

# Can You Teach a Person to Fish? Evidence From a Microfinance Privatisation Experiment PRELIMINARY, DO NOT CITE

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## Abstract

This paper examines an innovation to the delivery of “self-help group” microfinance services in East Africa using a randomized control trial. NGO-trained agents who help found and administer the groups play a key role in their operation, and we find that privatised entrepreneurs providing the self-help group services outperform their NGO-compensated counterparts along several dimensions. They cost the NGO less, administer more profitable groups, and the households with access to privately-delivered groups borrow and save more, invest more in businesses, and may have higher consumption. In trying to explain these impacts, we find evidence that cream-skimming plays an important role.

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As the Chinese proverb suggests, it is generally better to teach a person to fish rather than give a person a fish. What is less clear, however, is whether it is feasible to teach people to fish, especially in the provision of social services in developing countries. Such social services constitute a sizable amount of foreign aid, both governmental and non-governmental, and this aid involves both training and financial support.<sup>1</sup> The extent of resources allocated to such programs motivate an important question: Can such training-based programs flourish or even survive, once financial support has been removed?

This paper examines exactly that question in the context of an innovation to the provision of NGO-sponsored microfinance services in East Africa. The status quo provision was for the NGO to pay a salary to agents providing financial services, whereas the innovation required agents to become private entrepreneurs, earning their own remuneration from the clients. We evaluate this innovation using a randomized control trial in which agents in the control areas followed the status quo “give a fish” program, while those in the treatment village followed the “teach to fish” innovation. Both samples were “taught to fish”, in the sense that both received the identical initial training, but only the treatment sample was forced to earn their own remuneration.

The results are powerful and encouraging for the prospects of teaching to fish. Initially, the private entrepreneurs provide fewer services and reach fewer clients, but they converge to the levels of the status quo agents after a year. Their microfinance groups are substantially (26 log points) more profitable, and the households in the areas they serve show shifts toward business activity. Specifically, these households have higher levels of: savings from business activities; credit, especially to business owners; employees; and business investment. They spend a higher fraction (about 5 percentage points) of their time on their business, while spending correspondingly less in agriculture. Finally, these impacts are witnessed despite the facts that clients must pay for the services under the private entrepreneur model. Nonetheless, the evidence suggests that these households in the treatment villages actually spend and consume more.

The microfinance program evaluated involves the promotion of self-help groups (SHGs), an increasingly common method of providing access to microfinance services to the “poorest of the poor” in developing countries. These small groups of 10-25 typically meet on a regular basis to collect savings, lend to members with interest, maintain an emergency “safety net” fund, and share profits from lending activity. That is, they effectively operate as small,

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<sup>1</sup>Private NGO data is less easily available than public data. According to the OECD’s Creditor Report System, total public foreign aid donated for social service activities comprised \$50 billion of total aid in 2010. Of this, \$38 billion was for training related activities. Social service activities comprise of education, health services, population programmes, water and sanitation, other social infrastructure/services, and financial services.

independent, quasi-formal, self-financing credit cooperatives. The groups do not receive outside financial resources but only assistance from the outside agents who found and help administer the groups over roughly their first year of operation. These agents in turn receive training from Catholic Relief Services, the NGO sponsoring the program. CRS is a major non-governmental development organization, and their particular variety of SHGs are called SILCs (saving and internal lending committees), but their groups are representative of other similar SHG programs sponsored by other agencies in the developing world, including CARE, OxFam, Plan, World Vision, and perhaps most importantly, NABARD, a large government agency in India. SHGs reach an estimated 100 million clients, and this number has grown dramatically in recent years.<sup>2</sup> Hence, the type of program we study is itself of great interest.

We study a large randomized experiment involving 234 agents who started a total of over 5700 groups serving over 100,000 members across 11 districts in Kenya, Tanzania and Uganda. Each agent spent one year working as “field agents” (FAs), during which they established and assisted a fixed number of groups and received both training and compensation from CRS. After completing this course and passing their examination, most agents immediately became “private service providers” (PSPs), essentially entrepreneurs needing to start new groups and negotiate payments from the group members in order to receive remuneration. They carried with them credentials showing their successful completion of training, but their compensation from CRS was rapidly phased out. In contrast, a random sample was informed that they would remain on as FAs for an additional year, receiving a higher payment from CRS but not allowed to charge their clients. This randomization was performed at a geographic level, so that no PSPs were forced to compete with FAs.

In evaluating this experiment, our study uses several sources of data. First, eight quarters of MIS data on the financials and membership of the groups themselves are available through required quarterly reports to CRS. These data allow us to examine group performance. Second, these data are supplemented biannually by a brief questionnaire on the agent characteristics and experiences. Third, a baseline survey of village-level key informants. Fourth, a stratified two-period before-after panel of 10 households in each of 192 villages served by the program. These data include allow us to assess the impact of the innovation on the members themselves. Since each of the datasets also pre-date the randomization they are helpful in assessing whether the assignment was truly random *ex post*. The data indeed show few significant baseline differences in observables across treatment and control, certainly within the range of expected type I errors. We are therefore confident that assignment was truly random.

Our analysis of the group and agent level MIS shows that PSP treatment leads to signif-

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<sup>2</sup>The NABARD program alone has grown from 146,000 clients in 1997 to 49 million in 2010.

icantly higher group profitability after six months, and after one year increases profitability by approximately 28%. After three months, a PSP works with four fewer groups and 75 fewer clients than a traditional FA on average, but this difference shrinks over time and after one year the numbers are statistically indistinguishable, with point estimates actually implying *more* groups and clients served. The total amount of savings, number of loans, total credit disbursed and agent pay all show similar patterns: they start out significantly lower but increase over time. With savings and credit, the increase is due to both increases within groups and increases in the number of groups. Agent pay differs in that even after one year, PSP earnings remains significantly (\$50 per quarter, exchange rate valued, or nearly one-third ) below the pay of FAs. Nonetheless, both agent attrition and group failure remained extremely low, making the teaching-to-fish privatization an extremely cost-effective method of delivering services.

An important question, however, is how the benefits to households compared under the privatized entrepreneur approach relative to the status quo. Here the results are even more encouraging. Under the PSP approach members are required to actually pay for services, and so one might have suspected that the benefits to households would be smaller. Instead, we see the microfinance institutions being significantly more effective in delivering outcomes promoted by microfinance. The PSP treatment leads to nearly \$28 (or 50 percent) more credit per household. Several measures of business activity are significantly higher as a result of the PSP treatment. Nearly twice as much savings reportedly comes from business profits and nearly twice as much savings is reportedly done for the purpose of financing existing businesses. These reports correspond with observed business decisions. Business investment is nearly twice as high (\$20 per household). Likewise, the number of employees hired is nearly twice as high as in the FA villages, although this number is still quite low (just 0.22 employees per business in FA villages). Households spend about 5 percent more of their working time on business activities and the fraction of time spent in agriculture is correspondingly lower. Although we do not measure significant impacts on income, the measured increases of over ten percent for both total expenditures (\$213) and consumption (\$181) are marginally significant.

The natural question is why not only the groups themselves flourish under the PSP, but why they also lead to greater impact. . We find evidence suggesting that the PSP program may target services to a more entrepreneurial subset of the village population. The households who join (and leave) SILC under the PSP program are different from those under the FAs. The members of PSP SILCs tend to have higher levels of existing business income and higher reported discount factors, relative to the members of FA SILCs. Those who join spent more time running businesses, while those who left had lower levels of consumption,

lower savings, and more hours spent as wage workers. Thus, it appears that PSPs, whether intentional targeting or simply because of the required fees, target a more affluent, business-oriented membership. We examine a second channel, finding little evidence that PSPs work harder than FAs. PSPs are more likely to administrate groups that are in their own villages. They tend to spend less effort advertising the services and resolving conflicts among members. Again, these may be the result of better targeting of services.

The results of this paper contribute to several related literatures. First, a number of recent papers have examined attempted moves toward economically sustainable approaches, including privatisation, in the provision of services in developing countries. The literature has naturally found mixed results depending on the program and the context. Several authors have emphasized that cost recovery can reduce access or take-up. Most relevant, Morduch (1999) conjectured that an emphasis on sustainability and cost recovery will limit microfinance’s ability to reach the poorest households. Our results that privatized groups serve different members are consistent with the willingness-to-pay arguments against privatization. However, we also find that overall membership is unchanged, and that the groups themselves are more effective. In addition, the cost-savings itself is an important benefit in expanding programs elsewhere. Kremer and Miguel (2007) examined economically “sustainable” deworming programs, and found that a cost-sharing program for deworming drugs reduced take-up by 80 percent, while educational programs were largely ineffective. Problems with the privatized investment and sustainability of clean water sources in developing countries, have also been well examined (Kremer et al., 2011). On both of these studies, the public good aspect provides higher justification for sustained subsidies. Not all prior empirical evidence has been negative, however, even for services with a public good aspect. Focusing on Argentina, admittedly a middle-income country, ? found that privatization of water supplies reduced child mortality, especially in poor areas. We view the results on the PSP initiative as another success story that can be informative for future decisions.

Second, there is a burgeoning literature evaluating the impacts of a variety of microfinance interventions in different countries. There are different theories of microfinance. Some follow the traditional narrative by modeling credit that enables entrepreneurship, investment and growth (e.g., ? or Buera et al. (2012)), while others emphasize consumption smoothing or simply borrowing to increase current consumption at the expense of future consumption (e.g., Kaboski and Townsend (2011), Fulford (2011)). The empirics have yielded conflicting results. In the Phillipines, Karlan and Zinman (2010) found that microfinance led to fewer businesses and fewer workers hired. In Thailand, Kaboski and Townsend (2012) found large increases in consumption, hiring workers, and wages consistent with the entrepreneurship story, but they only find small impacts on investment and no significant impact on

entrepreneurship. Banerjee et al. (2011) found only marginal increases in investment and no impacts on consumption in India. In Morocco, Crépon et al. (2011) finds increases in income, expenses, and labor, but their study is not well-designed for finding increases in entrepreneurship. In Mongolia, Attanasio et al. (2011) find substantial increases in entrepreneurship, but only among females and the less educated, and only when microfinance loans are joint liability. We add to these conflicting results, one potential explanation for varying results. Our results suggest that delivery mode and incentives faced by institutions may greatly alter the impact of microfinance.

Finally, given the rising importance of SHGs, within the broader microfinance literature several other recent papers focus specifically on SHGs.<sup>3</sup> Goldston (2012) examines the role of local politicians and elections in determining the disbursal of credit in Indian SHGs. This highlights another distinction between SHGs supported from outside, and privately supplied SHGs. Deininger and Liu (2008), Deininger and Liu (2009) evaluated the impact of Indian SHGs using a propensity score matching approach in India. They find increases in nutrition, income and asset accumulation at 2.5-3 years of exposure but only increases in nutrition and female empowerment over shorter horizons. On the other hand, several recent studies of CARE's VSLA (village saving and loan associations) program in Africa (Malawi, Uganda, and Ghana) have yielded few impacts. Recent work with Oxfam's version is ongoing. These mixed results across different programs are not inconsistent with our findings that investment and expenditures rely greatly on the incentives faced by the organizers. Fafchamps and La Ferrara (2011) examine SHGs in an urban setting (Nairobi, Kenya) and emphasize the risk-sharing role of SHGs. They show that group members do not assortatively match ex ante but risk-sharing creates high correlation ex post. We show that the composition of members to the costs of membership. **XX cite Sujata Visaria when receive paper**

The remainder of the paper is organized as follows. Section 2 describes the program, experiment, data and methods, while Section 3 presents the results and analysis. We conclude in Section 4.

## 1 Program and Methods

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

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<sup>3</sup>There is a larger literature on rotating savings and credit associations (ROSCAs), where members contribute fixed amounts in each meeting, and the total contribution is given to a different member of the group each week. Since there is no standing fund, the administration of these organizations are dramatically simpler (one only needs to keep track of who has already received the payment). Thus ROSCAs often arise spontaneously. The potential uses of ROSCAs are substantially more limited than SHGs, however. For example, there is substantially less flexibility, and there is no net saving or borrowing at the individual level.

## 1.1 SILC Program and PSP Innovation

The particular brand of SHGs promoted by Catholic Relief Services are called SILCs (savings and internal lending committee). A typical SILC is a group of between 10 and 30 members (a mean of 19) who meet regularly to save, lend to members, and maintain a social fund for emergencies. In principle, SILCs allow those with limited access to financial services to save and borrow in small amounts, earn interest on savings, and lend flexibly. SHGs have gained wider 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, 2011).

Although the SHGs often have broader missions, their primary operations are as ASCAs (accumulating savings and credit associations).<sup>4</sup> ASCAs are distinct from the more well-known ROSCAs (rotating savings and credit associations). In ROSCAs members bring fixed contributions, one member receives the pot each meeting, and each member gets an opportunity to be the recipient. The ROSCA arrangement requires little to no recordkeeping and no central holding of funds. ASCAs instead operate like small credit unions: members are allowed to save in flexible amounts and loans are also made flexibly. The advantage over credit unions is that they 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, Uganda, and Tanzania, meetings are generally weekly, and a typical (median) weekly deposit would be \$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. Not all funds are lent out as loans; a portion are retained as a social fund available for emergency loans. Funds accumulate due to both savings, interest on repaid loans, and fines for late payments/other violations, and these funds are held centrally. The funds follow cycles of typically six months or one year. At the end of each cycle, all loans must be repaid, and the total fund is 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 that of school fees, Christmas, or some other time where cash is needed.

Beyond their greater flexibility, the fact that funds can accumulate allows for some members to be net savers; others may be net borrowers. The greater flexibility also makes the administration of ASCAs is far more complicated than ROSCAs. They require strict record

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<sup>4</sup>These additional “self-help” objectives may be female empowerment, social outreach, assist in infrastructure investments, providing services such as technical training or marketing assistance, fostering participation in local politics, or simply fostering stronger community bonds.

keeping to keep track of savings, loans, loan payments of various amounts, and payouts due.<sup>5</sup> They also require judgement of who should receive loans, how much they should receive, and interest rates. Risks of default are also potentially greater, since some members may borrow disproportionately, and this magnifies the importance of decisions on membership. Meetings are involved, requiring counting and verifying the starting fund, member deposits of varying amounts, loan payments of varying amounts; and loan disbursements; and the complexity increases as the fund grows over the course of the cycle. Given their complexity, ASCAs do not arise endogenously like ROSCAs do. Instead the role of trained field agents in founding, administering, and training the members themselves is critical.<sup>6</sup> The services provided by field agents to these groups include initial training and then follow-up supervision on the areas of: leadership and elections; savings, credit and social fund policies and procedures; development of constitution and by-laws; record-keeping; and meeting procedures; and conflict resolution.

CRS has traditionally catalyzed this process by training field agents (FA) to start SILC groups. FA trainees are recruited from the more educated segment of existing SILC members. They receive initial training and begin forming groups within a month, and then receive refresher training three additional times, as well as monitoring from a supervisor over the course of a year. CRS provides training to field agents in all of the above-mentioned services that FAs provide. Monitoring is done by checking over the constitutions and record books and sitting in on meetings of SILC groups of their trainee FAs occasionally (generally at least once a month, rotating groups). During the training phase, agents are required to form 10 groups. At the end of the training phase, the agents take an exam, and 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 expected number of groups also increases to 10 additional groups. Both during and after the training phase, agents must report summary accounting data for each group (e.g., group name, number of members, total loans, total credit, profits, payouts, defaults, etc.) on a quarterly basis following a standardized MIS system. Beyond this data collection, there is little additional oversight from CRS after the training phase.

Into this existing SILC promotion program, CRS introduced the PSP delivery innovation, in which fully trained FAs would be certified as such and transition to being private

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<sup>5</sup>Each SILC keeps its records in a single ledger divided into seven sections: 1) Register, 2) Social Fund ledger, 3) Savings Ledger, 4) Fines Ledger, 5) Loan Ledger, 6) Cash-Book and 7) Statement of SILC Worth.

<sup>6</sup>Additionally, funds require a means for safekeeping the unlent, accumulated savings. CRS provides SILCS with an iron money box with three separate locks. One member holds the box, while three other members hold the keys separately to ensure safety of the fund.



entrepreneurs who earn payment for their services from the SILC groups themselves rather than CRS. PSPs negotiate their own payment from the SILC members, and the most common form of payment is a fixed fee per member per meeting. For PSPs, after certification their payments are phased out linearly over four months (e.g., 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 required resources needed to subsidize SILCs, thereby improving both the long term sustainability of the groups and CRS' ability to expand the program. A second goal is to develop local capabilities, and so the longer term hope is to also transition the training of FAs to an eventual network or guild of PSPs. The implementation of this delivery model is a large-scale Gates Foundation-funded program that involves training roughly close to 750 agents who will found roughly 14,000 SILCs reaching nearly 300,000 members. The field agents are recruited in three waves over three years, as different local partners (typically Catholic diocese) in different regions of Kenya, Tanzania, and Uganda enter the expansion.

## 1.2 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 were certified in December 2009-January 2010. Agents in the second wave were recruited in either October 2009 (Kenya and Tanzania) and January 2010 (Uganda).<sup>7</sup> They were certified the following year in the October and January 2011, respectively. The second wave of agents represented expansion of the program to new areas, typically new regions in the country. After certification, those randomized as FAs earned the above mentioned monthly payments with the assignment of starting/assisting 10 additional groups, which was chosen to compare well with anticipated PSP earnings after certification. (Unfortunately, PSP earnings fell short of these anticipations as discussed in Section 3.1.)

The research includes data from Kenya, Tanzania and Uganda across multiple regions within each country. Within each region, a local partner supervised the implementation of the program in conjunction with CRS and our research team.<sup>8</sup> 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). Both partner

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<sup>7</sup>The original plan was for all three countries to begin in October 2009, but the partners in Uganda experienced operational delays.

<sup>8</sup>The first wave partners operate in Mombasa and Malindi in Kenya and Mwanzaa and Shinyanga in Tanzania. Within Kenya, the second wave included expansion in Mombasa and Malindi, as well as new partners in Eldoret and Homa Hills. In Tanzania, the second wave expanded in three existing areas and added a partner in Mbulu. The Ugandan sample, all second wave, included partners in Gulu, Kasese, Kyenjojo and Lira.

organizations and their agents were notified of the particular randomized assignment just prior to certification. Out of concern for human subjects, the FAs were informed that they would remain FAs for an additional 12 months before delayed PSP assignment. The geographical level was chosen to ensure that FAs and PSPs would not be competing in the same area: sublocation in Kenya, ward in Tanzania, and subcounty in Uganda. Relatively more of these 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 yielded relatively more agents into the evaluation sample, but similar numbers of FAs were chosen across each sample in 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.

From among the expansion agents recruited, the initial sample included all agents that had not yet been certified at the time of the initial randomization. A smaller sample of the recruits were excluded due to death or dropouts. The original year 1 sample included 65 agents in Kenya and Tanzania. In Kenya, the randomization 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 260 agents from Kenya, Tanzania and Uganda. In Kenya, there were 71 PSPs and 24 FAs spread across 4 partners, in Tanzania there were 44 PSPs and 19 FAs spread across three partners, and in Uganda there were 41 PSPs and 26 FAs distributed across 4 partners.<sup>9</sup>

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, Catholic Relief Services was strongly resistant to such a control. For that reason, however, 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.

### 1.3 Data

The data collected come from four different sources. First, the MIS system collects book-keeping accounting data at the level of SILC group. These data include total membership,

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<sup>9</sup>The randomization contained only a fraction 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.

savings, credit, losses, interest rates, profitability, and share outs, as well as agent name. They are collected on a quarterly basis, and were extracted from the MIS system into a rectangular database layout, where each record is a group. In order to pool the data across countries, we use exchange rates to put currency values into a dollar equivalents. We analyze these data at the level of the SILC groups, but we also aggregate to the level of the FA/PSP agents who operate them and analyze at this level.

The second source of data, an agent level survey, supplements the MIS with agent level characteristics collected in January 2009 (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., questions on targeting of groups, time spent with groups, and negotiation of payments) collected then and every six months thereafter. ) and additional group level data collected every 6 months covering membership characteristics, delivery of services, and compensation scheme.

The third and fourth sources of data are based on a set of 220 villages chosen. The villages were selected as followed. A subset of 220 agents was chosen among the full year sample of 248 second wave agents in April 2010. These agents were stratified across country (83 of 96 in Kenya, 74 of 82 in Tanzania, and 61 of 70 in Uganda), but otherwise chosen randomly. During this time, the agents were all in their training phase and had to be notified of their random assignment. For each of the 220 agents chosen, a village was randomly selected among the list of villages in which they currently ran at least one SILC. In May 2010, a key informant survey was administered to the village chief. This survey 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.

Village censuses were then matched with list of known SILC members in order to select a sample for the fourth data source, the household survey. From the list of SILC and non-SILC members, a sample of five households with known SILC members and five households without known SILC members were chosen with weights assigned appropriately based on their proportions in the matched village census list. For households with known 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). In June, July, and August of 2010, the baseline survey was conducted among 2190 households in Eastern Kenya, Tanzania, Uganda, and Western Kenya, respectively. (One village in Uganda was inaccessible and could not be surveyed.) In June and July of 2011, a resurvey of the same households was conducted in Kenya and Tanzania, approximately nine months after the agents had received certification. In October of 2011, Uganda was

resurveyed, again 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, and indicators of female empowerment and community participation, and questions about risk-aversion and discounting. Table 1 presents some summary statistics in the baseline for households with SILC members and households without. 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 we cannot distinguish 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 was self-identified 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. Expenditures include broad measures on food, beverages and tobacco (past week, both home produced, purchased, and received as a gift), non-durables such as utilities, fuel, transportation, communication, health (including free health care), and personal services (past 30 days), and less frequent, more durable expenses such as clothing, household items, education, land, agricultural investment, business investment, and social expenses (e.g., funerals, bride price). 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/childrearing, and market labor.

Our measures of income probably suffer from the most measurement difficulties. Recall is one major issue. To account for seasonalities, we asked for income over the past 12 months in the following categories: business, agriculture, market labor (cash and in-kind), gifts/transfers, and other sources. These data were collected separately for the respondent personally and the household overall. 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, however, 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. We focus on the respondents income since it is presumably better measured, and respondents (many of whom are SILC members) are more likely to be impacted directly by SILC.

## 1.4 Empirical Methods

We use simple regression methods tailored toward the different data sets. We first discuss our verification of the randomization, and then our estimation of impacts.

### 1.4.1 Baseline Randomization

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:

$$X_{i,n} = \alpha + \gamma wave_i + \theta TREAT_n + \varepsilon_{i,n}$$

where  $i$  indexes either 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  $wave_i$ .  $TREAT_n$  is a dummy for whether subdistrict  $n$  received the PSP program, so that  $\theta = 0$  is the null for the test of random assignment. For the agent level data, we cluster the standard errors by region, while for the household data, we cluster them by village.

Table 2 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, number of children or dependents across the two samples. We do, however, see a significant higher fraction of PSPs receiving secondary education, and correspondingly with primary education 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 only use the first wave, and data are weighted appropriately. Hence, a simple mean comparison suffices:

$$X_{j,n} = \alpha + \theta TREAT_n + \varepsilon_{j,n}$$

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

Table 3 shows similar results for the household characteristics. Again, the assignment of treatment appears to have been random with respect to the underlying characteristics of households, with the exception of education. Here, we see that the fraction of people whose highest attainment is completed primary school is significantly lower (0.08), and some of this is because the fraction with some secondary is somewhat higher (0.03). Here again, we believe this education result to be purely random.

We do several exercises to ensure that our results are not driven by the higher schooling of either the agents or recipients in PSP areas. First, in all regressions we include dummies for highest education attained. 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 Mombassa in Kenya and TAHEA in Mwanzaa, Tanzania. Without these two areas, the baseline analysis produces no significant differences in either the agent or household databases. All of our significant results are robust to dropping these two areas, as we show in Appendix B. Third, we examine the impact of dividing the sample by average village education rather than PSP/FA treatment. We discuss those results below.

### 1.4.2 Measuring Impact

Our estimation approaches differs slightly depending on the data source.

#### Agent and Group Impacts

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

$$Y_{idnt} = \alpha_{dt} + \gamma wave_i + X_i\beta + \theta TREAT_n + \varepsilon_{itdn} \quad (1)$$

$$Y_{idnt} = \alpha_{dt} + \gamma wave_i + X_i\beta + \sum_{s=1}^4 \theta_s TREAT_{ns} + \varepsilon_{itdn} \quad (2)$$

Here  $Y_{itdn}$  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  $\alpha_{dt}$ , a dummy for the wave of agent  $i$ ,  $wave_i$ , and the above agent  $i$  characteristics (gender, age, schooling dummies, number of dependents, and number of children),  $X_i$ . Given six quarters of data, we look for both an overall effect  $\theta$  (top regression equation), and duration  $s$  specific treatment effects 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 agent.

#### Household Level Data

For the household level data, we simply have two cross sections. Using the panel aspect of the data is problematic along two fronts. First, the requirement of a balanced panel would reduce the number of households by nearly 20 percent. Second, with two time periods, allowing for household heterogeneity amounts to effectively differencing the data. This

exacerbates any measurement error issues. Instead, we focus on the endline data to estimate impact using the following regression equation:

$$Y_{jdn} = \alpha_d + X_j\beta + \theta TREAT_n + \varepsilon_{jdn} \quad (3)$$

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  $X_j$  summarized in Table 3 (gender; age and age-squared; schooling dummies; and the number of adult men, women and children in the household). Again,  $\theta$  is the measure of impact. For the household data, we cluster standard errors by village.

Here the impact of treatment is evaluated at the village level, essentially, without reference to SILC membership. The primary reason for this is that SILC membership itself is naturally endogenous. A secondary reason is that some impacts may spillover to non-members.

In the results section, we focus exclusively on the estimates of  $\theta$  and  $\theta_s$ . Tables 4, 5, and 6 give examples of the full regression estimates for the agents, groups, and households, however.

### 1.4.3 Reasons for Impact

We use multiple ways to explore three potential explanations for the impacts: (1) improved member selection by agent or households, (2) improved effort by agent, and (3) improved effort by members.

The agent questionnaire gives several measures of agent’s 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, the distance traveled to the group.

Second, using the baseline household characteristics, which are not effected by the treatment, we look at how the characteristics of SILC members changes.

Finally, we have measures of overall effort, which come from the time use data.

**xx Need to follow up on this.**

## 2 Results

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

## 2.1 Impact on Agents and Groups

Table 7 presents the agent level results for various measures. The first row presents the overall impact  $\theta$  from equation (1). On average, PSPs start 2.5 fewer groups and earn \$100 less each quarter, both of which are significant at a one percent level. The negative coefficients on the number of members reached and number of loans administered, -43 and -19, are significant at a ten percent level, indicating that PSPs may lead to fewer intermediation services provided overall. Based on these numbers, one might be skeptical that the PSP program will expand SILC services as well as the FA program.

The remaining rows, which present the duration-specific estimates of  $\theta_s$  from equation (2), offer stronger insight: PSPs start off more slowly than FAs, but they improve over time. PSPs do significantly worse over the first two-quarters (six months) in starting groups, reaching members, and providing loans, total credit and accumulating savings in their groups. By the third quarter, however, PSPs are indistinguishable from FAs along these dimensions, and by the fourth quarter all the point estimates are positive, and considerably sized, albeit insignificant. The remaining two impacts also show increases over time. Agent pay of PSPs remains lower than FAs, with PSPs earning \$50 less (significant at a 10 percent level), even in the fourth quarter. In contrast, the profits of groups increase over time, and they are substantially higher, by a total of \$157 per agent, by the fourth quarter. This higher profitability of PSPs' groups in the fourth quarter is significant at a one percent level.

Clearly, PSPs are slower in starting groups, and a question is whether it is the number of groups driving the dynamics in members, saving and lending. To evaluate this, Table 8 presents the group level regressions. Here the impact coefficient is the impact of PSPs on the typical group. Overall, we don't see significant differences at the group level, indicating that the differences in members, credit and savings in Table 7 are indeed driven by the number of groups. At the group level, the significant differences for PSPS are that profits per group are \$5 higher on average, while agent pay per group is \$4 less. Comparing the profits to savings in the control implies a return on savings of about 24 percent. Since savings is unaffected at the group level, the additional profits amounts to a return of 32 percent on savings. The lower income per group together with the higher group profitability of may indicate that the FA remuneration exceeds the perceived value of FA SILCs to its members, assuming PSP pay reflects the clients valuation of their services. Finally, the lower agent pay and higher profits per group in Table 7, and the overall values for profits and pay at the agent level in Table 8 are certainly not explained simply by the difference in number of groups. Indeed, the difference suggests that PSPs with the most groups have the lower pay per group and higher profitability.

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 or clients they reach, the savings they mobilize, and the credit their groups provide. They earn substantially less, especially starting out, but their groups are more profitable.

Nevertheless, given the substantially lower cost to the NGO of PSPs relative to FAs, after only one year, the PSPs costs per member reached are substantially lower than the FA costs. In the training year, both FAs and PSPs earn an average of \$518. In the year after certification, the cost of FAs amounts to \$660, while that of PSPs is only the phase in value of the first quarter, \$83. Thus, over two year cost of PSPs is just about half (i.e.,  $(518 + 83)/(518 + 660)$ ) of the cost of FAs, and these numbers are likely to fall over time. Since both reach similar numbers of members, the relative cost of PSP per member reached are identical, but if PSPs continue to grow relative to FAs, then these costs could fall even more rapidly.

A legitimate concern might be PSP retention, given their much lower earnings. So far the data show very little dropout of PSPs or FAs, however.

## 2.2 Impact on Households

Although the PSP program appears to be cost effective in reaching households and providing services, another important question is whether it leads to similar impacts for those households. We now turn to the household data to evaluate the relative impacts of PSP-run SILCS on households. We examine savings, credit, and productive decisions before examining an overall impact on income and expenditures.

Table 9 presents the impact estimates of  $\theta$  in equation (3) for savings behavior. We see no impact on aggregate savings overall, nor the savings of business owners, but the reported source and use of savings are both impacted. The PSP program leads to an additional \$16 of savings (per household in the village) coming from business profits, but has no impact on the amount of savings coming from agriculture or wage income. This estimate is significant at the 0.05 percent level. Similarly, an additional \$17 per household was saved by households reporting using savings for existing businesses, and this estimate is strongly significant, at the 0.01 percent level. These estimates are substantial in percentage terms, amounting to increases of over 100 and 400 percent, respectively, relative to in the FA villages. Thus, PSPs seem to have important impacts on reported business-oriented savings.

The impact on borrowing is examined in Table 10. Here we see that the PSP program led to substantially higher levels of borrowing. The estimate of \$28 dollars amounts to an increase of almost 70 percent, and it is significant at a 5 percent level. The comparably-sized estimate of \$26 for reported business owners amounts to an even higher percentage increase, and it is highly significant at a 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 SILC are \$5 higher in PSP villages (significant at a 5 percent level). We see that borrowing from informal sources is actually playing a larger role, with a coefficient of \$8 that is strongly significant at a one percent level. The point estimate on formal credit is also larger, although not significantly so. The reported purpose for borrowing is also impacted by the PSP program with an additional \$8 of credit for agricultural activities and \$10 of credit for existing businesses (both significant at the one percent level). Both of these are increases in over 200 percent. In contrast, there is no impact on credit for new businesses.

Table 11 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 less in agriculture. We find no significant impact of the PSP program on new business starts (although the power of the test is clearly weak).<sup>10</sup> We do however find significant impacts on the intensive margins of business. Business investment rises by \$20 per household in response to PSPs. Thus, business investment under the PSP treatment is roughly twice its level with the FA control. Likewise, hours spent in business is higher by 4 per week, a difference of almost 50 percent relative to in the FA control villages. The number of non-household members employed by the households in the sample is low overall (0.23 per household) with most households employing no outside workers. Still, the coefficient of 0.12 employees per household, significant at a 5 percent level, represents an increase of over 60 percent relative to the FA level. We do not see a corresponding significant increase in the hours spent as an employee, however. Respondents may be less likely to work as employees than other household members. The point estimate is positive but insignificant, and small relative to the mean. Finally, we look at agricultural decisions. Although credit for agricultural activities had been positively impacted by PSPs, the relative effect on agriculture investment is insignificant, and agricultural investment remains substantially larger than business investment. PSPs lead to fewer hours spent in agriculture relative to FAs, however. The coefficient of -3 (hours per week of the respondent) nearly offsets the positive impact on hours spent in business.<sup>11</sup>

In Table 12, we examine two simple summary measures of welfare: income and expenditures. The evidence is somewhat weak, and our experiment lacks power along these dimensions. For both total income and business income, the point estimates are substantial

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<sup>10</sup>The insignificant point estimate would indicate that the fraction of households starting new businesses in PSP areas was 6 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.

<sup>11</sup>Another dimension along which we examined the impact on households was in risk-sharing and food security. We found no significant differences in this area, however, and so we do not present the results.

but entirely insignificant. The point estimates for total expenditures (consumption plus investment) and for just consumption are positive and substantial, and marginally significant at a ten percent level. The values indicate that average annual household expenditures and consumption were \$212 and \$188 higher in PSP villages, increases of over ten percent relative to expenditures and consumption in FA villages. We do not want to overstate these point estimates. The significance is marginal, and to attribute to the increased savings, or investment and labor in business, returns on investment and labor would need to be extremely high.

### 2.3 Reasons for Differential Impact

We explore evidence relevant to three hypotheses of why PSPs lead to larger impacts on households.

The first hypothesis is that PSPs put forth more effort because, as private entrepreneurs, their pay depends on their performance. Using several measures of effort: distance traveled to SILCs, frequency of services provided, number and range of services provided, we find no real evidence of this channel. In the first quarter after randomization – when PSPs have fewer groups – PSP groups are significantly more likely to be within the PSPs own village, and less likely to be within 5 km. We find that are significantly less likely to provide services such as sensitizing the community to the SILC program or resolving disputes, but this may simply be due to the members of the groups being better known. By the third quarter, PSPs are indistinguishable from FAs. Again, fact that payments received are substantially lower for PSPs may explain these no difference results, if the substitution effect of lower returns to working offsets the income effect of lower income.

The second hypothesis is that the clients themselves put forth more effort, perhaps as a wealth effect response to the cost of services or for behavioral reasons (people may work harder when they actually pay for services). To evaluate this hypothesis, we looked at total time spent working per week. While the composition of hours was impacted by the PSP program (recall Table 11), the overall total number of hours was not significantly effected.

The final hypothesis we consider is that the PSP-led SILCs may cater to different members than the FA-led SILCs. If PSP members are more financially sound, than the SILCs themselves may be more profitable and may provide stronger intermediation among the more entrepreneurial population. We find strong suggestive evidence along this dimension.

We first examine the agent and group level dataset. Table 13 shows that although PSPs were no more likely to target their populations based on demand, they were 10 percentage points less likely to choose their target populations based on connections. This result is significant at a one percent level. Perhaps the fact that FA services are free to members

make them more likely to be provided to close social connections than the services of PSPs which come at a cost.

Next we investigate how the baseline characteristics of households who were SILC members in the endline depended on the PSP treatment. Recall, these baseline characteristics are exogenous to the randomized treatment, and so they allow us to consider pure selection. Table 14 shows that members of PSP SILCs had higher incomes, higher business incomes, and suffered less from hyperbolic discounting than members of FA SILCs. These impacts are only marginally significant, however, at a ten percent level.

Table 15 focuses on those agents who joined SILC between the baseline and endline. Again, focusing on exogenous baseline characteristics, those in PSP areas who joined were households who spent an average of four hours less per week working in agriculture, marginally significant at a ten percent level. They also had incomes nearly 50 percent higher than those who joined SILCs in FA areas, and this is significant at a 5 percent level.

Finally, in Table 16, we examine those people who left SILC between the baseline and endline. The respondents who left PSP SILCs had significantly lower savings (\$173), lower expenditures (\$868 a year), lower consumption (\$843 a year), and worked substantially more hours as employees (9 per week, 5 percent significance level). Again, these numbers are substantial in percentage terms.

In sum, we find substantial evidence that the PSPs provide services to a wealthier, more business-oriented population to provide services. Whether this is driven by PSPs themselves, or simply a result of the fees charged is still an open question.

### 3 Conclusion

We have presented evidence from a randomized experiment of an innovation for privatized entrepreneurs and member purchased self-help group services. The somewhat surprising results indicate that 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. Moreover, the groups they founded and administered led ultimately to more credit and were more profitable. Moreover, the privately provided groups had relatively stronger impacts in terms of the narrative microfinance dimensions of business entrepreneurship and investment, perhaps even increasing levels of consumption substantially.

The program is an important example of a successful privatization program – cost effective in terms of enabling NGO resources to stretch further reaching greater numbers of people.

The apparent channels are also potential lessons for future programs. It does not appear that it was mainly driven by the increased effort from improved incentives toward agents or members making either of them put forth greater effort.

Instead, it appears to be driven by a difference in the population served by privatized providers. Privatized providers target social connections less. The fact that private service providers cater to a more business-oriented population is perhaps more important. On the pro-side, this may target the services to those who benefit most from them, and indeed better targeting may help improve the functioning of the groups. On the other hand, it may create problems of cream-skimming, especially if NGOs are still interested in providing services to the truly poorest of the poor. Such considerations may be more broadly important in moves toward sustainability or privatization.

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**Table 1: Summary Statistics of MIS Data**

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<b>Variables</b>	<b>Obs</b>	<b>Mean</b>	<b>Std. Dev</b>	<b>Min</b>	<b>Median</b>	<b>Max</b>
<i>Group Data:</i>						
Members	15259	19.40	7	4	18	61
Savings <sup>a</sup>	15259	336.02	395	0	211.70	9425.50
Outstanding Loans	15258	10.96	8	0	10	41
Value of Outstanding Loans <sup>a</sup>	15258	330.12	430	0	185.52	6804.30
Profit / Loss <sup>a</sup>	8219	25.81	32	-160.26	10.95	124.38
Agent Pay <sup>a</sup>	13800	5.70	7	0	4.89	71.4
Dummy for Kenya	15259	.56	.50			
Dummy for Tanzania	15259	.30	.46			
Dummy for Uganda	15259	.13	.34			
Dummy for Wave	15259	.76	.43			
Dummy for PSP	15259	.66	.47			
<i>Agent Data:</i>						
Members	846	349.92	180	13	320	1105
Savings <sup>a</sup>	846	6060.61	5650	145.78	4627.51	46863.95
Outstanding Loans	846	197.70	140	0	167	859
Value of Outstanding Loans <sup>a</sup>	846	5953.60	6160	0	4269.16	45399.01
Profit / Loss <sup>a</sup>	846	250.73	345	-160.26	15.57	1643.56
Agent Pay <sup>a</sup>	846	92.94	105	0	62.86	902.62
Number of Groups	846	18.04	8	1	17	54
Dummy for Kenya	846	.48	.50			
Dummy for Tanzania	846	.36	.48			
Dummy for Uganda	846	.16	.37			
Dummy for Wave	846	.78	.42			
Dummy for PSP	846	.68	.47			

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a. All monetary values are in U.S. Dollars

**Table 2. Randomization Results**

	Male	Age	Primary	Primary Complete	Secondary	Tertiary	Languages	Children	Dependents
Treatment	-.07	-.11	0.00	-.13***	.13**	0.00	.09	-.08	-.31
	(.07)	(1.06)	(.02)	(.04)	(.06)	(.04)	(.07)	(.37)	(.57)
Observations	240	235	239	239	239	239	240	240	239
Control Mean	.69	36.08	.012	.463	.425	.100	1.94	4.59	6.38
Sample Mean	.64	35.76	.010	.359	.519	.108	2.06	4.50	6.16
Median		36					2	4	6

**Table 11: Effect of PSP Treatment on Provision of Services**

	Community Sensitization	Training Meetings	Meeting Monitoring	Book Keeping	Conflict Resolution	Shareout Support	Linking to Other Services	Other Training	Other
Treatment	-.10**	-.01	.04	-.05	-.12**	.01	-.08	-.09**	-.01
	(.05)	(.05)	(.06)	(.05)	(.05)	(.06)	(.05)	(.04)	(.01)
Observations	4234	4234	4234	4234	4234	4234	4234	4234	4234
	Bi-Weekly Meetings		Within Community		0-5 km	5-10 km	10-20 km	20 or more km	
Treatment	.05		.05*		-.06*	0.00	0.00	.02	
	(0.05)		(0.03)		(0.04)	(0.04)	(0.03)	(0.01)	
Observations	4207		4212		4212	4212	4212	4212	



# Preliminary Results (with zeros)

## 1 Introductionary table

Table 1: Randomization Results Regressors

	PSP			FA			PSP-FA
	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.	Mean $\Delta$
Age	43	14	1362	42	13	536	0.31
Age Squared	2014	1284	1362	1986	1319	536	29
Gender	0.61	0.49	1363	0.58	0.49	534	0.03
# Adult Men	1.6	1.1	1380	1.5	1.10	539	0.06
# Adult Women	1.5	0.92	1380	1.6	0.95	539	-0.05
# Kids	2.6	2.0	1380	2.7	1.9	539	-0.07
No Schooling	0.22	0.41	1363	0.19	0.40	534	0.02
Some Primary	0.23	0.42	1363	0.21	0.41	534	0.02
Primary Completed	0.41	0.49	1363	0.49	0.50	534	-0.08***
Some Secondary	0.08	0.28	1363	0.05	0.22	534	0.03**
Secondary Completed	0.03	0.17	1363	0.04	0.20	534	-0.01
Technical after Secondary	0.02	0.14	1363	0.01	0.11	534	0.01
University	0.01	0.09	1363	0.00	0.06	534	0.00

## 2 Baseline

Table 2: Baseline Summary of Savings Activity

	Savings		Source			Purpose		
	Total	Business Owners	Business Profit	Sell Agric. Product	Salary or Wage	New Agric. Activity	New Non-Agric. Activity	Existing Business
PSP	1.1	-7.3	-5	3	-4	-5	4	3
s.e.	(19)	(22)	(9)	(9)	(7)	(14)	(4)	(9)
FA mean	137	156	35	25	16	40	5	16
Sample mean	137	156	32	29	17	38	8	20
Median	49	75	0	0	0	0	0	0
Observations	1877	857	1877	1877	1877	1877	1877	1877

### 3 Endline

Table 6: Endline Summary of Savings Activity

	Savings		Source			Purpose		
	Total	Business Owners	Business Profit	Sell Agric. Product	Salary or Wage	New Agric. Activity	New Non-Agric. Activity	Existing Business
PSP	16	-4.8	16**	4	6	0.51	-2	17***
s.e.	(16)	(21)	(7)	(13)	7	(11)	(2)	(5)
FA mean	132	156	15	41	10	39	4	4
Sample mean	141	153	24	37	15	37	3	15
Median	61	83	0	0	0	0	0	0
Observations	1891	865	1891	1891	1891	1891	1891	1891

Table 7: Endline Summary of Credit Activity

	Credit		Source			Purpose		
	Total	Business Owners	SILC	Formal	Informal	Agric. Activity	Expanding Business	Start New Business
PSP	28**	26***	5**	16	8***	8***	10***	2
s.e.	(11)	(8)	(2)	(10)	(3)	(3)	(3)	(1)
FA mean	41	32	7	22	10	4	4	2
Sample mean	56	50	10	30	16	8	10	3
Median	11	15	0	0	0	0	0	0
Observations	1891	865	1891	1891	1891	1891	1891	1891

Table 8: Endline Summary of Productive Decisions

	Start New Business	Business Investment	Hours spent in Business	Employees (non-HH)	Hours spent as Employee	Agric. Investment	Hours spent in Agric.
	PSP	0.05	20***	4**	0.12**	0.94	4
s.e.	(0.06)	(5.7)	(1)	(0.1)	(2)	(9.4)	(1)
FA mean	0.2	22	9	0.11	14	67	31
Sample mean	0.24	35	12	0.19	15	69	29
Median	0	0	0	0	10	28	30
Observations	1891	1891	1891	1891	1891	1891	1891

Table 9: Endline Summary of Income Activity

	Respondent		Household		Total Expenditures	Total Consumption
	Total Income	Business Income	Total Income	Business Income		
PSP	131	10	43	11	212*	188*
s.e.	(85)	(12)	(59)	(14)	(111)	(109)
FA mean	358	54	511	67	1598	1561
Sample mean	451	62	524	75	1717	1664
Median	196	0	320	0	1394	1356
Observations	1891	1891	1891	1891	1891	1891

## 4 Reasons

Table 10: Baseline Characteristics for Endline SILC members

	Total Income	Business Income	Hyperbolic disc
PSP	83*	29*	-0.09*
s.e.	(45)	(16)	(0.05)
FA mean	246	49	0.45
Sample mean	305	70	0.51
Median	142	0	1
Observations	1250	1250	1250

Table 11: Baseline Characteristics for non-SILC to SILC members

	Hours spent in Agric.	Income Respondent
PSP	-4*	137**
s.e.	(2)	(55)
FA mean	30	213
Sample mean	27	308
Median	25	135
Observations	387	387

Table 12: Baseline Characteristics for SILC to non-SILC members

	Total Saving	Total Expenditure	Total Consumption	Hours as Employee
PSP	-173*	-868**	-843**	9**
s.e.	(102)	(434)	(407)	(4)
FA mean	311	2215	2116	7
Sample mean	178	1544	1465	14
Median	78	1338	1251	0
Observations	105	105	105	105

**Table 7: Agent Level Results:**

	Groups	Members	Savings	Loans	Loan Value	Profit / Loss	Agent Pay
Treatment	-2** (1)	-44** (23)	-640 (670)	-19 (17)	-640 (680)	7 (30)	-100*** (7)
Prior to Treatment	0 (0)	-19 (14)	-890** (390)	-9 (10)	-960 (410)**	8 (12)	
Treatment Quarter 1	-4*** (1)	-76*** (23)	-1680** (690)	-53*** (16)	-1960*** (680)	-88*** (29)	-149*** (5)
Treatment Quarter 2	-2*** (1)	-48** (22)	-1360** (690)	-28* (17)	-1260* (710)	33 (29)	-107*** (8)
Treatment Quarter 3	-1 (1)	-20 (29)	570 (820)	12 (22)	650 (850)	26 (41)	-52*** (16)
Treatment Quarter 4	3 (2)	11 (43)	570 (820)	36 (37)	2060* (1690)	154*** (63)	-41** (23)
Observations	700	700	700	700	700	700	700
Control Mean	19	375	6200	204	5940	211	164

**Table 8: Group Level Results:**

	Members	Savings	Loans	Loan Value	Profit / Loss	Agent Pay
Total	0 (1)	0 (29)	0 (1)	-5 (29)	5* (2.3)	-4*** (1)
Prior to Treatment	-1* (1)	-71*** (26)	-1 (1)	-78*** (27)	0 (2)	
Treatment Quarter 1	1 (1)	-14 (31)	0 (1)	-40 (30)	-1 (3)	-7*** (1)
Treatment Quarter 2	0 (1)	-34 (32)	0 (1)	-33 (33)	5** (2)	-5*** (1)
Treatment Quarter 3	0 (1)	40 (33)	1 (1)	43 (34)	7*** (3)	-2*** (1)
Treatment Quarter 4	-2** (1)	25 (46)	0 (1)	28 (52)	6* (4)	-3*** (1)
Observations	13149	13149	13148	13148	6841	12171
Control Mean	20	326	11	313	21	9

Clustered standard errors in parenthesis

\*\*\* denotes significance at 99% level

\*\* denotes significance at 95% level

• denotes significance at 90% level

**Table 2: Summary Statistics of Survey Data**

<b>Variables</b>	<b>Obs</b>	<b>Mean</b>	<b>Std. Dev</b>	<b>Variables</b>	<b>Obs</b>	<b>Mean</b>	<b>Std. Dev</b>
<i>Agent's Targeting Criteria</i>				<i>Services Provided</i>			
Proximity	5122	.40	.49	Community Sensitization	5122	.39	.49
Connections	5122	.54	.50	Training Meetings	5122	.48	.50
Ability to Pay	5122	.32	.47	Meeting Monitoring	5122	.70	.46
Need	5122	.76	.43	Book Keeping	5122	.68	.47
Demand	5122	.28	.45	Conflict Resolution	5122	.51	.50
Other	5122	.01	.10	Support at Shareout	5122	.44	.50
<i>Compensation Scheme<sup>a</sup></i>				Linking To Other Programs	5122	.23	.42
Fixed Fee Per Member	1932	.64	.48	Training on Other Topics	5122	.15	.36
Fixed Fee Per Group	1932	.03	.16	Other Services	5122	.01	.08
Share of Savings	1932	.004	.06	<i>Distance to Group</i>			
Share of Profits	1932	.04	.19	Within Community	5096	.23	.42
Other Scheme	1932	.30	.46	Within 5 km	5096	.30	.46
<i>Other<sup>a</sup></i>				5-10 km	5096	.32	.46
Difficulty Negotiating Payment	2296	.37	.48	10-20 km	5096	.13	.34
Difficulty Collecting Payment	2038	.32	.47	20 or more km	5096	.02	.14
				<i>Frequency of Meetings</i>			
				Meet at least Bi-weekly	5088	.44	.50

a. Sample includes only PSPs

**Table 9: Clients Targeted by PSPs / FAs**


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	Target	Target Connections	Target Need	Target Demand	Target Other
	Proximity				
Treatment	-.025	-.098***	-.026	-.025	-.003
	(.023)	(.023)	(.017)	(.021)	(.004)
Observations	2176	2176	2176	2176	2176
Control Mean	.448	.541	.885	.254	.005
Sample Mean	.486	.534	.839	.236	.005

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**Table 10: Relationship Between Compensation Scheme and Group Outcomes:**


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	Members	Savings	Loans	Loan Value	Profit / Loss	Agent Pay
<i>Compensation Scheme</i>						
Fixed Fee Per Member	2.42**	158.24*	.20	70.46	-.42	2.27***
	(1.23)	(83.69)	(1.19)	(80.54)	(2.57)	(.87)
Fixed Fee Per Group	1.22	116.29	-1.85	94.85	2.38	2.38
	(1.93)	(107.23)	(1.76)	(134.75)	(4.77)	(2.08)
Share of Savings	1.92	193.16	-1.62	230.21	2.19	2.68
	(2.18)	(295.93)	(2.44)	(284.94)	(3.24)	(3.17)
Share of Profits	-.79	-42.94	-.18	-73.75	2.30	-.43
	(1.53)	(71.80)	(1.50)	(78.19)	(3.85)	(.77)

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Standard errors in parenthesis

\*\*\* denotes significance at 99% level

\*\* denotes significance at 95% level

• denotes significance at 90% level