the village level.

Additionally, we also focus on the potential heterogeneous impacts of the intervention. To do so, we rely on an extended regression model as follows:

$$Y_{ij} = \alpha + \beta_0 T_j + \gamma X_{ij} + \theta x_{ij} + \beta_1 T_j x_{ij} + \delta_j + \varepsilon_{ij} \quad (2)$$

where x_{ij} is a dummy variable that equals one if household i or village j matches the characteristic under analysis and is zero otherwise. In this model, β_0 measures the ITT effect for those with $x_{ij} = 0$, while $(\beta_0 + \beta_1)$ will capture the effect among those with $x_{ij} = 1$. Note that characteristic x_{ij} will often be a subset of matrix X_{ij} .

Since the take up levels were not perfect (see Sub-Section 5.1), we also estimate the treatment-on-the-treated effect (TOT). Fortunately, access to PRIDER’s administrative records on the activities of the savings groups allows us to identify the participation status of each individual originally sampled in our baseline survey. To obtain the TOT effects, we instrument actual membership to a savings group with the original random assignment to the treatment (T_j). This estimator is obtained from a two-stage procedure, where the second stage implies:

$$Y_{ij} = \alpha + \omega \widehat{T}_{ij} + \gamma X_{ij} + \delta_j + \varepsilon_{ij} \quad (3)$$

\widehat{T}_{ij} is a dummy variable that equals one if someone in the household is (or used to be) a member of a savings group. The first stage assumes that affiliation to a savings group (\widehat{T}_{ij}) is greatly determined by PRIDER’s targeting strategy which selected villages at random. Thus, since T_j is exogenous to the preferences and abilities of individuals in treated localities, it is the perfect candidate to instrument \widehat{T}_{ij} :

$$\widehat{T}_{ij} = \tau_1 + \tau_2 T_j + \delta_j + \mu_{ij} \quad (4)$$

Thus, the coefficient ω in (3) measures TOT effects of the intervention. Although we mostly focus on the ITT effects, Appendix B reports the results for the TOT estimator (see Tables B.1-B.9).⁸

⁸As expected, the TOT estimator is approximated by dividing the ITT estimator by the take up rate.

5 Results

5.1 Take-up

The expansion of the program was deployed in three rounds, between September 2014 and January 2017 (see Section 3.2). The number of VSLAs created augmented in an exponential way within this period and then remained flat once PRIDER moved on to other areas. By January 2017, 171 savings groups with a total of 2176 members were working in 120 villages.

Our ability to appropriately measure the effect of savings groups depends on the take-up rate in our random survey sample. A low take-up rate in the surveyed sample imposes limitations to the statistical power of the study (see, for example, Bruhn and McKenzie [2009]). We measure participation in the VSLA both at the household and individual levels relying on PRIDER administrative records. Figure 3 shows that take-up rates in our sample follow a growth pattern similar to that of the creation of VSLAs, with a steep boost during the first months of group creation and posterior flattening of the growth pattern. Average take-up rates reach substantial levels in our sample: 14.3% of those interviewed at baseline joined a savings group and the take up rate rises significantly to 23.5% when we consider participation of any member of the household⁹.

Table 1 tries to assess if observable characteristics are good predictors of take-up rates at the household level. We estimate the probability of joining a VSLA as a function of household head characteristics as well other household-level variables (e.g., number of children and ratio of household members to bedrooms) as measured at baseline. Surprisingly, having a female head is the only relevant characteristic that can predict higher affiliation to the savings groups. The increase in take-up rates is about 12 percentage points when the household head is a woman, which is equivalent to an adoption rate 50% higher than the average at the household level.

Column 1 shows that other characteristics of the household head, such as her or his age, marital status, education, level of entrepreneurship and level of financial knowledge, as well as characteristics of the household such as language spoken at home, number of children or the ratio of household's members per bedroom, do not have a significant influence on participation in VSLAs. However, the second column shows that previous participation in village banks has a positive and large effect (8.6 percentage points) on the probability to join a savings group. This result reveals that previous experience or geographical exposure to similar group mechanisms encourages households to join a VSLA. It may also suggest that the VSLA methodology is offering additional and valued services relative to village banks. Prior to the arrival of PRIDER to the area, village banks may have been the best alternative available to cover agricultural households' financial needs. Once the VSLA model is offered, village banking clients choose to join a savings group, potentially overcoming the

⁹Related studies of VSLAs in Africa reported take up rates between 32-37% [Beaman et al., 2014; Karlan et al., 2017]. Ksoll et al. [2016] report a 45% take up rate in treatment areas of Northern Malawi, but including a 21% rate in control areas.

limited flexibility offered by the village banking model.

5.2 Treatment Impacts

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We first focus on the primary goal of the program, fostering access to financial services in rural areas. Relying on self-reported survey data, Table 2 shows that access to the savings groups does not lead to changes in savings balances. The treatment is also ineffective in changing savings attitudes: a large majority of the experimental sample manifests that they have trouble saving, but the treatment did not improve the situation. Access to VSLAs may foster access to formal savings products by bringing rural households closer to financial services. However, the probability to save formally, already low in the control group (5 percent), is not affected by the treatment.

Even though we fail to find treatment impacts on self-reported total saving *balances* (see Table 2), average contributions to the VSLAs increase over time (see panel c in Figure 2). The accumulation patterns observed in the VSLAs' administrative records suggest that households could be increasingly setting money aside through the group. The lack of an impact on saving balances could thus respond to a paired effect on spending or investing. Later in this sub-section we analyze household's expenditures and living conditions.

Table 3 focuses instead on the effect of the treatment on access to credit. Merging together monthly loan balances from the credit bureau records as well as from the VSLAs' transactional records, we can get a full credit profile for the households in our sample. Thus, we evaluate the impact of the treatment both on the probability of having a loan as well as on the amount owed. Since the data allow us to observe outstanding debt by lender, we further disaggregate treatment impacts by the source of credit.

In general, the probability to have a loan in the past 12 months and the level of monthly outstanding debt remained unchanged after being exposed to the treatment. On average, the probability to have a pending loan with the VSLA significantly increases one and two years after the beginning of the intervention. Although not significant, there is also an average negative effect on the probability to have a loan with a regulated financial institution. Similarly, the total level of debt is unaffected by the treatment, but monthly outstanding balances with VSLAs did significantly increase 12 and 24 months after exposure to the treatment. Monthly debt held with non-regulated lenders also increases substantially, but only two years after the launch of the program. Notice also that we estimate a negative treatment impact on the debt held with regulated lenders, but the point estimate is too noisy to become significant. All in all, the results in Table 3 suggest a modest shift away from bank and regulated microfinance institutions and towards loans from NGOs, cooperatives, and VSLAs.

Building on the results on take-up that showed that women are more likely to join a VSLA, Table 4 tests if the treatment also has differential effects on access to credit by sex, when looking at the credit outcomes 2 years after the intervention. Since the administrative records from the credit bureau and the VSLAs allow us to observe the full credit history of both survey respondents and his/her partner, we can pool the data at the individual level and add an interaction effect with sex to the model as in (2). Interestingly, we find a significant 4-percentage point increase in the probability to have a loan in the past 12 months among females.

Table 4 also identifies differential effects by sex in the probabilities to have a loan from non-regulated lenders and the VSLAs: women tend to move away from formal lenders and substitute their services through the VSLA. The second panel in Table 4 confirms this pattern, with females reducing their debt portfolio from regulated lenders and relying more on the VSLA. Men, instead, significantly increased the portfolio they kept with both non-regulated lenders and the VSLA.

The introduction of VSLAs may also have differential effects depending on the household's past credit history. For instance, providing access to the VSLA model to previously unbanked households can discourage them to engage with other formal financial institutions once their demands for financial services are satisfied. However, it is also possible that joining a VSLA allows these previously excluded households to develop good repayment habits that may make them more likely to approach and more attractive to formal lenders. Self-management of the savings groups may also allow individuals to absorb important financial concepts by participating in real-life financial decisions related to their own resources, which could help them overcome financial literacy and trust issues that restrict their demand for formal financial services.¹⁰

Table 5 presents heterogeneous treatment impacts by access to credit from regulated formal lenders, as measured during the 24 months prior to the launch of the intervention. In general, the treatment did not yield a significant change in the probability to have a loan in the past 12 months. However, access to regulated lenders is significantly reduced among those who were unbanked prior to the intervention. In terms of the amount of debt held, the second panel shows that these households are also more likely to become more reliant on non-regulated lenders and VSLAs, which supports a discouragement effect towards formal regulated lenders in the market.

The impact of the treatment may also differ depending on the level of development in the district. Differential levels of initial access to financial services and economic opportunities may yield heterogeneous effects of the access to VSLAs on financial inclusion patterns. For instance, the treatment may foster very limited access to formal sources of credit in poorer areas, where the financial sector is less developed. In richer areas, access to credit through the savings groups may push treated households to search for complementary funding for larger investments. Table 6 shows that the

¹⁰The creation of the savings groups implies a training component to the leaders on regular and sustainable management of the group's finances, including decisions on the amount of mandatory monthly savings, the interest rate paid for loans, selecting group members who will take out loans, among others. But even members who are not trained, participate in the meetings and take part of these choices.

effect on the probability to get a loan from other lenders and the amount of borrowing undertaken does not vary by the district’s poverty level. If anything, we observe that households in poorer areas tend to significantly increase their level of debt with non-regulated microfinance lenders and VSLAs.

All in all, access to VSLAs weakly expands access to credit among females but has no significant average global effects. Moreover, the treatment seems to discourage the bancarization of households that had no pre-treatment access to loans from formal regulated lenders. Average monthly debt does not seem to change one or two years after the delivery of the intervention. However, relative to the control group, treated households experience changes in their loan portfolio, with increased debt from NGOs, cooperatives, and VSLAs and reduced debt from bank and regulated microfinance institutions. This substitution effect is stronger among females and previously unbanked households.

POVERTY AND VULNERABILITY

As mentioned above, we fail to find an effect on saving balances a year after the intervention. However, lack of higher frequency data does not let us rule out that both savings deposits and withdrawals could increase due to the treatment, facilitating households’ consumption and or investment. Tables 7 and 8 present the estimated ITT effects on consumption spending, asset ownership and housing quality, and vulnerability.

Table 7 reports that there are no significant effects on household expenditures or asset ownership (cell phone, TV, radio or sound equipment, computer, bicycle or motorcycle). The lack of an impact on monetary poverty is in line with previous findings on the effect of savings groups in African countries such as Mali, Ghana, Malawi, and Uganda [Beaman et al., 2014; Karlan et al., 2017].¹¹.

In turn, savings groups do seem to foster quality-improving housing investments. Indeed, the effect on housing quality supports the idea that savings groups facilitate the accumulation of lump-sums of money required to make investments in indivisible projects such as those related to housing upgrading [Kaboski and Townsend, 2011]. Relative to the control group, treated households improve the quality of their houses by 0.12 S.D, which is mostly explained by increases in the likelihood to have good quality floors (4-percentage point) and roofs (2-percentage points). This effect seems to be more robust in poorer districts, but the last column of Table 7 rules out that there are significant differences by poverty level.

One of the central goals of financial inclusion efforts among impoverished households is the possibility to reduce their exposure to negative shocks. Indeed, access to VSLAs in rural Peru reduces households’ vulnerability as shown in Table 8: the treatment yields a 5-percentage point decrease

¹¹Ksoll et al. [2016] do find that VSLAs reduce poverty in Malawi when measured using the USAID Poverty Assessment Tool (PAT). The PAT which encompasses a list of twenty simple indicators that are chosen to *predict* household expenditure.

in the probability to face a negative shock in the past 12 months. This effect is almost entirely driven by households in poorer districts, who experience a 9.4-percentage point drop in the probability to experience a bad shock. The treatment is particularly effective to reduce the likelihood of events such as hunger, death, and job loss among poorer households, which highlights the program’s ability to protect households’ food safety and income flows. Our results are in line with Beaman et al. [2014], who show that savings groups led to significant improvements in food safety and consumption smoothing in Mali.

PRODUCTIVE ACTIVITIES AND LABOR MARKET PARTICIPATION

Access to savings and credit through the VSLAs can also have important effects on productive activities undertaken by the household both directly and indirectly. On one hand, the provision of cheap and adequate financial services may foster household’s investment in agricultural activities, with potential consequences on their participation in non-agricultural production. Additionally, the technical guidance on agricultural businesses provided by the program may also lead households to further specialize and shy away from other market activities, especially if the latter were performed by necessity, to complement family income. On the other hand, the social capital and networks developed within the group and across savings groups in the village may foster greater job stability and availability of job opportunities, both in the agricultural and non-agricultural sector. Interactions with the group may also have the added advantage of access to information about market opportunities, prices, technology, among others.

Table 9 presents the treatment impacts on the choice to participate in productive and non-productive activities. On average, access to VSLAs only yields a significant impact on the number of harvested crops. However, the results among poorer households exhibit an interesting pattern of specialization: the treatment reduces the probability that the household owns a non-agricultural business (with a corresponding drop in the number of businesses) while reducing the number of animals and crops held by the family unit. The reduction in the level of diversification among poorer households suggests that the intervention is providing them with the means to focus on activities for which they have a comparative advantage or an idiosyncratic preference.

Table 10 presents the effects of the treatment on the extensive and intensive margins of labor market participation. On average, we identify an increase in the probability to work of about 5-percentage points. Increased labor market participation is mostly driven by female household members in the treatment group, who are 6.5-percentage points more likely to engage in paid or unpaid work. This speaks about the program’s ability to contribute to female empowerment and is in line with the greater levels of take-up identified among women.

6 Conclusions

Reaching the rural poor with adequate savings and credit services has proven to be extremely difficult for formal financial institutions around the world, even after incorporating lessons from the technologies put forward by the microfinance movement. The rural poor often have to resort to (and many times prefer) informal mechanisms to handle their financial needs, including crop diversification, borrowing from friends and neighbors, saving cash at home, among others. Village banks and VSLAs are probably the most popular organized group efforts to promote financial inclusion among the rural poor. While both mechanisms are based on peer support and pressure, VSLAs have a more salient focus on savings-based services [Karlan et al., 2014].

This study provides estimates of the impact of VSLAs based on a clustered randomized evaluation conducted in rural Peru. We worked with COFIDE, the Peruvian Development Bank, that has been promoting savings groups in Peruvian rural communities for fifteen years, and took advantage of the expansion of their operations in Ayacucho between 2014 and 2016. Consistent with the evidence available for Africa, we find no effects on average income or expenditures. However, we find that the savings groups reduce exposure to idiosyncratic shocks and facilitate housing improvements. Households in treated villages increased the quality of their houses in 0.125 SD, particularly investing on their floors and roofs. Similarly, households in treated villages show a 5-percentage point decrease in the probability to face a negative shock in the past 12 months and that figure is twice as high for households in poorer districts. The results among poorer households point towards an interesting pattern of specialization in agricultural activities due to the treatment, with a reduction in the level of diversification. This suggests that the VSLAs provide rural households the means to focus on activities in which they have a comparative advantage or for which they have a specific preference. Savings groups also seem to favor women’s empowerment as women in treated villages increase their access to credit (mainly through loans from the savings group) and their labor market participation.

The introduction of VSLAs has no impact on the likelihood to use formal financial services. In fact, we observe that, among the unbanked, the promotion of VSLAs *discourages* access to loans from formal financial institutions and microfinance lenders. We interpret these results as an indication that access to an attractive and effective savings technology reduces the need for credit among our sample of poor rural households.

A key policy question would be whether these groups could serve as a platform to reach the rural poor with additional complementary support that can bring them above the poverty line and with a better connection to the formal financial system, considering the strengths of group-based interventions stated in Diaz-Martin et al. [2020]. Along those lines, it would be key to understand better the role of the external promoters of the VSLAs (in this case, COFIDE and the field officers).

It may be the case that these agents not only help solve coordination problems that facilitate the creation of the groups, but they also transmit financial knowledge to the group members or provide other types of non-financial services valued by the members. Understanding their role in facilitating the development of these group is fundamental to explain why these savings groups did not develop spontaneously and to predict their sustainability over time, once the direct role of the coordination agent fades away.

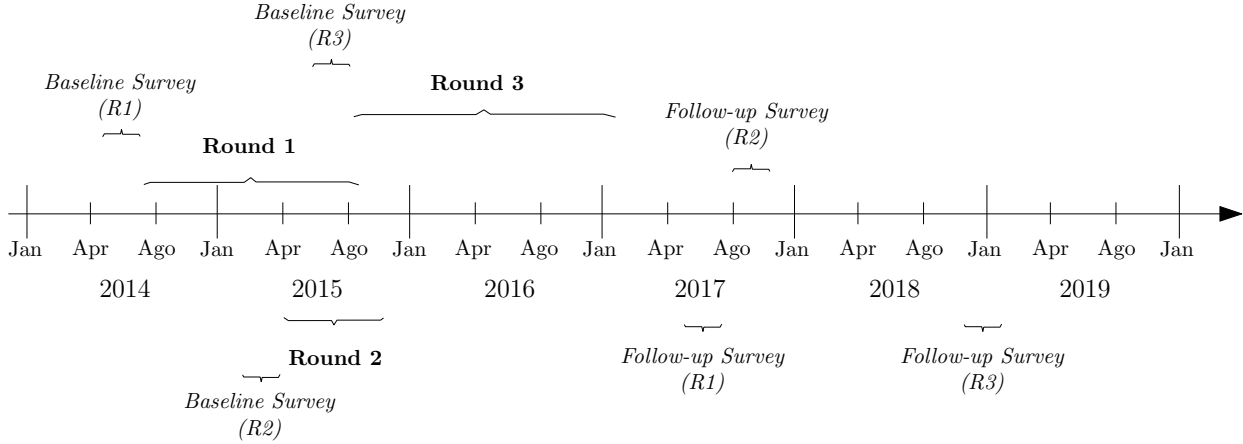
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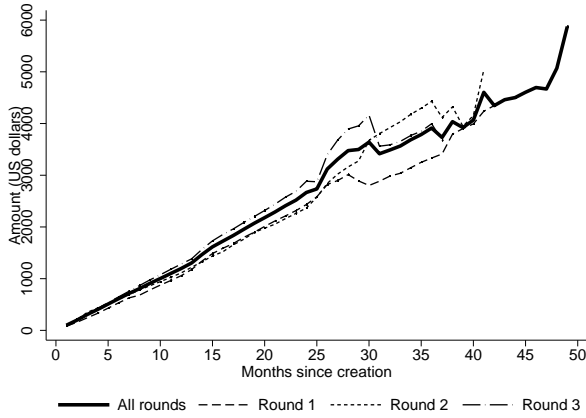
Figures and Tables

Figure 1: Study Timeline

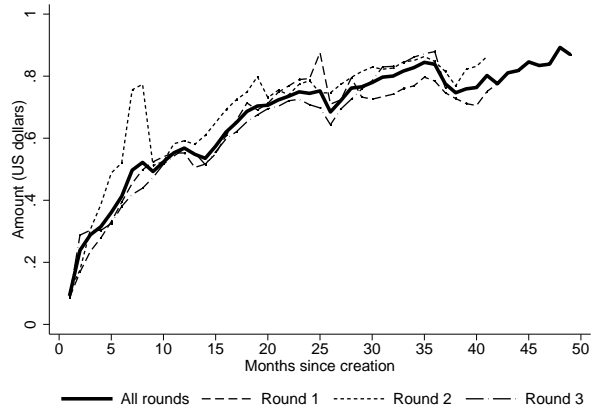


Note: Implementation activities in **bold** and data collection activities in *italics*.

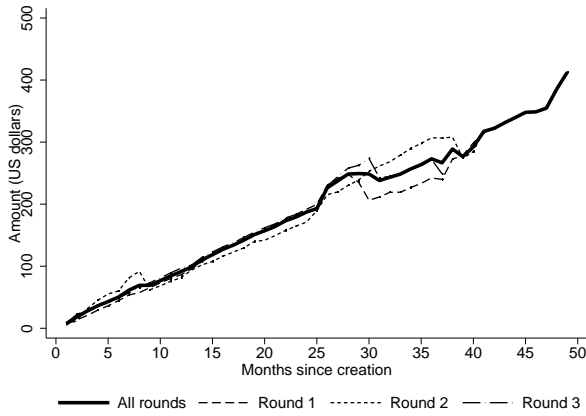
Figure 2: Monthly Evolution of VSLAs



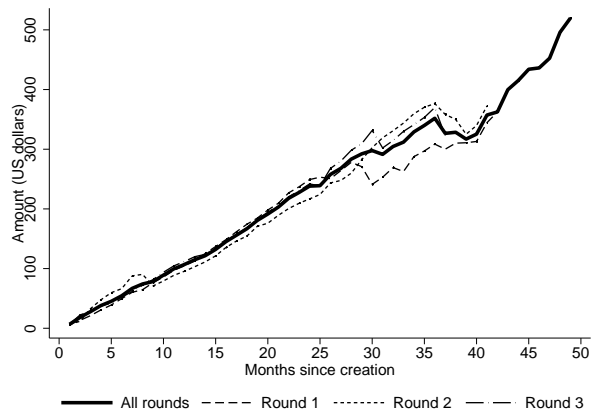
(a) Cumulative capital by months of life



(b) Percentage of clients with a loan



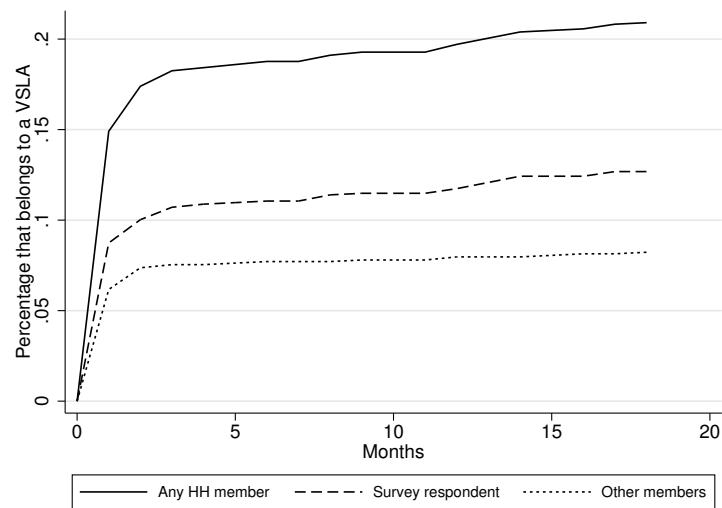
(c) Average contribution per member



(d) Average loans per member

Note: Panels (c) and (d) show the average of the accumulated amounts of contributions and loans granted divided by the number of members of the savings group, respectively. Amounts expressed in dollars using a fixed exchange rate from August 2014.

Figure 3: Percentage household members belonging a VSLA



Note: Accumulated percentage up to 18 months after the creation of the corresponding VSLAs.

Table 1: Determinants of affiliation to VSLAs

	Household	
	(1)	(2)
Age of household head	-0.002 (0.001)	-0.002* (0.001)
Gender of household head: Female	0.120** (0.051)	0.119** (0.050)
Marital status of household head: Married	0.060 (0.046)	0.061 (0.045)
Most spoken language at home: Quechua	0.016 (0.035)	0.019 (0.036)
Educational level of household head: without instruction	-0.008 (0.045)	-0.005 (0.044)
Entrepreneurship level of respondent	-0.012 (0.017)	-0.014 (0.017)
Level of financial knowledge of respondent	0.000 (0.013)	-0.001 (0.014)
Confidence level of respondent: in people	0.018 (0.015)	0.016 (0.015)
Confidence level of respondent: in friends	-0.005 (0.016)	-0.005 (0.016)
Confidence level of respondent: in acquaintances	0.009 (0.020)	0.010 (0.020)
Number of children	0.011 (0.010)	0.010 (0.010)
Ratio of household members to bedrooms	-0.008 (0.010)	-0.008 (0.010)
Wealth index		-0.005 (0.010)
Previous participation in village banks		0.086* (0.044)
R2	0.083	0.086
Mean dependent variable	0.229	0.229
Observations	1169	1169

Note: All regressions include village characteristics such as population size, literacy rate, households with drains, households with electrical energy, education center with secondary level or less, and health center, as well as district fixed effects, the value of the dependent variable level at baseline, and a dummy that indicates whether the same person answered the baseline and the follow-up survey. We also include a dummy that indicates if an observation has missing data at the village level. In addition, we include a dummy that controls whether or not the respondent is the head of the household. *** p<0.01,** p<0.05,* p<0.1.

Table 2: ITT Effects on Financial Inclusion and Participation

	Observations	Control	ITT
Current savings balance (USD)	1827	124.630 (17.319)	11.977 (22.159)
Saved formally	1827	0.055 (0.008)	0.002 (0.011)
Regrets spending money instead of saving it	1827	0.759 (0.014)	-0.020 (0.024)
Wants to save a little more	1827	0.719 (0.015)	0.005 (0.024)
Hide the money so others don't spend it	1827	0.647 (0.016)	-0.009 (0.024)

Note: All regressions include village characteristics such as population size, literacy rate, households with drains, households with electrical energy, education center with secondary level or less, and health center, as well as district fixed effects, the value of the dependent variable level at baseline, and a dummy that indicates whether the same person answered the baseline and the follow-up survey. We also include a dummy that indicates if an observation has missing data at the village level. Regressions also includes controls for rounds. Clustered errors at the village level in parentheses. *** p<0.01,** p<0.05,* p<0.1.

Table 3: ITT Effects on Access to Credit

	Observations	Control	1 year	2 years
Had a loan in the past 12 months	1999	0.464 (0.016)	0.023 (0.017)	0.006 (0.019)
Regulated lender	1999	0.389 (0.015)	-0.017 (0.013)	-0.023 (0.017)
Non-regulated lender	1999	0.145 (0.012)	0.002 (0.013)	0.005 (0.014)
VSLA	1999	0.000 (0.007)	0.102*** (0.013)	0.123*** (0.014)
Outstanding Monthly Debt (USD)	1999	1152.706 (88.573)	56.292 (81.406)	19.359 (104.905)
Regulated lender (USD)	1999	879.245 (71.968)	33.908 (63.609)	-86.844 (82.121)
Non-regulated lender (USD)	1999	207.220 (32.652)	10.646 (32.864)	92.123** (36.168)
VSLA (USD)	1999	0.000 (1.049)	7.707*** (1.270)	11.171*** (1.811)

Note: Mean of the dependent variable in the control group calculated for the second year after the intervention. Debt amounts expressed in dollars using a fixed exchange rate from August 2014, and winsorised at the 1% and 99% levels. All regressions include village characteristics such as population size, literacy rate, households with drains, households with electrical energy, education center with secondary level or less, and health center, as well as district fixed effects, and the value of the dependent variable level at baseline. We also include a dummy that indicates if an observation has missing data at the village level. Regressions also includes controls for rounds. Clustered errors at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: ITT Effects on Individual Access to Credit After Two Years, by Sex

	Observations	Control	Sex		
			Male	Female	P-value
Had a loan in the past 12 months	3585	0.299 (0.011)	0.006 (0.019)	0.042** (0.020)	0.147
Regulated lender	3585	0.239 (0.010)	-0.018 (0.018)	-0.007 (0.014)	0.612
Non-regulated lender	3585	0.087 (0.007)	0.017 (0.011)	-0.017 (0.013)	0.039
VSLA	3585	0.000 (0.005)	0.064*** (0.010)	0.094*** (0.012)	0.007
Outstanding Monthly Debt (USD)	3585	625.566 (44.411)	43.422 (86.851)	-47.946 (57.004)	0.371
Regulated lender (USD)	3585	468.838 (35.901)	-16.181 (71.758)	-82.633** (41.595)	0.428
Non-regulated lender (USD)	3585	98.848 (14.350)	58.232** (22.567)	25.541 (21.566)	0.268
VSLA (USD)	3585	0.000 (0.327)	3.246*** (0.825)	4.308*** (0.713)	0.261

Note: Mean of the dependent variable in the control group calculated for the second year after the intervention. Debt amounts expressed in dollars using a fixed exchange rate from August 2014, and winsorised at the 1% and 99% levels. All regressions include village characteristics such as population size, literacy rate, households with drains, households with electrical energy, education center with secondary level or less, and health center, as well as district fixed effects, and the value of the dependent variable level at baseline. We also include a dummy that indicates if an observation has missing data at the village level. Regressions also includes controls for rounds. Clustered errors at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: ITT Effects on Access to Credit After Two Years, by Household's Previous Access to Credit from Regulated Lenders

	Observations	Control	Pre-Treatment Access to Regulated Lenders		
			Access	No access	P-value
Had a loan in the past 12 months	1999	0.464 (0.016)	0.008 (0.026)	0.007 (0.026)	0.972
Regulated lender	1999	0.389 (0.015)	0.014 (0.030)	-0.045** (0.021)	0.117
Non-regulated lender	1999	0.145 (0.012)	0.038 (0.029)	-0.014 (0.014)	0.108
VSLA	1999	-0.000 (0.007)	0.152*** (0.022)	0.106*** (0.015)	0.041
Outstanding Monthly Debt (USD)	1999	1152.706 (88.573)	-49.261 (260.984)	55.130 (77.708)	0.709
Regulated lender (USD)	1999	879.245 (71.968)	-142.879 (205.769)	-49.897 (61.562)	0.673
Non-regulated lender (USD)	1999	207.220 (32.652)	119.711 (78.970)	75.135** (37.163)	0.619
VSLA (USD)	1999	-0.000 (1.049)	17.137*** (3.114)	7.639*** (1.835)	0.004

Note: Mean of the dependent variable in the control group calculated for the second year after the intervention. Debt amounts expressed in dollars using a fixed exchange rate from August 2014, and winsorised at the 1% and 99% levels. All regressions include village characteristics such as population size, literacy rate, households with drains, households with electrical energy, education center with secondary level or less, and health center, as well as district fixed effects, and the value of the dependent variable level at baseline. We also include a dummy that indicates if an observation has missing data at the village level. Regressions also includes controls for rounds. Clustered errors at the village level in parentheses. *** p<0.01,** p<0.05,* p<0.1.

Table 6: ITT Effects on Access to Credit After Two Years, by District Poverty

	Observations	Control	Level of Poverty		
			Low	High	P-value
Had a loan in the past 12 months	1999	0.464 (0.016)	0.017 (0.029)	-0.001 (0.025)	0.641
Regulated lender	1999	0.389 (0.015)	-0.025 (0.027)	-0.022 (0.023)	0.917
Non-regulated lender	1999	0.145 (0.012)	0.012 (0.025)	0.001 (0.018)	0.722
VSLA	1999	-0.000 (0.007)	0.141*** (0.023)	0.112*** (0.018)	0.321
Outstanding Monthly Debt (USD)	1999	1152.706 (88.573)	27.318 (182.867)	14.209 (122.863)	0.932
Regulated lender (USD)	1999	879.245 (71.968)	-82.864 (143.189)	-89.415 (100.754)	0.989
Non-regulated lender (USD)	1999	207.220 (32.652)	102.736 (64.051)	85.238* (43.639)	0.798
VSLA (USD)	1999	-0.000 (1.049)	14.704*** (3.101)	8.882*** (2.249)	0.141

Note: Mean of the dependent variable in the control group calculated for the second year after the intervention. Debt amounts expressed in dollars using a fixed exchange rate from August 2014, and winsorised at the 1% and 99% levels. All regressions include village characteristics such as population size, literacy rate, households with drains, households with electrical energy, education center with secondary level or less, and health center, as well as district fixed effects, and the value of the dependent variable level at baseline. We also include a dummy that indicates if an observation has missing data at the village level. Regressions also includes controls for rounds. Clustered errors at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: ITT Effects on Living Conditions by District's Level of Poverty

	Observations	Control	Average	Level of poverty		
				Low	High	P-value
Monthly expenditure (log)	1827	5.632 (0.042)	-0.023 (0.063)	-0.019 (0.099)	-0.026 (0.088)	0.962
Food expenditure (log)	1827	4.723 (0.096)	-0.137 (0.166)	-0.078 (0.258)	-0.176 (0.230)	0.785
Other expenditure (log)	1827	4.225 (0.051)	-0.050 (0.087)	-0.059 (0.135)	-0.043 (0.122)	0.934
Household assets index	1827	-0.000 (0.036)	0.013 (0.048)	0.110 (0.090)	-0.050 (0.059)	0.155
Housing quality index	1827	-0.000 (0.043)	0.125** (0.054)	0.149 (0.106)	0.110* (0.060)	0.751
High quality material in walls	1827	0.043 (0.008)	0.011 (0.010)	0.014 (0.021)	0.009 (0.011)	0.850
High quality material in floor	1827	0.106 (0.011)	0.041** (0.017)	0.043 (0.032)	0.039* (0.021)	0.927
High quality material in roof	1827	0.021 (0.007)	0.018** (0.009)	0.027 (0.017)	0.012 (0.010)	0.474

Note: High quality material in walls: brick, cement, or stone mixed with lime or cement. High quality material in floors: parquet, wood, cement, tiles, or asphalt sheets. High quality material in roofs: reinforced concrete. All regressions include village characteristics such as population size, literacy rate, households with drains, households with electrical energy, education center with secondary level or less, and health center, as well as district fixed effects, the value of the dependent variable level at baseline, and a dummy that indicates whether the same person answered the baseline and the follow-up survey. We also include a dummy that indicates if an observation has missing data at the village level. Regressions also includes controls for rounds. Clustered errors at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: ITT Effects on Household Vulnerability by District's Level of Poverty

	Observations	Control	Average	Level of poverty		
				Low	High	P-value
Experienced issue during past 12 months	1827	0.861 (0.012)	-0.050* (0.028)	0.017 (0.043)	-0.094** (0.039)	0.071
Hunger	1827	0.369 (0.016)	-0.038 (0.028)	0.037 (0.049)	-0.088** (0.037)	0.051
Disease	1827	0.420 (0.016)	-0.036 (0.027)	-0.020 (0.044)	-0.047 (0.036)	0.644
Death	1827	0.104 (0.009)	-0.047*** (0.016)	-0.054* (0.028)	-0.043** (0.019)	0.750
Job loss	1827	0.424 (0.017)	-0.047 (0.031)	-0.005 (0.053)	-0.074* (0.040)	0.321
Theft	1827	0.112 (0.011)	0.005 (0.016)	-0.004 (0.029)	0.012 (0.018)	0.631
Damage or loss at home	1827	0.201 (0.013)	-0.021 (0.022)	-0.063* (0.036)	0.006 (0.030)	0.151
Labor shortage	1827	0.393 (0.016)	-0.026 (0.033)	0.000 (0.055)	-0.043 (0.045)	0.551
Crop loss	1827	0.654 (0.016)	-0.015 (0.033)	0.017 (0.055)	-0.036 (0.044)	0.476
Livestock loss	1827	0.314 (0.015)	-0.028 (0.026)	-0.043 (0.043)	-0.017 (0.035)	0.650

Note: All regressions include village characteristics such as population size, literacy rate, households with drains, households with electrical energy, education center with secondary level or less, and health center, as well as district fixed effects, the value of the dependent variable level at baseline, and a dummy that indicates whether the same person answered the baseline and the follow-up survey. We also include a dummy that indicates if an observation has missing data at the village level. Regressions also includes controls for rounds. Clustered errors at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: ITT Effects on Productive Activities by District's Level of Poverty

	Observations	Control	Average	Level of poverty		
				Low	High	P-value
Owns a non-agricultural business	1827	0.062 (0.008)	0.001 (0.013)	0.040 (0.025)	-0.025* (0.014)	0.031
Number of non-agricultural business	1827	0.068 (0.010)	-0.006 (0.015)	0.025 (0.030)	-0.027** (0.014)	0.115
Number of harvested crops	1827	1.599 (0.060)	-0.220* (0.117)	-0.035 (0.175)	-0.340** (0.157)	0.198
Sells harvested crops	1827	0.258 (0.015)	0.014 (0.026)	0.023 (0.042)	0.007 (0.035)	0.775
Number of animals	1827	2.041 (0.189)	-0.429 (0.284)	0.374 (0.359)	-0.954** (0.445)	0.033

Note: All regressions include village characteristics such as population size, literacy rate, households with drains, households with electrical energy, education center with secondary level or less, and health center, as well as district fixed effects, the value of the dependent variable level at baseline, and a dummy that indicates whether the same person answered the baseline and the follow-up survey. We also include a dummy that indicates if an observation has missing data at the village level. Regressions also includes controls for rounds. Clustered errors at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

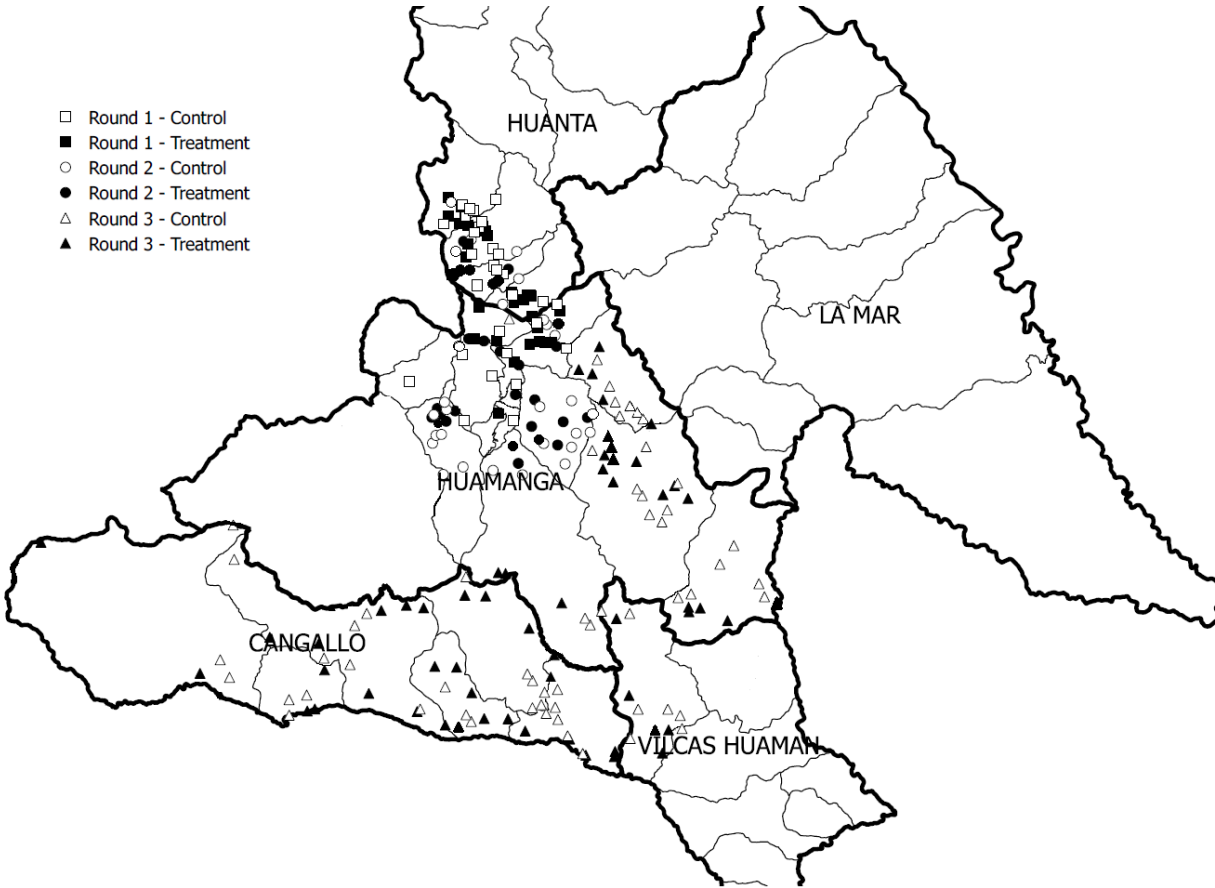
Table 10: ITT Effects on the Extensive and Intensive Margins of Labor Market Participation, by Sex

	Observations	Control	Average	Sex		
				Male	Female	P-value
Has paid/unpaid work	1827	0.706 (0.015)	0.049** (0.024)	0.028 (0.030)	0.065** (0.032)	0.357
Agricultural and livestock	1827	0.608 (0.016)	0.033 (0.027)	0.004 (0.034)	0.056 (0.035)	0.242
Household business	1827	0.120 (0.011)	-0.009 (0.014)	-0.033 (0.022)	0.008 (0.020)	0.206
Dependent	1827	0.133 (0.012)	0.005 (0.016)	0.002 (0.027)	0.008 (0.021)	0.882
Hours dedicated to paid/unpaid work	1827	28.772 (0.927)	1.072 (1.500)	-1.102 (1.963)	2.659 (1.895)	0.128
Agricultural and livestock	1827	21.634 (0.763)	0.914 (1.306)	-0.002 (1.749)	1.639 (1.564)	0.418
Household business	1827	3.114 (0.416)	-0.211 (0.485)	-1.013 (0.743)	0.338 (0.727)	0.227
Dependent	1827	4.024 (0.429)	0.298 (0.588)	-0.080 (1.007)	0.574 (0.746)	0.611

Note: All regressions include village characteristics such as population size, literacy rate, households with drains, households with electrical energy, education center with secondary level or less, and health center, as well as district fixed effects, the value of the dependent variable level at baseline, and a dummy that indicates whether the same person answered the baseline and the follow-up survey. We also include a dummy that indicates if an observation has missing data at the village level. Regressions also includes controls for rounds. Clustered errors at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

A Additional Figures and Tables

Figure A.1: Villages included in the experiment



Note: The map represents the treated and control villages in the department of Ayacucho, including provinces Huanta, Huamanga, Cangallo and Vilcas Huaman. The black filled shapes represent the treated villages, while the unfilled shapes represent the control villages. The squares represents the first round; circles are the second round and triangles are the third round.

Table A.1: Villages randomization balance check

Variable	Control mean	T-C	N
Total population	236.838 [133.501]	-6.312 [17.311]	235
Percentage of households with adequate drainage service	0.692 [0.249]	0.007 [0.035]	235
Percentage of households that have electricity by public grid	0.358 [0.327]	0.027 [0.029]	235
Literacy rate	0.711 [0.075]	0.011 [0.008]	235
There is any health facility	0.171 [0.400]	0.016 [0.047]	235
There is a full equipped hospital	0.000 [0.000]	0.000 [0.000]	235
There is a health center	0.000 [0.000]	0.000 [0.000]	235
There is a posta	0.171 [0.400]	0.016 [0.047]	235
There is a school of any educational level	0.692 [0.464]	-0.031 [0.064]	235
There is a school of early childhood educational level	0.265 [0.443]	-0.011 [0.057]	235
There is a school of primary educational level	0.598 [0.492]	-0.056 [0.058]	235
There is a school of secondary educational level	0.137 [0.345]	-0.001 [0.042]	235

Note: Significance levels (* 10%; ** 5%; *** 1%) captured through OLS estimation accounting for clustered (district) standard errors. Standard errors (deviations) of coefficients (control means) are in brackets.

Table A.2: Balance - Individual characteristics and productive activities

Variable	Control mean	T-C	N
Age	41.577 [12.501]	0.251 [0.584]	2369
Education: At least secondary education	0.190 [0.392]	-0.017 [0.020]	2369
Quechua as most used language at home	0.823 [0.382]	-0.003 [0.028]	2369
Marital status: married or cohabitant	0.830 [0.376]	0.025 [0.016]	2369
Housing quality index (floor, ceiling, wall)	0.000 [1.000]	0.125 [0.085]	2369
Asset Index	0.000 [1.000]	0.059 [0.057]	2369
Family farm unit	26.188 [21.024]	1.901 [1.390]	2369
Family business	4.184 [14.490]	-0.164 [0.702]	2369
Dependent work	8.851 [16.275]	-1.237 [1.010]	2369
Domestic activities	15.132 [13.739]	0.467 [0.756]	2369
Has a non-agricultural family business	0.165 [0.371]	0.029 [0.024]	2369
Agricultural	0.859 [0.348]	0.053 [0.025]**	2369
Livestock	0.742 [0.438]	0.041 [0.029]	2369
Forestry	0.052 [0.221]	0.001 [0.016]	2369
Sell at least part of the agricultural crop	0.350 [0.477]	0.017 [0.040]	2369

Note: Significance levels (* 10%; ** 5%; *** 1%) captured through OLS estimation accounting for clustered (village) standard errors. Standard errors (deviations) of coefficients (control means) are in brackets.

Table A.3: Balance - Business attitudes, social capital and household vulnerability

Variable	Control mean	T-C	N
Entrepreneurship index	0.000 [1.000]	0.081 [0.068]	2369
Financial knowledge index	0.000 [1.000]	0.008 [0.056]	2369
Confidence index	0.000 [1.000]	0.131 [0.062]**	2369
In people, in general	-0.000 [1.000]	0.095 [0.060]	2369
In friends	0.000 [1.000]	0.113 [0.061]*	2369
Among his acquaintances	-0.000 [1.000]	0.128 [0.058]**	2369
Problems experienced at home in the last year (No.)	1.759 [1.709]	0.085 [0.120]	2369
Long illnesses or death	0.271 [0.445]	0.003 [0.023]	2369
Job loss	0.178 [0.383]	0.013 [0.026]	2369
Loss of crops or livestock	0.558 [0.497]	0.034 [0.033]	2369
Others	0.410 [0.492]	0.012 [0.031]	2369

Note: Significance levels (* 10%; ** 5%; *** 1%) captured through OLS estimation accounting for clustered (village) standard errors. Standard errors (deviations) of coefficients (control means) are in brackets.

Table A.4: Balance - Attitudes towards savings and credit history

Variable	Control mean	T-C	N
Spends it	0.541 [0.499]	0.007 [0.026]	2369
Saves it	0.384 [0.487]	0.003 [0.028]	2369
Frequently saves after expenses	0.545 [0.498]	0.024 [0.028]	2369
Frequently regrets spending	0.688 [0.463]	0.023 [0.026]	2369
Frequently wants to save more than his/her partner	0.538 [0.499]	0.018 [0.033]	2369
Frequently saves his/her money separately	0.335 [0.472]	0.048 [0.027]*	2369
Hides it inside the home	0.298 [0.458]	0.027 [0.028]	2369
Keep it out of the home	0.052 [0.221]	0.018 [0.013]	2369
At least one loan was approved	0.254 [0.436]	0.049 [0.027]*	2369
Credit source: formal	0.061 [0.239]	-0.002 [0.011]	2369
Credit source: informal	0.254 [0.436]	0.049 [0.027]*	2369

Note: Significance levels (* 10%; ** 5%; *** 1%) captured through OLS estimation accounting for clustered (village) standard errors. Standard errors (deviations) of coefficients (control means) are in brackets.

Table A.5: Number of individuals at baseline and follow-up

		Baseline			Follow-up		
		Treatment	Control	Total	Treatment	Control	Total
Round 1	Individuals	278	316	594	213	180	393
	<i>Villages</i>	<i>31</i>	<i>31</i>	<i>62</i>	<i>31</i>	<i>30</i>	<i>61</i>
Round 2	Individuals	281	260	541	235	216	451
	<i>Villages</i>	<i>27</i>	<i>27</i>	<i>54</i>	<i>27</i>	<i>27</i>	<i>54</i>
Round 3	Individuals	610	624	1234	494	489	983
	<i>Villages</i>	<i>62</i>	<i>62</i>	<i>124</i>	<i>62</i>	<i>61</i>	<i>123</i>
Total number of surveys		1169	1200	2369	942	885	1827
<i>Total number of villages</i>		<i>120</i>	<i>120</i>	<i>240</i>	<i>120</i>	<i>118</i>	<i>238</i>

Note: The two villages not reported in the end-line were lost due to access problems.

Table A.6: Attrition level at follow-up by treatment status

	Household		Respondent	
	Full sample (1)	Rounds 2 & 3 (2)	Full sample (3)	Rounds 2 & 3 (4)
Treatment	0.058** (0.023)	0.002 (0.023)	0.025 (0.023)	-0.015 (0.025)
R-squared	0.056	0.048	0.068	0.060
Mean in control	0.738	0.798	0.564	0.613
Observations	2369	1775	2369	1775

Note: All regressions include the census characteristics of each populated center used in randomization as controls and district fixed effects. We also include a dummy to control whether or not an observation has missing data for its characteristics at the village level. The variables in columns (1) and (2) correspond to the socioeconomic characteristics of the head of household. We include age, gender, marital status, most spoken language at home, educational level, entrepreneurship level, level of financial knowledge, confidence level in people, friends and acquaintances, and participation in savings groups as controls. Columns (2) and (4) include only those surveyed in round 2 and 3. Errors clustered at the community level. *** p<0.01, ** p<0.05, * p<0.1.

B Appendix: TOT Effects

Table B.1: TOT Effects on Financial Inclusion and Participation

	Observations	Control	TOT
Current savings balance (USD)	1827	132.787 (13.004)	62.319 (112.737)
Saved formally	1827	0.059 (0.006)	0.008 (0.057)
Regrets spending money instead of saving it	1827	0.752 (0.011)	-0.102 (0.125)
Wants to save a little more	1827	0.728 (0.011)	0.024 (0.124)
Hide the money so others don't spend it	1827	0.649 (0.012)	-0.045 (0.126)

Note: All regressions include village characteristics such as population size, literacy rate, households with drains, households with electrical energy, education center with secondary level or less, and health center, as well as district fixed effects, the value of the dependent variable level at baseline, and a dummy that indicates whether the same person answered the baseline and the follow-up survey. We also include a dummy that indicates if an observation has missing data at the village level. Clustered errors at the village level in parentheses. Regressions also includes controls for rounds. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.2: TOT Effects on Access to Credit

	Observations	Control	TOT	
			1 year	2 years
Had a loan in the past 12 months	1999	0.450 (0.012)	0.123 (0.085)	0.032 (0.100)
Regulated lender	1999	0.380 (0.012)	-0.091 (0.072)	-0.125 (0.092)
Non-regulated lender	1999	0.156 (0.009)	0.011 (0.069)	0.027 (0.077)
VSLA	1999	-0.000 (0.004)	0.557*** (0.050)	0.672*** (0.052)
Outstanding Monthly Debt (USD)	1999	1199.148 (66.875)	303.577 (438.721)	104.400 (560.626)
Regulated lender (USD)	1999	913.751 (54.322)	182.672 (341.282)	-467.856 (437.394)
Non-regulated lender (USD)	1999	230.492 (24.648)	57.434 (176.065)	497.004** (206.890)
VSLA (USD)	1999	-0.000 (0.711)	41.793*** (5.875)	60.580*** (8.065)

Note: Mean of the dependent variable in the control group calculated for the second year after the intervention. Debt amounts expressed in dollars using a fixed exchange rate from August 2014, and winsorised at the 1% and 99% levels. All regressions include village characteristics such as population size, literacy rate, households with drains, households with electrical energy, education center with secondary level or less, and health center, as well as district fixed effects, and the value of the dependent variable level at baseline. We also include a dummy that indicates if an observation has missing data at the village level. Regressions also includes controls for rounds. Clustered errors at the village level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.3: TOT Effects on Individual Access to Credit After Two Years, by Sex

	Observations	Control	Sex		
			Male	Female	P-value
Had a loan in the past 12 months	3585	0.294 (0.008)	0.038 (0.104)	0.234** (0.100)	0.133
Regulated lender	3585	0.238 (0.008)	-0.099 (0.099)	-0.038 (0.076)	0.602
Non-regulated lender	3585	0.092 (0.005)	0.097 (0.065)	-0.094 (0.072)	0.048
VSLA	3585	-0.000 (0.003)	0.367*** (0.045)	0.524*** (0.050)	0.009
Outstanding Monthly Debt (USD)	3585	643.031 (33.431)	240.481 (486.300)	-258.469 (313.625)	0.377
Regulated lender (USD)	3585	481.212 (27.019)	-95.608 (397.565)	-454.366* (237.375)	0.441
Non-regulated lender (USD)	3585	110.966 (10.802)	328.115** (133.571)	145.337 (120.829)	0.263
VSLA (USD)	3585	0.000 (0.230)	18.561*** (4.093)	23.912*** (3.565)	0.302

Note: Mean of the dependent variable in the control group calculated for the second year after the intervention. Debt amounts expressed in dollars using a fixed exchange rate from August 2014, and winsorised at the 1% and 99% levels. All regressions include village characteristics such as population size, literacy rate, households with drains, households with electrical energy, education center with secondary level or less, and health center, as well as district fixed effects, and the value of the dependent variable level at baseline. We also include a dummy that indicates if an observation has missing data at the village level. Regressions also includes controls for rounds. Clustered errors at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.4: TOT Effects on Access to Credit After Two Years, by Household's Previous Access to Credit from Regulated Lenders

	Observations	Control	Pre-Treatment Access to Regulated Lenders		
			Access	No access	P-value
Had a loan in the past 12 months	1999	0.450 (0.012)	0.034 (0.106)	0.044 (0.161)	0.959
Regulated lender	1999	0.380 (0.012)	0.056 (0.126)	-0.282** (0.136)	0.073
Non-regulated lender	1999	0.156 (0.009)	0.159 (0.124)	-0.087 (0.091)	0.102
VSLA	1999	0.000 (0.004)	0.646*** (0.058)	0.694*** (0.077)	0.595
Outstanding Monthly Debt (USD)	1999	1199.148 (66.875)	-202.022 (1080.343)	343.743 (483.800)	0.649
Regulated lender (USD)	1999	913.751 (54.322)	-597.827 (846.461)	-326.014 (384.771)	0.774
Non-regulated lender (USD)	1999	230.492 (24.648)	507.499 (341.963)	482.919** (239.562)	0.952
VSLA (USD)	1999	-0.000 (0.711)	72.402*** (10.552)	49.966*** (10.321)	0.103

Note: Mean of the dependent variable in the control group calculated for the second year after the intervention. Debt amounts expressed in dollars using a fixed exchange rate from August 2014, and winsorised at the 1% and 99% levels. All regressions include village characteristics such as population size, literacy rate, households with drains, households with electrical energy, education center with secondary level or less, and health center, as well as district fixed effects, and the value of the dependent variable level at baseline. We also include a dummy that indicates if an observation has missing data at the village level. Regressions also includes controls for rounds. Clustered errors at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.5: TOT Effects on Access to Credit After Two Years, by District Poverty

	Observations	Control	Level of Poverty		
			Low	High	P-value
Had a loan in the past 12 months	1999	0.450 (0.012)	0.092 (0.154)	-0.003 (0.126)	0.624
Regulated lender	1999	0.380 (0.012)	-0.144 (0.150)	-0.114 (0.115)	0.875
Non-regulated lender	1999	0.156 (0.009)	0.064 (0.136)	0.005 (0.091)	0.710
VSLA	1999	0.000 (0.004)	0.795*** (0.111)	0.599*** (0.054)	0.111
Outstanding Monthly Debt (USD)	1999	1199.148 (66.875)	151.725 (992.555)	76.736 (620.736)	0.947
Regulated lender (USD)	1999	913.751 (54.322)	-468.186 (767.219)	-467.662 (506.936)	1.000
Non-regulated lender (USD)	1999	230.492 (24.648)	578.249 (373.823)	449.399* (231.928)	0.762
VSLA (USD)	1999	-0.000 (0.711)	82.260*** (16.955)	47.782*** (9.057)	0.074

Note: Mean of the dependent variable in the control group calculated for the second year after the intervention. Debt amounts expressed in dollars using a fixed exchange rate from August 2014, and winsorised at the 1% and 99% levels. All regressions include village characteristics such as population size, literacy rate, households with drains, households with electrical energy, education center with secondary level or less, and health center, as well as district fixed effects, and the value of the dependent variable level at baseline. We also include a dummy that indicates if an observation has missing data at the village level. Regressions also includes controls for rounds. Clustered errors at the village level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.6: TOT Effects on Living Conditions by District's Level of Poverty

	Observations	Control	TOT	Level of Poverty		
				Low	High	P-value
Monthly expenditure (log)	1827	5.624 (0.031)	-0.120 (0.326)	-0.108 (0.521)	-0.127 (0.418)	0.978
Food expenditure (log)	1827	4.647 (0.072)	-0.713 (0.869)	-0.457 (1.371)	-0.861 (1.119)	0.819
Other expenditure (log)	1827	4.208 (0.038)	-0.258 (0.445)	-0.327 (0.712)	-0.219 (0.577)	0.907
Household assets index	1827	0.027 (0.027)	0.067 (0.246)	0.574 (0.483)	-0.225 (0.281)	0.154
Housing quality index	1827	0.110 (0.032)	0.658** (0.297)	0.833 (0.612)	0.559* (0.303)	0.684
Quality material in walls	1827	0.060 (0.006)	0.058 (0.054)	0.077 (0.113)	0.048 (0.052)	0.810
Quality material in floor	1827	0.122 (0.008)	0.211** (0.091)	0.237 (0.172)	0.196* (0.103)	0.837
Quality material in roof	1827	0.040 (0.005)	0.094** (0.048)	0.147 (0.103)	0.063 (0.048)	0.459

Note: High quality material in walls: brick, cement, or stone mixed with lime or cement. High quality material in floors: parquet, wood, cement, tiles, or asphalt sheets. High quality material in roofs: reinforced concrete. All regressions include village characteristics such as population size, literacy rate, households with drains, households with electrical energy, education center with secondary level or less, and health center, as well as district fixed effects, the value of the dependent variable level at baseline, and a dummy that indicates whether the same person answered the baseline and the follow-up survey. We also include a dummy that indicates if an observation has missing data at the village level. Regressions also includes controls for rounds. Clustered errors at the village level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.7: TOT Effects on Household Vulnerability by District's Level of Poverty

	Observations	Control	TOT	Level of Poverty		
				Low	High	P-value
Experienced issue during past 12 months	1827	0.846 (0.009)	-0.261* (0.147)	0.074 (0.229)	-0.452** (0.188)	0.080
Hunger	1827	0.367 (0.012)	-0.200 (0.144)	0.179 (0.265)	-0.418** (0.177)	0.061
Disease	1827	0.404 (0.012)	-0.188 (0.142)	-0.115 (0.237)	-0.231 (0.175)	0.693
Death	1827	0.088 (0.007)	-0.24*** (0.082)	-0.297* (0.155)	-0.214** (0.091)	0.640
Job loss	1827	0.413 (0.012)	-0.243 (0.160)	-0.045 (0.279)	-0.358* (0.191)	0.354
Theft	1827	0.120 (0.008)	0.028 (0.080)	-0.020 (0.151)	0.056 (0.084)	0.645
Damage or loss at home	1827	0.197 (0.010)	-0.112 (0.116)	-0.336 (0.211)	0.018 (0.145)	0.162
Labor shortage	1827	0.391 (0.012)	-0.136 (0.174)	-0.008 (0.288)	-0.209 (0.211)	0.571
Crop loss	1827	0.654 (0.012)	-0.077 (0.171)	0.084 (0.291)	-0.169 (0.210)	0.481
Livestock loss	1827	0.313 (0.012)	-0.144 (0.135)	-0.237 (0.238)	-0.092 (0.165)	0.613

Note: All regressions include village characteristics such as population size, literacy rate, households with drains, households with electrical energy, education center with secondary level or less, and health center, as well as district fixed effects, the value of the dependent variable level at baseline, and a dummy that indicates whether the same person answered the baseline and the follow-up survey. We also include a dummy that indicates if an observation has missing data at the village level. Regressions also includes controls for rounds. Clustered errors at the village level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.8: TOT Effects on Productive Activities by District's Level of Poverty

	Observations	Control	TOT	Level of Poverty		
				Low	High	P-value
Owns a non-agricultural business	1827	0.064 (0.006)	0.004 (0.066)	0.206 (0.137)	-0.113* (0.066)	0.034
Number of non-agricultural business	1827	0.068 (0.007)	-0.033 (0.075)	0.130 (0.160)	-0.127* (0.066)	0.126
Number of harvested crops	1827	1.512 (0.045)	-1.145* (0.622)	-0.262 (0.934)	-1.653** (0.770)	0.233
Sells harvested crops	1827	0.268 (0.011)	0.072 (0.136)	0.127 (0.220)	0.040 (0.169)	0.750
Number of animals	1827	1.844 (0.142)	-2.239 (1.471)	1.746 (1.903)	-4.561** (2.170)	0.037

Note: All regressions include village characteristics such as population size, literacy rate, households with drains, households with electrical energy, education center with secondary level or less, and health center, as well as district fixed effects, the value of the dependent variable level at baseline, and a dummy that indicates whether the same person answered the baseline and the follow-up survey. We also include a dummy that indicates if an observation has missing data at the village level. Regressions also includes controls for rounds. Clustered errors at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.9: TOT Effects on the Extensive and Intensive Margins of Labor Market Participation, by Sex

	Observations	Control	TOT	Sex		
				Male	Female	P-value
Has paid/unpaid work	1827	0.732 (0.011)	0.254** (0.126)	0.191 (0.202)	0.292** (0.142)	0.656
Agriculture and livestock	1827	0.624 (0.012)	0.173 (0.137)	0.033 (0.226)	0.254* (0.154)	0.377
Household business	1827	0.117 (0.008)	-0.045 (0.071)	-0.217 (0.155)	0.038 (0.090)	0.178
Dependent	1827	0.140 (0.009)	0.027 (0.084)	0.017 (0.180)	0.034 (0.093)	0.934
Hours dedicated to paid/unpaid work	1827	29.571 (0.696)	5.581 (7.730)	-6.966 (13.108)	12.087 (8.451)	0.179
Agriculture and livestock	1827	22.352 (0.573)	4.752 (6.713)	0.176 (11.527)	7.395 (6.951)	0.541
Household business	1827	2.975 (0.312)	-1.100 (2.510)	-6.681 (5.225)	1.602 (3.263)	0.208
Dependent	1827	4.244 (0.322)	1.550 (3.047)	-0.457 (6.612)	2.601 (3.315)	0.685

Note: All regressions include village characteristics such as population size, literacy rate, households with drains, households with electrical energy, education center with secondary level or less, and health center, as well as district fixed effects, the value of the dependent variable level at baseline, and a dummy that indicates whether the same person answered the baseline and the follow-up survey. We also include a dummy that indicates if an observation has missing data at the village level. Regressions also includes controls for rounds. Clustered errors at the village level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.