Can Self-Help Groups Really Be Self-Help?

Can Self-Help Groups Really Be "Self-Help"?

Brian Greaney [*] Joseph P. Kaboski [†] Eva Van Leemput [‡§¶]

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Abstract

This paper examines a cost-reducing innovation to the delivery of "Self-Help Group" microfinance services. These groups typically rely on outside agents to found and administer the groups although funds are raised by the group members. The innovation is to have the agents earn their payment by charging membership fees rather than following the status quo in which the agents are paid by an outside organization and instead offer free services to clients. The theory we develop shows that such membership fees could actually improve performance without sacrificing membership, simply by mitigating an adverse selection problem. Empirically, we evaluate this innovation in East Africa using a randomized control trial. We find that privatized entrepreneurs providing the self-help group services indeed outperform their NGO-compensated counterparts along several dimensions. Over time, they cost the NGO less and lead more profitable groups; also, households with access to privately-delivered groups borrow and save more, invest more in businesses, and may have higher consumption. Consistent with the theory, these privatized groups attract wealthier, more business-oriented members, although they attract no fewer members.

[*]Federal Reserve Bank of St. Louis.

[†]University of Notre Dame and NBER

[‡]University of Notre Dame

[§]Corresponding Author: Kaboski, Department of Economics, 717 Flanner Hall, Notre Dame, IN 46556, jkaboski@nd.edu. Research funded by the Bill & Melinda Gates Foundation grant to the U. of Chicago Consortium on Financial Systems and Poverty. We are thankful for comments received from presentations at BREAD/Federal Reserve Bank of Minnesota Conference and Clemson University. We have benefited from help from many people at Catholic Relief Services, especially Marc Bavois and Mike Ferguson. We have also benefitted from the work of excellent research assistants: Luke Chicoine and Katie Firth in the data collection, and Melanie Brintnall in the data analysis.

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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] Although SHGs are partially "self help" in that funds are raised *internally*, they are not fully self-help, since the programs generally depend on continued *outside* assistance from administrative agents in their founding and administration. This motivates an important question, especially in the context of the scalabilty and financial sustainability of these types of programs: Can cost reduction or recovery be effective in the delivery of these program? The question is common to many aid programs, and the answer is not obvious. Recent research has shown that small costs to clients greatly reduce both take up and program effectiveness in other aid programs.[2] We provide some theory and evidence, however, that a cost-recovery approach can actually be effective in the context of SHGs.

This paper examines an innovation to the provision of NGO-sponsored microfinance services in East Africa. The status quo delivery mechanism was a typical "continous subsidy" program, in which, after training agents, the NGO would continually pay these agents 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 eventually lower costs to the NGO, but to also expand access to services. Some programs already follow such an approach. A major World Bank/Indian government initiative with goals of reaching 70 million new households is an important example.[3] Hence, both the type of program and innovation we study are of great interest. We examine the impacts of this delivery innovation using a theoretical model and a randomized control trial 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

[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 bednets.

[3] The Rural Poverty Reduction Program in Andhra Pradesh, India, was a nearly $300 million project betwen 2000-2009, which has trained 140,000 "community professionals" (privatized providers), reaching 9 million women through 630,000 SHGs. In 2010, it was expanded to 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 years (World Bank, 2007, 2012).

being "self-help," in the sense of financial independence. Our theory shows how a simple method of cost-recovery via membership fees can actually help solve an adverse selection problem that can plague credit cooperatives, especially when services are freely provided. Empirically we find that the agents who charge membership fees, although their groups are slower to grow initially, reach the same number of clients after a year as the status quo agents who provide the services for free. The composition of the entrepreneurs' clientele is very different, however, with the clients themselves being more business-oriented and having a larger demand for financial services. The treatment effects of these cooperatives reinforce this selection. The private entrepreneurs' groups are ultimately more profitable, and the households in the areas they serve shift even more toward business activity. More specifically, the groups lead to higher levels of: savings from business activities; credit, especially to business owners; employees; and business investment. They lead to households in these areas spending 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 fact 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 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, including CARE, OxFam, Plan, World Vision, and perhaps most importantly, NABARD, a large government agency in India. In practice, SILCs are small groups of 10-25 members that 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. They do not receive external financial resources but 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.[4]

To fix ideas, we develop a simple theory of SHGs as savings and loan cooperatives that enable investment in larger-scale, more-productive activities. Investments are risky, however, and the population of potential members varies in its inherent rate of success. This success determines repayment; good types succeed and repay with a higher probability than bad types. The benefits to joining the cooperative depend on the average repayment rate and,

[4]The "self-help" goals of these groups are not limited to only 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.

therefore, the composition of the cooperative. Bad types have nothing to lose by joining the cooperative, but since they succeed less often, their potential benefits are also relatively small. In a cooperative with a high repayment rate, good types have potentially more to gain since the more productive investment that the cooperative enables is more likely to succeed. But they also have a higher outside option, so they can be worse off in a cooperative with a substantial amount of bad clients. Thus, bad types can drive out good types. However, since bad types also have less to gain, a membership fee can drive them out of the cooperative. It is perhaps not surprising to find that fees can discourage some people from joining; what is surprising is that they can induce others (the good types) to actually enter by improving the composition. In this way, membership fees can actually increase total surplus by mitigating this selection problem.

Empirically, we conducted a large randomized control trial involving 276 agents who started a total of over 5700 groups serving over 100,000 members across 11 districts in Kenya, Tanzania, and Uganda. All agents, drawn from local communities, spent one year working as a "Field Agent" (FA), during which time they established and assisted a fixed number of groups and received both training and compensation from CRS according to the status quo. After completing this course and passing an 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 follow the status quo, remaining 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.

Our study uses several sources of data to evaluate the experiment. First, eight quarters of accounting reports 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 semi-annually by a brief questionnaire on the agent characteristics and experiences. Third, a baseline survey of village-level key informants, and, fourth, a stratified two-period before-after panel of 10 households in each of 192 villages served by the program were conducted. These data 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 accounting reports shows that, by one year, PSP treatment increases group profitability by approximately 50%. After three months, a PSP works with three fewer groups and 65 fewer clients than a traditional FA on average, but this difference shrinks over time and after one year the numbers become statistically insignificant. 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, with the point estimates actually becoming positive. The negative impacts early on are driven largely by the lower number of groups; at the level of individual groups, PSP groups are indistinguishable from FA groups early on. The relative increase reflects both "catch up" in the number of groups, but also better performance of PSP groups over time. By 12 months PSP-run groups intermediate substantially more savings and credit and are more profitable. Nonetheless, PSPs earn significantly and substantially less than their FA counterparts. Accumulated over the first year, they earn about a quarter of what FAs receive. Nevertheless, per group payments converge by 12 months, and agent attrition is low. Overall, PSPs are substantially more cost effective, reducing costs of providing services by over 40 percent after two years.

The question of how the benefits to households compared under the privatized entrepreneur approach relative to the status quo is also important, however. 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 find that the PSP approach is significantly more effective in delivering outcomes promoted by microfinance. The PSP treatment leads to nearly $29 (or 70 percent) more credit per household. Several measures of business activity are significantly higher as a result of the PSP treatment. About twice as much savings reportedly comes from business profits and nearly four times 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 twice as high as in the FA villages, although this number is still quite low (just 0.11 employees per household 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 10 percent for both total expenditures ($208) and consumption ($184) are marginally significant.

In examining why the PSP groups not only flourish but also lead to greater impact on households, we find evidence in line with the above theory. Namely, the households who join (and leave) SILC under the PSP program are different from those under the FA program. The members of PSP SILCs tend to have higher pre-existing levels of income

and savings. They were already more business oriented, with higher pre-existing levels of business income and time spent working as entrepreneurs. Finally, they suffered less from hyperbolic discounting preferences. Thus, it appears that PSPs, whether through intentional targeting or simply because of the required fees, cater to a more affluent, business-oriented membership. Again, we stress that the fees did not change the fraction of people in a village who were members but only their composition. The relative benefits of the PSP program were also disproportionately concentrated on these higher income populations. Beyond this selection story, we found no evidence in favor of other channels. PSPs do not appear to work harder than FAs. Again, this may be the result of better targeting of services. We also found no evidence that the members themselves work harder overall. They changed the composition of their work hours (toward business and away from agriculture) but not the total number of hours. This suggests that beyond the incentive to recoup costs, other incentives that accompanied privatization may not have been important. In evaluating the improvement, it is difficult to determine whether privatization is necessary beyond simply charging fees. Theory suggests that catering to local market demand by charging multiple fees that screen could be beneficial. Such catering might be difficult in a centralized system. We find high variability in the fees charged, and, consistent with this theory, the impacts of the PSP program are closely tied with multiple fees being charged. However, we stress that these last results are non-experimental.

Related Literature

The results of this paper contribute to several literatures.

First, a number of recent papers have examined attempted moves toward financially self-sustaining approaches 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 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. Moreover, the theory suggests the possibility that the cost-savings and increased impact could be achieved without privatization by the NGO simply charging membership fees, and this is consistent with our lack of evidence on the importance of incentives of privatization along other dimensions.

Our finding that attempts to recoup program costs actually improve the efficacy of the service without lowering the number of clients served is strikingly different from what has

been found with cost-sharing in health-related services. 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. Cohen and Dupas (2010) find a 60 percent drop in uptake from introducing a 10 percent cost recovery program for insecticide-treated bed nets. Problems with the privatized investment and sustainability of clean water sources in developing countries have also been well-examined (Kremer et al., 2011). Moreover, in health-related services, the positive externality of treatment creates a public good aspect, improving the justification for high-coverage and sustained subsidies.[5] Our theory and evidence shows that financial services are qualitatively different along this dimension.

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., Ahlin and Jiang (2008), 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) report that microfinance led to fewer businesses and fewer workers hired. In Thailand, Kaboski and Townsend (2012) find large increases in consumption, hiring workers, and wages consistent with the entrepreneurship story, but they find only small impacts on investment and no significant impact on entrepreneurship. Banerjee et al. (2011) measured only marginal increases in investment and no impacts on consumption in India. In Morocco, Crépon et al. (2011) found increases in income, expenses, and labor, but their study is not well-designed for finding increases in entrepreneurship, since nearly all households were already operating their own technology. In Mongolia, Attanasio et al. (2011) measured substantial increases in entrepreneurship, but only among females and the less educated and only when microfinance loans are joint liability. Field et al. (2009) showed that longer grace periods were associated with more new businesses and higher business investment, though also higher default rates. We note that the details or policy of the microfinance institutions are important in these last two studies, as also found in the earlier work of Kaboski and Townsend (2005). Consistent with these papers, our results here suggest a possible explanation for the conflicting results in this literature: the delivery mode and incentives faced by institutions may greatly alter the impact of microfinance.

[5] 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, Galiani et al. (2005) 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.

Third, a large theoretical literature in development has examined the behavior of credit markets in the presence of asymmetric information, including the design of cooperatives and lending groups (e.g., Banerjee et al. (1994) and Ahlin and Townsend (2007), respectively). The seminal works of Stiglitz and Weiss (1981) and De Meza and Webb (1987) analyze the impact of adverse selection on the provision of credit, with the former showing how it could lead to underprovision and underinvestment and the latter giving results for overprovision and overinvestment. Our contribution is to show that two-part pricing can mitigate this adverse selection. Although closer to De Meza and Webb (1987), we differ in an important way. Namely, the outside option of projects varies by type. This unique feature drives our results and precludes any possibility of overinvestment (unless one takes into account the cost of intermediation), since the values of all projects exceed their opportunity costs.

Finally, given the rising importance of SHGs, within the broader microfinance literature several other recent papers focus specifically on SHGs.[6] 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 the outside and privately delivered 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 find only increases in nutrition and female empowerment over shorter horizons. Two recent randomized control trials of CARE's 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. 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*. In India, Casini and Vandewalle (2012) demonstrate that SHG composition can be linked to how much collective action is taken toward the provision of public goods. We show that the composition of members can be linked to the financial benefits of membership.

[6] 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 is 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 more limited than SHGs, however. For example, there is substantially less flexibility in the amount and timing of deposits and loans, and there is no net saving or borrowing, even at the individual level.

The remainder of the paper is organized as follows. Section 2 presents a simple theory of a credit cooperative and the potential impact of membership fees. Section 3 describes the program, experiment, data, and methods. Section 4 presents the results and analysis. We conclude in Section 5.

1 Model

We present a simple model of a SHG, as a credit cooperative operating in the face of adverse selection. The model is stylized, but it yields two important results. First, low-quality members can drive out high-quality members, lowering aggregate output. Second, membership fees can potentially solve this adverse selection and thereby increase aggregate output.

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 available 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 measure θ_L individuals 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, the expected payoff of type-H individuals with the small-scale project exceeds 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 technology. 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 they make decisions 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 and those borrowing for small-scale production. The member's type is unknown when making 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 3.4.

1.2 Individual Decisions

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

$$p_i \left(\overline{A}k - R_B k + R_D \right) \tag{1}$$

if she runs a large-scale project;

$$p_i \left(\underline{A} - R_B + R_D \right) \tag{2}$$

if she runs a small-scale project; and

$$R_D; \tag{3}$$

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

$$p_i \underline{A}$$

which we refer to as her outside option.

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 as shown in Figure 1. The willingness to pay thresholds can be solved by equating the value of borrowing and investing (i.e., (1) or (2) for the large- or small-scale projects,

respectively) with the value of simply saving (i.e., (3)) and solving for R_B.[7] For any $R_D \geq 0$, type-H individuals with the large-scale opportunity have the highest willingness to pay ($\overline{R}_{BH}$) while type-L financing the small-scale project have the lowest willingness to pay ($\underline{R}_{BL}$).

Whether type-L individuals have a higher willingness to pay for the large project than type-H have for the small project depends on R_D and parameter values. The interesting case, which we have plotted, is $\overline{R}_{BL} > \underline{R}_{BH}$, since it can lead to adverse selection; type-L individuals have a higher willingness to pay, even though their expected payout is lower (i.e., $p_H\underline{A} > p_L\overline{A}$). This higher willingness to pay arises from the fact that large projects are not fully collateralized. 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. Indeed, since $1/k$ is the collateral ratio for the large project, the $\overline{R}_{BL} > \underline{R}_{BH}$ condition always holds for sufficiently large k.[8]

As Figure 1 illustrates, the demand for loans depends on the parameters governing loan demand like π and k. The total supply of savings (per member) in the cooperative is one. These parameters determine the equilibrium R_B (and R_D). If πk is too large, then even a small number of Type-H individuals in the group can use all the funds for the large-scale project, $R_B = \overline{R}_{BH}$, and there will be no adverse selection problem. Alternatively, if πk is sufficiently small, some type-H individuals will finance the small-scale project, $R_B \leq \underline{R}_{BH}$, and so they can do no worse than their outside option. Hence, the presence of type-Ls cannot inhibit type-Hs from joining.

The interesting case occurs when $R_B = \overline{R}_{BL}$. One can simplify the analysis of this case, without losing the interest features of the equilibrium, with the following parameter assumption:

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

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

$$\begin{aligned} \overline{R}_{Bi} &= \overline{A} - \left(\frac{1-p_i}{p_i k}\right) R_D \\ \underline{R}_{Bi} &= \underline{A} - \left(\frac{1-p_i}{p_i}\right) R_D. \end{aligned}$$

[8] For $R_D = 0$, the condition always holds. The following condition is sufficient to ensure that it holds for any R_D:

$$\frac{1}{k} < \frac{p_L}{p_H}\left(\frac{1-p_H}{1-p_L}\right).$$

This can be easily verified by applying the formulas in Footnote 7.

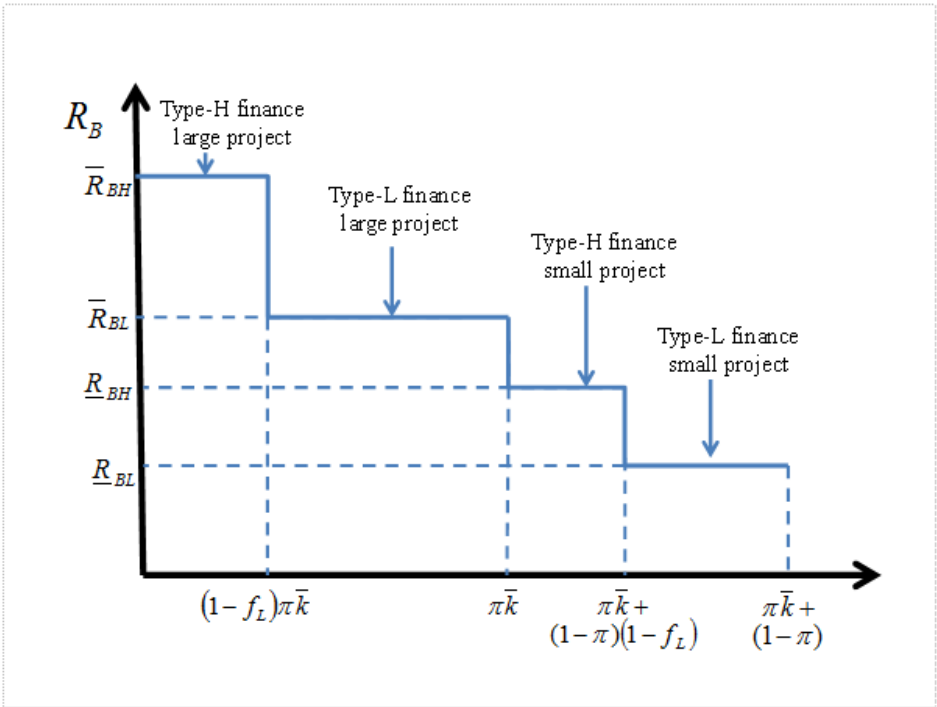

Fig. 1: (Per Member) Demand for Loans

Effectively, market clearing is simplified because the total demand for loans from those with large-scale projects equals the supply of savings ($\pi k \to 1$).[9]

Define f_L as the fraction of members of the cooperative that are type L. The break-even condition in per-member terms can be written and simplified as

$$\pi f_L p_L k R_B + \pi (1 - f_L) p_H k R_B = \pi f_L p_L R_D + \pi (1 - f_L) p_H R_D + (1 - \pi) R_D,$$

where the left-hand side is total loan repayments and the right-hand side is return on savings to depositors. We can define $p_{avg}(f_L) \equiv f_L p_L + (1 - f_L) p_H$ as the average probability of repayment, which is decreasing in f_L. Simplifying yields

$$\begin{aligned} \frac{p_{avg}(f_L)}{\pi p_{avg}(f_L) + (1 - \pi)} R_B &= R_D \\ \phi(f_L) R_B &= R_D. \end{aligned} \tag{4}$$

Here we have defined $\phi(f_L) \equiv \frac{p_{avg}(f_L)}{\pi p_{avg}(f_L)+(1-\pi)}$ as the effective repayment rate given the partial collateral of savings. Note that $\phi < 1$, so that $R_B > R_D$. The effective repayment is larger, the higher the actual probability of repayment $p_{avg}(f_L)$ and the lower the fraction of type-L in the cooperative, f_L.

[9] Formally, we analyze the model under the conditions $\lim_{\varepsilon \to 0^+} \pi\overline{k} + \varepsilon$ and $\lim_{\varepsilon \to 0^-} \pi\overline{k} + \varepsilon$.

One can easily solve for $\overline{R}_{BL}$ by setting (1) equal to zero for $i = L$ and substituting in (4):

$$\overline{R}_{BL}(f_L) = \frac{p_L \overline{A} k}{p_L k + (1 - p_L)\phi(f_L)}. \tag{5}$$

Now 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 income as a member and expected income if not a member of the cooperative. Since our focus on $R_B = \overline{R}_{BL}$ removes the small-scale project as a relevant option inside the cooperative, this can be expressed simply as

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

where $\tilde{p}_i$ indicates the success probability of the particular individual. In the case that $B(f_L; \tilde{p}_i) > 0$, the type-i individual joins. If $B(f_L; \tilde{p}_i) = 0$, the individual is indifferent. 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; but this advantage is larger, the greater the individual's probability of success. 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) The net benefit of joining declines in the proportion of membership that is type-L, $\frac{\partial B(f_L; \tilde{p}_i)}{\partial f_L} < 0$.
(iii) Type-H members are especially hurt by a high proportion of type-L members, $\frac{\partial B(f_L; \tilde{p}_H)}{\partial f_L} < \frac{\partial B(f_L; \tilde{p}_L)}{\partial f_L}$, *and their net benefit from joining a cooperative of all type-L members is smaller,* $B(1; \tilde{p}_H) < B(1; \tilde{p}_L)$.
(iv) For sufficiently low π *(*π *less than some* $\overline{\pi} \in (0, 1)$*), type-H individuals won't join a cooperative of all type-L members,* $B(1; \tilde{p}_H) < 0$.
(v) For sufficiently high π *(*π *greater than some* $\underline{\pi} \in (0, 1)$*), type-H individuals benefit more than type-L do from joining a cooperative of all type-H members,* $B(0; \tilde{p}_H) > B(0; \tilde{p}_L)$.
(vi) Intermediate values of π *exist where both (iv) and (v) apply, in particular, if* p_L *is sufficiently low; if* $p_L < \overline{p}_L$, $\overline{\pi} > \underline{\pi}$.

Proof of the proposition is straightforward and given in the 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. Type-Hs only benefit by financing the large project, so if the probability of getting a large project is small enough, they are worse off as members. Finally, when the composition is good, type-H individuals potentially have more to gain from financing large projects, since they succeed more often and earn a premium over the deposit rate. If they finance large-scale projects with a high enough probability, this more than compensates for their higher outside option.[10]

The top panel of Figure 2 shows these results graphically for an intermediate value of $\pi \in (\underline{\pi}, \overline{\pi})$. In such a case, although type-L 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 in the population is high enough, $\theta > \hat{f}_L$, then type-H never join and the equilibrium (denoted f_L^E) is $f_L^E = 1$. All type-L join, but type-H do not.[11]

1.4 Recouping Intermediation Costs

Now consider the possibility of recouping the intermediation cost, C, by introducing a flat membership fee F. We show how this could actually increase total output and the surplus of members.[12]

In the case of the upper panel of Figure 2, 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 a good membership, and a membership fee has the potential of driving

[10] 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.

[11] 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 f_L. Of course, in all cases there are also additional trivial equilibria where no one joins.

[12] For simplicity, 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 the 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 collateral.

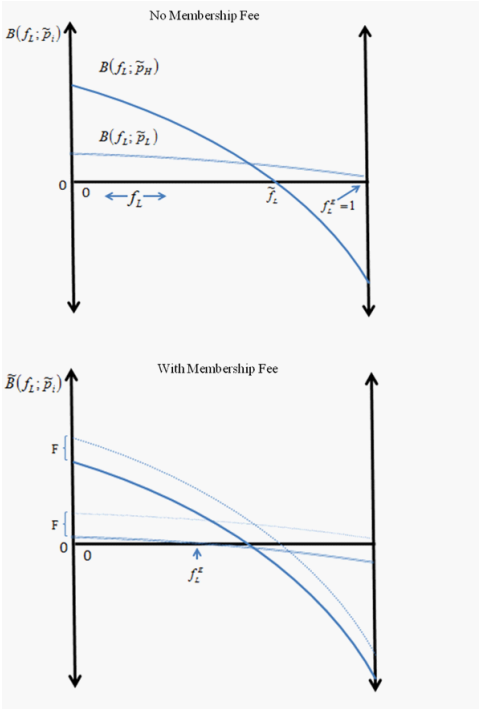

Fig. 2: Benefits of Joining vs. Fraction Type-L

out type-L individuals, thereby inducing type-H individuals to join. Define $\tilde{B}(f_L;\tilde{p}_H) = B(f_L;\tilde{p}_H) - F$. The membership F can ensure that the $\tilde{B}(f_L;\tilde{p}_H) = \tilde{B}(f_L;\tilde{p}_L)$ intersection is less than zero. If the relative benefits of type-H are high enough, this can actually increase average income even net of the payments. In such a case, illustrated in the lower panel of Figure 2, the unique equilibrium value of f_L^E is then at the point where type-L individuals are indifferent, $\tilde{B}(f_L;\tilde{p}_L) = 0$.[13]

We summarize this in the following proposition.

Proposition 2 *If $\pi \in (\underline{\pi}, \overline{\pi})$ and $\theta > \hat{f}_L$, there exists a membership fee F that induces some (or even all) type-L members to not join the cooperative and type-H members to join. For sufficiently low p_L and π, intermediate values of θ ($\hat{f}_L < \theta < \overline{\theta}$) exist, for which the total income in the economy net of fees increases for some F.*

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.

[13] Since total output is increasing 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 $f_L = 0$.

Consider now the optimal policy, optimal 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 $\tilde{B}(f_L;\tilde{p}_H) = \tilde{B}(f_L;\tilde{p}_L)$ in Figure 2 just below zero because it maximizes the number of type-L who enter, while ensuring that all type-H enter. This fee leaves the members with no surplus, however.

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 surplus than type-L members joining. In this case, the fee that maximizes total surplus to members is the one that maximizing 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 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, and we constrain $F > 0$. The cooperative can attract both types by offering a large F but smaller ϕ that is attractive to type-H, and a small F but larger ϕ that is attractive to type-H; and there are many such contracts that would accomplish this.[14] 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 instead starting two different cooperatives with different membership fees. The contracts that maximizes 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 $\phi(0)$ as an equilibrium, break-even value).

Denoting the output (net of fees) under this equilibrium with two different cooperatives as Y_2^*, the output (net of fees) under the single F^* fee equilibrium as Y_1^*, and the output under no fees as Y_0^*. The following proposition summarizes how the benefits of the program vary with fee structure for the examples discussed above.

Proposition 3 *If $\pi \in (\underline{\pi}, \overline{\pi})$ and $\theta \in \left(\hat{f}_L, \overline{\theta}\right)$, then the maximum output under two fees exceeds that under the single fee F^*. Likewise, the maximum output under a single fee F^* weakly exceeds that under no fee ($Y_2^* > Y_1^* > Y_0^*$).*

[14] There is also the possibility of adjusting R_B away from $\overline{R}_{BL}$, 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. Since individuals are risk neutral, this only affects ex post inequality but not their ex ante valuation.

The above proposition motivates a simple test in Section 3.3.

Finally, we note that while the multiple fees could potentially increase 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, exceeding the benefit of serving the type-H population, $(1-\theta_L)\,p_L(\overline{A}-\underline{A})$, social surplus is maximized with no cooperatives and no members. For a very low cost of intermediation C, 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 therefore 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.

1.5 Connection with Empirics and Extensions

The model is intentionally stylized, but one can consider interpretations and extensions that motivate the empirical measures used in the next section. In the context of the empirical setting, one might think of the small-scale project as a subsistence or self-employment work and the large-scale project as a more entrepreneurial investment opportunity. The adverse selection is captured by the probability of success in the model, but this can easily be reinterpreted as simply an unobserved productivity parameter. Neither productivity nor default rates are directly observed, however.

Instead, we observe selection based on income, business income and hours, savings, and discount rates. We also observe many of these same variables as outcome measures. Extensions that are less transparent but straightforward in principle accomodate a clearer mapping to these measures. Since the model works using a wedge between borrowing and savings, heterogeneity in initial wealth (e.g., savings or income) would lead to similar results as heterogeneity in productivity: That is, those with more assets would also have a higher willingness to pay for membership in a well-composed cooperative. Similarily, in a two-period model with both heterogeneous and stochastic discount factors, more patient individuals have a higher willingness to pay, and as an outcome will tend to save more, which also leads to more investment. In such a model, loans for low-probability-of-success projects could be replaced by consumption loans. In any case, the critical assumption is that such measures (income, savings, patience, etc.) are also positively correlated with the underlying heterogeneity driving the adverse selection.

For Proposition 3, the relative ranking predictions are on output, and the result captures

the idea that the benefits of the type-H group are larger than the benefits to the type-L group. We evaluate the predictions using measures of intermediation, which are available for a larger sample. If, in practice, the better types also have higher wealth and therefore more resources to save, borrow, etc., then such a test is natural. Again, higher levels of wealth could reflect higher initial wealth or higher past accumulation due to either higher income or higher savings rates.

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 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 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)).

Although SHGs often have broader missions, their primary operations are as ASCAs (accumulating savings and credit associations).[15] 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

[15] These additional "self-help" objectives may be female empowerment, social outreach, assistance in infrastructure investments, provision of services such as techical training or marketing assistance, fostering of participation in local politics, or simply fostering of stronger community bonds.

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 is retained as a social fund available for emergency loans. Funds accumulate through savings, interest on repaid loans, and fines for late payments/other violations; and these funds are held centrally. The funds follow cycles that are (typically) six months or a year long. 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 when 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 far more complicated than ROSCAs. They require strict record keeping to keep track of savings, loans, loan payments of various amounts, and payouts due.[16] They also require judgment of 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. Meetings are involved, requiring counting and verifying the starting fund, member deposits of varying amounts, loan payments of varying amounts, as well as loan disbursals; 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.[17] The services provided by field agents to these groups include initial training and then 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. CRS provides training to FAs in all of the above-mentioned services that they provide. 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,

[16] 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.

[17] 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.

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; 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) on a quarterly basis 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 being 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, the most common form of payment being a fixed fee per member per meeting.[18] 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 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 close to 750 agents who will found roughly 14,000 SILCs and reach nearly 300,000 members. The FAs are 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 in December 2009-January 2010. Agents in the second wave were recruited in either October 2009 (Kenya and Tanzania) or January 2010 (Uganda).[19] They were certified the following year in October and January 2011, respectively. The second

[18] Fees vary considerably across groups and savings levels vary considerably across groups and members. For those groups that charge fees, the median quarterly fee per member is $0.50, which amounts to about three percent of the median member's quarterly deposits.

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

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 4.1.)

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.[20] 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 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 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.

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 51 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 225 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 spread across 4 partners.[21]

[20] The first wave partners operated in Mombasa and Malindi (Kenya) and Mwanzaa and Shinyanga (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.

[21] 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

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, 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

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 (collected quarterly) include total membership, savings, credit, losses, interest rates, profitability, and share outs, as well as agent name. They 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 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 (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 every six months; additional group-level data were collected every 6 months that covered membership characteristics, delivery of services, and compensation 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 followed. A subset of 192 agents was chosen among the full-year sample of 225 second-wave agents in April 2010. These agents were stratified across country (83 of 96 in Kenya, 47 of 63 in Tanzania, and 62 of 67 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 192 agents chosen, a village was randomly selected among the list of villages in which they currently operated at least one SILC. In May 2010, a key informant survey was administered to the village chief. This survey

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.

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 a 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 1910 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 (including the Ugandan village not surveyed in the baseline) was conducted in Kenya and Tanzania, approximately nine months after the agents had received certification. In October of 2011 Uganda was resurveyed, 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. Table 1 presents some summary statistics in the baseline for households with SILC members and households 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 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" is 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 of 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 frequently, 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, 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 respondent's income since it is presumably better measured, and respondents (many of whom are SILC members) are more likely to be impacted 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, the validity of which those methods presume.

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:

$$Y_{idnt} = \alpha_{dt} + X_i\beta + \gamma wave_i + \delta PSP_n + \varepsilon_{itdn} \quad (6)$$

$$Y_{idnt} = \alpha_{dt} + X_i\beta + \gamma wave_i + \sum_{s=1}^{4} \delta_s PSP_{ns} + \varepsilon_{itdn} \quad (7)$$

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 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 eight quarters of data, we look for both an overall effect δ (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:

$$Y_{jdn} = \alpha_d + X_j\beta + \delta PSP_n + \varepsilon_{jdn}. \tag{8}$$

The outcomes Y_{jdn} for household j, living in district d and subdistrict n, depend 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). Again, δ is the measure of the treatment effect. 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 δ and δ_s. However, tables 2, 3, and 4 give examples of the full regression estimates for the agents, groups, and households.

2.4.2 Baseline Randomization

The above methods rely on the exogeneity of the PSP treatment, PSP, which ought to follow from our randomization. However, the success of the randomization must be first verified as well as possible, which we do 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. For the agent-level data, we cluster the standard errors by region, while for the household data, we cluster them by village.

Table 5 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 significantly higher fraction of PSPs receiving secondary education and correspondingly a 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.

Scond, Table 6 presents the randomization results for 23 village characteristics from the village key informant survey data. 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 FA villages and treatment PSP villages are all small and insignificant. The lone exception is animal disease within the past 5 years, which occured in 41 percent of PSP villages but only 21 percent of FA villages, statistically significant at the one percent level.

For the household-level data, we use only the first wave, and data are weighted appropriately. 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.

Table 7 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 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). 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 Mombasa in Kenya and TAHEA in Mwanzaa, Tanzania. Without these two areas, the baseline analysis produces insignificant differences in both the agent or household databases. The only exception is the

fraction of agents whose highest level of education is primary school completion; it is lower for PSPs but only significant at the ten percent level. All of our significant results are robust to dropping these two areas, as we show in Appendix A. 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 (8) do not show "impacts" in the baseline household data, i.e., prior to treatment. Table 8 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 ten percent level, and, again, we believe it to be purely random.

2.4.3 Reasons for Impact

Although the theory in Section 2 suggests that the differences could be driven by selection, we explore three potential explanations for the impacts using multiple methods. The three explanations are: (1) improved member selection by agent or households, (2) improved effort by agent, and (3) improved effort by members.

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

$$M_{jvn} = \alpha_v + X_j\beta + \eta_1 Z_j^{baseline} + \eta_2 PSP_n Z_j^{baseline} + \varepsilon_{jn}, \tag{9}$$

where M_{jvn} is endline membership (of household j in village v and subdistrict n) and $Z_j^{baseline}$ indicate 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, respectively, are above the median).[22] 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. 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.

[22] 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 the present 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.

For the second explanation, the agent questionniare 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 (6) and (7) above. The only difference is that these data are only available every six months, so our time-specific estimates in equation (7) are semi-annual rather than quarterly.

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

Finally, we look directly at payments themselves using the agent-level data. Although the choice of payment is not randomized and so potentially endogenous, the model in Section 2 suggests that it should add insight into the mechanism. Through this lense, we view the variation as potentially driven by exogenous differences in intermediation costs (C) across villages. These costs could reflect actual time/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 pursue this analysis by aggregating the data by village and distinguish villages where no fees are charged, villages where a single uniform fee is charged, and villages in which multiple fees are charged. We run regressions of the form

$$\begin{aligned} Y_{vdt} &= \alpha_{dt} + \gamma wave_d + \varpi_1 NoFeePSP_v \\ &\quad + \varpi_2 UniformPSP_v + \varpi_3 VariablePSP_v + \varepsilon_{itdn}, \end{aligned} \tag{10}$$

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. We run these regressions using both per-group values and village totals. 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. For the village totals, however, the theory suggests that the ability to vary fees should allow for more intermediation through a greater number of groups. Thus, ω_3 should exceed ϖ_2, including for membership.

3 Results

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

3.1 Impact on Agents and Groups

Table 9 presents the agent-level results for various measures. The first row presents the overall impact θ from equation (6). The dependent variables are accumulated stocks in Table 9. On average across the year, PSPs start 2.5 fewer groups, reach 46 fewer clients, and earn $152 less in payments per quarter, all of which are significant at the one percent level. 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 θ_s from equation (7), offer stronger insight, however: PSPs start off more slowly than FAs, but they improve over time. PSPs do significantly worse over the first three quarters (nine months) 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. Indeed, the point estimates are positive for savings, loans, and profits (loan value, or total credit, follows a similar pattern but is never statistically significant). Thus, by the end of the year, PSPs seem to be providing comparable levels of services as FAs. Payment for PSPs remains lower than FAs, however, with the gap in cumulative payments widening over time. Indeed, the average cumulative payment to PSPs at the end of the year is $540 less than the cumulative earnings of FAs (who accumulate an average of $708).

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 or whether the groups that PSPs start are systematically different. To evaluate this, Table 10 presents the group-level regressions. Here the dependent variables for savings, loans, loan value, profit, and payment have been normalized by the number of weeks of the current cycle to make them more comparable across groups started at different times. The impact coefficient is therefore the impact of PSPs on the typical group. We have multiplied the weekly flows by 13 so that they represent quarterly flows. Overall, for members, savings, and credit, we don't see significant negative differences at the group level early on, indicating that the early differences in members, credit, and savings in Table 9 are indeed driven by the number of groups. Nonetheless, we do see relative improvement of PSPs even at the group level. By the fourth quarter, the typical individual group of a PSP has significantly more savings, more credit, higher profits, and perhaps an extra member. In relative terms, these impacts are considerable, nearly 50 percent higher than the control means for savings and profit and over one-third higher for loan value. The point estimate for agent payment per group is still negative in the fourth quarter, but it is much smaller and no longer significant. In any case, if PSP payment indicates a willingness to pay for services on the part of members, then the fact that PSPs earn less per group even though their groups are more profitable may imply

that the FA remuneration exceeds the perceived value of services offered to their members.

In sum, PSPs appear to have slower starts, but within four quarters they appear to be stastistically indistiguishable 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 ultimately more profitable.

Nevertheless, given the substantially lower cost to the NGO of PSPs relative to FAs, after only one year the PSP 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 in the first quarter, $83. Thus, over two years, the cost of PSPs is just about half (i.e., $(518 + 83)/(518 + 660)$) of the cost of FAs. 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 about 10 percent fewer members (364 vs. 410 in the fourth quarter), so the cost of PSPs per member reached is just over half (57 percent) of what FAs cost. Since PSPs reach similar numbers of members by the end of the year, the relative cost of PSPs per members reached will again almost certaily 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. However, so far the data show very little dropout of PSPs or FAs. Indeed, this may indicate not that PSP pay is too low, but 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 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 11 presents the impact estimates of θ in equation (8) for savings behavior. We see no impact on aggregate savings overall or on 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 5 percent level. Similarly, an additional $16 per household was saved by households reporting 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 over

100 and 400 percent, respectively, relative to the FA villages. Thus, PSPs seem to have important impacts on reported business-oriented savings.

The impact on borrowing is examined in Table 12. Here we see that the PSP program led to substantially higher levels of borrowing. The estimate of $29 amounts to an increase of almost 70 percent, and it is significant at a 5 percent level. The comparably-sized estimate of $27 for reported business owners amounts to an even higher percentage increase, and it is highly 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 SILC are $5 higher in PSP villages (significant at the 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 the one percent level. The point estimate on formal credit is also larger, significant at the ten percent level. 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 of over 200 percent. In contrast, there is no impact on credit for new businesses.

Table 13 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).[23] 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 with the FA control. Likewise, time spent in business is higher by 3 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 0.12 employees per household, significant at a 5 percent level, represents an increase of over 50 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

[23]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.

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.

In Table 14, 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 but entirely insignificant. The point estimates for total expenditures (consumption plus investment) and for just consumption are positive and substantial, and marginally significant at the ten percent level. The values indicate that average annual household expenditures and consumption were $208 and $184 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. Their significance is marginal, and to attribute them to the increased savings or investment and labor in business, returns on investment and labor would need to be extremely high. (Although we do not present regressions using the components of consumption, much of the increase in consumption is food expenditures, which are measured on a weekly basis and multiplied by 52.)

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 risk-sharing or consumption smoothing. To evaluate this, we looked at several measures, including (1) probability of "ever going to sleep hungry," (2) probability of experiencing and various adverse shocks, and (3) "number of weeks until finances returned to normal" following an adverse shock.[24] The point estimates on PSP were generally positive, but none were significant. That is, we certainly see no 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 evaluate the evidence for differential selection motivated by the theory in Section 2. We also explore alternative hypotheses. According to the theory, PSP-led SILCs may cater to different members than FA-led SILCs. If PSP members are more financially sound, then 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 and weak evidence for the effect of incentives on other dimensons.

Table 15 presents the results of fixed-effect regressions of endline membership on baseline household characteristics and their interaction with the PSP treatment following equation

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

(9). The first row presents the estimates of 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 (with the exception of the linear discount factor, where less-patient households are likely to be members of SILCs). The second row 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 higher probability of SILC membership in PSP villages. These impacts are statistically significant. Not only are these coefficients statistically significant, but they are also economically significant. The mean impacts on the probability of membership range from 0.05 (business income) to 0.09 (income and hyperbolic discounting).

Table 15 represents only a selection of the most salient results from our selection analysis. We conducted a wide variety of alternative specifications including 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 overwhelmingly both the significant results and insignificant point estimates support the story of the results presented in Table 15. The PSP groups appear more attractive to agents that are wealthier (higher consumption, higher savings and credit) and more business-oriented (fewer hours in wage labor and agriculture, more investment). Finally, we note that the results are equally strong when village fixed effects are included, indicating 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 effort because, as private entrepreneurs, their pay depends on their performance. The top panel of Table 16 shows no evidence that PSPs behave differently in targeting services to villages closer to their home (proximity), villages with existing connections, or villages with greater perceived need (potentially altruism-driven) or demand (presumably profit-driven). In the bottom panel, we find little evidence of greater effort, using several measures: distance traveled to SILCs, infrequency of services provided (attending meetings at least biweekly rather than less frequently), and number of services provided. Indeed, the only significant estimates in Table 16 show that PSPs are more likely to work only part-time, less likely to work full-time, and more likely to

meet with their groups at least biweekly.

The second alternative 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 can only focus on the total time spent working per week. While the composition of hours was impacted by the PSP program (recall Table 13), 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. We interpet this population as the type-H individuals of our theory.

Whether this selection is driven by PSPs themselves or is simply a result of the fees charged is certainly still an open question. However, we pursue this further by looking at variation in fees offered across and within villages.

We first show the high level of fee variation across groups. To narrow our focus, 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. Nonetheless, Table 17 shows the substantial variation in fees in the data. The average fee is \$5.80/quarter per group, the standard deviation is \$4.60 per quarter, and the interquartile ratio is 3.1. Finally, a regression of group fee on village fixed effects explains 56 percent of the variation, indicating that just over half of the variation is across villages and just under half is within villages. Agent-specific fixed effects explain less than 40 percent of variation. Thus, the data indicate a high degree of price targeting.

We further examine how the level of intermediation services varies by the actual fees offered by PSPs. The fact that fees were not part of the randomization is an important caveat. We report the estimates from equation (10), which uses the group-level data aggregated within a village but distinguishing between villages where no groups charge fees (413 villages), villages where all groups charge the same fee (367 villages), and villages where different groups are charged different fees (428 villages).[25] Recall that these coefficients are all relative to the FA villages. The dependent variables for the results in Table 18 are all per-group values. Thus, they show how the typical group varies by the fees charged in the village.

[25] An assessment of baseline household characteristics of variable and uniform fee villages yielded significant differences, but they don't follow a clearly discernible pattern. Of the 28 outcome and control variables, 9 were significant; households in villages with variable fees had higher baseline levels of saving and more male-led households, but they had less business investment, less business credit, lower expenditures, lower consumption, and lower education levels (more primary as highest grade completed and less secondary and tertiary completed).

Except for profit, the positive 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. Both variable and uniform fees are positively related to higher credit, membership, and savings. The groups in PSP villages where no fees are charged are not statistically distinguishable from those in FA villages, except that the groups earn more profits (significant at the ten percent level). These 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.

The role of variable fees becomes of greater interest when examining the results in Table 19, where the dependent variables are aggregate village totals. 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 in which variable fees can cater to larger populations than uniform fees, yielding higher levels of intermediation and larger total impacts (recall Propostion 3). 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.

4 Conclusion

We have presented evidence for a theory of adverse selection in credit cooperatives and 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 ultimately led to more credit and were more profitable. The privately provided groups also 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 current and future SHG 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 a difference in the population served by privatized providers. PSPs cater to a more business-oriented population. 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. The distribution of benefits across the population and subpopulations is therefore an ongoing project of further investigation. A larger question is whether or not it is advisable to provide microfinance services to the less business-oriented populations.

Another remaining question is whether privatization matters beyond the incentives to charge fees that it provides. If not, as the theory suggests, the favorable outcome and reduced costs could be attained by NGOs without privatizing by simply charging membership fees. 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. A structural model of agent and household behavior may provide insights.

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5 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 Propositon 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 $\overline{R}_{BL}(0) = p_L\overline{A}k/\left(p_Lk + (1-p_L)\,\phi(0)\right)$ 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 $\overline{R}_{BL}(1) = p_L\overline{A}k/\left[p_Lk + (1-p_L)\phi(1)\right]$ and $\phi(1) \equiv \frac{p_L}{\pi p_L + (1-\pi)}$ yields, after much simplification

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

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

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

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

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

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:

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

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 this holds when the following inequality is satisfied:

$$\begin{aligned}
&p_H^3 p_L\left\{\pi\left(\overline{A}-\underline{A}\right)\left[\left(\overline{A}-\underline{A}\pi\right) - \left(\overline{A}-\underline{A}\right)\pi\right]\right\} \\
&+p_H^2 p_L^2\left\{\left[2\pi^2\left(\overline{A}-\underline{A}\right) + \overline{A}\pi\right]\left(\overline{A}-\underline{A}\right) + \overline{A}\pi\left(1-\pi\right)\left(3\underline{A}-2\overline{A}\right) + \underline{A}\left(\overline{A}\pi - \underline{A}\right)\right\} \\
&+p_H p_L^3\left\{\left(\overline{A}-\underline{A}\right)\pi\left[2\pi - \left(\overline{A}-\underline{A}\right)\pi\right] + (1-\pi)\left[3\overline{A}\underline{A}\left(1-\pi\right) - \overline{A}^2\right] + \overline{A}\pi\left(3\overline{A}-\pi\right)\right\} \\
&-p_L^4\left\{\overline{A}\underline{A}\left(1-\pi\right)^2\right\} > 0.
\end{aligned}$$

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

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

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.:

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

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 :

$$\begin{aligned}
Y_2^* &= (1-\theta)\left(p_H\overline{A} - F^*\right) + \theta p_L\overline{A} \\
Y_1^* &= (1-\theta)\left(p_H\overline{A} - F^*\right) + \theta p_L\underline{A} \\
Y_0 &= (1-\theta)\, p_H\underline{A} + \theta p_L\overline{A}.
\end{aligned}$$

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

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	1477	1573	1466	1616	11
Business Owner	0.55	0.5	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	0.4	0.49	0.44	0.5	-0.04
Secondary	0.11	0.32	0.10	0.31	0.01
Tertiary	0.02	0.13	0.03	0.16	-0.01
Observations	968		951		

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

Table 2: Sample Agent Level Regression

	Payment
Age	9***
	(3)
Age Squared	-0.08***
	(0.04)
Gender	11
	(8)
Primary Complete	45***
	(19)
Secondary	51***
	(21)
Tertiary	69***
	(24)
Languages	-14**
	(7)
Children	-2
	(1)
Financial Dependents	-0.25
	(1)
Cohort	-230***
	(33)
PSP * Quarter 1	-210***
	(11)
PSP * Quarter 2	-290***
	(12)
PSP * Quarter 3	-420***
	(16)
PSP * Quarter 4	-540***
	(17)
Observations	1080
R Squared	0.84

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

The results are estimated coefficients for a regression of cumulative agent payment on the stated independent variables. The baseline education level is less than primary complete. The regression is weighted by sampling weights. Standard errors are robust.

Table 3: Sample Group Level Regression

	Profit
Age	3
	(2)
Age Squared	-0.02
	(0.03)
Gender	3
	(7)
Primary Complete	7
	(12)
Secondary	13
	(13)
Tertiary	33**
	(16)
Languages	2
	(6)
Children	-3**
	(2)
Financial Dependents	2**
	(1)
Cohort	3
	(10)
PSP * Quarter 1	13
	(12)
PSP * Quarter 2	-7
	(10)
PSP * Quarter 3	9
	(8)
PSP * Quarter 4	25***
	(10)
Observations	18813
R Squared	0.03

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

The results are estimated coefficients for a regression of group profit on the stated independent variables. The baseline education level is less than primary complete. The regression is weighted by sampling weights. Standard errors are robust standard errors clustered by agent.

Table 4: Example of a Household Level Regression

	Total Credit
Treatment	29***
	(11)
Age	7***
	(3)
Age Squared	-0.07***
	(0.02)
Gender	-2
	(12)
Some Primary	47**
	(19)
Primary Complete	8
	(9)
Secondary	152***
	(41)
Tertiary	243**
	(100)
# Adult Males	-4
	(4)
# Adult Females	20***
	(8)
# Children	0.22
	(3)
Observations	1891
R Squared	0.08

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

The results are estimated coefficients for a regression of total credit on the stated independent variables. The baseline education level is no schooling. The regression is weighted by sampling weights. Standard errors are robust standard errors clustered by villages.

Table 5: Agent Level Randomization Results

	Age	Gender	Primary	Primary Complete	Secondary	Tertiary	Languages	Children	Financial Dependents
Treatment	-0.03	-0.07	-0.002	-0.13***	0.14***	-0.007	0.09	-0.09	-0.32
	(1)	(0.07)	(0.01)	(0.05)	(0.07)	(0.05)	(0.06)	(0.23)	(0.41)
Observations	236	241	240	240	240	240	241	241	240
Control Mean	36	0.69	0.01	0.46	0.43	0.10	2	5	6

***, **, * indicate statistical significance at the 1%, 5%, and 10% confidence level, 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 standard errors clustered by location.

Table 6: 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

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

Table 7: Household Level Randomization Results - Demographics

	PSP			FA			PSP-FA
	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.	Mean △
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***
Secondary	0.11	0.32	1363	0.09	0.29	534	0.02
Tertiary	0.03	0.16	1363	0.02	0.13	534	0.01

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

Table 8: Household Level Randomization Results - Outcomes

	PSP	s.e.	FA mean	Sample mean	Median	Obs.
Total Savings	-3	(20)	137	137	49	1877
Savings for Business Owners	-9	(22)	156	156	75	865
Savings from Business Profits	-5	(10)	35	32	0	1877
Savings from Agric. Profits	3	(9)	25	29	0	1877
Savings from Salary/wage	-4	(8)	16	17	0	1877
Savings used for New Agric. Activity	-7	(15)	40	38	0	1877
Savings used for New Non-Agric. Activity	4	(4)	5	8	0	1877
Savings used for Existing Business	2	(9)	16	20	0	1877
Total Credit	3	(14)	42	47	3	1877
Credit for Business Owners	14	(17)	41	54	6	865
Credit from SILC	0.48	(0.92)	3.7	4	0	1877
Credit from Formal Lenders	6	(13)	26	33	0	1877
Credit from Informal Lenders	-3	(3)	12	10	0	1877
Credit used for Agric. Activity	5	(6)	7	11	0	1877
Credit used to Expand Business	7	(5)	6	11	0	1877
Credit used to start New Business	0.14	(0.78)	1	1	0	1877
Start New Business	0.03	(0.04)	0.25	0.26	0	1877
Business Investment	-3	(1)	42	40	0	1877
Hours spent in Business	0.53	(2)	15	15	4	1877
Non-HH Employees	-0.12	(0.21)	0.42	0.32	0	1877
Hours spent in Employee	0.6	(2)	15	16	12	1877
Agric. Investment	6	(10)	48	53	11	1877
Hours spent in Agric.	-0.47	(2)	27	27	25	1877
Total Income	98*	(57)	274	346	189	1877
Business Income	9	(15)	59	65	0	1877
Total Expenditure	73	(118)	1454	1519	1118	1877
Total Consumption	74	(116)	1400	1466	1074	1877

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

Notes: The results are estimated coefficients for 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. The regressions are weighted by sampling weights. Standard errors are robust standard errors clustered by villages.

Table 9: Agent Level Results

	Groups	Members	Savings	Loans	Loan Value	Profit	Payment
All Quarters	-2.5***	-46***	-540	-27***	-430	-210	-370***
	(0.5)	(13)	(430)	(9)	(420)	(170)	(10)
Quarter 1	-3.3***	-65***	-920*	-42***	-910	-110	-210***
	(0.8)	(20)	(550)	(15)	(650)	(270)	(11)
Quarter 2	-2.4***	-50**	-1250	-38**	-1170	-670*	-290***
	(0.9)	(22)	(800)	(17)	(780)	(390)	(12)
Quarter 3	-3.0***	-52**	-650	-29	-700	-340	-420***
	(1.0)	(26)	(810)	(19)	(760)	(320)	(16)
Quarter 4	-1.3	-19	600	0.8	980	270	-540***
	(1.1)	(27)	(980)	(20)	(910)	(310)	(17)
Observations	1080	1080	1080	1080	1080	1080	1080
Control Mean	19	410	7140	220	6570	1960	450

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

Table 10: Group Level Results

	Members	Savings	Loans	Loan Value	Profit	Payment
All Quarters	0.6	62	1	38	10	-5***
	(0.4)	(41)	(1)	(24)	(8)	(1)
Quarter 1	0.3	26	-1	3	13	-9***
	(0.5)	(51)	(1)	(35)	(12)	(1)
Quarter 2	0.5	28	1	14	-7	-7***
	(0.4)	(43)	(1)	(22)	(10)	(1)
Quarter 3	0.7	65	2*	44	9	-4***
	(0.5)	(44)	(1)	(28)	(8)	(1)
Quarter 4	0.8*	110***	2	77***	25***	-1
	(0.4)	(45)	(1)	(31)	(10)	(2)
Observations	19509	18813	18813	18813	18813	17455
Control Mean	21	250	10	220	51	10

***, **, * indicate statistical significance at the 1%, 5%, and 10% confidence level, 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 regression is weighted by sampling weights. Standard errors are robust standard errors clustered by agent.

Table 11: Household Savings Results

	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	-3	16**	7	8	0.25	-2	16***
	(16)	(22)	(7)	(13)	(7)	(11)	(2)	(5)
Control 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

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

Notes: The results are estimated coefficients for 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. The regressions are weighted by sampling weights. Standard errors are robust standard errors clustered by villages.

Table 12: Household Credit Results

	Credit		Source			Purpose		
	Total	Business Owners	SILC	Formal	Informal	Agric. Activity	Expanding Business	Start New Business
PSP	29**	27***	5**	17*	8***	8***	10***	2
	(11)	(8)	(2)	(10)	(3)	(3)	(3)	(1)
Control mean	41	32	7	22	10	4	4	2
Sample mean	56	50	10	30	16	9	10	3
Median	11	15	0	0	0	0	0	0
Observations	1891	865	1891	1891	1891	1891	1891	1891

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

Notes: The results are estimated coefficients for 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. The regressions are weighted by sampling weights. Standard errors are robust standard errors clustered by villages.

Table 13: Household Productive Decisions Results

	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***	3**	0.12**	0.97	4	-3*
	(0.06)	(6)	(2)	(0.05)	(2)	(9)	(2)
Control 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

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

Notes: The results are estimated coefficients for 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. The regressions are weighted by sampling weights. Standard errors are robust standard errors clustered by villages.

Table 14: Household Income Results

	Total Income	Business Income	Total Expenditures	Total Consumption
PSP	131	11	208*	184*
	(85)	(12)	(113)	(111)
Control mean	358	54	1598	1561
Sample mean	451	62	1717	1664
Median	196	0	1394	1356
Observations	1891	1891	1891	1891

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

Notes: The results are estimated coefficients for 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. The regressions are weighted by sampling weights. Standard errors are robust standard errors clustered by villages.

Table 15: Endline Membership Selection on Baseline Characteristics

	Income	Business Income	Positive Savings	Positive Hrs. in Business	Linear Discount Factor,β	Hyperbolic Discount Factor, δ
Characteristic	5e-06	-2e-06	-0.005	0.01	-0.12*	-0.06
	(3e-06)	(0.0001)	(0.09)	(0.06)	(0.07)	(0.05)
PSP*Characteristic	0.0002***	0.0003*	0.23**	0.12*	0.07	0.17***
	(6e-06)	(0.0002)	(1)	(0.07)	(0.9)	(0.06)
Observations	1877	1877	1877	1877	1877	1877

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

Notes: The results are estimated coefficients for a regression of endline membership on on the stated baseline outcome, their interaction effects with 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. The regressions are weighted by sampling weights and include village fixed effects. Standard errors are robust standard errors clustered by villages.

Table 16: Effect of PSP Treatment on Agent Effort / Behavior

Targeting							
	Proximity	Connections	Need	Demand	Other		
All Quarters	0.05	0.00	-0.03	-0.01	-0.01		
	(0.07)	(0.05)	(0.04)	(0.04)	(0.01)		
Quarters 1 & 2	0.01	0.02	-0.04	0.03	-0.01		
	(0.06)	(0.07)	(0.05)	(0.05)	(0.01)		
Quarters 3 & 4	0.11	-0.02	-0.03	-0.05	-0.01		
	(0.09)	(0.09)	(0.07)	(0.06)	(0.01)		
Observations	4250	4250	4250	4250	4250		
Control Mean	0.34	0.55	0.85	0.30	0.01		
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	0.5	-0.07	0.21***	-0.05	-0.18**	0.02	0.06*
	(0.6)	(0.23)	(0.06)	(0.08)	(0.08)	(0.05)	(0.04)
Quarters 1 & 2	-0.4	0.14	0.26***	-0.09	-0.23**	0.06	0.03
	(0.6)	(0.24)	(0.08)	(0.10)	(0.10)	(0.06)	(0.05)
Quarters 3 & 4	2**	-0.50	0.11*	0.03	-0.08	-0.05	0.11**
	(1)	(0.49)	(0.06)	(0.05)	(0.12)	(0.10)	(0.05)
Observations	162	162	151	151	151	151	4222
Control Mean	5	3	0.06	0.21	0.67	0.06	0.41

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

The results are estimated coefficients for a regression of the stated outcome on a PSP 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 sampling weights. Standard errors are robust standard errors clustered by agent.

Table 17: Variation in Fees Charged by PSPs

Mean	5.8
Standard Deviation	4.6
Interquartile Ratio	3.1
Fraction Within-Village Variation	0.44
Fraction Between-Village Variation	0.56
Fraction Within-Agent Variation	0.62
Fraction Between-Agent Variation	0.38

These summary data are based on fees charged in the 4th quarter. We use only data from PSP groups that charged fees, and we trim the lowest and highest five percent tails.

Table 18: Effect of "Village Type" on Per Group Outcomes

	Members	Savings	Loans	Loan Value	Profit	Observations
No Fee	-0.1	32	1	23	22*	413
	(0.4)	(35)	(1)	(25)	(13)	
Uniform Fee	2.2***	39*	1	64***	17***	367
	(0.4)	(22)	(1)	(20)	(5)	
Variable Fee	2.0***	120***	4**	120***	24***	428
	(0.4)	(36)	(2)	(32)	(7)	
Observations	1864	1864	1864	1864	1864	
Control Mean	21	250	8	200	50	

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

The results are estimated coefficients for a regression of the stated outcome (aggregated at 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 baseline village type is village served by an FA. The regressions are weighted by sampling weights.. Standard errors are robust.

Table 19: Effect of "Village Type" on Total Outcomes

	Groups	Members	Savings	Loans	Loan Value	Profit	Observations
No Fee	-0.4***	-9***	57	-0.2	11	54	413
	(0.2)	(3)	(115)	(3)	(86)	(47)	
Uniform Fee	-1.1***	-20***	-190***	-7***	-170***	-43***	367
	(0.1)	(3)	(53)	(2)	(48)	(12)	
Variable Fee	1.9***	43***	990***	32***	940***	180***	428
	(0.2)	(5)	(150)	(6)	(130)	(29)	
Observations	1864	1864	1864	1864	1864	1864	
Control Mean	2.4	51	610	21	530	120	

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

The results are estimated coefficients for a regression of the stated outcome (aggregated at the village level) on a village type dummy, location-date fixed effects, and the cohort of the agent working in that village. The baseline village type is village served by an FA. The regressions are weighted by sampling weights. Standard errors are robust.

Appendix (Not for Publication)

A1: Randomization Results Excluding Mombassa and Tahea

	Age	Gender	Primary	Primary Complete	Secondary	Tertiary	Languages	Children	Financial Dependents
Treatment	-1	-0.11	0.01	-0.08*	0.08	-0.01	0.02	-0.15	-0.16
	(1)	(0.08)	(0.01)	(0.05)	(0.06)	(0.06)	(0.05)	(0.27)	(0.50)
Observations	193	197	196	196	196	196	197	197	196
Control Mean	35	0.72	0	0.32	0.55	0.13	2	5	6

***, **, * indicate statistical significance at the 1%, 5%, and 10% confidence level, 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 standard errors clustered by location.

A2: Agent Level Results Without Mombassa and Tahea

	Groups	Members	Savings	Loans	Loan Value	Profit	Payment
All Quarters	-3***	-52***	-220	-25***	-17	-34	-360***
	(0.6)	(14)	(460)	(11)	(470)	(170)	(11)
Quarter 1	-4***	-75***	-970	-45***	-950	-94	-210***
	(1)	(22)	(590)	(16)	(730)	(300)	(12)
Quarter 2	-3***	-53**	-820	-33*	-630	-420	-290***
	(1)	(24)	(800)	(18)	(820)	(310)	(15)
Quarter 3	-3***	-58**	-83	-31	-250	-48	-410***
	(1)	(29)	(890)	(22)	(900)	(280)	(19)
Quarter 4	-2	-22	980	6	1720	430	-540***
	(1)	(30)	(1180)	(24)	(1080)	(370)	(19)
Observations	874	874	874	874	874	874	874
Control Mean	21	430	7480	230	6830	1940	440

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

A3: Group Level Results Without Mombassa and Tahea

	Members	Savings	Loans	Loan Value	Profit	Payment
All Quarters	0.8*	78*	0.6	50*	16*	-4***
	(0.5)	(46)	(1)	(27)	(9)	(0.8)
Quarter 1	0.4	37	-2	9	17	-9***
	(0.5)	(56)	(1)	(39)	(13)	(0.5)
Quarter 2	0.7	37	0.3	23	-4	-6***
	(0.5)	(48)	(1)	(25)	(12)	(0.7)
Quarter 3	1*	82	1	55*	16*	-2***
	(0.5)	(50)	(1)	(33)	(9)	(0.9)
Quarter 4	0.9*	140***	2	98***	31***	0.1
	(0.5)	(52)	(1)	(37)	(11)	(2)
Observations	16251	15721	15721	15721	15721	14424
Control Mean	21	260	10	230	50	9

***, **, * indicate statistical significance at the 1%, 5%, and 10% confidence level, 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 standard errors clustered by agent.

A4: Household Savings Results Without Mombassa and Tahea

	Savings		Source			Purpose		
	Total	Business Owners	Business Profit	Sell Agric. Product	Salary or Wage	New Agric. Activity	New Non-Agric. Activity	Existing Business
PSP	31*	-8	15**	10	12*	6	0.03	19***
	(16)	(24)	(8)	(15)	(6)	(10)	(2)	(6)
Control mean	113	148	20	39	8	32	3	6
Sample mean	134	144	29	41	16	35	2	18
Median	50	64	0	0	0	0	0	0
Observations	1720	783	1720	1720	1720	1720	1720	1720

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

Notes: The results are estimated coefficients for 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. The regressions are weighted by sampling weights. Standard errors are robust standard errors clustered by villages.

A5: Household Credit Results Without Mombassa and Tahea

	Credit		Source			Purpose		
	Total	Business Owners	SILC	Formal	Informal	Agric. Activity	Expanding Business	Start New Business
PSP	31**	27**	5**	20*	6**	10***	11***	2
	(13)	(12)	(2)	(12)	(3)	(4)	(4)	(1)
Control mean	46	38	9	26	10	6	5	2
Sample mean	63	54	12	35	15	11	11	3
Median	11.8	16	0	0	0	0	0	0
Observations	1720	783	1720	1720	1720	1720	1720	1720

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

Notes: The results are estimated coefficients for 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. The regressions are weighted by sampling weights. Standard errors are robust standard errors clustered by villages.

A6: Household Productive Decisions Results Without Mombassa and Tahea

	Start New Business	Business Investment	Hours spent in Business	Employees (non-HH)	Hours spent as Employee	Agric. Investment	Hours spent in Agric.
PSP	0.07*	19***	4**	0.14***	-2	5	-2
	(0.04)	(7)	(1)	(0.05)	(2)	(12)	(2)
Control mean	0.2	28	11	0.1	17	72	29
Sample mean	0.2	39	13	0.2	15	73	29
Median	0	0	0	0	9	21	30
Observations	1720	1720	1720	1720	1720	1720	1720

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

Notes: The results are estimated coefficients for 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. The regressions are weighted by sampling weights. Standard errors are robust standard errors clustered by villages.

A7: Household Income Results Without Mombassa and Tahea

	Total Income	Business Income	Total Expenditures	Total Consumption
PSP	151	13	174	149
	(116)	(16)	(142)	(134)
Control mean	393	65	1696	1595
Sample mean	488	69	1759	1647
Median	165	0	1387	1266
Observations	1720	1720	1720	1720

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

Notes: The results are estimated coefficients for 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. The regressions are weighted by sampling weights. Standard errors are robust standard errors clustered by villages.