

Frisancho Robles, Verónica; Valdivia, J. Martín

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Savings groups reduce vulnerability, but have mixed effects on financial inclusion

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Verónica Frisancho
Martín Valdivia

Inter-American Development Bank
Department of Research and Chief Economist

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Verónica Frisancho*
Martín Valdivia**

* Inter-American Development Bank

** Grupo de Análisis para el Desarrollo (GRADE)

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Abstract

This paper evaluates the impact of the introduction of savings groups 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 savings groups after more than two years of exposure. 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 savings groups increases female labor market participation and, in poorer areas, it fosters greater specialization in agricultural activities. Access to savings groups also leads to a four-percentage point increase in access to credit among women, mainly driven by access to the group's loans. However, the introduction of savings groups has no impact on the likelihood of using 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

JEL Classification: C93, D24, D92, G22, O12, O13, O16, Q12, Q14

1 Introduction

Most of the rural poor around the world remain excluded from the financial system, which limits their capacity to access 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. Even when those institutions are present, however, 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 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 also reflects preferences for attributes offered by informal services such as their convenience, ease 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.

This study evaluates the effectiveness of Village Saving and Loan Associations (VSLAs) or savings groups, 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; instead, all funds come from group members' savings. We partnered with COFIDE, the Peruvian Development Bank that has been promoting savings groups in Peruvian rural communities for 15 years, and leverage the expansion of their program in rural villages of Ayacucho between 2014 and 2016. Relying on a clustered randomized control trial, we evaluate the impact of the introduction of savings groups on poverty, vulnerability, and financial inclusion outcomes in rural Peru.

We look at a variety of financial and social indicators drawn from a survey applied about 30 months after the beginning of the promotion of the savings groups, and credit history data obtained from one of the leading credit bureaus in Peru. Consistent with some of the previous studies on savings groups 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.

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

However, the introduction of savings groups has no impact on the likelihood of using formal financial services. On the contrary, it *discourages* access to loans from formal financial institutions and microfinance lenders among the unbanked. While savings groups offer access to small loans, the model mainly responds to the need to save by offering a means to do so collectively. Thus, we interpret the negative treatment effect on access to formal credit among the unbanked as suggestive evidence of a substitution effect of formal credit: savings groups offer an attractive and effective savings technology that may reduce the need for credit among poor rural households initially excluded from formal financial markets.

Due to the increased popularity of savings groups 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 each household member, especially in Africa (Karlan et al., 2017, Ksoll et al., 2016, Beaman et al., 2014). However, savings groups remain rare in other parts of the global 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 and a half years, on average) for the treatment to play a role. Second, we focus on a previously underexplored 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 to and use 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 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

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, and they often struggle to find formal institutions willing or able to provide basic financial services suitable for their needs.

The rural poor instead tend to satisfy their needs for financial services by relying on a variety of non-institutional and decentralized informal mechanisms such as savings groups or VSLAs. VSLAs are savings-led microfinance groups, very similar to Rotating Savings and Credit Associations (ROSCAs) (Besley et al., 1993), and they are made up of 6-15 individuals who commit to saving a certain amount every month. Relative to ROSCAs, the VSLA technology adds flexibility to its rules (Karlan et al., 2017). First, members can save more than the minimum period amount requested at each monthly meeting. Second, members are no longer forced to take the total pot based on a pre-determined order; instead, loans are granted on demand. Loans are charged an interest rate so that the group's savings can earn interest. Interest gains are paid out at the end of a predefined cycle. Savings groups also have a solidarity fund that can be used to help a member in need of interest-free support.

The VSLA model shares a few features with the village banking model such as the group meetings component and development of a network and the mandatory savings component. However, savings groups do not rely on any external funding source, and the loans provided do not seem to be the main focus of the association, as is the case under the village banking model. Instead, VSLAs are self-managed groups that can be thought of as a social savings technology, where commitment to the group facilitates compliance with savings goals.

While there is abundant evidence on the effects of other microfinance models on poverty reduction (Banerjee et al., 2015, Crépon et al., 2015), little is known about the impact of savings groups on household welfare. Despite their increasing rate of adoption in the rural developing world and their popularity among donors, there are only a few rigorous experimental studies that are able to address endogeneity and selection issues on program roll-out and treatment take-up to accurately measure the role of savings groups in household well-being.

Most of the literature on the effects of savings groups on the finances and welfare of the poor has been concentrated in rural Africa. In general, these studies find that savings groups do not have significant effects on poverty. In a multicountry study of three randomized control tri-

als in Ghana, Malawi, and Uganda, Karlan et al. (2017) find that savings groups do not generate significant effects on income, household expenditures, food security, or asset ownership in rural areas. They argue that the increase in savings balances due to participation in a VSLA is significant but fails to be transformative, as it only represents 7% of the average household's monthly consumption expenditure. In contrast, Ksoll et al. (2016) find a sizeable impact of VSLAs on savings, corresponding to a 153% increase relative to the control group. This effect translates into a statistically significant impact on household expenditures, meals consumed per day, and number of rooms in the dwelling for Malawi.

In line with Karlan et al. (2017), Beaman et al. (2014) fail to find significant effects on income or expenditure, but they show that VSLAs in Mali generate positive effects on food safety, consumption smoothing, and precautionary savings (mostly through livestock). The authors find that the savings groups facilitated the reallocation of resources to the lean season, without an overall effect on aggregate food consumption over the year.

Savings groups additionally include a mentoring/training component provided by the external organization that forms the groups. Field agents in charge of the program placement provide basic financial education lessons to self-manage the group's transactions, but they may also provide more specific training on productive activities or other topics. Moreover, the cooperative component of the model provides access to new networks and information that could influence participants' aspirations, investments, choices, and empowerment levels. In some settings, the VSLA model is especially targeted to women, which can reinforce potential effects on economic and social female empowerment.

Indeed, Karlan et al. (2017) show that providing access to the VSLA technology improves households' non-agricultural businesses outcomes and women's empowerment levels. Households in the treatment group invest more in non-farm activities, which tend to be short-term seasonal businesses. Female participants also experience an increase in their self-reported influence on household decisions, but only in villages that were not experiencing a drought. However, both these effects are small and statistically weak. Beaman et al. (2014) did not find significant effects on women empowerment, while Ksoll et al. (2016) do not explore gendered effects of the model.

All in all, the experimental evidence available for Africa suggests that the local financial market intermediation improvements put forward by the VSLA model may have positive effects on poverty, resilience, household's investments in productive activities, and female empowerment levels. However, the presence and the magnitude of these effects vary depending on the context and the implementer. On one hand, this social savings technology may activate different channels depending on the setting. On the other hand, Beaman et al. (2014) shows that the positive effects of the program on savings, housing quality, and food security in Mali were only realized when recruitment activities were organized and directly structured by an NGO, while the implementation

of the model with soft support of the NGO did not yield substantial benefits. This suggests that both the external agents' identity and implementation strategy play an important role in the effects of savings groups on household welfare, either by providing needed financial literacy or external legitimacy to the mechanism.

Interestingly, the existing evidence is silent about the effects of providing access to the VSLA model on financial inclusion outcomes and demand for formal financial services. Since the self-managed group may contribute to developing good financial habits and behavior, beneficiary households could graduate from the model over time and reach out to formal service providers that are able to provide additional financial services. Alternatively, local and simplified access to basic and flexible financial services may discourage households' demand for formal services.

A few recent papers highlight potential threats to the sustainability of the VSLA model over time. For instance, Le Polain et al. (2018) find that members in Congo mainly convert their savings into loans to avoid the risks of storing cash, thus sacrificing the group's level of capital accumulation. Moreover, even if information asymmetries are reduced, the group's rules for allocating credit are not usually guided by expected repayment, but rather reflect risk diversification and other subjective criteria. Cassidy and Fafchamps (2018) focus instead on the tendency of the groups to have relatively low heterogeneity in terms of the productive activities of their members. This limits the possibility of intermediation between agricultural and non-agricultural households, increasing the relative concentration of common risks associated with the group's main activity.

Studies about savings groups are scarce for developing contexts other than African countries. While the microcredit movement has had an unprecedented growth in Latin America, the main targets of their lending services and technologies are informal microenterprises in urban and peri-urban areas. With the exception of a handful of organizations that rely on the village banking model, few microfinance institutions work in remote rural areas where the main productive activities revolve around the agricultural calendar. While the VSLA model has many characteristics that make it suitable for successfully reaching underserved populations in areas with weak or non-existent local financial markets, it has rarely been tested in Latin America. Our paper contributes to the literature by providing evidence about the effects of savings groups in rural Peru. Beyond the standard measured outcomes related to poverty, consumption-smoothing, and risk-coping, this study relies on unique administrative records that allows us to contribute to the discussion on the role of savings groups in financial inclusion and demand for formal financial services, topics not yet explored in the literature.

3 Context and Experimental Design

3.1 *The Intervention*

The Development Finance Corporation (COFIDE), a development bank in Peru, attempts to support financial inclusion in rural areas of the country. In 2005, it created the Inclusive Program for Rural Business Development (PRIDER by its acronym in Spanish), which promotes the establishment of savings groups, which they call UNICAs (Uniones de Crédito y Ahorro in Spanish). The program aims to draw on the strength of interpersonal relationships in rural communities to foster the creation and sustained growth of these savings groups. PRIDER has been operating in nine different regions of the country, most of them located in the Peruvian highlands.²

Savings groups, also known as self-help groups or VSLAs, 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 10 to 30 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 group 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's 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 and to improve their income-generating capacity. In addition to the financial intermediation that is facilitated, its savings groups model offers additional support activities aimed at contributing to families' overall development. For instance, those activities focus on restoring weak social ties and strengthening participants' self-esteem, facilitating access to agricultural inputs, offering management tools, and building social capital through savings groups and productive associations. The program also offers technical guidance and advice on agricultural businesses.

Once PRIDER identifies candidate villages in which to introduce the savings group model, its officers assess the community's degree of interest and identify potential members. A savings group is set up when officers identify at least 10 people interested in becoming members. The group chooses its representatives, 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

² PRIDER 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.

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 savings group 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 corresponding financial profit accumulated 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. Its plan to boost the creation of savings groups covered four provinces: Huamanga, Huanta, Cangallo, and Vilcashuaman. Eligible villages were randomized into the treatment between 2014 and 2016 with the final goal of creating 120 saving groups in the four targeted provinces. Due to staff capacity constraints, the expansion of the program was rolled out in three phases. Villages located in Huamanga and Huanta were served during 2014 and 2015, respectively, while Cangallo and Vilcashuaman were only reached in 2016.

Figure 1 chronologically organizes the intervention activities (in bold) and 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, 30 months 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 a high risk of violence associated with local terrorist groups. Additionally, a village was eligible only if it was defined as rural, according to the 2007 Population and Housing Census, and recorded a population above 150. PRIDER pre-identified 260 villages that met 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.³ 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 tests 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.4 in the Appendix provide basic descriptive statistics, as well as balancing tests of the randomization at the 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 42. 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.⁴ On average, households in the sample spend 27 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 12 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 respondents report saving money outside the household.

3.4 Data and Measurement

To conduct the baseline survey, we randomly chose 10 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 chose a block of houses on the edge of the town square and picked one as a starting point. The first house with a resident willing 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 a given sex and age quota at the village level.⁵

³ Using census data, we construct variables such as the presence 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.

⁴ The difference is dedicated to self-consumption and bartering.

⁵ The quota was established ex ante so as to replicate the global distributions of sex and age in each village.

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.

Thirty months 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.⁶ The final sample in the follow up survey consists of 1,827 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 30 months after the baseline. However, the household re-contact rate in the treatment group was higher in treatment villages (81%) than in control villages (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, either in the full sample or 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 is credit bureau administrative records. We are able to match both our respondents as well as their partners through their national identification numbers. These data include 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 record pending loan balances, the type of loan, and its status. We set the months that are considered pre and post-intervention based on the date in which the first VSLA was created in the village. Loans with regulated institutions

⁶ Surveyors 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.

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 come from Equifax, covering the months between March 2015 and March 2020.

A third source of data is the transactional records of the savings groups' 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 savings groups transactional records, are converted to August 2014 dollars.

Aggregate records at the group level from PRIDER indicate that, by October 2018, 171 VSLAs had been created in the targeted provinces and 159 of them were still in operation. These savings groups comprised 2,261 members, with 65% female participation. On average, each savings group started out with financial capital of 98.7 dollars, which increased by 5,135% 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 more than 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:

$$Y_{ijk} = \alpha + \beta T_{jk} + \gamma X_{ijk} + \delta_k + \varepsilon_{ijk} \quad (1)$$

where Y_{ij} is the outcome variable for household i located in village j in district k . T_{jk} 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_{ijk} is a matrix that includes individual as well as 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_k , while ε_{ijk} is the error term. We assume that the errors are independent between villages, but not within a village, and thus implement the Huber-White correction at the village level.

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

$$Y_{ijk} = \alpha + \beta_0 T_{jk} + \gamma X_{ijk} + \theta x_{ijk} + \beta_1 T_{jk} x_{ijk} + \delta_k + \varepsilon_{ijk} \quad (2)$$

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

Since the take-up levels were not perfect (see Subsection 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_{jk}). This estimator is obtained from a two-stage procedure, where the second stage implies:

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

where T_{ijk} 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 with a savings group (\widehat{T}_{ijk}) is largely determined by PRIDER's targeting strategy, which selected villages at random. Thus, since T_{jk} is exogenous to the preferences and abilities of individuals in treated localities, it is the perfect candidate for instrumenting \widehat{T}_{ijk} :

$$\widehat{T}_{ijk} = \tau_1 + \tau_2 T_{jk} + \lambda_k + \mu_{ijk} \quad (4)$$

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).⁷

5 Results

5.1 Take-up

The expansion of the program in treatment villages was deployed in three rounds, between September 2014 and January 2017 (see Section 3.2). The number of savings groups created augmented in an exponential way within this period and then remained flat once PRIDER moved on to other

⁷ As expected, the TOT estimator is approximated by dividing the ITT estimator by the take-up rate.

areas. By January 2017, 171 savings groups with a total of 2,176 members were working in 120 treatment villages.

Our ability to appropriately measure the effect of savings groups depends on the take-up rate in our random survey sample in treated villages. A low take-up rate in the surveyed sample imposes limitations on the statistical power of the study (see, for example, Bruhn and McKenzie (2009)). We measure participation in the VSLA at both the household and individual levels relying on PRIDER administrative records. Figure 3 shows that take-up rates in our treated sample follow a growth pattern similar to that of the creation of savings groups, with a steep boost during the first months of group creation and subsequent flattening of the growth pattern. Average take-up rates in the treatment group reach substantial levels: 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 household.⁸

Table 1 tries to assess if observable characteristics are good predictors of take-up rates at the household level. Focusing on the sample of households in treated villages, we estimate the probability of joining a savings group 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 with 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 savings groups. However, the second column shows that previous participation in village banks has a positive and large effect (8.6 percentage points) on the probability of joining a savings group. This result reveals that previous experience or geographical exposure to similar group mechanisms encourages households in treated areas to join a savings group. It may also suggest that this 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 savings group model is offered, village banking clients choose to join a savings group, potentially overcoming the limited flexibility offered by the village banking model.

⁸ Related studies of savings groups 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.

5.2 Treatment Impacts

Financial Inclusion

We first focus on the primary goal of the program, fostering access to financial services in rural areas, as measured from self-reported survey data. Given the potential bias when referring to delicate information such as personal savings, Table 2 shows that access to the savings groups does not lead to changes in self-reported savings balances. The treatment is also ineffective in changing savings attitudes: a large majority of the experimental sample reports that they have trouble saving, but the treatment did not improve their situation. Access to savings groups may foster access to formal savings products by bringing rural households closer to financial services. However, the probability of saving formally, already low in the control group (5 percent), does not seem to be affected by the treatment.

Even though we fail to find treatment impacts on self-reported total saving *balances* (see Table 2), average contributions to savings groups increase over time (see panel c in Figure 2). The accumulation patterns observed in the savings groups' administrative records suggest that households could be increasingly setting money aside through the group. The lack of an impact on saving balances suggested by self-reported data could thus respond to measurement error and/or a paired effect on spending or investing. Later in this subsection 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 savings groups' 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 and 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 of having 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 of having 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 of having a loan with a regulated financial institution. Similarly, the total level of debt is unaffected by the treatment, but monthly outstanding balances with savings groups 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 savings groups.

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 two years after the intervention. Since the administrative records from the credit bureau and the savings groups allow us to observe the full credit history of both a survey respondent and his/her partner, we can pool the data at the individual level and add an interaction effect with sex to the model in (2). Interestingly, we find a significant 4-percentage point increase in the probability of having a loan in the past 12 months among females.

Table 4 also identifies differential effects by sex on the probabilities of having a loan from non-regulated lenders and the savings groups: 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. In contrast, men significantly increased the portfolio they kept with both non-regulated lenders and the VSLA.

The introduction of savings groups 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 from engaging 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 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.⁹

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 of having a loan in the past 12 months, irrespective of past credit history. However, access to regulated lenders is significantly reduced among those who were unbanked (i.e., did not have a credit history with regulated formal lenders 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 savings groups, which supports a discouragement effect towards formal regulated lenders in the market.

⁹ The creation of the savings groups implies a training component for 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, and selecting group members who will take out loans, among other issues. Even members who are not trained, however, participate in the meetings and take part in these choices.

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 savings groups 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 effect on the probability of getting 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 savings groups.

All in all, access to savings groups 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 savings groups 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 monthly 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). However, we find a significant reduction of out-of-pocket expenditures on health problems, which is driven by households in poorer districts. This treatment effect may be indicating that the treatment reduces households' health problems due to improved preventive health behavior.

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 of having 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 significant differences by poverty level.

One of the central goals of financial inclusion efforts among impoverished households is the possibility of reducing their exposure to negative shocks. Indeed, access to savings groups in rural Peru reduces households' vulnerability as shown in Table 8. First, notice that 86% of the households in the control group report experienced a negative shock in the past 12 months, with crop losses being the most common (65%), followed by health shocks (42%). Then, we find the treatment reduces by 5 percentage points the probability of facing any such shock. 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 in reducing the likelihood of idiosyncratic events such as hunger, death, and job loss among poorer households, but it shows no effect on crop and livestock losses, which are usually more connected to aggregate climate shocks. These effects are also aligned with the drop in health expenditures among poorer households. 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 savings groups can also have important direct and indirect effects on productive activities undertaken by the household. On one hand, the provision of cheap and appropriate financial services may foster the household's investment in agricultural activities, with potential consequences for 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 providing access to information about market opportunities, prices, and technology, among other possibilities.

Table 9 presents the treatment impacts on the choice to participate in productive and non-productive activities. On average, access to savings groups 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 raised and crops kept by the family unit. The reduction in the level of

diversification among poorer households is consistent with an improvement in available financial services, and it 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. This result is in contrast to the increased investment in non-farm activities found by Karlan et al. (2017) and it is probably explained by contextual differences and the agricultural focus of the implementing agency in Peru.

Table 10 presents the effects of the treatment on the extensive and intensive margins of labor market participation. On average, we identify an increase of about 5 percentage points in the probability of working. 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 either 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. Regardless of working status, the program does not yield any changes in the total average number of hours dedicated to paid work.

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, and saving cash at home, among others. Village banks and savings groups 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, savings groups have a more salient focus on savings-based services (Karlan et al., 2014).

This study provides estimates of the impact of savings groups based on a clustered randomized evaluation conducted in rural Peru. We worked with COFIDE, the Peruvian Development Bank, which has been promoting savings groups in Peruvian rural communities for 15 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 monetary poverty, as measured by average income or expenditures. However, we find that the savings groups reduce exposure to idiosyncratic shocks and facilitate housing improvements, variables connected to common measures of multidimensional poverty. Households in treated villages increased the quality of their houses by 0.125 SD, particularly investing in their floors and roofs. Similarly, households in treated villages show a 5-percentage point decrease in the probability of facing 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, another effect that is consistent with improved access to financial services. This suggests that the savings groups 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 savings groups has no impact on the likelihood of using formal financial services. In fact, we observe that, among the unbanked, the promotion of savings groups *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 reported in Diaz-Martin et al. (2020). Along those lines, it would be key to understand better the role of the external promoters of the savings groups (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 that they also transmit financial knowledge or provide other types of non-financial services valued by the members. Understanding their role in facilitating the development of these groups is fundamental to explaining 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.

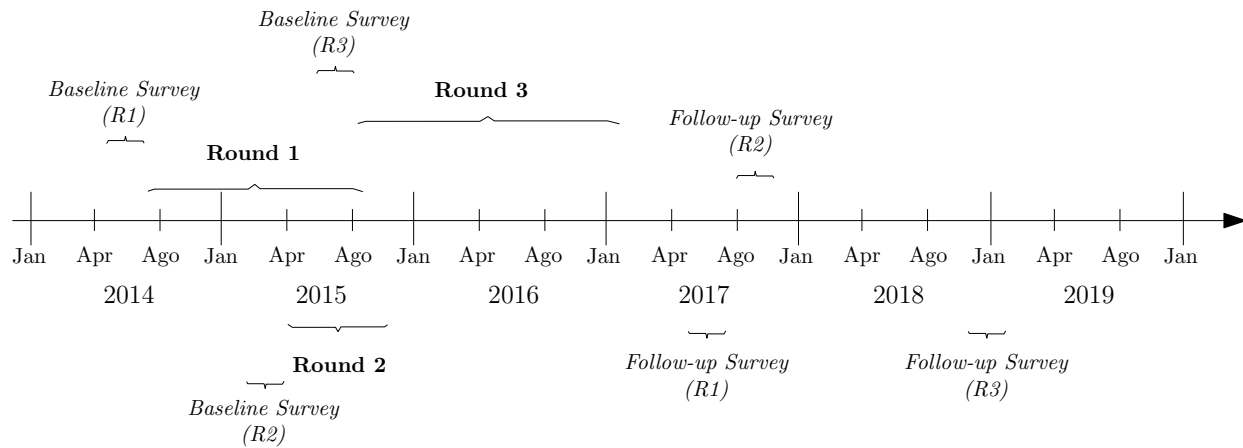
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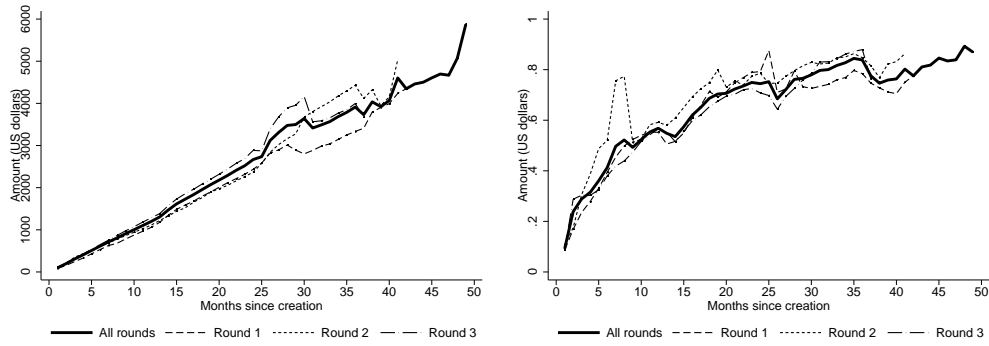
Figures and Tables

Figure 1. Study Timeline



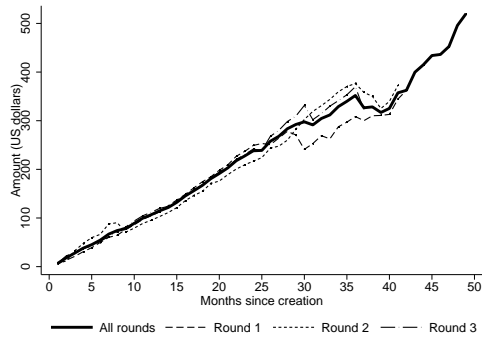
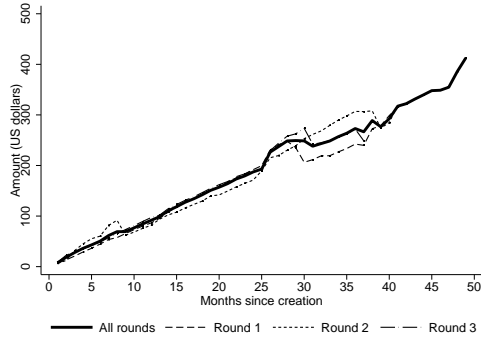
Note: Implementation activities in **bold** and data collection activities in *italics*.

Figure 2. Monthly Evolution of savings groups



(a) Cumulative capital by months of life

(b) Percentage of clients with a loan

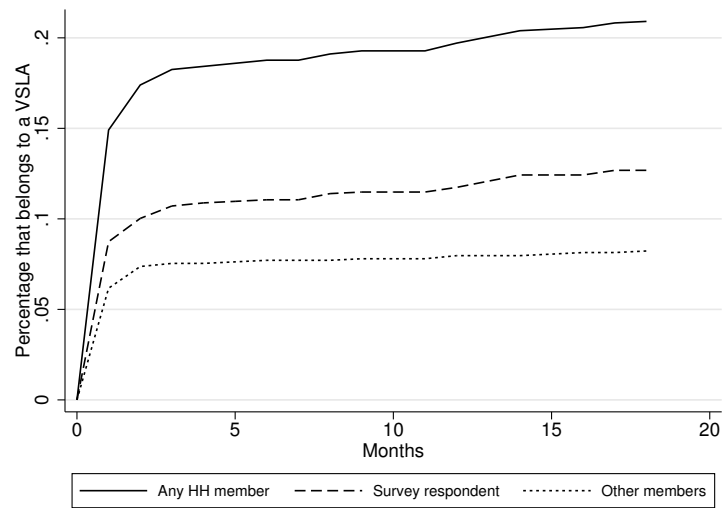


(c) Average contribution per member

(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 to a Savings Group



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

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. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

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. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

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

	Observations	Control	Sex		P-value
			Male	Female	
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. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

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		P-value
			Access	No access	
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)	17.14***† † † (3.114)	7.64***† † † (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. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

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)	14.70***††† (3.101)	8.88***††† (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. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

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
Health expenditure (log)	1827	-0.454 (0.134)	-0.368* (0.213)	0.269 (0.340)	-0.785***††† (0.272)	0.018
Other expenditure (log)	1827	3.954 (0.056)	-0.082 (0.091)	-0.206 (0.151)	-0.001 (0.128)	0.333
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. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

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. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

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. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

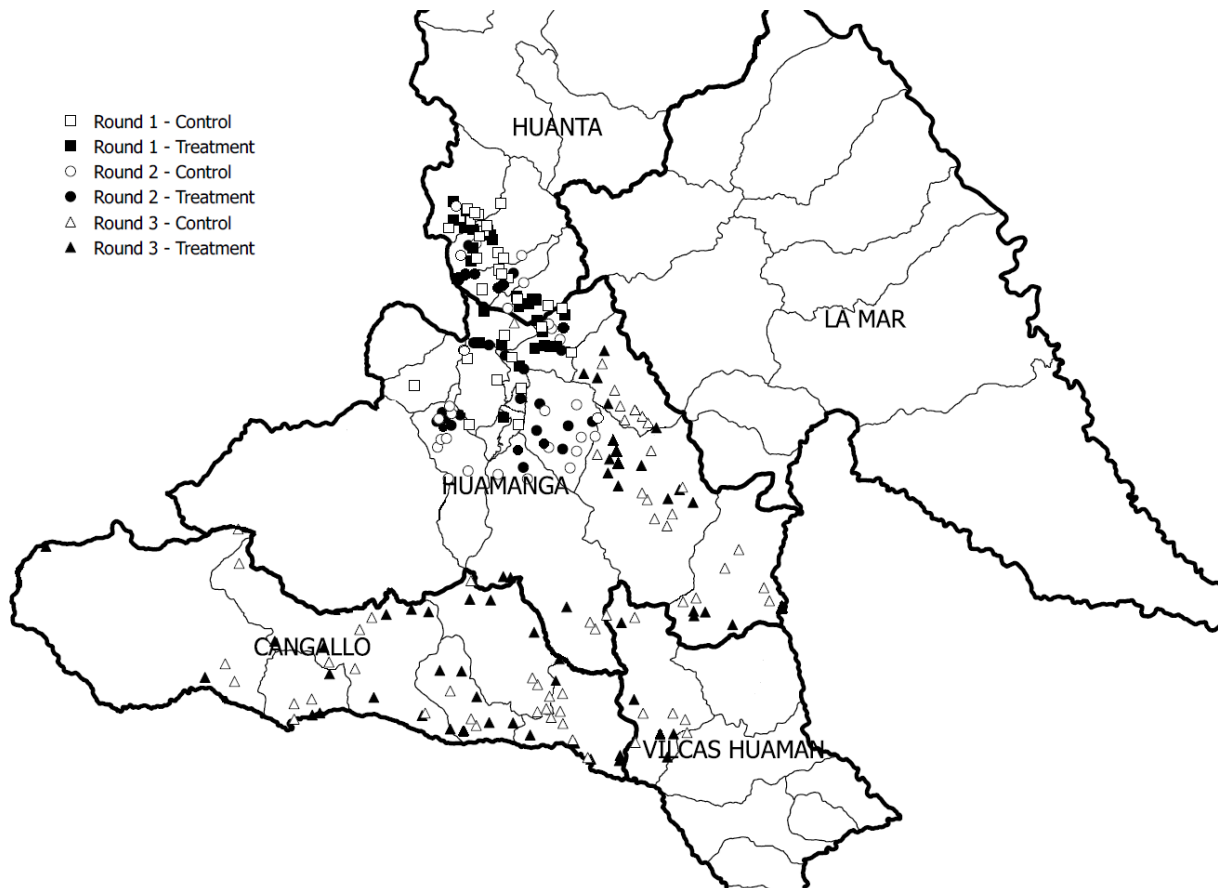
Table 10. ITT Effects on the Extensive and Intensive Margins of Labor Market Participation, by Sex

	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. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

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 (hours)	26.188 [21.024]	1.901 [1.390]	2369
Family business (hours)	4.184 [14.490]	-0.164 [0.702]	2369
Dependent work (hours)	8.851 [16.275]	-1.237 [1.010]	2369
Domestic activities (hours)	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>	31	31	62	31	30	61
Round 2	Individuals	281	260	541	235	216	451
	<i>Villages</i>	27	27	54	27	27	54
Round 3	Individuals	610	624	1234	494	489	983
	<i>Villages</i>	62	62	124	62	61	123
Total number of surveys		1169	1200	2369	942	885	1827
<i>Total number of villages</i>		120	120	240	120	118	238

Note: The two villages not reported in the follow-up 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 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. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

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. Clustered errors at the village level in parentheses. Regressions also includes controls for rounds. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values

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)	18.56***††† (4.093)	23.91***††† (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. Clustered errors at the village level in parentheses. Regressions also includes controls for rounds. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

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)	72.40***††† (10.552)	49.97***††† (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. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

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)	82.26***††† (16.955)	47.78***††† (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. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

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
Health expenditure (log)	1827	-0.638 (0.101)	-1.926* (1.117)	1.260 (1.832)	-3.755***†† (1.350)	0.023
Other expenditure (log)	1827	3.922 (0.042)	-0.429 (0.472)	-1.105 (0.826)	-0.039 (0.610)	0.312
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
High quality material in walls	1827	0.060 (0.006)	0.058 (0.054)	0.077 (0.113)	0.048 (0.052)	0.810
High quality material in floor	1827	0.122 (0.008)	0.211**† (0.091)	0.237 (0.172)	0.196* (0.103)	0.837
High 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. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

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)	-0.244*** (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. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

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. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

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
Agricultural 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
Agricultural 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. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.