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Abstract: We provide an experimental and theoretical evaluation of a cost-reducing innovation in the delivery of "self-help group" microfinance services, in which privatized agents earn payments through membership fees for providing services. Under the status quo, agents are paid by an outside donor and offer members free services. In our multi-country randomized control trial we evaluate the change in this incentive scheme on agent behavior and performance, and on overall village-level outcomes. We find that privatized agents start groups, attract members, mobilize savings, and intermediate loans at similar levels after a year but at much lower costs to the NGO. At the village level, we find higher levels of borrowing, business-related savings, and investment in business. Examining mechanisms, we find that self-help groups serve more business-oriented clientele when facilitated by agents who face strong financial incentives.

Keywords: Microfinance, Self-Help Groups, Privatized Delivery
JEL classification: O1,O12,O16

1 Introduction

Over the past several decades microfinance services have expanded tremendously in developing countries. An increasingly common method of providing access to microfinance to the "poorest of the poor" are self-help groups (SHGs). In their most common form, SHGs essentially act as tiny savings and loan cooperatives. Currently these SHGs reach an estimated 100 million clients and this number has grown dramatically in recent years; active plans will nearly double this number by 2017.[1] Groups are not fully "self-help", though. Although all funds are raised *internally*, the groups generally depend on *outside* assistance from administrative agents in their founding and continued administration. This motivates an important question: Can cost reduction or recovery in the delivery of *self*-help microfinance programs be effective in making them more fully self-help? The issue is common for many aid programs concerned with scalabilty, financial sustainability, and other types of aid, but the answer is not obvious. Recent research has shown that small costs to clients greatly reduce both take-up and program effectiveness.[2] We provide a theory and evidence that microfinance is different, even in very poor populations: A cost-recovery approach can actually be effective for SHGs.

The paper examines an innovation to the provision of NGO-sponsored microfinance services in three East African countries: Kenya, Tanzania, and Uganda. The status quo delivery mechanism was a typical "continuous subsidy" program, in which the NGO would train agents and then continually pay them a wage for starting up a fixed number of SHGs and providing financial services. In contrast, the innovation cut off payments to these agents after training, forcing them to become private entrepreneurs who start up any number of SHGs and earn their remuneration from their members. The hope was to not only lower costs to the NGO but also to expand access to services. Some programs already follow such an approach. A major World Bank/Indian government initiative with a goal of reaching 70 million new households is an important example.[3] Hence, the

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[1]The National Bank for Agriculture and Rural Development (NABARD) program in India alone has grown from 146,000 clients in 1997 to 49 million in 2010.

[2]See Kremer and Miguel (2007) for an example with deworming pills or Cohen and Dupas (2010)'s analysis of insecticide-treated bed nets. These health-related programs have positive externalities that finance does not.

[3]The Rural Poverty Reduction Program in Andhra Pradesh, India, was a nearly $300 million project between 2000 and 2009, which has trained 140,000 "community professionals" (privatized providers), and reached 9 million women through 630,000 SHGs. In 2010, it was expanded into a nationwide program, the National Rural Livelihoods Mission, which is spending a combined $5.1 billion from the Indian government and $1 billion from the World Bank over seven

types of program and innovation we study are both of great interest. We examine the impact of this delivery innovation using a randomized control trial and a theoretical model in which control areas received the status quo program, while treatment areas received the private entrepreneur innovation.

The results are powerful and encouraging for the prospects of self-help groups indeed being "self-help", in the sense of being financially independent. Our randomization allows us to estimate the causal impacts on outcomes. The number of groups started by the treated agents, who charge fees, are slower to grow initially. However, after one year they reach the same number of clients and have more-profitable groups than the control agents, who provide their services for free. Moreover, the privatization treatment improves the outcomes of clients along many dimensions, leading to higher levels of: savings from business activities; credit, especially to business owners; employees; and business investment. It leads to households spending a higher fraction of their time on their business, and correspondingly less in agricultural activities. These impacts are witnessed despite the fact that clients must pay for services under the private entrepreneur model. (Point-estimates of impacts on household income and consumption are insignificant.) Finally, the composition of the entrepreneurs' clientele is very different; the clients of the treated agents are more business-oriented and have a larger demand for financial services ex ante. Thus, the cost-sharing treatment appears to aim the program toward agents who are more likely to use the financial services for business purposes–the traditional goal of microfinance. Thus, cost-sharing could have important distributional consequences for NGOs with objectives beyond average impact and financial sustainability.[4]

To understand these results, we develop a theory in which cost recovery via membership fees can actually help solve an adverse selection problem that can plague credit cooperatives, especially when services are freely provided. Indeed, varying membership fees within a village can outperform a single membership fee. We provide suggestive evidence supportive of the role of membership fees: fees are strongly associated with greater levels of intermediation and agents appear to target their fees, varying them substantially both within and across villages. In contrast, we

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years (World Bank, 2007, 2012).

[4]We note two considerations, however. First, the entire population is quite poor. Second, we do not find that the program increases food vulnerability or negative responses to adverse shocks.

find no evidence that agent behavior along other dimensions drives these results. Because our experiment lacks variation in fees that is independent of other incentives, however, our results cannot definitively prove the proposed mechanism.

The specific variety of SHGs that we evaluate empirically are called SILCs (Saving and Internal Lending Committees). SILCs are promoted by Catholic Relief Services (CRS), a major non-governmental development organization, and are representative of other similar SHG programs sponsored by other agencies in the developing world.[5] In practice, SILCs are small groups of 10 to 25 members that typically meet on a regular basis to (1) collect savings, (2) lend to members with interest, (3) maintain an emergency "safety net" fund, and (4) share profits from lending activity. They do not receive external financial resources, only assistance from the outside agents who found and help administer the groups. In this sense, they effectively operate as small, independent, quasi-formal, self-financing credit cooperatives.[6]

Our empirical findings come from a large multi-country randomized controlled trial involving 276 agents who started a total of over 5,700 groups serving over 100,000 members across 11 districts in Kenya, Tanzania, and Uganda. All agents underwent a training phase. Upon completion, agents in the randomized treatment areas immediately became "Private Service Providers" (PSPs): entrepreneurs who need to start new groups and charge fees to group members in order to receive remuneration. In the control areas, field agents (FAs) received wages from CRS for establishing and administering a set number of groups but they were not allowed to charge their clients. This randomization was performed at a geographic level, so that treated agents did not compete with agents in the control group. Several sources of data verify that the randomization was indeed random.

In the post-treatment data PSP treatment increases group profitability by approximately 50 percent after one year. After three months, a PSP works with 4 fewer groups and 78 fewer clients, on average, than a traditional FA, but by one year the differences become statistically insignificant.

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[5]These agencies include CARE, OxFam, Plan, World Vision and perhaps most importantly, NABARD, a large government agency in India.

[6]The "self-help" goals of these groups are not limited to self-intermediation. (Indeed, mature groups in some regions actually leverage their funds through outside loans.) They are also intended to help local communities by building social capital, empowering women, and fostering improved collective action. These aspects–at least in the data we study–are relatively minor compared with the financial activities.

The total amount of savings, number of loans, total credit disbursed, and profits all show similar patterns: They start out lower but increase over time and after one year the difference is statistically insignificant. PSPs earn only one-sixth of what FAs earn over the first year, but their earnings increase over time and agent attrition is low (less than 2 percent in either group). Overall, PSPs are substantially more cost effective, reducing the costs of providing services by over 40 percent after two years.

When considering the innovation's benefits to households, the results are even more encouraging. Despite the fact that households actually pay for services, the PSP approach is significantly more effective in delivering outcomes promoted by microfinance. (We estimate these impacts at the village level, where assignment is random, without reference to membership which is clearly non-random). On an intent-to-treat, per-household basis, the PSP leads to nearly $27 (or 50 percent) more credit, $19 (or 90 percent) more business investment, 0.13 (110 percent) more employees hired, and 3 more hours per week (33 percent) in business. The point estimate of $15 (5 percent) more savings, is insignificant, but the $17 estimates for savings from business profits and savings for business purposes are significant.

Related Literature

This paper contributes to several strands of literature. First, we contribute to a literature on cost recovery in development programs. Previous research on health-related cost sharing (i.e., Kremer and Miguel (2007) for deworming pills, Cohen and Dupas (2010) for insecticide-treated bed nets, and the simulations of Kremer et al. (2011) for clean water sources) highlighted the problem of small costs lowering the number of clients served.[7] Morduch (1999) conjectured that an emphasis on cost recovery in microfinance would limit its ability to reach the poorest households. We find that although costs alter the type of clients, the number of members is unchanged (and the cost-savings itself is important to expanding programs elsewhere). Low take-up of health interventions can lead to lower impacts, even on remaining clients, because the interventions have positive externalities. In contrast, we find higher aggregate impacts, and our theory suggests that

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[7]Not all prior empirical evidence has been negative, however, even for services with a public-good aspect. In Argentina, Galiani et al. (2005) found that privatization of water supplies reduced child mortality, especially in poor areas.

financial services are qualitatively different than health interventions along this dimension.

Second, there are different theories of microfinance and a burgeoning empirical literature that has yielded mixed results regarding its impacts. Some theories follow the traditional narrative by modeling credit that enables entrepreneurship, investment, and growth (e.g., (Ahlin and Jiang, 2008), (Buera et al., 2012)), while others emphasize consumption smoothing or simply borrowing to increase current consumption (e.g., (Kaboski and Townsend, 2011), (Fulford, 2011)). Empirically, although there is evidence of very high returns to capital for some entrepreneurs,[8] with a few exceptions, the impacts of microfinance on consumption and entrepreneurial activity have generally been small (e.g., (Banerjee et al., 2015), (Crépon et al., 2011), (Kaboski and Townsend, 2012) (Karlan and Zinman, 2010)). Still, Kaboski and Townsend (2005) found that program details matter for impacts, as was the case for two recent studies with sizable impacts on businesses: Attanasio et al. (2011) and Field et al. (2009), who find impacts for joint liability loans and loans with repayment grace periods, respectively. We show that the delivery mode and incentives faced by institutions can greatly alter the entrepreneurial impact of microfinance.

Third, a large theoretical literature has examined credit markets under asymmetric information, including the design of cooperatives and lending groups (e.g., Banerjee et al. (1994), Ahlin and Townsend (2007), Wang (2013)). Seminally, Stiglitz and Weiss (1981) and De Meza and Webb (1987) analyze the impact of adverse selection on the provision of credit. The former show how it could lead to underprovision when entrepreneurs vary in the dispersion of returns, and the latter show that when entrepreneurs differ in their expected returns, adverse selection could lead to overprovision. Although our agents differ in their expected returns, as in De Meza and Webb (1987), our model has no room for overinvestment because we have a supply of funds in equilibrium, with the value of all projects exceeding their opportunity costs because agents with low returns also have low opportunity costs.[9] Our contribution is to show how two-part pricing can mitigate adverse selection in such a setting.

Finally, there is a recent literature on SHGs. Two recent randomized control trials of CARE's

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[8]For example, de Mel et al. (2008) finds returns of 55 to 63 percent annually, substantially greater than market interest rates.

[9]The membership fees we propose are distinct from collateral because they are sunk; i.e., they are not contingent on borrowing or repayment.

VSLA (Village Saving and Loan Associations) program found significant positive short-run impacts on food consumption in Malawi (Ksoll et al., 2012), and consumption, financial services, and assets in Burundi (Bundervoet, 2012). Evaluations of OxFam's SHG program are ongoing but have found fewer impacts.

The remainder of the paper is organized as follows. Section 1 describes the program, experiment, data, and methods. Section 2 presents the results and evidence of selection. Section 3 develops a simple theory of a credit cooperative and the potential impact of membership fees, and suggestive tests of the role of membership fees in the data. Section 4 concludes.

2 Program and Methods

This section describes the operation of the SHG programs we study. We then document the details of the experiment, our data, and our regression equations.

2.1 SILC Program and PSP Innovation

Recall that the SHGs promoted by Catholic Relief Services are called SILCs (savings and internal lending committees). A typical SILC is a group of between 10 and 25 members who meet regularly to save, lend to members, and maintain a social fund for emergencies. SILCs allow those with limited access to financial services to save and borrow in small amounts, while earning interest on savings and borrowing flexibly. SHGs have gained wide support among development organizations because, in contrast to many traditional microfinance institutions, they emphasize savings as well as credit. Research has shown that many people in developing countries lack adequate savings capabilities, and some even value savings accounts that pay negative interest (e.g., Dupas and Robinson (2012)).

The advantage over more formal financial institutions is that SILCs are formed and meet locally, allowing members to avoid transportation and transaction costs that are prohibitive for those who save and borrow small amounts. For SILCs in Kenya, Tanzania, and Uganda, meetings are generally weekly, with a median weekly deposit of $1.25. A typical loan would be $20 for 12 weeks at a 12-week interest rate of 10 percent. The loan would be uncollateralized except for the

personal savings in the fund.[10] Funds accumulate through savings, interest on repaid loans, and fines for late payments/other violations. These funds are held centrally. The funds follow cycles that generally last one year. All loans must be repaid at the end of each cycle, and the total fund is then temporarily dissolved with payouts to members made in proportion to their total savings contributed over the cycle. For SILC, the timing of payouts is typically arranged to coincide with school fees, Christmas, or some other time when cash is needed.

SILCs offer greater flexibility than rotating savings and credit associations (ROSCAs), and the fact that funds can accumulate in a SILC allows for some of its members to be net savers, while others are net borrowers. The greater flexibility also makes their management nontrivial. They require strict record keeping to keep track of savings, loans, loan payments of various amounts, and payouts due. They also require judgment regarding who should receive loans, how much they should receive, and how to set interest rates. Risks of default are also potentially greater, since some members may borrow disproportionately, and this magnifies the importance of decisions on membership. In contrast to ROSCAs, SILCs do not arise spontaneously. Given their complexity, the role of trained field agents in founding, administering, and training the members themselves is critical. The services provided by field agents to these groups include initial training and follow-up supervision in the areas of leadership and elections; savings, credit, and social fund policies and procedures; development of a constitution and by-laws; record-keeping; meeting procedures; and conflict resolution.

CRS has traditionally catalyzed this process by training field agents (FAs) to start SILC groups. FA trainees are recruited from the more educated segment of existing SILC members. They receive initial training, begin forming groups within a month, and then receive refresher training three additional times; they are also monitored by a supervisor over the course of a year.[11] During the training phase, agents are required to form 10 groups. At the end of the training phase, the agents take an exam; if they pass they are certified. FAs receive a monthly payment during the training phase ($48 in Kenya, $31.50 in Tanzania, and $50 in Uganda), but this payment increases after completion of the training phase (to $54, $59.50, and $65, respectively). The required stock of

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[10]Not all funds are lent out as loans; a portion is retained as a social fund available for emergency loans.

[11]Monitoring is done by checking over the constitutions and record books and occasionally sitting in on meetings of SILC groups of trainee FAs (generally at least once a month, rotating groups).

groups also increases by 10 additional groups, which they meet with regularly. Both during and after the training phase, agents must report quarterly summary accounting data for each group (e.g., group name, number of members, total loans, total credit, profits, payouts, defaults) following a standardized MIS system. Beyond this data collection, there is little additional oversight from CRS after the training phase.

CRS introduced the PSP delivery innovation into this existing SILC promotion program; in the new program, fully trained FAs are certified as such and transition to PSPs, private entrepreneurs who earn payment for their services from the SILC groups themselves rather than from CRS. PSPs negotiate their own payment from the SILC members, with the most common form of payment being a fixed fee per member collected at each meeting.[12] After certification, payments from CRS to PSPs are phased out linearly over four months (75 percent of the training payment in the first month, 50 percent in the second month, etc.). CRS' goal with this innovation is to lower the resources needed to subsidize SILCs, thereby improving both the long-term sustainability of the groups and CRS' ability to expand the program. The initial implementation of this delivery model was a large-scale Gates Foundation–funded program that involved training close to 750 agents to found roughly 14,000 SILCs and reach nearly 300,000 members. The FAs were recruited in three waves over three years, as different local partners (typically Catholic dioceses) in different regions of Kenya, Tanzania, and Uganda enter the expansion.

2.2 Experimental Design

The research focuses on the outcomes of a randomized set of FAs/PSPs from the first two of these waves. Agents in the first wave were recruited and began training in January 2009. This first wave was certified between December 2009 and January 2010. Agents in the second wave were recruited in either October 2009 (Kenya and Tanzania) or January 2010 (Uganda).[13] They were certified the following year, in October 2010 and January 2011, respectively. The second wave of agents represented an expansion of the program to new areas. After certification, those

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[12]For those groups that charge fees, the median quarterly fee per member is $0.50, which amounts to about 3 percent of the median member's quarterly deposits.

[13]The original plan was for all three countries to begin in October 2009, but the partners in Uganda experienced operational delays.

agents randomized as FAs earned monthly payments mentioned previously, which were chosen to compare well with anticipated PSP earnings after certification, and were required to start or assist 10 additional groups. (Unfortunately, PSP earnings fell short of these anticipations, as discussed in Section 4.1.)

The research includes data from multiple regions across Kenya, Tanzania, and Uganda. Within each region, a local partner supervised the implementation of the program in conjunction with CRS and our research team.[14] The randomization was stratified by country and assignment was done on a geographical basis, with all agents within a given geographical entity receiving the same assignment (FA or PSP). Treatment was assigned at the subdistrict level, with 50 subdistricts assigned to be served by the traditional FA program, and 108 subdistricts by the new PSP model. CRS had the goal of moving fully to the PSP delivery model in order to reduce costs, and all agents were recruited under the auspices of the PSP program. Both the partner organizations and their agents were notified of the particular randomized assignment just prior to certification. FAs did not remain FAs beyond the 12-month experimental phase, and out of concern for human subjects, the FAs were informed that they would transition to PSP assignment after 12 months.[15] The geographical levels were chosen to ensure that FAs would not compete against PSPs: sublocations in Kenya, wards in Tanzania, and subcounties in Uganda. (Within any area, PSPs could and did compete amongst themselves, however, including charging different fees.) The randomization was stratified by partner, with relatively more PSP regions.[16]

From among the expansion agents who were recruited, the initial sample included all agents who had not yet reached the certification step at the time of the initial randomization. The original year-1 sample included 51 agents in Kenya and Tanzania. In Kenya, the stratified randomization

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[14]The first-wave partners operated in Mombasa and Malindi (Kenya) and Mwanzaa and Shinyanga (Tanzania). Within Kenya, the second wave included expansion into Mombasa and Malindi, as well as new partners in Eldoret and Homa Hills. In Tanzania, the second wave expanded into three existing areas and added a partner in Mbulu. The Ugandan sample, all second wave, included partners in Gulu, Kasese, Kyenjojo, and Lira.

[15]In principle, this might reduce the likelihood of measuring differences between PSPs and FAs, since FAs may have behaved like PSPs (e.g., targeted clients, offering better services) in anticipation of this transition. However, while we do find significant differences in outcomes and selection, we do not find differences in targeting or services (see Section 2.3). Instead, we attribute the differences to fees, which were definitely not charged by FAs in anticipation.

[16]Relatively more of the geographical regions were assigned to PSPs for two reasons. First, the PSP program is less costly for the NGO. Second, the expectation was that the variance in outcomes would be higher under the PSP program. The second wave added relatively more agents into the evaluation sample, but similar numbers of FAs were chosen across each sample in an attempt to spread the costs of randomization. Because the randomization was done at a geographical level, the ratios of FAs to PSPs are not necessarily consistent across partners or countries.

yielded a total of 9 PSPs and 9 FAs spread across two partners, while in Tanzania there were 20 PSPs and 13 FAs spread across two partners. The year-2 sample included 225 agents from Kenya, Tanzania, and Uganda. In Kenya there were 71 PSPs and 24 FAs spread across four partners, in Tanzania there were 44 PSPs and 19 FAs spread across three partners, and in Uganda there were 41 PSPs and 26 FAs spread across four partners.[17]

One downside of the experiment is that it lacks a "true" control, in the sense of a set of villages receiving no SILCs whatsoever. Unfortunately, from an evaluation design perspective, CRS declined to create pure control groups. For that reason, we can only make statements about impacts of the PSP program relative to the FA variety, but we have no experimental evidence on absolute impacts.

2.3   Data

Data were collected from four sources. The first, the MIS system, collects book-keeping accounting data at the level of SILC group. These group-level data (collected quarterly) include total membership, savings, credit, losses, interest rates, profitability, and payouts, as well as agent name and village. In order to pool the data across countries, we use exchange rates to put currency values into dollar equivalents. We analyze these data at the level of the SILC groups, but we also aggregate to (1) the level of the FA/PSP agents who operate them and (2) the level of village and analyze at these levels.

The second source of data, an agent-level survey, supplements the MIS with agent-level characteristics (e.g., age, education, languages, work and family background, importance of FA income, and labor) as well as a smaller set of questions (e.g., on targeting of groups, time spent with groups, and negotiation of payments) collected every six months; additional group-level data were collected every six months covering membership characteristics, delivery of services, and the com-

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[17]The randomization does not contain all of the recruited agents, particularly in the first year, for several reasons. First, the randomized evaluation was introduced somewhat late in the process (late December 2009). For the first wave, some partners had already certified their trained FAs as PSPs, and these were naturally excluded. Second, a small number were lost due to death or failure of the certification test. Finally, the initial randomized sample contained 268 agents, but unfortunately the agents from two of the partners in Tanzania had to be dropped from the sample after the partners ignored randomization assignments. These partners constituted 6 FAs and 8 PSPs in the first wave (from just one partner) and 29 PSPs and 6 FAs in the second wave.

pensation scheme. Unfortunately, response rates on this survey were relatively low, so the sample is not as large and may suffer from biases in response rates.

The third and fourth sources of data are based on a set of 192 randomly chosen villages. The villages were selected as follows. A subset of 192 agents was chosen among the full-year sample of 225 second-wave agents in April 2010.[18] During this time, the agents were all in their training phase and had yet to be notified of their random assignment. For each of the 192 agents chosen, a village was randomly selected from among the villages in which they operated at least one SILC. In May 2010, a key informant survey was administered to that village chief. This survey (the third source of data) collected data on village infrastructure and proximity to important institutions (schools, markets, health clinics, banks, etc.), chief occupations, history of shocks to the village, and, most importantly, a village census of households.

Our fourth source of data, a household survey, obtained representative village samples to enable a comparison of village means, without reference to membership in order to identify causal "intention to treat" impacts. The data were stratified over likely initial SILC members and non-members using the village census.[19] In June, July, and August of 2010, the baseline survey was conducted among 1,920 households in eastern Kenya, Tanzania, and Uganda, respectively. (One village in Uganda was inaccessible and could not be surveyed.) A resurvey of the same households was conducted in Kenya and Tanzania (along with the Ugandan village not surveyed in the baseline) in June and July of 2011, approximately nine months after the agents had received certification. Uganda was resurveyed in October of 2011 , also nine months after agents had received certification. The household survey contained detailed data on household composition, education, occupation and businesses, use of financial services (especially SILC), expenditures, income, response to shocks, and time use, as well as some gross measures of assets, indicators of female empowerment and community participation, and questions about risk-aversion and discounting.

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[18]These agents were stratified across country (83 of 96 in Kenya, 47 of 63 in Tanzania, and 62 of 67 in Uganda), but they were otherwise chosen randomly.

[19]Village censuses were matched with a list of known SILC members in order to select a sample for the fourth data source, the household survey. These members were members of agent groups during the training phase. From the list of SILC and non-SILC members, a sample of five households with matched SILC members and five households with no matched SILC members were chosen with weights assigned appropriately based on their proportions in the matched village census list. For households with matched SILC members, the respondent is the SILC member, while for the others it was generally the spouse of the head of household (appropriate since SILC members are disproportionately women). See Section A.10 in the online appendix for more detailed information on the construction of the weights.

Table A.1 in the online appendix presents some summary statistics in the baseline for households with and without SILC members. Although the population is quite poor, SILC members tend to be somewhat better off on a number of dimensions. Naturally, membership itself is endogenous, so this member vs. non-member comparison cannot separate the roles of selection and impact.

The data are high quality, but measurement error is always a concern with household-level survey data in a developing country. Our working definition of "household" relies on self-identification and is based on joint concepts: both eating from the same pot and living in the same home or compound. Among the data collected, expenditures, time use, and income are the most difficult to measure. Our measures of income probably suffer from the most measurement difficulties.[20] We focus on the respondent's income since it is presumably better measured, and respondents (many of whom are SILC members) are more likely to be affected directly by SILC.

2.4 Empirical Methods

We use simple regression methods tailored toward the different data sets. We first present our methods for estimating impact and then discuss our verification of the randomization.

2.4.1 Measuring Impact

Our estimation approaches differ slightly depending on the data source.

Agent and Group Impacts

For the agent-level data, we use the following regression equations:

$$Y_{idnt} = \alpha_{dt} + X_i\beta + \gamma wave_i + \delta PSP_n + \varepsilon_{itdn} \tag{1}$$

$$Y_{idnt} = \alpha_{dt} + X_i\beta + \gamma wave_i + \sum_{s=1}^{4} \delta_s PSP_{ns} + \varepsilon_{itdn} \tag{2}$$

Here, Y_{idnt} represents the outcome for agent i in district d, subdistrict n at time t. The outcomes we examine from the MIS data are total members, savings, number of loans, value of loans, profits, and agent pay. Here we control for several things by adding district-time fixed effects, α_{dt}; a dummy for

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[20]See Section A.10 in the online appendix for measurement of these variables.

the wave of agent i, $wave_i$; and the above agent i characteristics (gender, age, schooling dummies, number of dependents, and number of children), X_i. The variable PSP_n is a dummy that is positive for households in treatment villages during the four quarters of treatment, while PSP_{ns} is specific to quarter s. Given eight quarters of data, we look for both an overall effect, δ (equation (1)), and duration-specific treatment effects, δ_s (equation (2)), for each of the four treatment quarters.

For the group-level data, the data are no longer aggregated across agents. We use the identical regression, however, except i now represents group i. For these regressions, the standard errors on estimates are clustered by subdistrict.

Household-Level Data

For the household-level data, we simply have two cross-sections. Rather than first differencing, which could exacerbate measurement error, we simply add the baseline outcome variable as a control and estimate impact using the following regression equation:[21]

$$Y_{jdn} = \alpha_d + X_j\beta + \rho Y_{jdn,t-1} + \delta PSP_n + \varepsilon_{jdn}. \tag{3}$$

The outcomes Y_{jdn} for household j, living in district d and subdistrict n, depend on a district-specific fixed effect, the characteristics of the household X_j (gender; age and age-squared; schooling dummies; and the number of adult men, women, and children in the household), and the baseline value for the outcome $Y_{jdn,t-1}$. Again, δ is the measure of the treatment effect. For the household data, we cluster standard errors by subdistrict, the level of treatment. We have 147 subdistricts in the household data.

Here, the impact of treatment is evaluated at the village level, without reference to SILC membership. The primary reason for this is that SILC membership itself is naturally endogenous. A secondary reason is that the overall impact of SILC could involve spillover impacts (either positive or negative) on non-members.

In the results section, we focus exclusively on the estimates of δ and δ_s.[22]

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[21]Ignoring the panel aspect of the data and simply using the endline data expands the sample somewhat and produces very similar results.

[22]Section A.2 in the online appendix provides full regression results for a sample of each of the agent, group, and household regressions.

2.4.2 Baseline Randomization

The above methods rely on the exogeneity of the PSP treatment, PSP, which ought to follow from our randomization. We verify the randomization using several methods.

First, using the baseline data, we verify that the randomization was successful in terms of observables. We do this using three data sets: the village-level key informant data, the agent-level data (both MIS and agent characteristics), and the household data.

For the agent-level data, we focus on a simple regression on the data used for explanatory variables:

$$X_{i,n} = \alpha + \gamma wave_i + \delta PSP_n + \varepsilon_{i,n},$$

where i again indexes the agent and n indexes the subdistrict in which the agent operates. We present the results for our independent variables used below. We control for the wave using $wave_i$. PSP_n is a dummy for whether subdistrict n received the PSP program, so that $\delta = 0$ is the null for the test of random assignment. We cluster the standard errors by subdistrict, the level of randomization.

Table 1 shows the baseline estimates for agents operating in the treatment (PSP) and control (FA) areas. We see no significant differences in gender, age, languages spoken, or number of children or dependents across the two samples. We do, however, see a significantly higher fraction of PSPs receiving secondary education and a correspondingly lower fraction of PSPs with primary school completion as the highest schooling attained. We believe this to be a purely random result rather than a problem with the implementation of the randomization.

For the household-level data, we use only the first wave, and data are weighted appropriately (to account for the stratified sampling across likely members and non-members). Hence, a simple mean comparison suffices:

$$X_{j,n} = \alpha + \delta PSP_n + \varepsilon_{j,n}.$$

Here, j indexes household j, and the null of $\delta = 0$ is again the test for random assignment.

The top panel of Table 2 shows similar results for the household characteristics. Again, the assignment of treatment appears to have been random with respect to the underlying characteristics

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Table 1: Agent-Level Randomization Results

	Age	Gender	Primary	Primary Complete	Secondary	Tertiary	Languages	Children	Financial Dependents
PSP	-0.33	-0.06	0.00	-0.14***	0.12*	0.02	0.09	-0.33	-0.40
s.e.	(1.2)	(0.07)	(0.01)	(0.05)†	(0.07)	(0.05)	(0.08)	(0.36)	(0.6)
FA Mean	36	0.69	0.01	0.46	0.43	0.10	1.9	4.6	6.4
Obs.	223	227	226	226	226	226	227	227	226

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated coefficients for a regression of the stated outcome on a PSP dummy and the following controls: age, age squared, gender, dummies for schooling (i.e., primary completed, secondary, and tertiary with a baseline of less than primary complete), number of languages spoken, number of children, number of financial dependents, cohort, and location fixed effects. The regressions are weighted by sampling weights. Standard errors are robust and clustered by subdistrict.

of households, with the exception of education. Here, we see that the fraction of people whose highest attainment is primary school completion is significantly lower (0.08), and some of this is because the fraction with some secondary schooling is somewhat higher (0.02). Again, we believe this education result to be purely random.

We perform several exercises to ensure that our results are not driven by the higher schooling of either the agents or recipients in PSP areas. First, we include dummies for highest education attained in all regressions. Of course, if there are also significant differences in unobservables, this would not be sufficient. Second, the significant difference in education is concentrated in districts served by two partners: the Archdiocese of Mombasa in Kenya and Tahea in Mwanzaa, Tanzania. All of our significant results are robust to dropping these two areas, as Tables A.13 to A.17 in Section A.8.1 of our online appendix show. Third, we examine the impact of dividing the sample by average village education rather than PSP/FA treatment. We discuss those results below.

Finally, we verify that our outcomes from equation (3) do not show "impacts" in the baseline household data–i.e., prior to treatment. The bottom panel of Table 2 verifies this for the 27 outcome variables we examine. Again, we see that the differences across the control and treatment are small and insignificant. The only exception is income, which is substantially higher in the PSP villages. This is only significant at the 10 percent level, and, again, we believe it to be purely random.[23]

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[23]Randomization results for 23 village characteristics from the village key informant survey data also support that the randomization was indeed random (Table A.5 of the online appendix). These data include the village population, the presence of various infrastructure, services, and facilities in the village–including financial institutions–and whether the village had experienced various natural disasters in the past five years. The differences between the control

Table 2: Household-Level Randomization Results - Baseline Demographics and Outcomes

Demographics/ Controls	PSP Mean	PSP Std. Dev.	PSP Obs.	FA Mean	FA Std. Dev.	FA Obs.	PSP-FA Mean △
Age	43	13	1362	43	14	536	0.31
Age Squared	2006	1277	1362	2007	1336	536	29
Gender	0.61	0.49	1363	0.57	0.50	534	0.03
# Adult Men	1.6	1.1	1380	1.5	1.1	539	0.06
# Adult Women	1.5	0.92	1380	1.6	0.96	539	-0.05
# Kids	2.6	2.0	1380	2.7	1.9	539	-0.07
No Schooling	0.21	0.41	1363	0.22	0.42	534	0.01
Some Primary	0.23	0.42	1363	0.20	0.40	534	0.02
Primary Completed	0.41	0.49	1363	0.48	0.50	534	-0.08*** †
Secondary	0.12	0.32	1363	0.09	0.28	534	0.02
Tertiary	0.03	0.16	1363	0.02	0.13	534	0.01

Outcomes (measured pre-treatment)	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.	△ Coeff.
Total Savings	138	299	1380	134	291	539	-2.5
Savings for Business Owners	159	284	624	151	322	252	-8.6
Savings from Business Profits	30	169	1380	35	203	539	-5.1
Savings from Agric. Profits	31	216	1380	23	114	539	-0.26
Savings from Salary/wage	17	105	1380	16	153	539	-2.1
Savings used for New Agric. Activity	38	224	1380	38	170	539	-6.6
Savings used for New Non-Agric. Activity	9.2	125	1380	5.3	31	539	3.9
Savings used for Existing Business	22	201	1380	16	186	539	2.1
Total Credit	48	214	1380	42	273	539	3.0
Credit for Business Owners	61	231	624	40	108	252	14
Credit from SILC	4.1	17	1380	3.7	18	539	0.48
Credit from Formal Lenders	36	211	1380	26	269	539	5.8
Credit from Informal Lenders	9.0	26	1380	12	51	539	-3.3
Credit used for Agric. Activity	13	131	1380	6.8	56	539	4.9
Credit used to Expand Business	13	127	1380	6.1	46	539	6.6
Credit used to Start New Business	1.5	19	1380	1.4	17	539	0.14
Start New Business	0.27	0.44	1380	0.26	0.44	539	0.03
Business Investment	39	147	1380	41	143	539	-2.8
Hours spent in Business	15	19	1380	15	19	539	0.53
Non-HH Employees	0.29	2.0	1380	0.42	4.3	539	-0.12
Hours spent as Employee	16	18	1380	15	17	539	0.60
Agric. Investment	57	457	1380	44	126	539	5.8
Hours spent in Agric.	27	15	1380	27	15	539	-0.47

****, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and †
indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively.
The top panel presents the household randomization results for the demographic controls and the bottom panel presents
the outcome variables. The last column of the bottom panel shows the estimated coefficients from a regression of the
stated outcome on a PSP dummy and the following controls: age, age squared, gender, number of men, woman and
children in the household, dummies for schooling (i.e., some primary, primary completed, secondary, and tertiary with
a baseline of no schooling). All regressions utilize sampling weights. After weighting, the sample is representative
at the village level, including all households within FA or PSP villages irrespective of SILC membership. Standard
errors are robust and clustered by subdistrict.

2.4.3 Reasons for Impact

Using multiple methods, we explore three potential explanations for the impacts: (1) improved member selection by agents or households, (2) improved effort by agents, and (3) improved effort by members.

For the first explanation, our methodological approach is to include an interaction between a dummy for the PSP treatment with baseline variables, which are exogenous with respect to the randomized treatment, in regressions explaining endline membership. That is, we run

$$M_{jvn} = \alpha_v + X_j\beta + \eta_1 Z_j^{baseline} + \eta_2 PSP_n Z_j^{baseline} + \varepsilon_{jn}, \tag{4}$$

where M_{jvn} is endline membership (of household j in village v and subdistrict n) and $Z_j^{baseline}$ indicates various baseline household characteristics (income; business income; a dummy for whether the household had positive savings; a dummy for whether the household had positive hours in business; and dummies for whether the household's estimated linear discount factor and hyperbolic discount factor are above the median).[24] Note that the fixed effects α_v are village-specific, so that the coefficient of interest η_2 will be identified from within-village variation in membership.[25] The η_2 coefficient estimates differential selection in PSP villages, i.e., the extent to which endline membership is more closely related to $Z_j^{baseline}$ in villages with PSPs.

For the second explanation, the agent questionnaire gives several measures of agents' behavior, including how households were targeted for new groups (based on demand, need, proximity, local connections, etc.), and three measures of "effort": the frequency of services provided to the group, the type of services provided, and the distance traveled to the group. We examine these as outcome variables in the agent-level regression equations, equations (1) and (2) above. The only difference is that these data are only available every six months, so our time-specific estimates in equation (2) are semi-annual rather than quarterly.

FA villages and treatment PSP villages are all small and insignificant. The lone exception is animal disease within the past five years, which occurred in 41 percent of PSP villages but only 21 percent of FA villages, statistically significant at the 1 percent level.

[24] The hyperbolic (δ^{hyp}) and linear (β^{lin}) discount rates are estimated by using indifference valuations (V) between time 0 and time t using the following formulas:

$$V_0 = \delta^{hyper}\beta^t V_t.$$

We obtained two estimates using two sets of questions: (1) tradeoffs between 0 and 1 month together with 12 and 13 months and (2) 0 and 3 months together with 12 and 15 months. We used the average of the two estimates.

[25] Alternative regressions without these fixed effects yield very similar results.

For the third explanation, we have data on the total hours per week spent working from the household time-use data. Although admittedly limited, hours working does give us some information on the respondents' overall levels of effort.

3 Results

We evaluate the impacts of the PSP program on PSP agents and groups themselves first and then on households. Finally, we examine potential explanations for the differential impact of PSPs.

3.1 Impact on Agents and Groups

Table 3 presents the agent-level results for various measures. These coefficients can be interpreted as treatment effects on the agents and the overall level of services intermediated by them.[26] As we are performing multiple testing, we also include Bonferroni corrections at a statistical level of $\frac{\alpha}{m}$ where α and m represent the significance level and the number of regressions in each table, respectively. The first row presents the overall impact δ from equation (1). With the exception of agent payments, which are quarterly flows, the dependent variables are accumulated stocks. On average across the four quarters, PSPs start 2.6 fewer groups, reach 62 fewer clients, and earn $150 less in payments per quarter, all of which are significant at the 1 percent level. Based on these numbers, one might be skeptical that the PSP program will expand SILC services as well as the FA program will.

The remaining rows, which present the duration-specific estimates of δ_s from equation (2), offer stronger insight, however: PSPs start off more slowly than FAs, but they improve over time. This may be demand driven, as the PSP service is not free, but it may also be a supply side strategy of PSPs as they, for example, attempt to learn about their markets slowly. PSPs do significantly worse over the first three quarters in starting groups, reaching members, and intermediating loans, but these differences narrow over time and by the fourth quarter of treatment are not statistically distinguishable. Thus, by the end of the year, PSPs seem to be providing levels of services com-

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[26]With respect to the clients themselves, these treatment effects on the agents could encompass both the selection of different clients and causal impact on the clients. This estimation does not distinguish between the two.

Table 3: PSP Impacts on Agent-Level Outcomes

	Groups	Members	Savings	Loans	Loan Value	Profit	Earnings
All Quarters	-2.6***	-62***	-1090	-35*	-1050	-400	-150***
s.e.	(0.93) †††	(24) †	(800)	(18)	(730)	(310)	(5.0) †††
Quarter 1	-3.6***	-78***	-1150*	-46**	-1280**	-290	-160***
s.e.	(0.91)†††	(25)††	(650)	(18)†	(650)	(230)	(5.2)†††
Quarter 2	-2.4***	-63***	-1740*	-45**	-1720*	-870*	-150***
s.e.	(0.91)†	(23)†	(940)	(19)	(920)	(490)	(6.4)†††
Quarter 3	-3.1***	-70***	-1320	-37*	-1300*	-540	-140***
s.e.	(1.1)††	(27)†	(870)	(20)	(750)	(380)	(6.3)†††
Quarter 4	-1.6	-40	-170	-12	55	130	-150***
s.e.	(1.3)	(29)	(1090)	(22)	(910)	(370)	(4.9)†††
FA Mean	20	430	7610	230	7100	2140	180
Obs.	865	865	865	865	865	865	865

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated coefficients for a regression of the stated group-level outcome on a PSP (the randomized treatment) or PSP*Quarter dummy and the following controls: age, age squared, gender, number of languages spoken, number of children, number of financial dependents, dummies for schooling (i.e., primary completed, secondary, and tertiary with a baseline of less than primary complete), cohort, and location-date fixed effects. The regression is weighted by sampling weights. Standard errors are robust and clustered by subdistrict.

parable to the FAs'. Payment for PSPs remains lower than for FAs, however, with the gap in cumulative payments widening over time. Indeed, if we calculate the cumulative payment to PSPs at the end of the year, the average is $550 less than the $708 average cumulative earnings of FAs.[27]

The smaller effects on the level of services in the early years are driven entirely by the fact that PSPs have fewer groups. Indeed, running comparable regressions at the group level yields no statistically negative impacts of PSPs, even early on (Table A.7 of the online appendix). Over time, we see relative improvements in membership, savings, loans, and profits over time, even at the group level. By the fourth quarter, a PSP's typical individual group has more savings, more credit, higher profits, perhaps an extra member, and statistically indistinguishable agent pay per group. In relative terms, these group-level differences are considerable–nearly 50 percent higher than the control means for savings and profit and 25 percent higher for loan value.

In sum, PSPs appear to have slower starts, but within four quarters they appear to be statistically indistinguishable from FAs in terms of the number of groups they start, clients they reach, savings

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[27]Table A.18 in Section A.8.2 of the online appendix shows the unweighted regression in Table 3. The point estimates and significance levels are highly robust.

they mobilize, and credit their groups provide. They earn substantially less, especially starting out, but their groups are ultimately more profitable.

Nevertheless, given the PSP's substantially lower cost to the NGO relative to FAs, after only one year the PSP's costs per member reached are substantially lower than the FA's costs. In the training year, both FAs and PSPs earn an average of $518. In the year after certification, FA costs amounts to $714, while PSP costs are only the first-quarter phase-in value: $65. Thus, over two years, the cost of a PSP is just about half (i.e., $(518+65)/(518+714)$) of the cost of an FA. Since the cost of additional years is zero for PSPs, these numbers will almost certainly continue to fall over time. Averaged over the course of the first year, PSPs reach 62, or about 15 percent, fewer members (368 vs. 430), so the per-member cost of the PSP program is just over half (55 percent) of what the FA program costs. Since PSPs reach similar numbers of members by the end of the year, the relative cost of PSPs per-members reached will also almost certainly fall over time. If PSPs continue to grow relative to FAs, then their relative cost could fall even more rapidly.

A legitimate concern might be PSP retention, given their much lower earnings. So far, however, the drop out rates are very low across the board–1.6 percent (3 of 185) for PSPs and 1.1 percent (1 of 91) for FAs. In fact, this may indicate not that PSP pay is too low, but rather that FAs were paid more than the minimum amount needed to retain them.

3.2 Impact on Households

Although the PSP program appears to be cost effective in reaching households and providing services, another important question is whether it has similar effects on the households it reaches. We now turn to the household data to evaluate the relative impacts of PSP-run SILCs on households.[28] We first examine savings, credit, and productive decisions before examining the overall impact on income and expenditures.

Table 4 presents the impact estimates of δ in equation (3) for savings and borrowing behavior. With respect to savings (top panel), we see no significant impact on overall aggregate savings, notably including no effect on the savings of business owners, but the reported source and use of

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[28]Here we have the village-level analog to the interpretation caveat in footnote 26. The causal impacts on villages encompass both causal changes on household-level behavior (e.g., reason for saving) and changes in the relative importance of behavior (e.g., levels of saving) across households.

savings are both impacted. The PSP program leads to an additional $17 of savings (per household in the village) coming from business profits (significant at the 5 percent level), but it has no impact on the amount of savings coming from agriculture or wage income. Similarly, an additional $16 per household was saved by households that report using savings for existing businesses, and this estimate is significant at the 1 percent level. These estimates are substantial in percentage terms, amounting to increases of 120 percent and 400 percent, respectively, relative to the FA villages. Thus, PSPs seem to have important effects on reported business-oriented savings.

Table 4: PSP Impact on Endline Household Savings and Credit

			Source			Purpose		
PANEL I: Savings								
	Total	Business Owners	Business Profit	Sell Agric. Product	Salary or Wage	New Agric. Activity	New Non-Agric. Activity	Existing Business
PSP	15	-3.7	17**	-3.9	8.5	0.52	-2.2	16***
s.e.	(13)	(16)	(6.2)†	(8.7)	(5.3)	(11)	(2.5)	(4.7)†††
FA Mean	131	152	14	39	10	38	4.2	4.0
Sample Mean	141	153	24	37	15	37	2.6	15
Median	61	83	0	0	0	0	0	0
PANEL II: Credit								
	Total	Business Owners	SILC	Formal	Informal	Agric. Activity	Expanding Business	Start New Business
PSP	27**	23***	4.4**	15	8.6***	7.5***	9.0***	1.9
s.e.	(11)	(8.5)†	(2.0)	(9.7)	(2.9)††	(2.8)†	(3.0)††	(1.3)
FA Mean	41	32	7.6	22	10	4.6	3.5	1.7
Sample Mean	56	50	10	30	16	8.7	9.9	3.0
Median	11	15	0	0	0	0	0	0
Obs.	1891	865	1891	1891	1891	1891	1891	1891

*** , **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated "intent to treat" coefficients for a regression of the stated outcome on a PSP dummy (the randomized treatment), the baseline outcome and the following controls: age, age squared, gender, number of men, woman and children in the household, dummies for schooling (i.e., some primary, primary completed, secondary, and tertiary with a baseline of no schooling). The regressions are weighted by sampling weights. After weighting, the sample is representative at the village level, including all households within FA or PSP villages irrespective of SILC membership. Standard errors are robust and clustered by subdistrict.

Turning to credit (the bottom panel), we see that the PSP program led to substantially higher levels of borrowing. The estimate of $27 is an increase of over 60 percent relative to the FA mean, and it is significant at a 5 percent level. The comparably-sized estimate of $23 for reported business owners amounts to an even larger percentage increase, which is significant at the 1 percent level

despite the much smaller sample size. The additional credit does not come exclusively from SILC, although per-household levels of credit from SILCs are $4 higher in PSP villages (significant at the 5 percent level). We see that borrowing from informal sources actually plays a larger role, with a coefficient of $9 that is strongly significant at the 1 percent level.[29] The reported purpose for borrowing is also affected by the PSP program with an additional $8 of credit for agricultural activities and $9 of credit for existing businesses (both significant at the 1 percent level). Both of these are increases of over 100 percent. In contrast, there is no impact on credit for new businesses.[30]

Table 5 delves more deeply into the impact of PSPs on the productive decisions of households. In general, PSPs lead to relatively more positive impacts on business efforts but, if anything, fewer positive impacts on agriculture. We find no significant impact of the PSP program on new business starts (although the power of the test is clearly weak).[31] We do, however, find significant impacts on the intensive margins of business. Business investment rises by $20 per household in response to the PSP treatment. Thus, business investment under the PSP treatment is roughly twice its level under the FA control. Likewise, time spent in business is higher by three hours per week, a difference of about 33 percent relative to FA control villages. The number of non-household members employed by the households in the sample is low overall (0.19 per household), with most households employing no outside workers. Still, the coefficient of 0.13 employees per household, significant at a 5 percent level, represents an increase of over 100 percent relative to the FA level. We do not see a significant corresponding increase in the hours spent as an employee, however. Respondents may be less likely than other household members to work as employees. The point estimate is positive, but insignificant and small relative to the mean. Finally, we look at agricultural decisions. Although credit for stated agricultural activities had been positively impacted by PSPs,

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[29]While some studies have found that microfinance substitutes for or crowds out informal borrowing (e.g., (Banerjee et al., 2015)), other programs have led to increases (e.g., (Kaboski and Townsend, 2011), (Kaboski and Townsend, 2012)). It is possible that funds are either leveraged, relent to others, or that repayment itself leads people to borrow from additional sources.

[30]Table A.20 in Section A.8.2 of the online appendix shows the equivalent unweighted regressions from Table 4. On the savings side, we find similar point estimates on savings coming from business profits and savings going to an existing business with the same statistical significance levels. With regards to credit, we lose significance on credit for business owners and credit from SILC.

[31]The insignificant point estimate would indicate that the fraction of households starting new businesses in PSP areas was 5 percentage points higher. The rates of business ownership and business starts are high in the data. In the endline sample, 42 percent of households own a business and 24 percent reported starting a new business in the past 12 months.

the relative effect on agriculture investment is insignificant, and agricultural investment remains substantially larger than business investment. The lack of an impact on agriculture may be due to the fact that loan duration was typically shorter than crop cycles. PSPs lead to fewer hours spent in agriculture relative to FAs, however. The coefficient of –3 (hours per week per respondent) nearly offsets the positive impact on hours spent in business.[32]

Table 5: PSP Impact on Endline Household Productive Decisions

	Start New Business	Closed Business	Business Investment	Hours spent in Business	Employees (non-HH)	Hours spent as Employee	Agric. Investment	Hours spent in Agric.
PSP	0.05	-0.17***	20***	3.4**	0.13**	0.87	4.2	-2.8*
s.e.	(0.05)	(0.06)†	(5.7)†††	(1.5)	(0.05)	(1.5)	(9.9)	(1.4)
FA Mean	0.20	0.49	21	9.4	0.11	14	67	32
Sample Mean	0.24	0.42	35	12	0.19	15	69	29
Median	0	0	0	0	0	10	28	30
Obs.	1891	865	1891	1891	1891	1891	1891	1891

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated "intent to treat" coefficients for a regression of the stated outcome on a PSP dummy (the randomized treatment), the baseline outcome and the following controls: age, age squared, gender, number of men, woman and children in the household, dummies for schooling (i.e., some primary, primary completed, secondary, and tertiary with a baseline of no schooling). The regressions are weighted by sampling weights. After weighting, the sample is representative at the village level, including all households within FA or PSP villages irrespective of SILC membership. Standard errors are robust and clustered by subdistrict.

We have also examined two simple summary measures of welfare: income and expenditures. Unfortunately, however, the evidence is weak, and our experiment lacks power along these dimensions. The point estimates are substantial for total income, business income, total expenditures, and total consumption (roughly 25 percent increases in total income and business income, and roughly 10 percent in consumption and expenditures) but they are not significant, with the exception of total expenditures, and that is only at the ten percent level. Nonetheless, the fact that these estimates are not negative might be at least somewhat surprising, since clients must pay for their services out of pocket.[33]

One concern we had was that the PSP-led groups might have a greater impact on business-oriented behavior, but that this may come at the expense of other potential benefits of the program, such as consumption smoothing (through risk-sharing, credit, or dissaving in response to shocks)

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[32]Table A.21 in the online appendix shows the unweighted regressions for Table 5. We lose significance on business investment, the number of employees hired and hours spent in agriculture.

[33]The results are shown in Table A.8 in the online appendix.

or financing the purchase of durables. To evaluate this, we looked at several measures, including (1) the probability of "ever going to sleep hungry," (2) the probability of experiencing various adverse shocks, and (3) the "number of weeks until finances returned to normal" following an adverse shock.[34] The point estimates on PSP were generally positive, but none were significant. That is, we certainly do not see evidence that PSPs underperform along this dimension. Instead, we view the two programs as comparable along this front.

3.3   Reasons for Differential Impact

In this section, we explore potential mechanisms that may drive the observed impacts. We find strong suggestive evidence that PSP programs cater to different people, and very weak evidence for the effect of incentives on other dimensions.

Table 6 presents the results of endline membership regressions on baseline household characteristics and their interaction with the PSP treatment following equation (4). The top panel shows the results for regressions with village fixed effects that highlight within-village selection, while the results in the bottom panel combine both within- and across-village selection. Focusing on the top panel, the first row presents the estimates of the direct impact of the household characteristic ($\hat{\eta}_1$). It shows that in general, characteristics such as income, business income, positive hours working in business, positive savings, and discount rates do not strongly predict membership. The second panel shows the differential selection within PSP treated villages ($\hat{\eta}_1$). Here higher baseline incomes, higher baseline business incomes, spending time working in business, having positive levels of savings, and having higher hyperbolic discount factors (i.e., suffering less from hyperbolic discounting) were all associated with a higher probability of SILC membership in PSP villages. These impacts are both statistically and economically significant. The mean impacts on the probability of membership range from 0.02 (business income) and 0.20 (positive savings). Finally, our five measures are not independent, so the last column shows the results for the first principle component across all five measures. It is, again, strongly significant and quantitatively important.

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[34]The one exception was the probability of a business failure. If business investments are risky as our theory assumes, this is understandable, since the PSPs are making more business investments.

Table 6: Endline Membership Selection on Baseline Characteristics

	Income	Business Income	Positive Savings	Positive Hrs. in Business	Linear Disc. Factor, β	Hyperbolic Disc. Factor, δ	1st Princ. Comp.
Characteristic	-5e-05	-2e-05	-0.006	0.01	-0.12	-0.06	-0.01
s.e.	(4e-05)	(1e-04)	(0.08)	(0.06)	(0.08)	(0.04)	(0.02)
PSP*Characteristic	2e-04***	3e-04*	0.23**	0.12*	0.07	0.17***	0.10***
s.e.	(6e-05)††	(2e-04)	(0.09)†	(0.07)	(0.09)	(0.06)†	(0.03)††
Village FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
PSP	-0.08	-0.05	-0.30***	-0.11*	-0.10**	-0.08*	-0.03
s.e.	(0.05)	(0.05)	(0.10)††	(0.07)	(0.05)	(0.05)	(0.05)
Characteristic	-8e-05*	-6e-05	-0.17*	0.02	-0.16**	-0.02	-0.02
s.e.	(4e-05)	(8e-05)	(0.10)	(0.06)	(0.07)	(0.06)	(0.02)
PSP*Characteristic	1e-04**	3e-04*	0.30***	0.15**	0.11	0.09	0.09***
s.e.	(6e-05)	(2e-04)	(0.10)††	(0.07)	(0.08)	(0.06)	(0.03)††
Village FE	No	No	No	No	No	No	No
Obs.	1877	1877	1877	1877	1877	1877	1877

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated coefficients for a regression of household endline membership on on the stated baseline outcome, their interaction effects with a PSP dummy (the randomized treatment) and the following controls: age, age squared, gender, number of men, woman and children in the household, dummies for schooling (i.e., some primary, primary completed, secondary, and tertiary with a baseline of no schooling). The regressions are weighted by sampling weights and are reported both with and without village fixed effects. After weighting, the sample is representative at the village level, including all households within FA or PSP villages irrespective of SILC membership. Standard errors are robust and clustered by subdistrict.

Table 6 represents only a selection of the most salient results from our selection analysis. We conducted a wide variety of alternative specifications and included many different selection variables (i.e., $Z_j^{baseline}$) such as hours spent in agriculture or wage labor, presence and levels of credit, levels of savings, business investment, expenditures, and consumption. We also ran separate regressions, replacing membership with dummies for leavers (baseline members but endline non-members) or joiners (baseline non-members but endline members) separately and running regressions without fixed effects. Significance varied considerably across these different exercises, but both the significant results and the insignificant point estimates overwhelmingly support the story of the results presented in Table 6. The PSP groups appear more attractive to agents who are wealthier (i.e., who have higher consumption, higher savings and credit) and more business-oriented (i.e., who have fewer hours in wage labor and agriculture and more investment). Finally, we note that the fact that the results are equally strong when village fixed effects are included suggests that the selection we uncover is driven by patterns within the village, not differential

membership rates across villages.

Nonetheless, we investigate alternative hypotheses. The first alternative hypothesis is that PSPs behave differently in either their targeting or their effort. The top panel of Table 7 shows no evidence that PSPs behave differently in targeting services to villages closer to their home (proximity), villages with which they have existing connections, or villages with greater perceived need (potentially driven by altruism) or demand (presumably driven by profit). In the bottom panel, we find no evidence of greater effort as measured by distance traveled to SILCs or number of services provided. Indeed, the only significant estimates in Table 7 show that PSPs are more likely to work only part-time, less likely to work more than part-time, and more likely to meet with their groups at least biweekly. It is nonetheless possible that unobserved effort varied across the two regimes.

Table 7: Impact of PSP Treatment on Agent Effort/ Behavior

Targeting	Proximity	Connections	Need	Demand	Other		
All Quarters	0.02	-0.03	-0.04	-0.03	-0.01**		
s.e.	(0.06)	(0.05)	(0.04)	(0.04)	(0.00)		
Quarters 1 & 2	0.01	-0.04	0.01	0.02	-0.01		
s.e.	(0.05)	(0.06)	(0.05)	(0.06)	(0.01)		
Quarters 3 & 4	0.03	-0.03	-0.10*	-0.08	-0.01		
s.e.	(0.09)	(0.07)	(0.05)	(0.07)	(0.01)		
FA Mean	0.32	0.54	0.84	0.30	0.01		
Obs.	3816	3816	3816	3816	3816		
Effort / Work Time	Average Distance	Average # Services	Work l.t. Half Time	Work Half Time	Work g.t. Half Time	Work Full Time	Biweekly Meetings
All Quarters	0.43	-0.05	0.25***	-0.11	-0.17*	0.02	0.08**
s.e.	(0.63)	(0.27)	(0.08)††	(0.09)	(0.09)	(0.04)	(0.04)
Quarters 1 & 2	-0.38	0.11	0.31***	-0.14	-0.24*	0.06	0.04
s.e.	(0.67)	(0.29)	(0.10)††	(0.13)	(0.12)	(0.05)	(0.06)
Quarters 3 & 4	2.3*	-0.42	0.13*	-0.04	-0.01	-0.08	0.13***
s.e.	(1.2)	(0.48)	(0.07)	(0.06)	(0.11)	(0.10)	(0.05)†
FA Mean	4.5	2.8	0.04	0.21	0.68	0.06	0.41
Obs.	146	146	135	135	135	135	3792

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated coefficients for a regression of the stated agent-level outcome on a PSP (the randomized treatment) or PSP*Half dummy and the following controls: age, age squared, gender, number of languages spoken, number of children, number of financial dependents, dummies for schooling (i.e., primary completed, secondary, and tertiary with a baseline of less than primary complete), cohort, and location-date fixed effects. The regressions are weighted by country-specific sampling weights. Standard errors are robust and clustered by subdistrict.

The second alternative hypothesis is that the clients themselves put forth more effort. To evaluate this hypothesis, we can only focus on the total time spent working per week. While the composition of hours was impacted by the PSP program (recall Table 5), the overall total number of hours was not significantly affected.

In sum, we find no evidence that either the agents or members worked harder in response to the incentives of the PSP program, but we do find substantial evidence that the PSPs provide services to a wealthier, more business-oriented population.

4   The Role of Membership Fees

Given the evidence that membership selection is based on business-oriented characteristics, we develop a model to provide a potential mechanism. More specifically, in Section 3.1, we develop a stylized model of a credit cooperative operating in the face of adverse selection where low-quality members can drive out high-quality members, lowering entrepreneurial activity (and aggregate output). Section 3.2 formulates the theoretical role of membership fees, showing that they can potentially solve this adverse selection and thereby increase entrepreneurial activity (and aggregate output).[35] We then empirically test the model predictions in Section 3.3 and we find that both agent intermediation outcomes and household outcomes are strongly associated with fees, consistent with the theory.[36]

4.1   Model

We develop a one-period static equilibrium designed to capture a group cycle. In practice, groups generally continue beyond the cycle, but neither members nor groups need to commit to future participation.[37] Since the outcomes of interest involve entrepreneurial activity–rather than consumption smoothing or risk-sharing, for example–we model agents with entrepreneurial projects

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[35]We present these results but relegate the technical derivations and proofs to our online appendix (see Section A.9).

[36]Thus the impact of our model credit cooperative is related to the charging of fees. A smoking gun empirical test of the model would involve randomizing membership fees. Lacking such exogenous variation - the model was developed ex post to understand the impacts.

[37]Bond and Rai (2009) show how the repeated game nature of lending groups can give incentives for future repayment in a model with strategic repayment. We rule out strategic repayment.

who, for simplicity and clarity, are risk neutral. In the data, we measured selection based on income, business activity, savings, and other measures of patience. In the model, we capture this using differences in average productivity and default. Although neither are directly observed in our data, higher productivity implies high average income and business income in the model, and the first principal component of our measures in the data is strongly negatively correlated with business failure and negative income shocks.[38]

4.1.1 Environment

Consider an economy with two stochastic project technologies that differ in their scale and productivity. The small-scale project requires one unit of capital, which it transforms, when successful, into $\underline{A}$ units of output. A large-scale project transforms $k > 1$ units of capital into $\overline{A}k$ units of output. The large-scale project is more productive in that $\overline{A} > \underline{A}$.

There is a unit measure of individuals, who are each endowed with one unit of capital for operating the technologies. All individuals have access to the small-scale project, but each person has access to the more productive large-scale project only with probability π. The individuals are divided into two types, $i \in \{L, H\}$ that differ in their inherent probability of success. A fraction of individuals, θ_L, have a lower probability of success in production, p_L, while the remaining $1 - \theta_L$ individuals succeed with probability $p_H > p_L$, where $p_L, p_H \in (0, 1)$. We assume that $\theta_L p_L < (1 - \theta_L) p_H$, so that the total number of potential successful type-H people exceeds the number of successful type-L people. When individuals fail, production yields zero output. In order to yield interesting results regarding adverse selection, we make the stronger assumption that $p_H \underline{A} > p_L \overline{A}$. That is, we assume the expected payoff of type-H individuals with the small-scale project exceeds that of type-L individuals with the large-scale project.

Since the large-scale project is more productive, but no individual is endowed with enough capital to operate it, there is potential demand for intermediation services. We model the timing and operation of a credit and savings cooperative as follows. Individuals decide whether to become members of a cooperative before finding out whether they have access to the large-scale technol-

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[38]The model has direct implications for default rates and returns on savings as well. Default rates are notoriously difficult to measure empirically, but we do find higher profitability among the PSP groups.

ogy. Members of a credit cooperative deposit their capital into the cooperative with a promised gross return of R_D. Individuals then find out whether they have access to the large-scale technology, and decide of whether to borrow at a gross interest rate of R_B, which equates demand for loans with available deposits. The cooperative is able to effectively distinguish between individuals borrowing for large-scale production and those borrowing for small-scale production. The member's type is unknown to the cooperative when it makes loan decisions, however, so that all borrowers pay the same borrowing rate. Unsuccessful members default on their loans and also forfeit their savings. Successful members repay their loans and then receive the return, R_D, on their savings, which effectively dissolves the fund.

Finally, consider that there is a minimum intermediation cost of C needed to remunerate the agent administrating the cooperative, but we assume that this cost is paid by an outside organization until Section 3.2.

4.1.2 Individual Decisions

Individuals simply maximize their expected income. A member of the cooperative receives in expectation

$$p_i \left(\overline{A}k - R_B k + R_D \right) \tag{5}$$

if she runs a large-scale project;

$$p_i \left(\underline{A} - R_B + R_D \right) \tag{6}$$

if she runs a small-scale project; and

$$R_D \tag{7}$$

if she simply saves. An individual not in the cooperative can neither invest in the large-scale activity nor save, so she simply earns

$$p_i \underline{A}$$

which we refer to as her outside option.

4.1.3 Equilibrium

An equilibrium must satisfy three conditions: (1) individuals' choices regarding joining the cooperative–whether and which project to undertake–must be optimal; (2) given R_B and R_D, the cooperative must earn zero profits; and (3) the market for funds must clear.

Given individuals' optimization and R_D, the demand for credit (per member) is a step function, involving willingness-to-pay thresholds that can be solved by equating the value of borrowing and investing (i.e., (5) or (6) for the large- or small-scale projects, respectively) with the value of simply saving (i.e., (7)) and solving for R_B.[39] Type-H individuals with the large-scale opportunity have the highest willingness to pay ($\overline{R}_{BH}$) while type-L individuals financing the small-scale project have the lowest willingness to pay ($\underline{R}_{BL}$). The ordering of the other thresholds depends on R_D and other parameter values. We focus on the interesting case, in which type-L individuals have a higher willingness to pay even though their expected payout is lower (i.e., $p_H \underline{A} > p_L \overline{A}$) because this case can lead to adverse selection. This arises because Type-Ls fail more often, and so they have less to gain, but limited liability (from less than full collateral) can give them a higher willingness to pay. Moreover, to simplify the analysis without losing the interesting features of the equilibrium, we focus on instances when market clearing is simplified because the total demand for loans from those with large-scale projects equals the supply of savings ($\pi k \to 1$).[40]

Defining f_L as the fraction of members of the cooperative who are type L, we can derive a break even condition for the cooperative showing that the borrowing rate must exceed the savings rate because of incomplete repayment:

$$\phi(f_L)R_B \quad = \quad R_D. \tag{8}$$

---

[39]The borrowing thresholds for type-i individuals for the large- and small-scale project, respectively, are

$$\overline{R}_{Bi} \quad = \quad \overline{A} - \left(\frac{1-p_i}{p_i k}\right)R_D$$

$$\underline{R}_{Bi} \quad = \quad \underline{A} - \left(\frac{1-p_i}{p_i}\right)R_D$$

[40]Formally, we make the following parameter assumption:

$$\pi k = 1 + \varepsilon.$$

where epsilon is an arbitrarily small, positive number, and we analyze the model under the conditions $\lim_{\varepsilon \to 0^+} \pi \overline{k} + \varepsilon$ and $\lim_{\varepsilon \to 0^-} \pi \overline{k} + \varepsilon$.

where $\phi(f_L) \equiv \frac{p_{avg}(f_L)}{\pi p_{avg}(f_L)+(1-\pi)} < 1$ is the effective repayment rate given the partial collateral of savings. Note that ϕ is increasing in f_L, since Type-Ls fail, and therefore, default more often.

We define $B(f_L; \tilde{p}_i)$ as the type-i individual's net benefit of joining the cooperative–i.e., the difference between the expected incomes of members and nonmembers in the cooperative. Given our simplifications, we can easily derive

$$
\begin{aligned}
B(f_L; \tilde{p}_i) &= \pi\tilde{p}_i \left[\left[\overline{A} - \overline{R}_{BL}(f_L) \right] k + \overline{R}_{DL}(f_L) \right] + (1-\pi)\overline{R}_{DL}(f_L) - \tilde{p}_i\underline{A} \\
&= \tilde{p}_i \left(\overline{A} - \underline{A} \right) + \left[(1 - \pi + \pi\tilde{p}_i)\phi - \tilde{p}_i \right] \overline{R}_{BL}(f_L),
\end{aligned}
$$

where $\tilde{p}_i$ indicates the success probability of the particular individual (see Section A.9 in the online appendix). There are two forces at work in this equation. One force, clearly seen in the first term, is the fact that the cooperative allows the more productive large-scale projects to be financed, which is always an advantage and becomes larger as the individual's probability of success increases. The second term captures the compositional force, which depends on the average success rate in the cooperative compared to the individual's own success rate. The smaller the average success rate, the larger the wedge between borrowing and savings rates. For type-H individuals, this force is (weakly) negative, while for type-L individuals it is (weakly) positive.

Examination of $B(f_L; \tilde{p}_i)$ leads to the major results formalized in the following proposition.

Proposition 1 *Given the assumptions above,*

(i) Type-L individuals always join, $B(f_L; \tilde{p}_L) > 0$.

(ii) Intermediate values of π exist at which type-H individuals won't join a cooperative of all type-L members, $B(1; \tilde{p}_H) < 0$, although they benefit more than type-L do from joining a cooperative of all type-H members, $B(0; \tilde{p}_H) > B(0; \tilde{p}_L)$.

Proof of the proposition is straightforward and given in Section A.9 in the online appendix, but we offer some simple intuition here. Type-L individuals can only do better by joining, since both of the above-mentioned forces are positive for them. A poor composition lowers the benefits of joining because higher default rates lower the savings rate relative to the borrowing rate (i.e., lower ϕ). This wedge matters more for type-H, however, since borrowers only pay the borrowing rate and earn the savings rate when successful, and they succeed more often. Moreover, the type-H

individuals have a higher outside option, so they benefit less from joining a cooperative with poor composition. Finally, when the composition is good, type-H individuals can have more to gain from financing large projects, since they succeed more often and earn a premium over the deposit rate.[41]

The left panel of Figure 1 shows these results graphically for an intermediate value of $\pi \in (\underline{\pi}, \overline{\pi})$. In such a case, although type-L individuals always join, type-H join only if type-L are less than some $\hat{f}_L$, defined by the root $B(\hat{f}_L; \tilde{p}_H) = 0$. If the proportion of type-L individuals in the population is high enough, $\theta > \hat{f}_L$, then type-H individuals never join and the equilibrium (denoted f_L^E) is $f_L^E = 1$. That is, all type-L join, but no type-H do.[42]
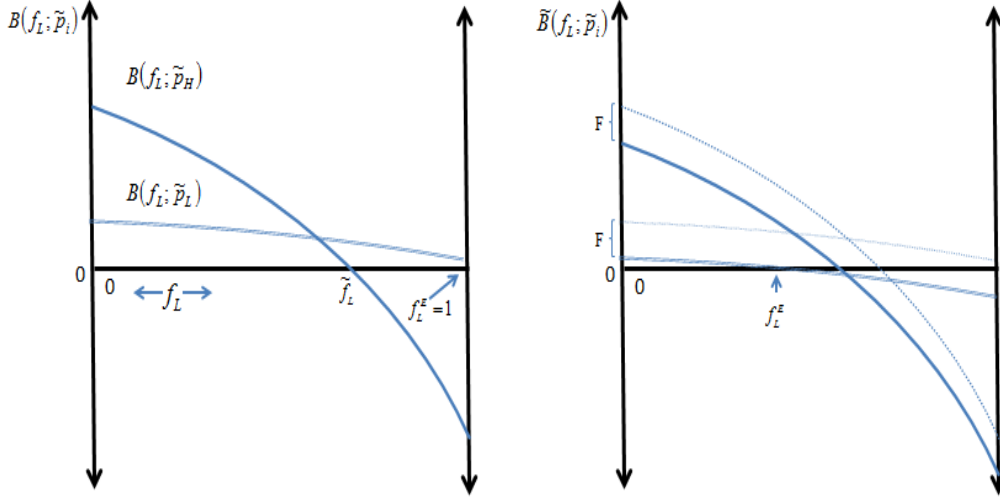
Figure 1: Benefits of Joining vs. Fraction Type-L

4.2 Theoretical Results on Membership Fees

Now consider the possibility of recouping the intermediation cost, C, by introducing a flat membership fee $F > 0$. We show how this could actually increase total output and the surplus of

---

[41]If π is too high, type-H will always join. If it is too low, their benefits of joining will not exceed Type-L's. The requirement for intermediate values of π underscores the fact that the results rely on individual's having uncertainty over being a net borrower or net saver. If the timing were such that individuals knew whether they had a large- scale project before joining, then type-H individuals with the large-scale project would always join.

[42]If $\theta < \hat{f}_L$, multiple equilibria exist: two stable equilibria at either $f_L = 1$ or $f_L = \theta$, and an equilibrium, $f_L = \hat{f}_L$, that is unstable to perturbations around $\hat{f}_L$. Of course, in all cases there are also additional trivial equilibria where no one joins.

members.[43]

In the left panel of Figure 1, the benefit at lower levels of f_L is higher for type-H individuals. They therefore have a higher willingness to pay for membership in a cooperative with lower levels of type-H members. A membership fee has the potential of driving out type-L individuals, thereby inducing type-H individuals to join. Define $\tilde{B}(f_L; \tilde{p}_i) = B(f_L; \tilde{p}_i) - F$. The membership fee, F, can ensure that the intersection of $\tilde{B}(f_L; \tilde{p}_H) = \tilde{B}(f_L; \tilde{p}_L)$ is less than zero. If the relative benefits of type-H are high enough, this can actually increase average income even net of payments. In such a case, illustrated in the right panel of Figure 1, the unique equilibrium value of f_L^E is at the point where type-L individuals are indifferent, $\tilde{B}(f_L; \tilde{p}_L) = 0$.[44]

We summarize this in the following proposition, and we give the details of parameter value requirements in Section A.9 of the online appendix.

Proposition 2 *There exist values of π, θ, and p_L satisfying Proposition 1 such that a membership fee, F, that induces some (or even all) type-L members not to join the cooperative will induce type-H members to join and increase the total income in the economy net of fees.*

Of course, if C is too large (that is, if it exceeds the potential benefits of type-H members, $(1 - \theta_L) B(0; \tilde{p}_H)$), requiring the cooperative to recoup costs through a flat membership fee would make the cooperative financially unsustainable.

Consider now the optimal policy, in the sense of maximizing total output. Total output is increasing in the number of individuals who finance the large-scale project, but the average output gain is larger for type-H individuals. The optimal single fee sets the intersection of $\tilde{B}(f_L; \tilde{p}_H) = \tilde{B}(f_L; \tilde{p}_L)$ in Figure 1 just below zero because it maximizes the number of type-L who enter, while also ensuring that all type-H enter. This fee leaves the members with no surplus, however.

---

[43]We do not include the cost, C, in the capital resource constraint of the model in order to maintain the simplicity of our stylized assumption that $\pi k - \varepsilon$ equals the amount of deposits available for loans. One could motivate these by an additional stylized assumption: introduce an initial endowment of $D > C$ output. We need to further assume that it cannot be used for investment, nor is it storable across the period of the model. Otherwise, the fund could demand this as collateral. We stress that entry costs differ from collateral in two important ways: (1) collateral is kept by the borrower in the case of repayment, and (2) for small π, entry costs are less than full collateral.

[44]Since total output is increasing in the number of agents who finance the large-scale project, the single fee that maximizes total output sets the $\tilde{B}(f_L; \tilde{p}_H) = \tilde{B}(f_L; \tilde{p}_L)$ just below zero. This maximizes the number of type-L who enter, while ensuring that all type-H enter. This leaves the members with no surplus, however. Since type-L members will never earn any surplus with the membership fee, the single fee that maximizes total surplus to members is the one that makes type-L members indifferent at $f_L = 0$.

Alternatively, we can solve for the equilibrium that maximizes total surplus. Under the assumption that $\theta_L < \frac{p_H}{p_L+p_H}$, type-$H$ members joining adds more to the surplus than type-L members joining. In this case, the fee that maximizes total surplus to members is the one that maximizes the surplus of type-H members. This is the lowest fee that keeps type-L members out; that is, it solves $\tilde{B}(0; \tilde{p}_L) = 0$. Call this F^*. The loss of type-L members does not lower the surplus because for any membership fee equilibrium in which type-H join, the surplus of type-L members is zero.

Finally, consider more flexible contracts that can achieve the first-best in the sense of maximizing total output by having everyone join the cooperative. A cooperative could effectively screen by offering two different contracts, $\{F, \phi\}$, which have the flavor of two-part tariffs. The cooperative can attract both types by offering a large F together with a large ϕ, which is attractive to type-H, and a small F together with a small ϕ, which is attractive to type-L.[45] Naturally, the output (net of fees) would be maximized since all individuals would be in the cooperative.

A similar equilibrium, where total output is maximized and everyone joins the cooperative, could also be achieved by starting two different cooperatives with different membership fees. The contracts that maximize the member surplus in the cooperative attracting type-H members would charge $F = F^*$ (and have $\phi(0)$ as an equilibrium, break-even value), and the contract maximizing member surplus in the cooperative attracting type-L would have $F = 0$ (and have $\phi(1)$ as an equilibrium, break-even value).

Denote output (net of fees) under this equilibrium with two different cooperatives as Y_2^*, output (net of fees) under the single F^* fee equilibrium as Y_1^*, and output under no fees as Y_0^*; the following proposition summarizes how the benefits of the program vary with fee structure in these three examples.

Proposition 3 *For values of π and θ satisfying Proposition 2, the maximum output under two fees exceeds that under the single fee, F^*. Likewise, the maximum output under a single fee, F^*, exceeds that under no fee ($Y_2^* > Y_1^* > Y_0^*$).*

Propositions 2 and 3 motivate a simple test in Section 3.3.

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[45]There are many such contracts that would accomplish this. There is also the possibility of adjusting R_B away from $\underline{R}_{BL}$, which we have focused on. In particular, any $R_B \in \left[\underline{R}_{BH}, \overline{R}_{BH}\right]$ for the first contract and $R_B \in \left[\underline{R}_{BL}, \overline{R}_{BL}\right]$ for the second would accomplish this. Since individuals are risk neutral, this only affects ex post inequality, not their ex ante valuation.

Finally, we note that while the varying membership fees could potentially increase the total surplus of individuals, by including both types, the true social surplus would be net of the cost of financial intermediation, C. For a very high C that exceeds the benefit of serving the type-H population, $(1 - \theta_L) p_H (\overline{A} - \underline{A})$, social surplus is maximized with no cooperatives and no members. For a very low intermediation cost, C, that is less than the benefits of serving the type-L population, $\theta_L p_L (\overline{A} - \underline{A})$, social surplus is maximized with two cooperatives and everyone served. For intermediate values of C, social surplus is maximized with only one cooperative serving the type-H individuals.

One might thus interpret the model as illustrating a rationale for potentially excluding the poorest of the poor from microfinance: The benefits of their receiving microfinance do not exceed the costs, and their participation may actually drive out potential recipients who would benefit more.

4.3 Empirical Evidence on Membership Fees

The model suggests a strong role for membership fees in leading to the relative impacts of PSPs. In this section, we examine this empirically by focusing directly on these fees.

Our agent-level data contains information on PSP payments by the groups themselves. Although these payments are not randomized and so are potentially endogenous, our model suggests that variation could be driven by differences in intermediation costs (C) across villages which could reflect actual time and labor costs or the time and labor costs net of any altruistic motive (e.g., family, friends). Variation in either could plausibly be exogenous.

We document substantial fee variation across groups.[46] The quarterly per-group fee is $5.80, on average; the standard deviation is 4.60; and the interquartile ratio is 3.1. Moreover, we find that 56 percent of fee variation occurs across villages, with just under half (44 percent) occurring within villages. Groups within the same village can be charged different fees, either by a single PSP or by different PSPs. PSP-specific fixed effects explain less than 40 percent of variation, however. Thus, the data indicate a high degree of price targeting within and across villages and by individual PSPs.

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[46]To match the timing of the household data, we use only those groups that charged fees in the fourth quarter of the randomization, and we trim the lower and upper 5 percent of outliers.

We perform two analyses to test the role of membership fees. First, we evaluate the extent to which financial intermediation in the agent data is associated with the charging of fees. Second, we examine whether the household outcomes are driven by the fee structure of villages.

4.3.1 Agent Outcomes

The model has predictions for economies of households, which we interpret as villages. To pursue the analysis further, we therefore aggregate the agent-level data by village and distinguish between villages where no fees are charged (413 villages), villages where a single uniform fee is charged (367 villages), and villages with variable fees–i.e., different groups are charged different fees (424 villages). We run regressions of the form

$$
\begin{aligned}
Y_{vdt} \;=\; & \alpha_{dt} + \gamma wave_d + \varpi_1 NoFeePSP_v \\
& + \varpi_2 UniformPSP_v + \varpi_3 VariablePSP_v + \varepsilon_{itdn,}
\end{aligned}
\tag{9}
$$

where Y_{vdt} are the same MIS outcomes (total members, total groups, savings, number of loans, value of loans, profits, and agent pay) aggregated by village, v. First, we run this regression using the per-group averages. The role of fees in the theory suggests that for the per-group averages, both ϖ_2 and ϖ_3 should be bigger than ϖ_1. That is, fees should enable higher levels of services, except perhaps for membership. Recall that these coefficients are all relative to the FA villages.

The results are shown in Table 8. They show how the typical group varies by the fees charged in the village. Except for loans, the positive significant estimates are all concentrated on the villages that charge fees, especially those with variable fees. Thus, fees seem to be closely related to the level of services that individual groups provide. Villages with uniform fees have significantly higher membership, and those with variable fees have significantly higher credit, membership, savings, and profits. The groups in PSP villages where no fees are charged are not statistically distinguishable from groups in FA villages. These PSP villages may be a combination of villages where PSPs offer free services out of social connections or altruism and villages in which PSPs anticipate introducing fees at a later date. Altruism would be an exogenous source of variation in the context of the model, while delayed fees might be potentially endogenous if correlated with willingness to pay, for example.

Table 8: Effect of "Village Type" on Per-Group Outcomes

	Members	Savings	Loans	Loan Value	Profit	Obs.
No Fee	-0.71	26	0.70	19	22	413
s.e.	(0.63)	(45)	(1.3)	(35)	(16)	
Uniform Fee	1.6**	26	0.26	44	14	367
s.e.	(0.69)	(33)	(1.1)	(31)	(8.3)	
Variable Fee	1.6**	110**	3.3	100**	20*	424
s.e.	(0.74)	(55)	(2.0)	(46)	(10)	
FA Mean	21	250	8.0	200	50	
Obs.	1760	1760	1760	1760	1760	

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated coefficients for a regression of the stated outcome (aggregated from the MIS group-level data to the village level and divided by the number of groups operating in the village) on a village type dummy, location-date fixed effects, and the cohort of the agent working in that village. The omitted village type is village served by an FA. The regressions are weighted by sampling weights. Standard errors are robust and clustered by subdistrict.

Second, we run regression (9) using the aggregate village totals. The theory suggests that the ability to vary fees should allow for more intermediation through a greater number of groups. Thus, ϖ_3 should exceed ϖ_2, even for membership. Again, these coefficients are all relative to the FA villages.

Table 9: Effect of "Village Type" on Total Outcomes

	Groups	Members	Savings	Loans	Loan Value	Profit	Obs.
No Fee	-0.43**	-9.1**	77	-1.3	18	64	413
s.e.	(0.19)	(4.2)	(140)	(4.2)	(110)	(55)	
Uniform Fee	-1.1***	-20***	-200**	-8.8***	-200**	-45**	367
s.e.	(0.14)†††	(3.4)†††	(88)	(3.3)††	(80)†	(19)	
Variable Fee	2.0***	46***	1020***	33***	960***	180***	424
s.e.	(0.23)†††	(5.2)†††	(210)†††	(7.4)†††	(180)†††	(35)†††	
FA Mean	2.4	51	610	21	530	120	
Obs.	1760	1760	1760	1760	1760	1760	

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated coefficients for a regression of the stated outcome (aggregated from the MIS group-level data to the village level) on a village type dummy, location-date fixed effects, and the cohort of the agent working in that village. The omitted village type is village served by an FA. The regressions are weighted by sampling weights. Standard errors are robust and clustered by subdistrict.

Table 9 presents the results and highlights the role of variable fees. Here, the positive estimates are almost exclusively in the villages where variable fees are charged. Indeed, uniform fees are associated with fewer groups, members, and services than in FA villages, but variable fees are

associated with more groups, members, savings, loans, credit, and profits. This is consistent with the theory that variable fees can cater to larger populations than uniform fees, yielding higher levels of intermediation and larger total impacts (recall Proposition 3).

4.3.2 Household Outcomes

Next, we examine whether the fee structure of villages appears to be related to household outcomes. Recall that we have household data for only a small subset of villages. Among these, only two villages charge a single uniform fee, so we cannot divide the sample into three groups. Instead, we simply distinguish between PSP villages where fees are charged (952 villages) and PSP villages where no fees are charged (318 villages).

We first examine whether the charging of fees is related to underlying baseline characteristics by running regressions of the form

$$Y_{jdn0} = \alpha_d + X_j\beta + \delta PSP_{fee} + \varepsilon_{jdn}$$

where the baseline (time=0) outcome for household j (living in district d and subdistrict n), Y_{jdn0} depends on a district-specific fixed effect, and the characteristics of the household, X_j (gender; age and age-squared; schooling dummies; and the number of adult men, women, and children in the household). PSP_{fee} is a dummy for PSP villages where a fee is charged. The coefficient δ is relative to those PSP villages where no fees are charged. We add the district-specific fixed effects to control for any regional heterogeneity, since our fee analysis cannot fall back on randomization for exogeneity.

Using the above equation, we examine 27 different outcomes.[47] For the vast majority of these, we find that the PSP villages are statistically indistinguishable from the baseline, with the exception that villages where fees are charged significantly have higher savings from business profit, have more credit from SILC and informal sources, and spend more hours in businesses than FA villages in the baseline. Our household results nevertheless control for any baseline differences, but we interpret our results as evidence that fee variation is reasonably exogenous.

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[47]The results are reported in Tables A.9, A.10, and A.11 in Section A.6 of the online appendix.

We then examine how the endline results in Tables 4 and 5 are concentrated among the different fee villages. We run regressions that account for any baseline differences and take the form

$$Y_{jdn1} = \alpha_d + X_j \beta + Y_{jdn0} + \delta_1 PSP_{no\,fee} + \delta_2 PSP_{fee} + \varepsilon_{jdn}$$

The endline outcomes Y_{jdn1} (for household j living in district d and subdistrict n), depend on a district-specific fixed effect; the baseline outcome, Y_{jdn0}; and the characteristics of the household, X_j (gender; age and age-squared; schooling dummies; and the number of adult men, women, and children in the household). $PSP_{no\,fee}$ is a dummy for PSP villages where no fee is charged, and PSP_{fee} is a dummy for PSP villages where a fee is charged. The coefficients δ_1 and δ_2 are all relative to the FA villages.

The results are reported in Tables 10 and 11, which are exact analogs to Tables 4 and 5. We find that the overall positive results from the PSP are driven by the PSP villages where groups are charged a fee. For example, in Table 10, villages with fees use more savings for existing business, have more credit going to business owners, obtain more credit from SILCs, and use more credit for agricultural activities and expanding existing business. The results in Table 11 are a bit more mixed, however. We find a more statistically significant increase in business investment in PSP villages with fees, but the point estimate is actually smaller. We find fewer hours spent in agriculture. However, we find lower rates of business closures in villages without fees, and more hours spent as employees.[48]

To summarize, we find that the relative gains in intermediation and household outcomes that we measure for PSPs generally appear to be linked to the charging of fees, though not in every analysis. Although fees (rather than other aspects of PSP behavior) seem to be closely related to impacts, the high level of variability in fees suggest that it might be difficult to replicate the results of the privatization scheme by using a centrally mandated uniform fee, for example. A randomization on the fees themselves would add greater insight into these questions, however.

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[48]Though not reported in the tables, we actually find a marginally significant positive impact in total household consumption in villages where fees are charged. We also note, however, that selection itself seems to occur in PSP villages regardless of whether fees are charged. Indeed, the differences between the fee and no-fee villages are not statistically significant. This is, admittedly, puzzling evidence for our fee interpretation.

Table 10: PSP Impact on Endline Household Savings and Credit by Village Type

			Source			Purpose		
PANEL I: Savings								
	Total	Business Owners	Business Profit	Sell Agric. Product	Salary or Wage	New Agric. Activity	New Non-Agric. Activity	Existing Business
No Fee PSP	29	-1.8	8.8	-11	14	-0.44	1.0	6.9
s.e.	(18)	(32)	(7.0)	(12)	(9.3)	(15)	(3.3)	(4.3)
Fee PSP	-10	-25	12*	-3.2	3.4	-9.7	-3.6	15***
s.e.	(14)	(17)	(6.3)	(9.1)	(4.4)	(11)	(2.9)	(5.3)††
PANEL II: Credit								
	Total	Business Owners	SILC	Formal	Informal	Agric. Activity	Expanding Business	Start New Business
No Fee PSP	16	0.72	3.8	2.2	9.9	-0.83	7.0	6.1
s.e.	(19)	(26)	(2.9)	(15)	(7.5)	(4.3)	(7.5)	(4.0)
Fee PSP	15	21**	6.0***	4.1	5.8*	4.9*	6.2**	0.87
s.e.	(11)	(10)	(2.3)†	(9.1)	(3.1)	(2.9)	(3.1)	(1.4)
Obs.	1731	779	1731	1731	1731	1731	1731	1731

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated "intent to treat" coefficients for a regression of the stated outcome on two dummy variables: PSP households with no fee charged and PSP households with either a uniform or variable fee charged, the baseline outcome and the following controls: age, age squared, gender, number of men, woman and children in the household, dummies for schooling (i.e., some primary, primary completed, secondary, and tertiary with a baseline of no schooling). The regressions are weighted by sampling weights. After weighting, the sample is representative at the village level, including all households within FA or PSP villages irrespective of SILC membership. Standard errors are robust and clustered by subdistrict. All regressions include country and district fixed effects.

Table 11: PSP Impact on Endline Household Productive Decisions

	Start New Business	Closed Business	Business Investment	Hours spent in Business	Employees (non-HH)	Hours spent as Employee	Agric. Investment	Hours spent in Agric.
No Fee PSP	-0.03	-0.25***	26*	2.9	0.15	3.8**	-8.6	-3.4**
s.e.	(0.06)	(0.10)†	(14)	(1.9)	(0.11)	(1.6)	(11)	(1.6)
Fee PSP	0.06	-0.09	14**	1.8	0.09	1.4	1.2	-5.5***
s.e.	(0.07)	(0.07)	(6.4)	(1.7)	(0.07)	(1.2)	(10)	(1.7)†††
Obs.	1731	779	1731	1731	1731	1731	1731	1731

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated "intent to treat" coefficients for a regression of the stated outcome on two dummy variables: PSP households with no fee charged and PSP households with either a uniform or variable fee charged, the baseline outcome and the following controls: age, age squared, gender, number of men, woman and children in the household, dummies for schooling (i.e., some primary, primary completed, secondary, and tertiary with a baseline of no schooling). The regressions are weighted by sampling weights. After weighting, the sample is representative at the village level, including all households within FA or PSP villages irrespective of SILC membership. Standard errors are robust and clustered by subdistrict. All regressions include country and district fixed effects.

5   Conclusion

We have presented evidence from a randomized trial of an innovation for privatized entrepreneurs who earn remuneration by charging fees for membership to their clients. The somewhat surprising empirical results indicate that through this cost-saving innovation, these microfinance services can indeed be "self-help" in the sense that, after initial training, the group administration can be financed through client-based fees. Relative to the continuously NGO-subsidized model, the private entrepreneurs expanded services more slowly but ultimately reached similar numbers of people and provide similar levels of services. The privately provided groups also had relatively stronger impacts in terms of the narrative microfinance dimensions of business entrepreneurship and investment.

As an example of a successful cost-recovery innovation–successful in terms of its intended goal of enabling NGO resources to stretch further, reaching greater numbers of people–the reasons for success are important for current and future microfinance programs. It does not appear that it was driven by the increased effort from improved incentives toward agents or members putting forth greater effort. Instead, it appears to be driven by privatized entrepreneurs catering to a more business-oriented population, who are willing to pay fees. We have developed a theory of the role of fees in mitigating adverse selection into credit cooperatives. Consistent with the theory, membership fees seem to be tightly linked to impact.

On the pro-side, this may target the services toward those who most benefit from them, and, indeed, better targeting may help improve the functioning of the groups. On the other hand, though, aid programs may still be interested in reaching those with a lower willingness to pay, who may be the truly poorest. Such tradeoffs may be more broadly important in moves toward sustainability or privatization. The distribution of benefits across the population and subpopulations is therefore an ongoing project of further investigation. A larger question is whether it is advisable to provide microfinance services to all populations. PSPs might improve welfare by limiting credit access to hyperbolic discounters, for example. Conversely, targeting populations that are more business-oriented may be riskier, potentially leading to future microfinance runs (Bond and Rai, 2009).

Another remaining question is whether privatization matters beyond the incentives to charge

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fees that it provides. If not, as the theory suggests, the favorable outcome and reduced costs could be attained by NGOs simply charging membership fees without privatizing. If the incentives do matter, then heterogeneous responses of PSPs to these incentives may provide insights. Unfortunately, our current data do not offer exogenous variation in payments or PSP behavior to further evaluate these issues. Experimental evidence on fees is an area for further research, as is examining the generality of these results for the provision of other financial services to the poor.

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Appendix For Online Publication

This is an online appendix of additional empirical results, robustness tests, and mathematical proofs for the paper, "Can Self-Help Groups Really Be Self-Help?" by Greaney, Kaboski, and Van Leemput. We have organized the results into the following sections: A.1) summary statistics by member/non-member of SILC, A.2) sample regressions, A.3) additional randomization results, A.4) group-level results, A.5) additional household results, A.6) baseline randomization across fee vs. no fee, A.7) endline results for different PSP villages, A.8) additional robustness results, A.9) the mathematical appendix for the model, and A.10) data description.

A.1 Summary Statistics by Member/Non-Member

Table 1: Summary Statistics SILC versus non SILC

	SILC		Non-SILC		SILC - Non-SILC
	Mean	Std. Dev.	Mean	Std. Dev.	Mean △
Savings	153	371	131	263	24
Credit	48	165	45	236	1.2
Income	289	485	356	665	-68*
Consumption	1429	1516	1428	1538	1.2
Business Owner	0.55	0.50	0.36	0.48	0.19*** †††
No Schooling	0.22	0.41	0.21	0.41	0.01
Some Primary	0.26	0.44	0.22	0.41	0.04*
Primary Completed	0.40	0.49	0.44	0.50	-0.04
Secondary	0.11	0.32	0.10	0.31	0.01
Tertiary	0.02	0.13	0.03	0.16	-0.01
Obs.	968		951		

***, and **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The table presents baseline mean comparison results for households with SILC members and households without SILC members. All results utilize sampling weights.

A.2 Sample Regressions

Table 2: Sample Agent-Level Regression

	Earnings
PSP * Quarter 1	-160***
s.e.	(5.2)†††
PSP * Quarter 2	-150***
s.e.	(6.4)†††
PSP * Quarter 3	-140***
s.e.	(6.3)†††
PSP * Quarter 4	-150***
s.e.	(4.9)†††
Age	0.56
s.e.	(1.7)
Age Squared	-0.00
s.e.	(0.02)
Gender	-1.4
s.e.	(4.1)
Primary Complete	7.9
s.e.	(9.9)
Secondary	14
s.e.	(11)
Tertiary	9.8
s.e.	(12)
Languages	-1.3
s.e.	(3.8)
Children	-0.04
s.e.	(0.69)
Financial Dependents	-0.23
s.e.	(0.37)
Cohort	-3.9
s.e.	(12)
Obs.	865
R Squared	0.88

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated coefficients for a regression of the stated group-level outcome on a PSP*Quarter dummy and the following controls: age, age squared, gender, number of languages spoken, number of children, number of financial dependents, dummies for schooling (i.e., primary completed, secondary, and tertiary with a baseline of less than primary complete), cohort, and location-date fixed effects. The regression is weighted by sampling weights. Standard errors are robust and clustered by subdistrict.

Table 3: Sample Group-Level Regression

	Profit
PSP * Quarter 1	8.3
s.e.	(9.9)
PSP * Quarter 2	-13
s.e.	(12)
PSP * Quarter 3	2.3
s.e.	(10)
PSP * Quarter 4	22**
s.e.	(11)
Age	4.7**
s.e.	(2.3)
Age Squared	-0.05*
s.e.	(0.03)
Gender	5.2
s.e.	(8.2)
Primary Complete	13
s.e.	(11)
Secondary	15
s.e.	(12)
Tertiary	35**
s.e.	(16)
Languages	2.7
s.e.	(6.0)
Children	-4.0**
s.e.	(1.9)
Financial Dependents	1.8
s.e.	(1.3)
Cohort	3.9
s.e.	(10)
Obs.	15,747
R Squared	0.03

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. ††† , †† , and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated coefficients for a regression of the stated outcome on a PSP*Quarter dummy and the following controls: age, age squared, gender, number of languages spoken, number of children, number of financial dependents, dummies for schooling (i.e., primary completed, secondary, and tertiary with a baseline of less than primary complete), cohort, and location-date fixed effects. The regressions are weighted by sampling weights. Standard errors are robust and clustered by subdistrict.

Table 4: Example of a Household-Level Regression

	Total Credit
PSP	27**
s.e.	(11)
Total Credit Baseline	0.15*
s.e.	(0.08)
Age	6.0**
s.e.	(2.3)†
Age Squared	-0.06**
s.e.	(0.02)
Gender	-4.2
s.e.	(13)
Some Primary	43**
s.e.	(19)
Primary Complete	5.9
s.e.	(8.5)
Secondary	129***
s.e.	(40)††
Tertiary	218**
s.e.	(88)
# Adult Males	-4.6
s.e.	(4.3)
# Adult Females	20***
s.e.	(7.5)†
# Children	-1.4
s.e.	(2.8)
Obs.	1891
R Squared	0.10

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated "intent to treat" coefficients for a regression of the stated outcome on a PSP dummy (the randomized treatment), the baseline outcome and the following controls: age, age squared, gender, number of men, woman and children in the household, dummies for schooling (i.e., some primary, primary completed, secondary, and tertiary with a baseline of no schooling). The regressions are weighted by sampling weights. After weighting, the sample is representative at the village level, including all households within FA or PSP villages irrespective of SILC membership. Standard errors are robust and clustered by subdistrict.

A.3 Additional Randomization Results

Table 5: Key Informant Mean Comparisons

	PSP			FA			PSP-FA
	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.	Mean △
Population	1292	1466	139	1120	1166	55	171
Power Grid	0.27	0.44	139	0.22	0.42	55	0.04
Months Inaccessible	2.8	3.8	139	2.6	2.9	55	0.22
Bank Distance	27	28	139	23	17	55	3.5
Primary	0.74	0.44	139	0.65	0.48	55	0.09
Secondary	0.36	0.48	138	0.34	0.48	55	0.02
Post Secondary	0.06	0.24	136	0.07	0.25	54	-0.01
Hospital	0.43	0.50	137	0.44	0.50	55	-0.01
Factory	0.06	0.23	137	0.05	0.23	53	.0004
MFI	0.14	0.35	136	0.23	0.43	52	-0.09
Bank	0.02	0.15	137	0.02	0.14	54	0.003
ROSCA	0.76	0.43	132	0.65	0.48	52	0.11
ASCA	0.66	0.48	123	0.61	0.49	49	0.05
SACCO	0.16	0.37	138	0.11	0.32	55	0.05
FSA	0.05	0.23	122	0.06	0.23	51	-0.004
Mobile Money	0.12	0.33	137	0.10	0.31	55	0.02
Moneylender	0.19	0.39	132	0.15	0.36	54	0.04
Drought	0.58	0.35	121	0.61	0.38	51	-0.03
Flood	0.49	0.35	92	0.55	0.38	36	-0.06
Crop Failure	0.51	0.34	88	0.52	0.39	37	-0.01
Animal Disease	0.41	0.32	68	0.21	0.24	30	0.20***
Bandits	0.29	0.31	36	0.19	0.24	20	0.10
Violence	0.77	0.32	12	0.67	0.45	6	0.10

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively.

Table 6: Randomization Results Excluding Mombassa and Tahea

	Age	Gender	Primary	Primary Complete	Secondary	Tertiary	Languages	Children	Financial Dependents
PSP	-1.7	-0.11	0.01	-0.09	0.05	0.03	0.02	-0.48	-0.26
s.e.	(1.3)	(0.08)	(0.01)	(0.05)	(0.08)	(0.07)	(0.09)	(0.44)	(0.71)
FA Mean	35	0.72	0.00	0.32	0.55	0.13	2.0	4.6	6.2
Obs.	182	185	184	184	184	184	185	185	184

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. ††† , †† , and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated coefficients for a regression of the stated outcome on a PSP dummy and the following controls: age, age squared, gender, dummies for schooling (i.e., primary completed, secondary, and tertiary with a baseline of less than primary complete), number of languages spoken, number of children, number of financial dependents, cohort, and location fixed effects. The regressions are weighted by sampling weights. Standard errors are robust and clustered by subdistrict.

A.4 Group-Level Results

Table 7: PSP Impacts on Group-Level Outcomes

	Members	Savings	Loans	Loan Value	Profit	Payment
All Quarters	0.26	46	0.57	19	5.5	-4.4***
s.e.	(0.59)	(50)	(1.0)	(25)	(9.0)	(0.89)†††
Quarter 1	0.22	21	-1.7	-12	8.3	-9.3***
s.e.	(0.62)	(58)	(1.3)	(31)	(9.9)	(0.63)†††
Quarter 2	0.06	16	0.53	-0.07	-13	-6.7***
s.e.	(0.59)	(53)	(1.1)	(24)	(112)	(0.80)†††
Quarter 3	0.33	39	1.3	17	2.3	-3.5***
s.e.	(0.64)	(53)	(1.3)	(29)	(10)	(1.0)†††
Quarter 4	0.37	96*	1.5	58*	22**	-0.77
s.e.	(0.63)	(51)	(1.4)	(33)	(11)	(2.0)
FA Mean	21	240	9.9	230	53	9.5
Obs.	16,289	15,747	15,747	15,747	15,747	14,907

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated coefficients for a regression of the stated outcome on a PSP or PSP*Quarter dummy and the following controls: age, age squared, gender, number of languages spoken, number of children, number of financial dependents, dummies for schooling (i.e., primary completed, secondary, and tertiary with a baseline of less than primary complete), cohort, and location-date fixed effects. The regressions are weighted by sampling weights. Standard errors are robust and clustered by subdistrict.

A.5 Additional Household Results

Table 8: PSP Impact on Endline Household Income and Expenditures

	Total Income	Business Income	Total Expenditures	Total Consumption
PSP	111	9.2	190*	169
s.e.	(84)	(12)	(112)	(107)
FA Mean	358	54	1600	1512
Sample Mean	451	62	1717	1613
Median	196	0	1394	1314
Obs.	1891	1891	1891	1891

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. ††† , †† , and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated "intent to treat" coefficients for a regression of the stated outcome on a PSP dummy (the randomized treatment), the baseline outcome and the following controls: age, age squared, gender, number of men, woman and children in the household, dummies for schooling (i.e., some primary, primary completed, secondary, and tertiary with a baseline of no schooling). The regressions are weighted by sampling weights. After weighting, the sample is representative at the village level, including all households within FA or PSP villages irrespective of SILC membership. Standard errors are robust and clustered by subdistrict. Note that income and outcomes were not completely balanced in the baseline.

A.6 Baseline Randomization across Fee vs. No Fee

This subsection shows initial differences for the PSP villages in which fees were and were not charged.

Table 9: Baseline Household Savings and Credit across Fee vs. No Fee Villages

			Source			Purpose		
PANEL I: Savings								
	Total	Business Owners	Business Profit	Sell Agric. Product	Salary or Wage	New Agric. Activity	New Non-Agric. Activity	Existing Business
Fee PSP	34	21	32***	28	-5.0	35	8.2	42
s.e.	(42)	(43)	(12)†	(28)	(14)	(37)	(6.3)	(26)
PANEL II: Credit								
	Total	Business Owners	SILC	Formal	Informal	Agric. Activity	Expanding Business	Start New Business
Fee PSP	7.3	-56	1.5*	0.60	5.1*	-8.9	-18	0.45
s.e.	(18)	(34)	(0.87)	(18)	(2.8)	(16)	(13)	(1.1)
Obs.	1237	555	1237	1237	1237	1237	1237	1237

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The sample includes only PSP villages. The results are estimated coefficients for a regression of the stated outcome on a dummy that signifies whether fees were charged in the PSP village (the baseline are PSP villages in which no fees were charged) and the following controls: age, age squared, gender, number of men, woman and children in the household, dummies for schooling (i.e., some primary, primary completed, secondary, and tertiary with a baseline of no schooling). The regressions are weighted by sampling weights. Standard errors are robust and clustered by subdistrict.

Table 10: Baseline Household Productive Decisions across Fee vs. No Fee Villages

	Start New Business	Business Investment	Hours spent in Business	Employees (non-HH)	Hours spent as Employee	Agric. Investment	Hours spent in Agric.
Fee PSP	-0.003	1.9	3.7*	0.01	0.10	-10	-0.35
s.e.	(0.06)	(12)	(2.1)	(0.17)	(2.5)	(19)	(1.4)
Obs.	1237	1237	1237	1237	1237	1237	1237

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The sample includes only PSP villages. The results are estimated coefficients for a regression of the stated outcome on a dummy that signifies whether fees were charged in the PSP village (the baseline are PSP villages in which no fees were charged) and the following controls: age, age squared, gender, number of men, woman and children in the household, dummies for schooling (i.e., some primary, primary completed, secondary, and tertiary with a baseline of no schooling). The regressions are weighted by sampling weights. Standard errors are robust and clustered by subdistrict.

Table 11: Baseline Household Income and Expenditures across Fee vs. No Fee Villages

	Total Income	Business Income	Total Expenditures	Total Consumption
Fee PSP	-42	15	146	155
s.e.	(85)	(15)	(140)	(133)
Obs.	1237	1237	1237	1237

 ***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. ††², ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The sample includes only PSP villages. The results are estimated coefficients for a regression of the stated outcome on a dummy that signifies whether fees were charged in the PSP village (the baseline are PSP villages in which no fees were charged) and the following controls: age, age squared, gender, number of men, woman and children in the household, dummies for schooling (i.e., some primary, primary completed, secondary, and tertiary with a baseline of no schooling). The regressions are weighted by sampling weights. Standard errors are robust and clustered by subdistrict.

A.7 Additional Endline Results for Different PSP Villages

This subsection shows the endline income and expenditures results for household across different

village types.

Table 12: PSP Impact on Endline Household Income and Expenditures

	Total Income	Business Income	Total Expenditures	Total Consumption
No fee PSP	23	12	20	4.2
s.e.	(125)	(20)	(111)	(99)
Fee PSP	216	18	166*	153*
s.e.	(162)	(14)	(93)	(91)
Obs.	1731	1731	1731	1731

 ***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated "intent to treat" coefficients for a regression of the stated outcome on two PSP dummies which represent PSP villages in which no fees and fees were charged respectively, the baseline outcome and the following controls: age, age squared, gender, number of men, woman and children in the household, dummies for schooling (i.e., some primary, primary completed, secondary, and tertiary with a baseline of no schooling). The regressions are weighted by sampling weights. After weighting, the sample is representative at the village level, including all households within FA or PSP villages irrespective of SILC membership. Standard errors are robust and clustered by subdistrict.

A.8 Additional Robustness Results

A.8.1 Results without Mombassa and Tahea

Table 13: PSP Impacts on Agent-Level Outcomes without Mombassa and Tahea

	Groups	Members	Savings	Loans	Loan Value	Profit	Earnings
All Quarters	-3.2***	-67**	-530	-31	-480	-100	-150***
s.e.	(1.0)†††	(28)	(820)	(22)	(790)	(260)	(6.1)†††
Quarter 1	-4.4***	-87***	-950	-46**	-1190*	-200	-160***
s.e.	(1.0)†††	(28)††	(680)	(21)	(710)	(240)	(6.0)†††
Quarter 2	-2.8***	-63**	-1010	-36	-950	-500	-140***
s.e.	(1.0)††	(27)	(870)	(23)	(920)	(360)	(8.1)†††
Quarter 3	-3.7***	-75**	-540	-38	-700	-120	-140***
s.e.	(1.1)†††	(32)	(870)	(25)	(860)	(280)	(8.0)†††
Quarter 4	-2.0	-43	350	-5.6	920	410	-150***
s.e.	(1.4)	(35)	(1280)	(26)	(1030)	(440)	(5.9)†††
FA Mean	21	460	7930	250	7410	2110	180
Obs.	715	715	715	715	715	715	715

 ***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated coefficients for a regression of the stated outcome on a PSP or PSP*Quarter dummy and the following controls: age, age squared, gender, number of languages spoken, number of children, number of financial dependents, dummies for schooling (i.e., primary completed, secondary, and tertiary with a baseline of less than primary complete), cohort, and location-date fixed effects. The regressions are weighted by sampling weights. Standard errors are robust and clustered by subdistrict.

Table 14: PSP Impacts on Group-Level Outcomes without Mombassa and Tahea

	Members	Savings	Loans	Loan Value	Profit	Earnings
All Quarters	0.65	68	0.14	35	12	-3.2***
s.e.	(0.59)	(60)	(1.2)	(30)	(10)	(1.0)†††
Quarter 1	0.47	35	-2.0	-5.5	13	-8.7***
s.e.	(0.65)	(66)	(1.5)	(35)	(11)	(0.60)†††
Quarter 2	0.61	36	0.14	18	-7.1	-5.6***
s.e.	(0.58)	(63)	(1.5)	(27)	(12)	(0.89)†††
Quarter 3	0.77	59	0.50	31	11	-2.1**
s.e.	(0.61)	(63)	(1.5)	(34)	(11)	(1.1)
Quarter 4	0.69	130**	1.4	82.22**	30**	0.81
s.e.	(0.60)	(63)	(1.7)	(41)	(12)†	(2.4)
FA Mean	21	250	10	240	53	9.1
Obs.	13,805	13,377	13,377	13,377	13,377	12,573

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated coefficients for a regression of the stated outcome on a PSP or PSP*Quarter dummy and the following controls: age, age squared, gender, number of languages spoken, number of children, number of financial dependents, dummies for schooling (i.e., primary completed, secondary, and tertiary with a baseline of less than primary complete), cohort, and location-date fixed effects. The regressions are weighted by sampling weights. Standard errors are robust and clustered by subdistrict.

Table 15: PSP Impact on Endline Household Savings and Credit Without Mombassa and Tahea

			Source			Purpose		
PANEL I: Savings								
	Total	Business Owners	Business Profit	Sell Agric. Product	Salary or Wage	New Agric. Activity	New Non-Agric. Activity	Existing Business
PSP	32**	2.5	15**	8.2	15***	6.5	0.10	19***
s.e.	(13)	(21)	(6.8)	(9.2)	(4.9)††	(10)	(1.7)	(5.8)††
FA Mean	105	132	18	34	8.2	29	2.3	5.1
Sample Mean	133	141	28	40	16	34	2.3	17
Median	49	64	0	0	0	0	0	0
PANEL II: Credit								
	Total	Business Owners	SILC	Formal	Informal	Agric. Activity	Expanding Business	Start New Business
PSP	32**	22**	4.8**	21*	7.4**	10***	8.9***	1.6
s.e.	(13)	(11)	(2.4)	(12)	(2.9)†	(3.7)†	(3.4)†	(1.3)
FA Mean	43	34	8.2	24	9.9	5.7	4.2	1.8
Sample Mean	62	53	11	34	15	11	11	2.7
Median	11	15	0	0	0	0	0	0
Obs.	1702	779	1702	1702	1702	1702	1702	1702

*** , **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated "intent to treat" coefficients for a regression of the stated outcome on a PSP dummy (the randomized treatment), the baseline outcome and the following controls: age, age squared, gender, number of men, woman and children in the household, dummies for schooling (i.e. some primary, primary completed, secondary, and tertiary with a baseline of no schooling). The regressions are weighted by sampling weights. After weighting, the sample is representative at the village level, including all households within FA or PSP villages irrespective of SILC membership. Standard errors are robust and clustered by subdistrict.The sample includes all households within FA or PSP villages - excluding villages in Mombassa and Tahea - irrespective of SILC membership.

Table 16: Household Productive Decisions Results Without Mombassa and Tahea

	Start New Business	Closed Business	Business Investment	Hours spent in Business	Employees (non-HH)	Hours spent as Employee	Agric. Investment	Hours spent in Agric.
PSP	0.07**	-0.23***	21***	4.3***	0.16***	-0.69	-6.9	-1.6
s.e.	(0.04)	(0.06)†††	(6.0)†††	(1.6)†	(0.05)††	(1.9)	(13)	(1.5)
FA Mean	0.22	0.46	47	13	0.23	17	87	26
Sample Mean	0.23	0.39	53	15	0.25	16	95	26
Median	0	0	0	0	0	10	16	24
Obs.	1702	779	1702	1702	1702	1702	1702	1702

*** , **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated "intent to treat" coefficients for a regression of the stated outcome on a PSP dummy (the randomized treatment), the baseline outcome and the following controls: age, age squared, gender, number of men, woman and children in the household, dummies for schooling (i.e. some primary, primary completed, secondary, and tertiary with a baseline of no schooling). The regressions are weighted by sampling weights. After weighting, the sample is representative at the village level, including all households within FA or PSP villages irrespective of SILC membership. Standard errors are robust and clustered by subdistrict.The sample includes all households within FA or PSP villages - excluding villages in Mombassa and Tahea - irrespective of SILC membership.

Table 17: Household Income Results Without Mombassa and Tahea

	Total Income	Business Income	Total Expenditures	Total Consumption
PSP	127	15	224	200
s.e.	(107)	(15)	(138)	(130)
FA Mean	398	61	1635	1540
Sample Mean	487	68	1741	1630
Median	161	0	1366	1252
Obs.	1702	1702	1702	1702

 ***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. [†††], [††], and [†] indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated "intent to treat" coefficients for a regression of the stated outcome on a PSP dummy (the randomized treatment), the baseline outcome and the following controls: age, age squared, gender, number of men, woman and children in the household, dummies for schooling (i.e. some primary, primary completed, secondary, and tertiary with a baseline of no schooling). The regressions are weighted by sampling weights. After weighting, the sample is representative at the village level, including all households within FA or PSP villages irrespective of SILC membership. Standard errors are robust and clustered by subdistrict.The sample includes all households within FA or PSP villages - excluding villages in Mombassa and Tahea - irrespective of SILC membership.

A.8.2 Unweighted Regressions

This subsection shows the unweighted endline results for both the agent, group and household data.

Table 18: PSP Impacts on Agent-Level Outcomes

	Groups	Members	Savings	Loans	Loan Value	Profit	Earnings
All Quarters	-2.8***	-65***	-1250	-39**	-1210	-440	-150***
s.e.	(0.94)††	(24)†	(830)	(18)	(740)	(320)	(4.9)†††
Quarter 1	-3.7***	-80***	-1190*	-48***	-1340**	-310	-170***
s.e.	(0.91)†††	(25)†††	(650)	(18)†	(640)	(230)	(5.2)†††
Quarter 2	-2.6***	-67***	-1890*	-49**	-1880**	-910*	-150***
s.e.	(0.91)††	(23)††	(960)	(19)†	(940)	(490)	(6.3)†††
Quarter 3	-3.2***	-74***	-1520*	-42**	-1480*	-590	-150***
s.e.	(1.1)††	(27)††	(900)	(20)	(770)	(390)	(6.1)†††
Quarter 4	-1.8	-43	-420	-17	-160	69	-150***
s.e.	(1.3)	(30)	(1150)	(22)	(920)	(380)	(5.0)†††
FA Mean	20	430	7610	230	7100	2140	180
Obs.	865	865	865	865	865	865	865

****, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated coefficients for a regression of the stated agent-level outcome on PSP, the randomized treatment, or PSP*Quarter dummy and the following controls: age, age squared, gender, number of languages spoken, number of children, number of financial dependents, dummies for schooling i.e. primary completed, secondary, and tertiary with a baseline of less than primary complete), cohort, and location-date fixed effects. Agent-level outcomes are aggregated from the MIS data group-level outcomes. All regressions are unweighted, standard errors are robust and clustered by subdistrict.

Table 19: PSP Impacts on Group-Level Outcomes

	Members	Savings	Loans	Loan Value	Profit	Payment
All Quarters	0.26	47	0.55	17	5.6	-4.4***
s.e.	(0.60)	(52)	(1.0)	(26)	(9.2)	(0.91)†††
Quarter 1	0.25	21	-1.8	-14	8.6	-9.5***
s.e.	(0.63)	(60)	(1.3)	(32)	(10)	(0.65)†††
Quarter 2	0.06	17	0.53	-1.7	-12	-6.8***
s.e.	(0.60)	(55)	(1.1)	(25)	(12)	(0.81)†††
Quarter 3	0.32	38	1.3	15	1.9	-3.8***
s.e.	(0.65)	(54)	(1.3)	(29)	(10)	(1.0)†††
Quarter 4	0.38	99*	1.5	57*	22**	-0.69
s.e.	(0.64)	(54)	(1.4)	(34)	(11)	(2.1)
FA Mean	21	240	9.9	230	53	9.5
Obs.	16,289	15,747	15,747	15,747	15,747	14,907

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated coefficients for a regression of the stated group-level outcome on a PSP (the randomized treatment) or PSP*Quarter dummy and the following controls: age, age squared, gender, number of languages spoken, number of children, number of financial dependents, dummies for schooling i.e. primary completed, secondary, and tertiary with a baseline of less than primary complete), cohort, and location-date fixed effects. All regressions are unweighted, standard errors are robust and clustered by subdistrict.

Table 20: PSP Impact on Endline Household Savings and Credit

			Source			Purpose		
PANEL I: Savings								
	Total	Business Owners	Business Profit	Sell Agric. Product	Salary or Wage	New Agric. Activity	New Non-Agric. Activity	Existing Business
PSP	26*	8.7	18**	5.8	8.3	15	-1.4	14***
s.e.	(15)	(26)	(8.6)	(13)	(5.8)	(14)	(1.6)	(5.4)[†]
FA Mean	129	163	32	53	18	41	3.4	14
Sample Mean	140	157	40	56	20	48	2.4	21
Median	53	63	0	0	0	0	0	0
PANEL II: Credit								
	Total	Business Owners	SILC	Formal	Informal	Agric. Activity	Expanding Business	Start New Business
PSP	26**	22	2.4	19	6.4**	13*	9.8*	5.2
s.e.	(13)	(14)	(2.9)	(12)	(2.9)	(7.8)	(5.4)	(3.6)
FA Mean	59	53	16	30	11	11	11	3.2
Sample Mean	77	80	18	43	15	18	21	5.8
Median	16	21	0	0	0	0	0	0
Obs.	1891	865	1891	1891	1891	1891	1891	1891

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. [†††], [††], and [†] indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated "intent to treat" coefficients for a regression of the stated outcome on a PSP dummy (the randomized treatment), the baseline outcome and the following controls: age, age squared, gender, number of men, woman and children in the household, dummies for schooling (i.e. some primary, primary completed, secondary, and tertiary with a baseline of no schooling). All regressions are unweighted, standard errors are robust and clustered by subdistrict.

Table 21: PSP Impact on Endline Household Productive Decisions

	Start New Business	Closed Business	Business Investment	Hours spent in Business	Employees (non-HH)	Hours spent as Employee	Agric. Investment	Hours spent in Agric.
PSP	0.02	-0.10**	12	2.7**	0.05	-1.0	14	-0.28
s.e.	(0.03)	0.05	(9.1)	(1.2)	(0.06)	(1.2)	(26)	(0.90)
FA Mean	0.23	0.49	43	13	0.22	16	85	27
Sample Mean	0.24	0.42	51	14	0.24	15	93	27
Median	0	0	0	0	0	12	20	25
Obs.	1891	865	1891	1891	1891	1891	1891	1891

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. [†††], [††], and [†] indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated "intent to treat" coefficients for a regression of the stated outcome on a PSP dummy (the randomized treatment), the baseline outcome and the following controls: age, age squared, gender, number of men, woman and children in the household, dummies for schooling (i.e. some primary, primary completed, secondary, and tertiary with a baseline of no schooling). All regressions are unweighted, standard errors are robust and clustered by subdistrict.

Table 22: PSP Impact on Endline Household Income and Expenditures

	Total Income	Business Income	Total Expenditures	Total Consumption
PSP	1.6	-0.33	197*	182*
s.e.	(176)	(21)	(118)	(107)
FA Mean	579	92	1867	1738
Sample Mean	596	92	2001	1857
Median	207	0	1564	1457
Obs.	1891	1891	1891	1891

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated "intent to treat" coefficients for a regression of the stated outcome on a PSP dummy (the randomized treatment), the baseline outcome and the following controls: age, age squared, gender, number of men, woman and children in the household, dummies for schooling (i.e. some primary, primary completed, secondary, and tertiary with a baseline of no schooling). All regressions are unweighted, standard errors are robust and clustered by subdistrict.

A.8.3 Household Endline Results - No Baseline Controls

This subsection shows the endline result for the household data without controlling for initial conditions.

Table 23: PSP Impact on Endline Household Savings and Credit

			Source			Purpose		
PANEL I: Savings								
	Total	Business Owners	Business Profit	Sell Agric. Product	Salary or Wage	New Agric. Activity	New Non-Agric. Activity	Existing Business
PSP	16	-3.0	16**	-3.5	9.3	0.27	-2.2	16***
s.e.	(16)	(21)	(6.6)	(8.8)	(6.0)	(11)	(2.5)	(4.7)†††
FA Mean	131	152	14	39	10	38	4.2	4.0
Sample Mean	141	153	24	37	15	37	2.6	15
Median	61	83	0	0	0	0	0	0
PANEL II: Credit								
	Total	Business Owners	SILC	Formal	Informal	Agric. Activity	Expanding Business	Start New Business
PSP	29**	27***	4.5**	17*	7.9***	7.8***	10***	2.1
s.e.	(11)†	(9.0)††	(2.0)	(10)	(3.0)†	(2.9)†	(3.1)††	(1.3)
FA Mean	41	32	7.6	22	10	4.6	3.5	1.7
Sample Mean	56	50	10	30	16	8.7	9.9	3.0
Median	11	15	0	0	0	0	0	0
Obs.	1891	865	1891	1891	1891	1891	1891	1891

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated "intent to treat" coefficients for a regression of the stated outcome on a PSP dummy (the randomized treatment) and the following controls: age, age squared, gender, number of men, woman and children in the household, dummies for schooling (i.e. some primary, primary completed, secondary, and tertiary with a baseline of no schooling). The regressions are weighted by sampling weights. After weighting, the sample is representative at the village level, including all households within FA or PSP villages irrespective of SILC membership. Standard errors are robust and clustered by subdistrict.

Table 24: PSP Impact on Endline Household Productive Decisions

	Start New Business	Business Closed	Business Investment	Hours spent in Business	Employees (non-HH)	Hours spent as Employee	Agric. Investment	Hours spent in Agric.
PSP	0.05	-0.17***	20***	3.5**	0.12**	0.97	4.5	-2.8*
s.e.	(0.05)	(0.06)†	(6.0)†††	(1.4)	(0.05)	(1.5)	(10)	(1.4)
FA Mean	0.20	0.49	21	9.4	0.11	14	67	32
Sample Mean	0.24	0.42	35	12	0.19	15	69	29
Median	0	0	0	0	0	10	28	30
Obs.	1891	865	1891	1891	1891	1891	1891	1891

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated "intent to treat" coefficients for a regression of the stated outcome on a PSP dummy (the randomized treatment) and the following controls: age, age squared, gender, number of men, woman and children in the household, dummies for schooling (i.e. some primary, primary completed, secondary, and tertiary with a baseline of no schooling). The regressions are weighted by sampling weights. After weighting, the sample is representative at the village level, including all households within FA or PSP villages irrespective of SILC membership. Standard errors are robust and clustered by subdistrict.

Table 25: PSP Impact on Endline Household Income and Expenditures

	Total Income	Business Income	Total Expenditures	Total Consumption
PSP	131	11	208*	184*
s.e.	(86)	(12)	(115)	(109)
FA Mean	358	54	1600	1512
Sample Mean	451	62	1717	1613
Median	196	0	1394	1314
Obs.	1891	1891	1891	1891

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively. The results are estimated "intent to treat" coefficients for a regression of the stated outcome on a PSP dummy (the randomized treatment) and the following controls: age, age squared, gender, number of men, woman and children in the household, dummies for schooling (i.e. some primary, primary completed, secondary, and tertiary with a baseline of no schooling). The regressions are weighted by sampling weights. After weighting, the sample is representative at the village level, including all households within FA or PSP villages irrespective of SILC membership. Standard errors are robust and clustered by subdistrict. Note that income and outcomes were not completely balanced in the baseline.

A.8.4 Mean Comparison Endline

This subsection shows the endline mean comparison results for the agent-, group- and household-level data without any controls.

Table 26: Mean Comparison of PSP Impacts on Agent-Level Outcomes

	Groups	Members	Savings	Loans	Loan Value	Profit	Earnings
All Quarters	-2.4*	-49*	-630	-26	-490	-280	-150***
s.e.	(1.2)	(29)	(860)	(23)	(950)	(360)	(3.5)†††
Quarter 1	-5.1***	-120***	-2650***	-78***	-2620***	-650*	-170***
s.e.	(1.2)†††	(27)†††	(800)†††	(21)†††	(910)††	(390)	(3.1)†††
Quarter 2	-3.5***	-75***	-1760**	-46**	-1600*	-700**	-150***
s.e.	(1.2)††	(28)†	(840)	(22)	(900)	(340)	(4.6)†††
Quarter 3	-1.6	-27	-360	-0.80	150	-330	-140***
s.e.	(1.3)	(29)	(900)	(24)	(970)	(360)	(4.7)†††
Quarter 4	0.43	23	2240**	19	2080*	570	-150***
s.e.	(1.4)	(32)	(1030)	(26)	1170	(420)	(3.5)†††
FA Mean	20	430	7610	230	7100	2140	180
Obs.	865	865	865	865	865	865	865

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively.

Table 27: Mean Comparison of PSP Impacts on Group-Level Outcomes

	Members	Savings	Loans	Loan Value	Profit	Payment
All Quarters	0.10	62	1.0	41	9.5	-4.4***
s.e.	(1.0)	(42)	(1.2)	(33)	(9.5)	(0.84)†††
Quarter 1	-0.77	10	-1.1	-16	5.1	-8.6***
s.e.	(1.0)	(49)	(1.3)	(43)	(14)	(0.54)†††
Quarter 2	-0.11	11	0.60	-3.0	-4.5	-6.1***
s.e.	(1.0)	(42)	(1.3)	(31)	(8.8)	(0.69)†††
Quarter 3	0.35	68*	2.1*	63*	8.2	-3.4***
s.e.	(1.0)	(40)	(1.2)	(33)	(9.3)	(1.0)†††
Quarter 4	0.67	130***	1.8	99***	26**	-2.0
s.e.	(1.0)	(48)††	(1.4)	(38)†	(11)	(1.7)
FA Mean	21	240	9.9	230	53	9.5
Obs.	16,289	15,747	15,747	15,747	15,747	14,907

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively.

Table 28: Mean Comparison of PSP Impacts on Household Endline Outcomes

Outcomes (measured post-treatment)	PSP Mean	Std. Dev.	Obs.	FA Mean	Std. Dev.	Obs.	PSP-FA Mean △
Total Savings	145	249	1380	131	232	539	11
Savings for Business Owners	153	261	624	152	260	252	-4.1
Savings from Business Profits	28	172	1380	14	73	539	14*
Savings from Agric. Profits	36	115	1380	39	159	539	-4.8
Savings from Salary/wage	16	99	1380	10	113	539	6.0
Savings used for New Agric. Activity	37	196	1380	38	163	539	-2.4
Savings used for New Non-Agric. Activity	2.0	18	1380	4.2	30	539	-2.2*
Savings used for Existing Business	20	121	1380	4.0	47	539	16*** ††
Total Credit	62	222	1380	41	172	539	22**
Credit for Business Owners	58	168	624	32	100	252	26**
Credit from SILC	12	38	1380	7.6	24	539	4.3**
Credit from Formal Lenders	32	212	1380	22	158	539	10
Credit from Informal Lenders	18	56	1380	10	30	539	7.6*** ††
Credit used for Agric. Activity	10	104	1380	4.6	40	539	6.2
Credit used to Expand Business	12	102	1380	3.5	24	539	8.9**
Credit used to start New Business	3.6	41	1380	1.7	20	539	1.8
Start New Business	0.25	0.43	1380	0.20	0.40	539	0.05**
Closed Business	0.47	0.50	624	0.66	0.47	252	-0.19*** †††
Business Investment	41	130	1380	22	90	539	19*** ††
Hours spent in Business	13	23	1380	9.4	16	539	3.4*** ††
Non-HH Employees	0.23	0.86	1380	0.11	0.54	539	0.12*** ††
Hours spent in Employee	15	18	1380	14	18	539	0.83
Agric. Investment	70	171	1380	67	215	539	2.1
Hours spent in Agric.	28	14	1380	32	16	539	-2.6*** †††
Total Income	487	2443	1380	358	1495	539	129
Business Income	65	231	1380	54	218	539	11
Total Expenditure	1763	1548	1380	1600	1327	539	165**
Total Consumption	1652	1446	1380	1512	1243	539	144**

***, **, and * indicate statistical significance at the 1%, 5%, and 10% confidence levels, respectively. †††, ††, and † indicate statistical significance with a Bonferroni correction at the 1%, 5%, and 10% confidence levels, respectively.

A.9 Mathematical Appendix

We present some more details of the model results. First, we derive the bounds for $\underline{\pi}$ and $\overline{\pi}$ in Proposition 1. We start with $\underline{\pi}$. Define the additional benefit of type-H members as $\Delta(f_L) = B(f_L; \tilde{p}_H) - B(f_L; \tilde{p}_L)$. It is trivial to show that $\frac{d\Delta(f_L)}{df_L} < 0$ as stated in Proposition 1. We need to derive the conditions for $\Delta(0) > 0$.

$$\Delta(0) = (p_H - p_L)\left[\left(\overline{A} - \underline{A}\right) + (\pi\phi(0) - 1)\overline{R}_{BL}(0)\right] > 0$$

Substituting in $\overline{R}_{BL}(0) = p_L\overline{A}k / \left(p_L k + (1 - p_L)\phi(0)\right)$ and simplifying yields

$$-p_L\underline{A}k + \left[\left(\overline{A} - \underline{A}\right) + p_L\underline{A}\right]\phi(0) > 0$$

Now substituting in $\phi(0) = p_H / \left(\pi p_H + (1 - \pi)\right)$ and simplifying yields

$$\pi > \frac{p_L\underline{A}}{p_H\left(\overline{A} - \underline{A}\right) + p_L\underline{A}} = \underline{\pi} \in (0, 1).$$

Now consider $\overline{\pi}$. We can solve by deriving the conditions for $B(1; p_H) < 0$, which is

$$B(1; p_H) = p_H\left(\overline{A} - \underline{A}\right) + \left[(1 - \pi + \pi p_H)\phi(1) - p_H\right]\overline{R}_{BL}(1) < 0$$

Again, substituting $\overline{R}_{BL}(1) = p_L\overline{A}k / \left[p_L k + (1 - p_L)\phi(1)\right]$ and $\phi(1) \equiv \frac{p_L}{\pi p_L + (1 - \pi)}$ yields, after much simplification

$$\pi < \frac{p_H\underline{A} - p_L\overline{A}}{p_H\left(\overline{A} - \underline{A}\right) + p_H\underline{A} - p_L\overline{A}} \equiv \overline{\pi} \in (0, 1).$$

Clearly, $\overline{\pi} > \underline{\pi}$ if and only if[1]

$$p_H \underline{A} - p_L \overline{A} > p_L \underline{A}.$$

Here left-hand side measures the (per unit) capital production loss of adverse selection, while the right-hand side is the outside option of type-L. This condition always holds as $p_L \to 0$, and the upper bound on p_L is

$$p_L < \overline{p}_L = p_H \left(\frac{\overline{A}}{\overline{A} + \underline{A}} \right)$$

Next, we derive $\overline{\theta}$ from Proposition 2. First, notice that total surplus (net of F^*) is higher under F^* if and only if total output (net of F^*) is higher under F^*, since output is only distributed among members, and the outside options are always the same. Knowing that F^* leads to $f_L = 0$, we can express the condition that total output is higher under the F^* as:

$$(1 - \theta)\left(p_H \overline{A} - F^*\right) + \theta p_L \underline{A} > (1 - \theta) p_H \underline{A} + \theta p_L \overline{A}$$

Substituting in $F^* = B(0; \tilde{p}_L)$ and simplifying yields

$$\theta < \frac{p_H \left(\overline{A} - \underline{A}\right) - B(0; \tilde{p}_L)}{(p_L + p_H)\left(\overline{A} - \underline{A}\right) - B(0; \tilde{p}_L)} = \overline{\theta} \in (0, 1)$$

Next, we need to show that there exists an $\hat{f}_L$ such that:

$$B\left(\hat{f}_L; \tilde{p}_H\right) = 0 \Leftrightarrow \hat{f}_L = \frac{\left(\overline{A} - \underline{A}\right)\left[p_H^2 + p_H p_L \left(\frac{1-\pi}{\pi}\right)\right]}{(p_H - p_L)\left[p_H^2 \left(\overline{A} - \underline{A}\right) + p_L \overline{A} \left(\frac{1-\pi}{\pi}\right)\right]}$$

Then, substitute in for $B\left(0, \tilde{p}_L\right)$ and derive a condition for which $\hat{f}_L < \overline{\theta}$. One can show that

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[1]Recall that Footnote 8 has an additional sufficient condition of $\frac{1}{k} < \frac{p_L(1-p_H)}{p_H(1-p_L)}$ for adverse selection to hold. The economics of this is driven by the collateral ratio, however, are assumptions made to simplify market clearing therefore imply $\pi < \frac{p_L(1-p_H)}{p_H(1-p_L)}$. If this bound is less than $\overline{\pi}$ then sufficient conditions on parameters also exist to ensure that it exceeds $\underline{\pi}$. Namely, it requires $\underline{A}/\overline{A} < (1 - p_H)/(2 - p_H)$. Parameters satisfying this condition as well as the other conditions hold (e.g., $\underline{A} \to 0$). More generally, this condition constraining π depends on our $\pi k \to 1$, which was economically arbitrary but done for reasons of simplifying algebra.

this holds when the following inequality is satisfied:

$$p_H^3 p_L \left\{ \pi \left(\overline{A} - \underline{A} \right) \left[\left(\overline{A} - \underline{A} \pi \right) - \left(\overline{A} - \underline{A} \right) \pi \right] \right\}$$

$$+ p_H^2 p_L^2 \left\{ \left[2\pi^2 \left(\overline{A} - \underline{A} \right) + \overline{A} \pi \right] \left(\overline{A} - \underline{A} \right) + \overline{A} \pi \left(1 - \pi \right) \left(3\underline{A} - 2\overline{A} \right) + \underline{A} \left(\overline{A} \pi - \underline{A} \right) \right\}$$

$$+ p_H p_L^3 \left\{ \left(\overline{A} - \underline{A} \right) \pi \left[2\pi - \left(\overline{A} - \underline{A} \right) \pi \right] + \left(1 - \pi \right) \left[3\overline{A}\underline{A} \left(1 - \pi \right) - \overline{A}^2 \right] + \overline{A} \pi \left(3\overline{A} - \pi \right) \right\}$$

$$- p_L^4 \left\{ \overline{A}\underline{A} \left(1 - \pi \right)^2 \right\} > 0$$

As p_L is sufficiently close to 0, a sufficient but not necessary condition for $\hat{f}_L < \overline{\theta}$ is:

$$\left(\overline{A} - \underline{A} \pi \right) > \left(\overline{A} - \underline{A} \right) \pi \Leftrightarrow \pi < \frac{\overline{A}}{2\overline{A} - \underline{A}}.$$

Recall that we already defined an upper bound $\overline{\pi}$, and it is straightforward to show that the above bound exceeds this upper bound, i.e.:

$$\frac{\overline{A}}{2\overline{A} - \underline{A}} > \overline{\pi} = \frac{p_H \underline{A} - p_L \overline{A}}{p_H \left(\overline{A} - \underline{A} \right) + p_H \underline{A} - p_L \overline{A}}$$

Therefore the previous sufficient condition for $\hat{f}_L < \overline{\theta}$ is always satisfied as $p_L \to 0$.

Finally, the results from Proposition 3 are straightforward. Using Y_2^*, Y_1^* and Y_0 to denote total maximum output under two, one and zero fees, we have :

$$Y_2^* = \left(1 - \theta \right) \left(p_H \overline{A} - F^* \right) + \theta p_L \overline{A}$$

$$Y_1^* = \left(1 - \theta \right) \left(p_H \overline{A} - F^* \right) + \theta p_L \underline{A}$$

$$Y_0 = \left(1 - \theta \right) p_H \underline{A} + \theta p_L \overline{A}.$$

$Y_1^* > Y_0$, follows from the assumption that $\left(1 - \theta \right) p_H > \theta p_L$, while $Y_2^* > Y_1^*$ follows from $\overline{A} > \underline{A}$.

A.10 Data Description

A.10.1 Household Survey Data

Savings Measures

The measure of total savings is the sum of all savings the survey respondent records from: the SILC group, merry-go-round (a group that collects money from each member and gives it to one person in turn), a group of friends that lend with interest, a bank, a microfinance institution (MFI), a SACCO/Co-operative (organization that requires you to be a member, e.g., agricultural co-op or workplace co-op), mobile money, a secret hiding place, giving to a friend or family member to keep, crops or grains in storage, and other savings which need to be specified. For the savings coming from the merry-go-round, the respondent records the amount of money that he or she would receive when it is their time to cash out. For crops or grains in storage, the respondent records the amount of money they would receive if they would sell all of it. Total savings are recorded in the local currency and we convert data from local currencies into USD using exchange rates at the time of survey for each country. Total savings for business owners then is total savings as just described for those survey respondents who recorded to own a business in the baseline survey.

Besides breaking down the total amount of savings, the respondent is also asked what the most important source and purpose are for these savings. In the paper, we focused on savings coming from three sources: business profits, selling agricultural products and salary or wages. We define the main source of savings as total savings for those respondents that record business profits, selling agricultural products and salary or wages as being the most important source of any type of savings they have. For the main purpose of savings, we also focused on three: new agricultural activity, new non-agricultural business and improve an already existing business. Each purpose of savings is defined in a similar way as is the source of savings: it is total savings for respondents that record new agricultural activity, new non-agricultural business and improve an already existing business as being one of the main purposes of savings in the last 12 months.

Credit Measures

Total credit is the sum of all loan amounts received in the past 12 months from: the SILC group, an ASCA, a bank, an MFI, a SACCO/Co-operative, a moneylender, an employer, a buyer of products who gives you cash/input in advance, a local shop/supplier that allows you to take goods/services on credit, family and friends, goods/items on hire purchase, and other sources which need to be specified. Total credit is recorded in the local currency and we convert data from local currencies into USD using exchange rates at the time of survey for each country. Total credit for business owners then is total credit as just described for those survey respondents who recorded to own a business in the baseline survey.

We divided the source of credit into three main categories: credit coming from the SILC group, formal and informal credit. Formal credit is defined as the sum of all loan amounts received in the past 12 months from the more formal sources i.e. from ASCA, a bank, an MFI, SACCO/Co-operative or a moneylender. Informal credit are all loans in the past 12 months coming from informal lenders: an employer, a buyer of products who gives you cash/input in advance, a local shop/supplier that allows you to take goods/services on credit, family and friends, and goods/items on hire purchase. The purpose of credit is defined as total credit for those respondents that record farm inputs or improvements, expanding your business or starting up a new business as being one of the purposes. In the paper we focused mainly on these three purposes.

Time Use Measures

Weekly time-use measures for the respondent were constructed by asking for the number of rest days and work days in a typical week and then detailing the time-use separately for rest and work days across labor for own business, own farm, home production/childbearing, and market labor.

Consumption and Expenditure Measures

Expenditures are a sum of the following data. We have weekly spending data on food, beverages (alcoholic and non-alcoholic), and tobacco. Next, respondents record monthly spending on housing, transport and communication, health and medical care, and personal expenses. Finally the

survey asked for yearly spending on clothing and footwear, things for the house, education, live-stock/agriculture investment, business investment, social obligations, and land. We then convert the weekly and monthly data to yearly data and ad up all expenditures to a yearly measure. All expenditures are recorded in the local currency and we convert data from local currencies into USD using exchange rates at the time of survey for each country. In order to measure consumption we subtract all investments from the expenditures measure. These include both livestock/agricultural investment and business investment.

Income Measures

We measure total income as the total income in the past 12 months (to account for seasonalities). These data were collected separately for the respondent personally and the household overall. Besides reporting total income, the survey also asked to break down income by different activities: income from wage and salary, business income etc. This is where the measure for business income comes from. More formally, it is defined as income for the respondent earned from his/her business that is not a farm/agriculture in the past 12 months. Measurement of home production is another major issue, especially for agriculture. It is likely that home production was not considered income by respondents. Both income measures are substantially less than our measure of annual purchases, which exclude home-produced and gratis consumption. Finally, reported household incomes were only marginally higher than reported income of respondents. Thus, it appears there is also likely underreporting. Income is recorded in the local currency and we convert data from local currencies into USD using exchange rates at the time of survey for each country.

A.10.2 MIS Data

- All monetary variables (savings, loan value, profit, and earnings) are converted to US dollars using exchange rates at the time of data collection.

- Savings, profit, and earnings measure accumulated totals over the course of the entire cycle. Groups, members, loans, and loan value measure totals at the time of data collection.

- Savings, loans, loan value, profit, and earnings are reported in per quarter terms for group-level impacts and as a simple sum across groups each agent is working with at the time of data collection for agent-level impacts.

Membership Measures

Groups measures the number of groups each agent is working with at the time of data collection. This variable is measured only at the agent level. Members measures the number of members in each group at the time of data collection. For agent-level impacts we sum across each of the groups a particular agent is working with.

Savings and Loan Measures

Savings measures the total value of savings over the course of the current cycle, converted to US dollars using exchange rates at the time of data collection. Since savings accumulate over time, we report savings per quarter for the group-level impacts so groups that started at different times will be comparable. For agent-level impacts, we report savings summed across all groups with no adjustment made for weeks in each group's cycle. Loans gives the number of loans outstanding at the time of data collection. As with savings, we normalize the group-level results to loans per quarter since the number of outstanding loans is likely to grow over time. The agent-level results are summed across all groups the agent is currently serving with no adjustment made for length of current cycle. Loan value is the value of outstanding loans (converted to US dollars) at the time of data collection. The group and agent-level results are calculated in the same manner as savings and loans.

Profit and Earnings

Profit measures the total amount of profit earned over the course of the current cycle by each group (converted to US dollars). Profits include money earned from registration fees, fines, and interest earned from loans and can be positive or negative. For group-level impacts we normalize to profit per quarter, while for agent-level impacts we sum across groups with no such adjustment. Earnings measures the amount of money in US dollars that the group has paid to the agent over

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the course of the current cycle. The payment may come in several forms, most commonly as a fixed group/member fee, share of savings, or share of profits. For group-level impacts we report earnings per quarter, while for agent-level impacts we report the total across the agent's groups with no adjustment made for length of current cycle.

A.10.3 Weights

We create a set of weights based based on: (1) sampling weights and (2) country weights. First we create the sampling weights. In each village five SILC and five non-SILC households are sampled. However, these might not reflect the underlying data. For instance, imagine a village with 30 households of which 20 are SILC and 10 are non-SILC. If five household are sampled from each category, in this example, the non-SILC households would be "oversampled" compared to the non-SILC households i.e. 50% of the non-SILC households are repesented as opposed to only 25% of the SILC households. In order to take this into account, each observation will be weighted by the inverse of its probability of being sampled. In this example SILC household would receive a weight of four and non-SILC housholds two.

Example Village	SILC	Non-SILC
Census	20	10
Sample	5	5
Probabilty of being sampled	0.25	0.50
Weight (= Inverse probability)	4	2

Second, we create the country weights. Recall that the sample contains data from three countries: Kenya, Tanzania, and Uganda. If we break down the village data by country and treatment we have the following table:

	Kenya	Tanzania	Uganda	Total
PSP	65	34	39	138
FA	18	13	23	54
	83	47	62	192

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Now we see that in the overall sample the ratio of PSP vs FA is $\frac{138}{54} \approx 2.6$. Note that this ratio is not the same across countries. In Kenya it is $\frac{65}{18} \approx 3.6$, in Tanzania $\frac{34}{13} \approx 2.6$, and in Uganda: $\frac{39}{23} \approx 1.7$. This could lead to biased results when estimating the impact of PSP treatment. More specifically, we would be mostly pick up the treatment effects in PSPs in Kenya and Tanzania as their PSP to FA ratio is higher than in the overall sample. Therefore, we need to weigh the different villages such that the ratio in each country is the same as in the overall sample. In other words:

$$\frac{65 w_{PSP}^{K}}{18 w_{FA}^{K}} = \frac{34 w_{PSP}^{T}}{13 w_{FA}^{T}} = \frac{39 w_{PSP}^{U}}{23 w_{FA}^{U}} = \frac{138}{54}$$

and

$$65 w_{PSP}^{K} + 18 w_{FA}^{K} = 83$$
$$34 w_{PSP}^{T} + 13 w_{FA}^{T} = 47$$
$$39 w_{PSP}^{U} + 23 w_{FA}^{U} = 62$$