SAVINGS GROUPS REDUCE VULNERABILITY, BUT HAVE MIXED EFFECTS ON FINANCIAL INCLUSION[*]

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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 over a two year period. We find that savings groups channel expensive investments such as housing improvements and reduce households' vulnerability to idiosyncratic shocks, particularly among households in poorer districts. The treatment also induces changes in households' labor allocation choices: access to 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 to use formal financial services. On the contrary, it *discourages* access to loans from formal financial institutions and microfinance lenders among the unbanked.

KEYWORDS: Saving Groups, Village Savings and Loans Associations, Financial Inclusion, Impact Evaluation.

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

The rural poor thus often satisfy their needs for financial services by relying on informal mechanisms that are frequently linked to social networks. Their demand is not only driven by exclusion from financial markets, but it also reflects preferences for attributes offered by informal services such as their convenience, easiness of use, and trustworthiness [Lee et al., 2012]. Although financial inclusion efforts tend to focus on the provision of *formal* financial services, financial markets are increasingly embracing informal services and even building upon them as they tend to be flexible, innovative, and naturally tailored to the specific needs of their clients.

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, but instead all funds come from group members' savings. We partnered up with COFIDE, the Peruvian Development Bank that has been promoting savings groups in Peruvian rural communities for fifteen 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 coming from a survey applied about thirty months from 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.

However, the introduction of savings groups has no impact on the likelihood to use formal financial

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

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 it 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 big south such as Latin America. Even within the African context, results on their effectiveness are mixed. On one hand, no income or expenditure effects are found for savings groups in Ghana, Malawi, Uganda [Karlan et al., 2017], and Mali [Beaman et al., 2014]. However, Ksoll et al. [2016] report positive effects on household expenditures, meals consumed per day and number of rooms in the dwelling in northern Malawi while Karlan et al. [2017] and Beaman et al. [2014] find instead that savings groups do help households manage risk, either through consumption smoothing or food security. While Karlan et al. [2017] find positive effects on women's empowerment, these are absent in Beaman et al. [2014]. Increased human capital investments are present in India [Baland et al., 2020], but not in Mali [Beaman et al., 2014].

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

The remainder of this paper is organized in five sections. Section 2 discusses the literature of savings groups or VSLAs, while Section 3 describes the 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 providers capable of or willing to provide basic financial services suitable for their needs.

The rural poor 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 a savings-led microfinance group, very similar to Rotating Savings and Credit Associations (ROSCAs) [Besley et al., 1993]. They are formed by 6-15 individuals that commit to save a certain amount every month. Relative to the ROSCAS, the VSLA technology adds flexibility to their 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 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 out 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 with 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 on 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 trials 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 an 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 have 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' transactions, but they may also provide more specific training on productive activities or other topics. Moreover, the associative 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 where only materialized whenever 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 much benefits. This suggests that both the external agents' identity and implementation strategy play an important role on 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 the VSLA model

on financial inclusion outcomes and demand for formal financial services. Since the self-managed group may contribute to develop 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 of 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 to allocate 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 associations 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 groups' 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 target 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 to successfully reach underserved populations in areas with weak or nonexistent local financial markets, the VSLA model has been rarely 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 data that allows us to contribute to the discussion on the role of savings groups on financial inclusion and demand for formal financial services, a topic 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, tries to support financial inclusion in rural areas of the country. In 2005, they created the Inclusive Program for Rural Business Development (PRIDER by its acronym in Spanish), which promotes the establishment of 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 ten to thirty families from the same community. The group meets regularly to make savings contributions that are put together into a common fund. This fund is then used to support the economic activities and needs of their members through loans that pay an interest rate and thus provide a return on savings. Indeed, the operating rules require that all the funds collected should be completely lent out to members of the group at all times. The financial profit generated through lending is proportionally distributed at the end of each year depending on the individual level of savings.

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

Once PRIDER identifies candidate villages to introduce the savings group model, their officers gather the community's degree of interest and identify potential members. A savings group is set up when officers identify at least ten 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 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 to those who decide to abandon the

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[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.

group before the end of the group's working year: the leaving member gets back the value of her share, but she does not receive the accumulated financial profit corresponding during the year.

3.2 Study Timeline

In 2014, PRIDER decided to expand its operations in Ayacucho, an impoverished department in the southern part of Peru. Their plan to boost the creation of savings groups covered four provinces: Huamanga, Huanta, Cangallo, and Vilcashuaman. Eligible villages randomized into the treatment were intervened 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) as well as the data collection activities (in italics). Between 2014 and 2016, PRIDER progressively implemented the VSLA program in the study area. Prior to each of the three recruiting phases, we collected baseline data in the control and treatment villages. Follow-up surveys were conducted on a rolling basis, two years after each round of implementation had ended.

3.3 Sample Selection and Randomization

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

The final sample consisted of 240 eligible villages, which were stratified by district. Within each strata, the treatment was randomized at the village level. We first randomized the treatment a hundred times and measured the differences across control and treatment groups in terms of their pre-program characteristics at the village-level.[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 test between the treatment and control groups at the village level. Figure A.1 in the Appendix shows the geographical locations of the villages included in the experiment by treatment arm and implementation round.

Tables A.2-A.3 in the Appendix provide basic descriptive statistics, as well as balancing tests of the

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

randomization at household-level, relying on baseline survey records. Consistent with the random treatment assignment, we found few significant differences between both groups.

The average age of the sample is forty-two. Only 18% of the sample reported having at least full secondary while 82% said that the language most used at home was Quechua. Over 85% of the households report agricultural or livestock activities, although only 35% report selling at least part of their production in the market in exchange for money.[4] On average, households in the sample spend twenty-seven hours per week working in the family farm unit.

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

3.4   Data and Measurement

To conduct the baseline survey, we randomly chose ten households per village. This number was adjusted downwards in smaller villages. The sampling procedure in each village depended on the geographical extension and spatial distribution of dwellings. Since most of the villages did not have an updated cartography, the surveyor began the process in the town square of each village. She choose a block of houses on the edge of the town square and picked one as a starting point. The first house with willingness to respond becomes the first observation in that village. The team then skips three houses and moves clockwise until they find a second household willing to be surveyed. Once two houses were effectively surveyed in a block, the surveyor moves to the block on the opposed diagonal. This process continues until the limits of the village are reached. The team then returns to the town square to repeat the same process but going in the opposite direction. At the household level, the respondent is chosen among the head of the household and his/her partner so as to reach a given sex and age quota at the village level.[5]

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,

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[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.

age, gender, and education level.

Two years after each baseline survey round was completed, we revisited the 240 villages in the experimental sample and retraced the sample of households originally surveyed. The follow up questionnaire was similar to the baseline instrument and targeted the same respondent who was initially interviewed.[6] The final sample in the follow up survey consists of 1827 households (see Table A.5).

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

A second source of data are credit bureau administrative records. We are able to match both our respondents as well as their partners through their national identification numbers. These data includes individual-monthly level information on outstanding debt at both regulated and unregulated formal financial institutions. Even though the latter are not really informal lenders, they tend to relax the minimum requirements to get a loan at the cost of higher interest rates when compared to banks [Campion et al., 2010]. For each loan, these data records pending loan balances, the type of loan, and its status. We set the months that are considered pre and post intervention based on the date in which the first VSLA was created in the village. Loans with regulated institutions come from the Superintendency of Banks (SBS) during the period August 2012-April 2020. These records provide outstanding debt levels with banks and other supervised microfinance institutions. Loans obtained from unregulated financial institutions such as cooperatives and NGOs were obtained from two private credit bureaus that operate in Peru. Data for household heads comes from Sentinel and covers the periods August 2014 through August 2019. Partners' data instead comes from Equifax, covering the months between March 2015 and March 2020.

A third source of data are the transactional records of the savings groups' operations between October 2014 and October 2018. These data provide information on savings and loans balances at

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[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.

the individual-monthly level. All variables measured in soles, both from credit bureau and savings groups transactional records, are transformed to August 2014 dollars.

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

4 Estimation Strategy

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

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

where Y_{ij} is the outcome variable for household i located in village j. T_j is a dummy variable that equals one when the household belongs to a village selected to be supported by PRIDER to form a savings group. X_{ij} is a matrix that includes individual as well and village level characteristics, including the value of the outcome variable at baseline and dummies for each survey round. Implementation of an analysis of covariance (ANCOVA) to estimate the treatment effects leads to large improvements in power compared to a difference-in-difference specification [McKenzie, 2012]. The regression model includes fixed effects at the district level denoted by δ_j, while ε_{ij} is the error term. We assume that the errors are independent between communities, but not within a community and thus implement the Huber-White correction at the village level.

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

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

where x_{ij} is a dummy variable that equals one if household i or village j matches the characteristic

under analysis and is zero otherwise. In this model, β_0 measures the ITT effect for those with $x_{ij} = 0$, while $(\beta_0 + \beta_1)$ will capture the effect among those with $x_{ij} = 1$. Note that characteristic x_{ij} will often be a subset of matrix X_{ij}.

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

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

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

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

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 areas. By January 2017, 171 savings groups with a total of 2176 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 to the statistical power of the study (see, for example, Bruhn and McKenzie [2009]).

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

We measure participation in the VSLA both at the household and individual levels relying on PRIDER administrative records. Figure 3 shows that take-up rates in our 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 posterior 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 to the savings groups. The increase in take-up rates is about 12 percentage points when the household head is a woman, which is equivalent to an adoption rate 50% higher than the average at the household level.

Column 1 shows that other characteristics of the household head, such as her or his age, marital status, education, level of entrepreneurship and level of financial knowledge, as well as characteristics of the household such as language spoken at home, number of children or the ratio of household's members per bedroom, do not have a significant influence on participation in 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 to join 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.

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. Knowing the potential bias when referring to delicate information such as personal savings, Table 2 shows that access to the savings groups does

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[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.

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 manifests that they have trouble saving, but the treatment did not improve the situation. Access to savings groups may foster access to formal savings products by bringing rural households closer to financial services. However, the probability to save formally, already low in the control group (5 percent), 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 the 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 could thus respond to a paired effect on spending or investing. Later in this sub-section we analyze household's expenditures and living conditions.

Table 3 focuses instead on the effect of the treatment on access to credit. Merging together monthly loan balances from the credit bureau records as well as from the 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 as well as on the amount owed. Since the data allow us to observe outstanding debt by lender, we further disaggregate treatment impacts by the source of credit.

In general, the probability to have a loan in the past 12 months and the level of monthly outstanding debt remained unchanged after being exposed to the treatment. On average, the probability to have a pending loan with the VSLA significantly increases one and two years after the beginning of the intervention. Although not significant, there is also an average negative effect on the probability to have a loan with a regulated financial institution. Similarly, the total level of debt is unaffected by the treatment, but monthly outstanding balances with 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 survey respondents and his/her partner, we can pool the data at the individual level and add an interaction effect with sex to the model in (2). Interestingly, we find a significant 4-percentage point increase in the probability to have a loan in the past 12 months among females.

Table 4 also identifies differential effects by sex in the probabilities to have a loan from non-regulated lenders and the 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. Men, instead, 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 to engage with other formal financial institutions once their demands for financial services are satisfied. However, it is also possible that joining a VSLA allows these previously excluded households to develop good repayment habits that may make them more 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 to have 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 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.

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

All in all, access to savings groups weakly expands access to credit among females but has no

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

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 the 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 issues 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 to have good quality floors (4-percentage point) and roofs (2-percentage points). This effect seems to be more robust in poorer districts, but the last column of Table 7 rules out that there are significant differences by poverty level.

One of the central goals of financial inclusion efforts among impoverished households is the possibility to reduce their exposure to negative shocks. Indeed, access to 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 in 5-percentage points the probability to face 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 to reduce the likelihood of idiosyn-

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cratic events such as hunger, death, and job loss among poorer households, and show no effect on crop and livestock losses, 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 effects on productive activities undertaken by the household both directly and indirectly. On one hand, the provision of cheap and adequate financial services may foster household's investment in agricultural activities, with potential consequences on their participation in non-agricultural production. Additionally, the technical guidance on agricultural businesses provided by the program may also lead households to further specialize and shy away from other market activities, especially if the latter where performed by necessity, to complement family income. On the other hand, the social capital and networks developed within the group and across savings groups in the village may foster greater job stability and availability of job opportunities, both in the agricultural and non-agricultural sector. Interactions with the group may also have the added advantage of access to information about market opportunities, prices, technology, among others.

Table 9 presents the treatment impacts on the choice to participate in productive and non-productive activities. On average, access to 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 and crops held by the family unit. The reduction in the level of diversification among poorer households is consistent with an improvement in available financial services, and 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 context 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 in the probability to work of about 5-percentage points. Increased labor market participation is mostly driven by female household members in the treatment group, who are 6.5-percentage points more likely to engage in 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. Unconditional on the 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, 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, that has been promoting savings groups in Peruvian rural communities for fifteen years, and took advantage of the expansion of their operations in Ayacucho between 2014 and 2016. Consistent with the evidence available for Africa, we find no effects on 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 in 0.125 SD, particularly investing on their floors and roofs. Similarly, households in treated villages show a 5-percentage point decrease in the probability to face a negative shock in the past 12 months and that figure is twice as high for households in poorer districts. The results among poorer households point towards an interesting pattern of specialization in agricultural activities due to the treatment, with a reduction in the level of diversification, 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 to use 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 stated in Diaz-Martin et al. [2020]. Along those lines, it would be key to understand better the role of the external promoters of the 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 they also transmit financial knowledge to the group members or provide other types of non-financial services valued by the members. Understanding their role in facilitating the development of these group is fundamental to explain why these savings groups did not develop spontaneously and to predict their sustainability over time, once the direct role of the coordination agent fades away.

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Figures and Tables

Figure 1: Study Timeline
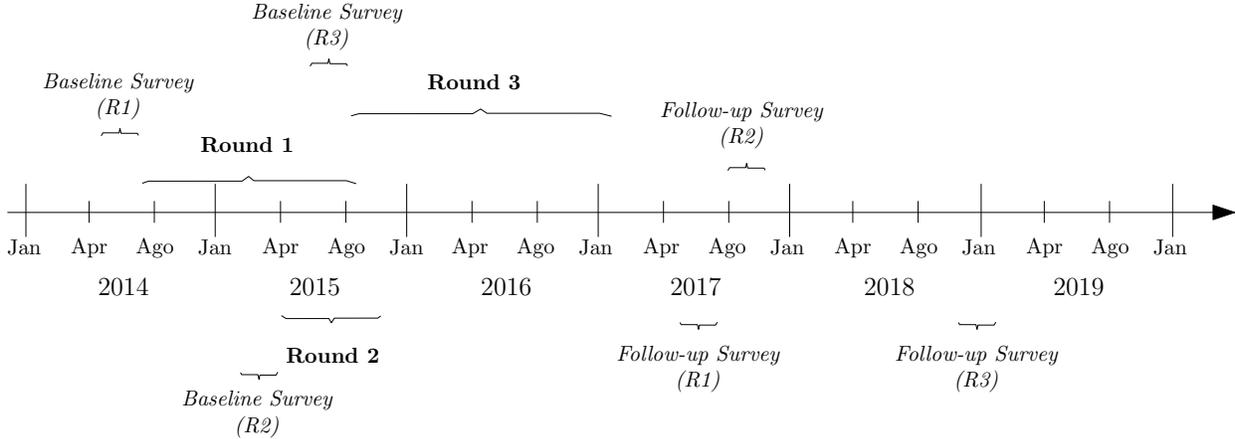
Note: Implementation activities in **bold** and data collection activities in *italics*.

Figure 2: Monthly Evolution of VSLAs
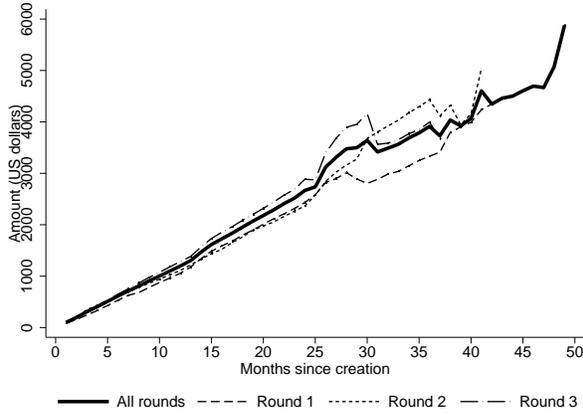
(a) Cumulative capital by months of life
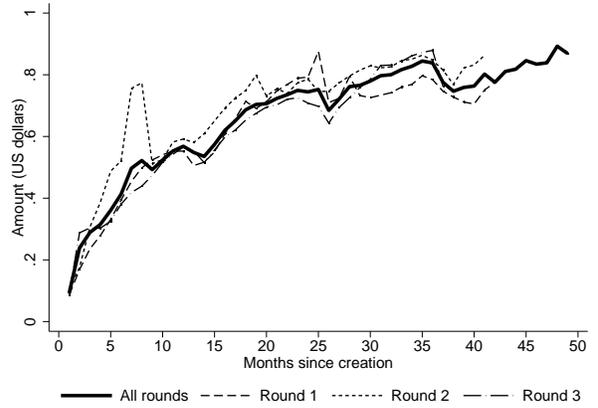
(b) Percentage of clients with a loan
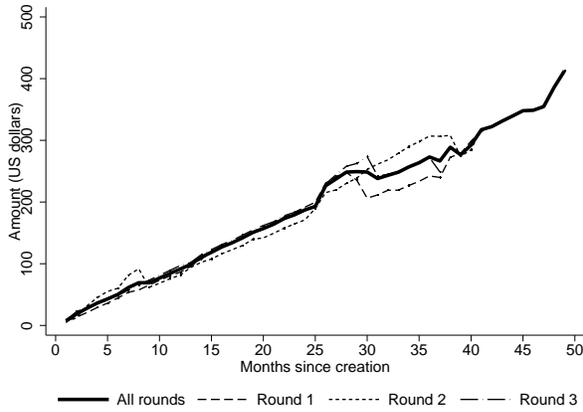
(c) Average contribution per member
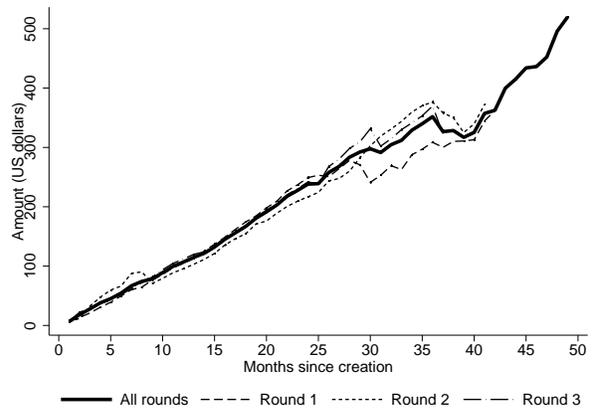
(d) Average loans per member

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

Figure 3: Percentage Household Members Belonging a VSLA
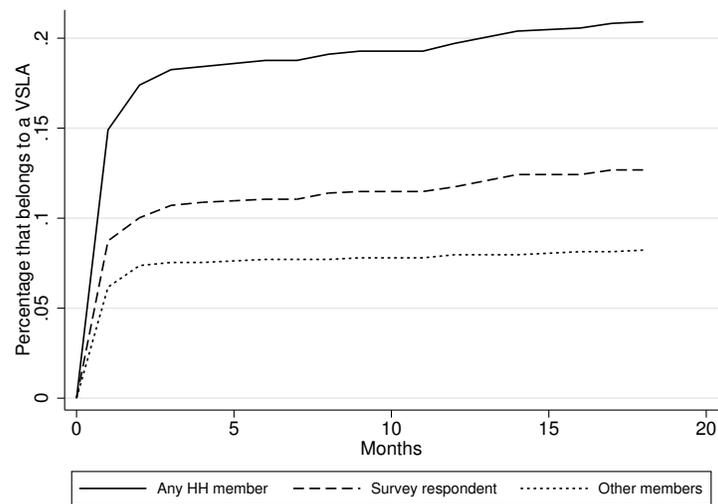
Note: Accumulated percentage up to 18 months after the creation of the corresponding VSLAs.

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Table 1: Determinants of Affilliation to VSLAs

	Household	
	(1)	(2)
Age of household head	-0.002	-0.002*
	(0.001)	(0.001)
Gender of household head: Female	0.120**	0.119**
	(0.051)	(0.050)
Marital status of household head: Married	0.060	0.061
	(0.046)	(0.045)
Most spoken language at home: Quechua	0.016	0.019
	(0.035)	(0.036)
Educational level of household head: without instruction	-0.008	-0.005
	(0.045)	(0.044)
Entrepreneurship level of respondent	-0.012	-0.014
	(0.017)	(0.017)
Level of financial knowledge of respondent	0.000	-0.001
	(0.013)	(0.014)
Confidence level of respondent: in people	0.018	0.016
	(0.015)	(0.015)
Confidence level of respondent: in friends	-0.005	-0.005
	(0.016)	(0.016)
Confidence level of respondent: in acquaintances	0.009	0.010
	(0.020)	(0.020)
Number of children	0.011	0.010
	(0.010)	(0.010)
Ratio of household members to bedrooms	-0.008	-0.008
	(0.010)	(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	11.977
		(17.319)	(22.159)
Saved formally	1827	0.055	0.002
		(0.008)	(0.011)
Regrets spending money instead of saving it	1827	0.759	-0.020
		(0.014)	(0.024)
Wants to save a little more	1827	0.719	0.005
		(0.015)	(0.024)
Hide the money so others don't spend it	1827	0.647	-0.009
		(0.016)	(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	0.023	0.006
		(0.016)	(0.017)	(0.019)
Regulated lender	1999	0.389	-0.017	-0.023
		(0.015)	(0.013)	(0.017)
Non-regulated lender	1999	0.145	0.002	0.005
		(0.012)	(0.013)	(0.014)
VSLA	1999	0.000	0.102***† † †	0.123***† † †
		(0.007)	(0.013)	(0.014)
Outstanding Monthly Debt (USD)	1999	1152.706	56.292	19.359
		(88.573)	(81.406)	(104.905)
Regulated lender (USD)	1999	879.245	33.908	-86.844
		(71.968)	(63.609)	(82.121)
Non-regulated lender (USD)	1999	207.220	10.646	92.123**††
		(32.652)	(32.864)	(36.168)
VSLA (USD)	1999	0.000	7.707***† † †	11.171***† † †
		(1.049)	(1.270)	(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		
			Male	Female	P-value
Had a loan in the past 12 months	3585	0.299	0.006	0.042**†	0.147
		(0.011)	(0.019)	(0.020)	
Regulated lender	3585	0.239	-0.018	-0.007	0.612
		(0.010)	(0.018)	(0.014)	
Non-regulated lender	3585	0.087	0.017	-0.017	0.039
		(0.007)	(0.011)	(0.013)	
VSLA	3585	0.000	0.064***†††	0.094***†††	0.007
		(0.005)	(0.010)	(0.012)	
Outstanding Monthly Debt (USD)	3585	625.566	43.422	-47.946	0.371
		(44.411)	(86.851)	(57.004)	
Regulated lender (USD)	3585	468.838	-16.181	-82.633**†	0.428
		(35.901)	(71.758)	(41.595)	
Non-regulated lender (USD)	3585	98.848	58.232**††	25.541	0.268
		(14.350)	(22.567)	(21.566)	
VSLA (USD)	3585	0.000	3.246***†††	4.308***†††	0.261
		(0.327)	(0.825)	(0.713)	

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		
			Access	No access	P-value
Had a loan in the past 12 months	1999	0.464	0.008	0.007	0.972
		(0.016)	(0.026)	(0.026)	
Regulated lender	1999	0.389	0.014	-0.045**†	0.117
		(0.015)	(0.030)	(0.021)	
Non-regulated lender	1999	0.145	0.038	-0.014	0.108
		(0.012)	(0.029)	(0.014)	
VSLA	1999	-0.000	0.152***†††	0.106***†††	0.041
		(0.007)	(0.022)	(0.015)	
Outstanding Monthly Debt (USD)	1999	1152.706	-49.261	55.130	0.709
		(88.573)	(260.984)	(77.708)	
Regulated lender (USD)	1999	879.245	-142.879	-49.897	0.673
		(71.968)	(205.769)	(61.562)	
Non-regulated lender (USD)	1999	207.220	119.711	75.135**†	0.619
		(32.652)	(78.970)	(37.163)	
VSLA (USD)	1999	-0.000	17.14***†††	7.64***†††	0.004
		(1.049)	(3.114)	(1.835)	

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	0.017	-0.001	0.641
		(0.016)	(0.029)	(0.025)	
Regulated lender	1999	0.389	-0.025	-0.022	0.917
		(0.015)	(0.027)	(0.023)	
Non-regulated lender	1999	0.145	0.012	0.001	0.722
		(0.012)	(0.025)	(0.018)	
VSLA	1999	-0.000	0.141***†††	0.112***†††	0.321
		(0.007)	(0.023)	(0.018)	
Outstanding Monthly Debt (USD)	1999	1152.706	27.318	14.209	0.932
		(88.573)	(182.867)	(122.863)	
Regulated lender (USD)	1999	879.245	-82.864	-89.415	0.989
		(71.968)	(143.189)	(100.754)	
Non-regulated lender (USD)	1999	207.220	102.736	85.238*†	0.798
		(32.652)	(64.051)	(43.639)	
VSLA (USD)	1999	-0.000	14.70***†††	8.88***†††	0.141
		(1.049)	(3.101)	(2.249)	

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	-0.023	-0.019	-0.026	0.962
		(0.042)	(0.063)	(0.099)	(0.088)	
Food expenditure (log)	1827	4.723	-0.137	-0.078	-0.176	0.785
		(0.096)	(0.166)	(0.258)	(0.230)	
Health expenditure (log)	1827	-0.454	-0.368*	0.269	-0.785***††	0.018
		(0.134)	(0.213)	(0.340)	(0.272)	
Other expenditure (log)	1827	3.954	-0.082	-0.206	-0.001	0.333
		(0.056)	(0.091)	(0.151)	(0.128)	
Household assets index	1827	-0.000	0.013	0.110	-0.050	0.155
		(0.036)	(0.048)	(0.090)	(0.059)	
Housing quality index	1827	-0.000	0.125**†	0.149	0.110*	0.751
		(0.043)	(0.054)	(0.106)	(0.060)	
High quality material in walls	1827	0.043	0.011	0.014	0.009	0.850
		(0.008)	(0.010)	(0.021)	(0.011)	
High quality material in floor	1827	0.106	0.041**†	0.043	0.039*	0.927
		(0.011)	(0.017)	(0.032)	(0.021)	
High quality material in roof	1827	0.021	0.018**†	0.027	0.012	0.474
		(0.007)	(0.009)	(0.017)	(0.010)	

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

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

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

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
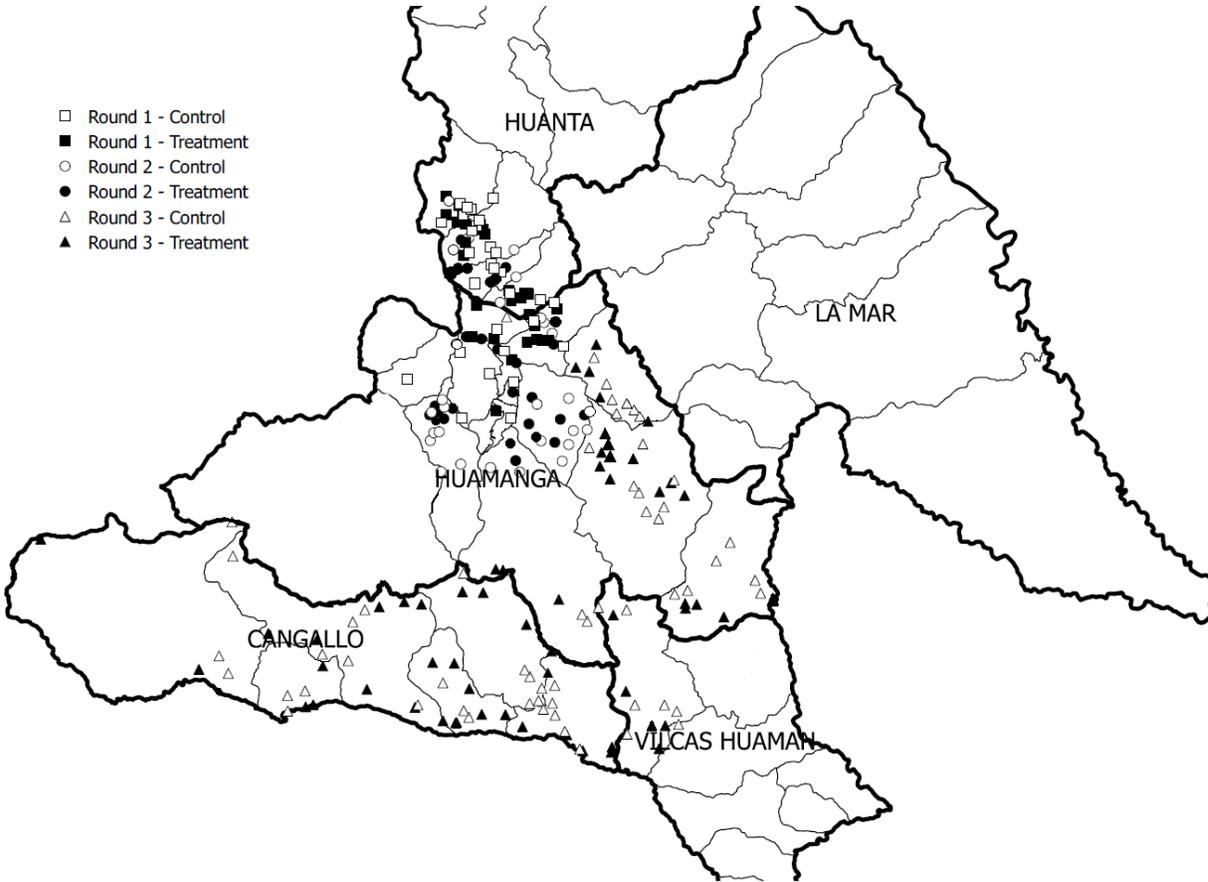
Note: The map represents the treated and control villages in the department of Ayacucho, including provinces Huanta, Huamanga, Cangallo and Vilcas Huaman. The black filled shapes represent the treated villages, while the unfilled shapes represent the control villages. The squares represents the first 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	-6.312	235
	[133.501]	[17.311]	
Percentage of households with adequate drainage service	0.692	0.007	235
	[0.249]	[0.035]	
Percentage of households that have electricity by public grid	0.358	0.027	235
	[0.327]	[0.029]	
Literacy rate	0.711	0.011	235
	[0.075]	[0.008]	
There is any health facility	0.171	0.016	235
	[0.400]	[0.047]	
There is a full equiped hospital	0.000	0.000	235
	[0.000]	[0.000]	
There is a health center	0.000	0.000	235
	[0.000]	[0.000]	
There is a posta	0.171	0.016	235
	[0.400]	[0.047]	
There is a school of any educational level	0.692	-0.031	235
	[0.464]	[0.064]	
There is a school of early childhood educational level	0.265	-0.011	235
	[0.443]	[0.057]	
There is a school of primary educational level	0.598	-0.056	235
	[0.492]	[0.058]	
There is a school of secondary educational level	0.137	-0.001	235
	[0.345]	[0.042]	

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

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

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

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
	Villages	*31*	*31*	*62*	*31*	*30*	*61*
Round 2	Individuals	281	260	541	235	216	451
	Villages	*27*	*27*	*54*	*27*	*27*	*54*
Round 3	Individuals	610	624	1234	494	489	983
	Villages	*62*	*62*	*124*	*62*	*61*	*123*
Total number of surveys		1169	1200	2369	942	885	1827
Total number of villages		*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**	0.002	0.025	-0.015
	(0.023)	(0.023)	(0.023)	(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, martial status, most spoken language at home, educational level, entrepreneurship level, level of financial knowledge, confidence level in people, friends and acquaintances, and participation in savings groups as controls. Columns (2) and (4) include only those surveyed in round 2 and 3. Errors clustered at the community level. *** $p<0.01$,** $p<0.05$,* $p<0.1$.

B Appendix: TOT Effects

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

	Observations	Control	TOT
Current savings balance (USD)	1827	132.787	62.319
		(13.004)	(112.737)
Saved formally	1827	0.059	0.008
		(0.006)	(0.057)
Regrets spending money instead of saving it	1827	0.752	-0.102
		(0.011)	(0.125)
Wants to save a little more	1827	0.728	0.024
		(0.011)	(0.124)
Hide the money so others don't spend it	1827	0.649	-0.045
		(0.012)	(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	0.123	0.032
		(0.012)	(0.085)	(0.100)
Regulated lender	1999	0.380	-0.091	-0.125
		(0.012)	(0.072)	(0.092)
Non-regulated lender	1999	0.156	0.011	0.027
		(0.009)	(0.069)	(0.077)
VSLA	1999	-0.000	0.557***†††	0.672***†††
		(0.004)	(0.050)	(0.052)
Outstanding Monthly Debt (USD)	1999	1199.148	303.577	104.400
		(66.875)	(438.721)	(560.626)
Regulated lender (USD)	1999	913.751	182.672	-467.856
		(54.322)	(341.282)	(437.394)
Non-regulated lender (USD)	1999	230.492	57.434	497.004**††
		(24.648)	(176.065)	(206.890)
VSLA (USD)	1999	-0.000	41.793***†††	60.580***†††
		(0.711)	(5.875)	(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	0.038	0.234**††	0.133
		(0.008)	(0.104)	(0.100)	
Regulated lender	3585	0.238	-0.099	-0.038	0.602
		(0.008)	(0.099)	(0.076)	
Non-regulated lender	3585	0.092	0.097	-0.094	0.048
		(0.005)	(0.065)	(0.072)	
VSLA	3585	-0.000	0.367***†††	0.524***†††	0.009
		(0.003)	(0.045)	(0.050)	
Outstanding Monthly Debt (USD)	3585	643.031	240.481	-258.469	0.377
		(33.431)	(486.300)	(313.625)	
Regulated lender (USD)	3585	481.212	-95.608	-454.366*†	0.441
		(27.019)	(397.565)	(237.375)	
Non-regulated lender (USD)	3585	110.966	328.115**††	145.337	0.263
		(10.802)	(133.571)	(120.829)	
VSLA (USD)	3585	0.000	18.56***†††	23.91***†††	0.302
		(0.230)	(4.093)	(3.565)	

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	0.034	0.044	0.959
		(0.012)	(0.106)	(0.161)	
Regulated lender	1999	0.380	0.056	-0.282**†	0.073
		(0.012)	(0.126)	(0.136)	
Non-regulated lender	1999	0.156	0.159	-0.087	0.102
		(0.009)	(0.124)	(0.091)	
VSLA	1999	0.000	0.646***†††	0.694***†††	0.595
		(0.004)	(0.058)	(0.077)	
Outstanding Monthly Debt (USD)	1999	1199.148	-202.022	343.743	0.649
		(66.875)	(1080.343)	(483.800)	
Regulated lender (USD)	1999	913.751	-597.827	-326.014	0.774
		(54.322)	(846.461)	(384.771)	
Non-regulated lender (USD)	1999	230.492	507.499	482.919**†	0.952
		(24.648)	(341.963)	(239.562)	
VSLA (USD)	1999	-0.000	72.40***†††	49.97***†††	0.103
		(0.711)	(10.552)	(10.321)	

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	0.092	-0.003	0.624
		(0.012)	(0.154)	(0.126)	
Regulated lender	1999	0.380	-0.144	-0.114	0.875
		(0.012)	(0.150)	(0.115)	
Non-regulated lender	1999	0.156	0.064	0.005	0.710
		(0.009)	(0.136)	(0.091)	
VSLA	1999	0.000	0.795***†††	0.599***†††	0.111
		(0.004)	(0.111)	(0.054)	
Outstanding Monthly Debt (USD)	1999	1199.148	151.725	76.736	0.947
		(66.875)	(992.555)	(620.736)	
Regulated lender (USD)	1999	913.751	-468.186	-467.662	1.000
		(54.322)	(767.219)	(506.936)	
Non-regulated lender (USD)	1999	230.492	578.249	449.399*†	0.762
		(24.648)	(373.823)	(231.928)	
VSLA (USD)	1999	-0.000	82.26***†††	47.78***†††	0.074
		(0.711)	(16.955)	(9.057)	

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	-0.120	-0.108	-0.127	0.978
		(0.031)	(0.326)	(0.521)	(0.418)	
Food expenditure (log)	1827	4.647	-0.713	-0.457	-0.861	0.819
		(0.072)	(0.869)	(1.371)	(1.119)	
Health expenditure (log)	1827	-0.638	-1.926*	1.260	-3.755***††	0.023
		(0.101)	(1.117)	(1.832)	(1.350)	
Other expenditure (log)	1827	3.922	-0.429	-1.105	-0.039	0.312
		(0.042)	(0.472)	(0.826)	(0.610)	
Household assets index	1827	0.027	0.067	0.574	-0.225	0.154
		(0.027)	(0.246)	(0.483)	(0.281)	
Housing quality index	1827	0.110	0.658**†	0.833	0.559*	0.684
		(0.032)	(0.297)	(0.612)	(0.303)	
High quality material in walls	1827	0.060	0.058	0.077	0.048	0.810
		(0.006)	(0.054)	(0.113)	(0.052)	
High quality material in floor	1827	0.122	0.211**†	0.237	0.196*	0.837
		(0.008)	(0.091)	(0.172)	(0.103)	
High quality material in roof	1827	0.040	0.094**†	0.147	0.063	0.459
		(0.005)	(0.048)	(0.103)	(0.048)	

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	-0.261*	0.074	-0.452**	0.080
		(0.009)	(0.147)	(0.229)	(0.188)	
Hunger	1827	0.367	-0.200	0.179	-0.418**†	0.061
		(0.012)	(0.144)	(0.265)	(0.177)	
Disease	1827	0.404	-0.188	-0.115	-0.231	0.693
		(0.012)	(0.142)	(0.237)	(0.175)	
Death	1827	0.088	-0.244***††	-0.297*	-0.214**†	0.640
		(0.007)	(0.082)	(0.155)	(0.091)	
Job loss	1827	0.413	-0.243	-0.045	-0.358*	0.354
		(0.012)	(0.160)	(0.279)	(0.191)	
Theft	1827	0.120	0.028	-0.020	0.056	0.645
		(0.008)	(0.080)	(0.151)	(0.084)	
Damage or loss at home	1827	0.197	-0.112	-0.336	0.018	0.162
		(0.010)	(0.116)	(0.211)	(0.145)	
Labor shortage	1827	0.391	-0.136	-0.008	-0.209	0.571
		(0.012)	(0.174)	(0.288)	(0.211)	
Crop loss	1827	0.654	-0.077	0.084	-0.169	0.481
		(0.012)	(0.171)	(0.291)	(0.210)	
Livestock loss	1827	0.313	-0.144	-0.237	-0.092	0.613
		(0.012)	(0.135)	(0.238)	(0.165)	

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

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

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.