What Are the Headwaters of Formal Savings? Experimental Evidence from Sri Lanka[#]

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Abstract.
When households increase their deposits in formal bank savings accounts, what is the source of the money? We combine high-frequency surveys with an experiment in which a Sri Lankan bank used mobile Point-of-Service (POS) terminals to collect deposits directly from households each week. In this context, the headwaters of formal savings are to be found in sacrificed leisure time: households work more, and work more on the wage market when savings options improve. These results suggest that the labor allocation channel is an important mechanism linking savings opportunities to income.

Keywords: savings, household decisionmaking, high-frequency surveys.

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1. INTRODUCTION.

In recent years, there has been a strong push - in developed and developing countries alike - to provide financial services that bring the savings of the unbanked into formal institutions. The surge in interest in formal savings stems from a recognition of its potential to improve the efficiency of the overall economy by increasing the depth of financial markets, and from increased understanding of its potential to improve the lives of people, most of them poor, who do not use banks. Half of adults worldwide remain unbanked, but 35 percent of the unbanked report obstacles to saving that can be overcome by improvements to products and regulation (Demirguc-Kunt and Klapper, 2012). Several recent studies suggest that overcoming the obstacles to savings can lead to very large positive effects on household expenditures (Kast et al 2012, Dupas and Robinson 2013, Brune et al 2013, Prina 2013, Schaner 2014).

The reasons behind these substantial impacts of formal savings on the well-being of the poor have been the subject of a great deal of theoretical and empirical work. Much attention has been focused on understanding why individuals fail to achieve their own savings goals, and how well-designed financial services help overcome these obstacles. Potential impediments to savings include problems with self-control arising from time inconsistent preferences (Laibson 1997) and other-control issues arising from pressure to make transfers to spouses or peers (Anderson and Baland, 2002). Commitment products (Benartzi and Thaler, 2003; Ashraf, Karlan, and Yin, 2005) and savings accounts that provide individuals within the household greater autonomy over savings (Ashraf, 2009; Schaner 2014) may overcome these obstacles. Even those models that use a neo-classical framework to explain the large effects of recent savings experiments (such as Dupas and Robinson (2013)) typically invoke production function non-convexities which prevent individuals from making high-return investments in productive inputs without savings instruments that allow them to accumulate the capital to do so. Under this framework, tools that help entrepreneurs accumulate savings can have transformative effects by allowing people to make lumpy investments and thereby achieve higher steady-state returns and, as a result, escape low-investment poverty traps (Banerjee and Newman 1993).

Studies of pro-poor savings products have paid less attention to an older and entirely neo-classical explanation for the impact of new savings technologies: even with time-consistent preferences and a concave production function, increases in the interest rate inherent in new savings products will change the intertemporal rate of substitution and therefore increase the incentive to earn today so as to enjoy interest-driven consumption in the future. This logic is at the heart of a number of workhorse

macroeconomic models such as Lucas and Rapping (1969) and Blanchard and Fischer (1989), and places emphasis on labor rather than investment as the pathway that connects improved savings options with increased incomes.

In this paper we present an experiment providing a powerful inducement to save, and using detailed, high-frequency survey data to capture changes in cash flows within households as money begins to flow into formal savings. We attempt to answer a simple question: what are the headwaters of formal savings? The answer to this question is highly informative as to the casual pathways behind savings impacts, because the different potential explanations have very distinct signatures in terms of intrahousehold cash flows. If overcoming self-control problems is key, we will find the headwaters of formal savings in decreased consumption. If other-control is central, then formal savings will flow from decreased net intra-household transfers and potentially decreased use of collective informal savings products that may involve implicit transfers through social insurance. If non-convexities in production are the driver, we will observe an initial transition period during which consumption, net transfers, or other investments decrease so as to generate savings balances, followed by lumpy investments in productive assets and then an increase in income resulting from improved marginal returns on capital. If the intertemporal substitution story is operative, we should instead see an immediate increase in labor as households respond to the higher interest rate.

We collaborated with a Sri Lankan bank to send staff door-to-door to offer households a weekly savings deposit collection service using a mobile Point-of-Service (POS) handset that prints out deposit receipts on the spot. The service was provided to randomly selected unbanked individuals living at least 5km from their nearest bank branch.[1] Starting five months before the collections began and continuing 13 months afterward, we collected detailed monthly survey data, including income, expenditures, transfers, and labor market activity data. We use the detailed survey data to analyze changes in the household's cash flows resulting from the savings treatment, along the lines of the analysis in Samphantharak and Townsend (2009).

The weekly POS deposit collection service led to large increases in formal savings. The number of transactions with formal financial institutions per month quadrupled as a result of the treatment; the flow of savings into bank accounts almost doubled; and overall savings increased by more than 15 percent per month, or about US$7. The treatment generates an immediate increase in total household

[1] The intervention is designed to open new savings opportunity for unbanked individuals. We screened our experimental sample such that participants satisfied three criteria: (i) they received income at least weekly, e.g., were self-employed or daily wage workers; (ii) they were accessible by a motorcycle and; (iii) they were unbanked (i.e., they had no formal bank account or had not used a bank account in the previous month.

income, and in earned income in particular. The increased income is used to fund the increased savings and to pay off loans. We see no change in transfers to or from other households, and no decrease in consumption.

The suggestion that increases in income are a source of savings rather than a result of savings leads us to examine labor market activity in more detail. The survey data for the main respondent show that labor hours, and particularly hours worked on the wage market, jump within a month of the offering of the treatment. Moreover, there is an immediate shift out of self-employment and into wage work, with the occupational shift persisting over time. In aggregate, we find an increase in earning from wage work and no increase – and indeed, in some periods a decrease – in self-employment income. The lack of any lag in the savings and income effects implies that the saving-invest-earn channel highlighted in the existing literature cannot be the only pathway through which savings accounts affect income. Instead, these results appear most consistent with the entirely neo-classical model in which new savings options represent a change in inter-temporal prices which affect the inter-temporal allocation of consumption and, correspondingly, labor (Blanchard and Fischer 1989).[2]

This shift from self-employment to wage work was counter to our initial expectations, but is again consistent with an entirely neo-classical model if households wishing to expand labor hours are confronted with constant marginal returns on the wage market versus diminishing marginal returns in self-employed microenterprise. To examine this possibility further, we split the sample into three subsamples based on activities reported during the baseline period: 1) individuals who are self-employed in the retail and service sectors, where we posit the ability to expand is more limited given the very local markets typical in our rural setting; 2) those active in the manufacturing sector, where sales are more easily made outside the local area; and 3) those not involved in any self employment. We find that those who are self-employed in the manufacturing sector are the source of all income increases. The initial increase in income comes from an increase in hours worked, but also, apparently, from an increase in intensity of work per hour. The data provide evidence that the labor / leisure channel is an important piece of the answer to both the question of the headwaters of savings and the question of the channel through which the availability of savings services affects expenditures.

[2] Banerjee et al (2013) develop a model to structure an analysis of the effects of an expansion of a micro credit program in India. They also show that the increase in credit can lead to an increase in labor. Intuitively, their effect comes from the fact that credit makes the purchase of durable goods possible, and hence raises the marginal benefit of consumption. In the Blanchard and Fischer framework, savings accounts increase the labor effort immediately because the higher interest rate increases the benefit of future consumption.

Of course, the various explanations for savings pathways need not be mutually exclusive. By the end of the study (24 months after treatment) we do begin to see some evidence of increased investment in manufacturing enterprises by those offered the savings accounts, consistent with the save-invest-earn channel. We can also use our detailed survey data to explore the self-control and other-control channels that have received most of the attention in the previous literature. We use individual information on the time-inconsistency of preferences at baseline, along with list experimental questions on the consumption of 'sin goods', to explore this pathway. The product allows the saver to interact privately with a bank staffer to make a deposit into the account solely in his or her own name, and thus may improve the ability of the account holder to deny the demands of others for transfers, particularly relative to very public savings options such as the rotating savings and credit associations (called *seetus*) that are ubiquitous in the Sri Lankan context. Following Anderson and Baland (2002), we use the income share of the respondent within the household to examine heterogeneity in responses. We also exploit an experiment in which we randomized the intensity of treatment (Crépon et al. 2013, Baird et al. 2015) within the *seetus* used by study households at baseline to examine the interaction between formal and informal savings, finding that if anything these two institutions prove to be complements in this setting. Overall, our results highlight the ability of savings to have a transformative effect on income even in an environment in which non-convexities in production do not appear to play a dominant role.

The remainder of the paper is structured as follows: Section 2 presents the background and design of the experiment, and Section 3 develops a simple framework to organize the analysis. Section 4 presents the impact of the program on savings, the "headwaters" of personal liquidity, and on entrepreneurial activity. Section 5 investigates the extent to which improvements in self-control and other-control may also be responsible for generating our results, and Section 6 concludes.

2. Survey and Experimental Design.

2.1. Sampling and Treatment.

In 2008, Sri Lanka's National Savings Bank (NSB) began a program of remote deposit collection targeted at business owners within one kilometer of NSB branches. The collections are made using a wireless Point-of-Service (POS) terminal carried by a bank agent traveling by motorcycle. The POS terminal allows deposits to be recorded in the bank's accounts immediately; at the same time, the depositor receives a deposit confirmation showing the new account balance. Agents typically visit a given merchant once per week.

For this project, NSB agreed to provide branches in the towns of Bandarawela and Mahiyangana with an additional POS terminal and agent to make collections from more remotely located rural areas. While NSB's goal for the initial POS program was to reach highly liquid merchants, the goal of this project was reaching lower-income, under-banked households in more remote locations. We agreed on selecting areas by first going five to 10 kilometers from the branch along the main roads leading away from each of the two towns, and then taking households within one kilometer of the main road. Selected households in these areas were offered weekly door-to-door deposit collection services. We began by conducting a screening survey in these areas, listing all economically active adults in the households. The screening survey collected information on the frequency of pay from employment, whether or not the individual had a bank account, and, if so, whether the individual had made any banking transaction within the past month, and whether the individual participated in one or more *seetus*, the rotating savings and credit associations that are common in Sri Lanka. We also noted whether the household was accessible by motorcycle, since that was a requirement for the collection service. A total of 5,081 individuals in 3,657 households were listed.

Before drawing the sample, we eliminated individuals whose profile made the weekly POS collection either infeasible or inappropriate. The first group included those living in houses that could not be reached by motorcycle. By this criterion, the house had to be within 500 feet of a point accessible to the agent via motorcycle. We also eliminated those who received income payments less frequently than once per week. The weekly collection is of most value where income streams are either daily or weekly. In practice, this means the sample is largely made up of individuals who are either self-employed or daily wage workers. This is important to keep in mind, because the daily paid workers may have more ability to change hours worked than, for example, monthly salary workers. Finally, we eliminated those who had made a deposit in a bank account within the past month, since we expected the behavior of those using the bank regularly would not be affected as much by the deposit collection services. Note that the last two criteria were applied at the individual level, so that if any listed member of the household received income payments at least weekly and had not used a bank account in the previous month, that individual remained eligible even if the criteria eliminated other household members.

NSB quite reasonably felt that their agents could not refuse a deposit from anyone approaching them while making their rounds. Therefore, we knew we would need to conduct the randomization at the level of clusters of houses. Agriculture is the primary activity in the sampled areas around both towns. Near Bandarawela, the main crops are tea and vegetables; near Mahiyangana, the

main crops are rice and vegetables. The households are scattered into small clusters rather than distinct villages. We identified 156 distinct clusters of households (hereafter referred to as "zones"), separated by at least a stretch of road long enough to make spillovers between treatment and control zones unlikely.

With these clusters in mind, we selected the project sample in several stages. Because we were particularly interested in the interaction between formal and informal savings, we began by selecting a sample of individuals who participate in a *seetu* in which all of the members reside within one kilometer of the initial respondent. We refer to these as "close *seetus*." The first step was to select an individual in each cluster who is a member of a close *seetu*. There were 93 zones with at least one member of a close *seetu*. We re-surveyed these 93 individuals to collect the list of names of other participants in their *seetu*.

The second step of the sampling was aimed at achieving gender balance. The listing yielded a much smaller number of women than men meeting the employment / earnings frequency criteria. We therefore next selected up to five women in each of the zones. There were more than five women in only two of the 156 zones, implying that we selected essentially all of the women in the listing who qualified by the earning frequency and bank transaction criteria. We then extended the sample by selecting one male randomly from each zone.[3] Finally, for each close *seetu* we then randomly selected 20 percent of its membership to participate in the survey. The resulting intake sample of 829 has 432 women and 397 men. Between August 2010, when the baseline was conducted, and November 2010, when the randomization was carried out, 34 respondents attrited from the survey, leading to an experimental sample of 795 who were included in the randomization. Twelve households did not answer at least two post-treatment survey waves, our criterion for inclusion in the panel analysis, leaving us with a final sample of 783 for analysis. In all, 44 percent of the sample (344) are members of close *seetus*, with the remainder (439) coming from the broader random selection.

The randomization of collection services was carried out at the zone level, stratifying and balancing on data from the baseline survey. We stratified on quartiles of total savings balances, above/below median of household expenditures and whether or not the zone included a close *seetu*. We then re-randomized to achieve balance on a set of variables related to savings behavior and income

[3] Because some zones have no males, we select up to two additional males per zone and use those to fill the remaining sample.

generation.[4] The study sample thus consists of 78 zones with 389 individuals for treatment and 78 zones with 406 individuals for control.[5]

Finally, we exploited the additional listing of *seetu* members to introduce a randomized saturation experiment in order to investigate the relationship between formal and informal savings instruments. We began from the group of 39 close *seetus* that had been selected into the treatment, each of which had 20% of its membership in the household survey. We then offered treatment to (but did not survey) an additional 20% of the members in one third of these groups and to an additional 40% of members in another one third of the groups. Therefore, we surveyed 20% of the members of all of the close *seetus* in the study. With respect to treatment, 45 of the close *seetus* are untreated, 13 are treated at 20%, 13 are treated at 40%, and 13 at 60% saturation. The comparison of the fortunes of the constant 20% of members studied across the randomized variation in the treatment saturation allows us to isolate externality effects of formal financial services on the welfare of the membership of informal savings networks.

In December 2010, we offered to open accounts at the local NSB branch for those selected into treatment. The account opening required that the individual come to the bank branch. During the first two weeks of the month, project research assistants were at the bank branches to help participants with the required paperwork. The minimum balance required to open an account is 500 LKR (~ US$4.50); we paid this for all of the treated individuals who opened an account.[6] Of the 389 individuals selected for treatment, 347 (89 percent) opened an account. POS collection services began December 13th, 2010, in both branches. One main road from town was serviced each day of the week.

2.2. Surveys and administrative data

We conducted a baseline and three additional pre-treatment surveys for the full sample at monthly frequency between August and November 2010. We randomly allocated 498 households to a sample in which we continued to conduct monthly surveys, and 297 households to a sample in which we conducted quarterly surveys. This both reduced survey costs and allowed us at least a partial test of whether survey frequency affects deposit or aggregate savings behavior. For both monthly and

[4] The variables on which we balanced were agricultural employment, education, gender, present bias, monthly income, whether the individual had a formal bank account, balances of formal and informal savings, total and informal monthly savings amounts, and whether anyone in the household was either a grantor or recipient of an outstanding informal loan.
[5] *Seetu* members who were brought into the study through the listing of all *seetu* members received treatment based on their zone of residence, not on the status of their respective *seetu* seed.
[6] The opening deposit is trivial relative to median income of the participants (22,000 LKR). The exchange rate during the course of the study was approximately 111 LKR per US dollar.

quarterly survey groups, we conducted surveys at the defined frequencies through November 2011. We then conducted monthly surveys of everyone in December 2011 and January 2012 and longer term follow-up surveys in July 2012 and January 2013. Thus, for the monthly survey sample, we have five pre-treatment, 15 post-treatment surveys (13 monthly plus the two semi-annual surveys); for the quarterly survey sample, we have four pre-treatment and eight post-treatment surveys (four at quarterly intervals plus two monthly surveys and the two semi-annual surveys). Six months into the main experiment, we began a series of unbundling experiments whose impact is described in de Mel et al. (2013). The unbundling experiment was conducted in a randomly selected and well-balanced subset of the control and weekly home visit treatment arms. To avoid confounding the primary results, we dropped the 192 treatment and 150 control individuals involved in the unbundling exercise as soon as that experiment began. Appendix Table 1 details the timing of the surveys, and shows which surveys are included in the sample we use here. The result is a full 30 months of data for the core sample (92 zones; 18 months at high frequency) and, for a subsample of 64 zones, data from the 12 months prior to the beginning of the unbundling experiment. Results are very similar if we use only the sample of 92 zones, but the precision of the short-term estimates is improved by the inclusion of the additional group that receives the core treatment for six months. Our analyses use individual-level fixed effects and we cluster standard errors at both the zone and individual level using the method developed by Cameron, Gelbach and Miller (2008) as a way of accounting for both the substantial autocorrelation present in high-frequency household data and the effect of local shocks.

We undertook this project with the aim of answering the simple but vexing question: what is the root source of money that is newly brought into the formal financial sector? When people begin to use formal savings, what other behaviors in the household change to allow this liquidity to be deposited in a bank? Candidate explanations are that saved capital is substituted from cash in the mattress, that greater discipline from formal savings causes expenditures to decrease, that formal savings come at the cost of informal mutual insurance networks, or that some new source of income is engendered by the savings. The survey was designed with these sources in mind (we include the cash flow component of the survey in Appendix B). The heart of the survey instrument is a cash flow analysis for both the household and for individual being sampled, who was always the respondent. In order to unpack the headwaters of formal finance, we need to be able to construct balances of financial flows at the household level. Thus, our survey was designed to capture monthly inflows (earned income, transfers from individuals and other transfers) and outflows (expenditures, transfers to other individuals, savings and loan payments).

Because we expect individual members of a household to have better information about their own earnings and transfers than those of other members of the household, we also ask about the respondent's individual cash flow. Questions asked of the individual respondent net out her/his transfers to and from other members of the household to calculate personal income, and then allocate this personal income to individual consumption items. Significantly, we ask the direct questions on the amount saved through various means (in the NSB account, in other formal accounts, in cash, in *seetus*, etc.) with regard to the individual respondent. Since savings decisions are likely made at the household level in many households, we make use of the aggregate household income and expenditure data. But we also use the more detailed individual data to focus on the outcomes of the individual respondent. The enumerators were trained to check that the sources of cash matched the uses of cash for the individual. Where the initial responses yielded differences, the enumerators pointed out the inconsistency and re-asked the income and expenditure questions.[7]

The decision to focus the detailed earnings questions on the activities of the participant him/herself represents a tradeoff. On the one hand, we focus on data the participant knows best. On the other hand, we will be somewhat limited in answering the "headwaters" question if the changes in income, expenditure, or savings come from changes in the behavior of other members of the household. That is, if we identify that increases in savings in banks are associated with increases in transfers from the spouse, we know only indirectly whether the spouse increased his income – and if so, we do not know how – or decreased his formal or informal savings. But the aggregate household data allow us to identify the sources of changes in savings arising from income and expenditure patterns of other household members up to a point.

In addition to the detailed survey data, we have administrative data from NSB for the accounts directly linked to the project. These detail each deposit and withdrawal, as well as other transactions (e.g., interest payments). We use the institutional data in this paper to study the determinants of uptake and account usage (Table A4) and institutional balances (Table 1); all of the remaining impact tables rely exclusively on information from the household surveys.[8]

[7] Samphantharak and Townsend (2009) assign mismatches in monthly income and expenditure to changes in cash holding. We measure cash holdings along with other financial assets. To the extent that individuals do not want to report increases in cash holdings, they might be tempted to increase reported expenditures instead. However, over years of conducting surveys in Sri Lanka, we have found respondents to be generally very willing to discuss financial matters openly, and we do not detect any patterns in the data which suggest significantly mis-reported cash balances.

[8] Our analysis of a subsequent unbundling experiment takes place within the treatment group and hence makes extensive use of the institutional data; see de Mel et al. (2013).

2.3 Balance and attrition

Appendix Table 2 examines correlates of survey attrition. Overall, the resurvey rates were high. Ignoring attrition from the sample that occurred prior to the assignment of treatment, only 12 out of 795 individuals were not surveyed at least twice pre-treatment and twice post-treatment (our criteria for inclusion in the panel sample). We also examine the 20 individuals who did not complete the last high-frequency survey (wave 18 in January of 2012). Most importantly, attrition does not differ in the treatment and control groups whether we examine simple differences between the two groups or we control for other covariates.

Appendix Table 3 examines the balance of the experiment. Treatment was assigned at the zone level, and the randomization was stratified on whether a zone contained a close *seetu* (meaning that all the members of the *seetu* were resident in that zone), as well as on average household expenditures and baseline average savings in each zone. Stratification was done using baseline (round 1) data, and we test for balance using the remaining pre-treatment waves (rounds 2-5). Appendix Table 3 shows balance on a range of variables measuring savings, income and expenditure. Only one of the 35 variables tested shows any difference, and then only at the 10 percent level.

Despite the excellent overall balance of the experiment, the number of units to which treatment was assigned was relatively small (156), and due to sampling rules intended to locate as many self-employed females as possible, there is substantial cross-zone variation within the sample in variables such as gender and *seetu* participation. For example, 28 zones contain all male and 10 zones all female core respondents. Potentially due to this clustering of gender by zone, the experiment is imbalanced when we look among men only or among women only.[9] Figure 1 shows the densities of one of the most severely imbalanced variables, the log of personal income. While the imbalances are not visually severe, the figure illustrates how the problems within men and within women subsamples counteract each other to lead to a full treatment/control comparison that is well balanced.[10] The four subgroups used later in the analysis (the self-employed in manufacturing, the self-employed in trade and services, and those not self-employed, and *seetu* members) are all well-balanced within subgroup.

[9] We used randomization inference techniques (Small et al. 2008) to re-run our randomization code 1,000 times and examine how the actual sub-group balance of our experiment compares to the distribution of subgroup treatment/control differentials that could have obtained given our sample and randomization routine. This illustrates that we were simply unlucky in realizing a group of treatment females (males) that are substantially richer (poorer) than the control (p-value < .01 for highly imbalanced variables such as household income among males).

[10] Interestingly, if we use only the data from the round 1 survey (on which the stratification was conducted), no significant signs of sub-group imbalance are observable. The imbalances result from a gap that opens up between treatment and control within men and within women in rounds 2-5, and hence is only observable because we collected multiple high-frequency pre-treatment surveys as suggested by McKenzie (2012).

Given the imbalance within gender, we avoid analysis by gender and discuss imbalance issues carefully when we move to the analysis of the *seetu* experiment in Section 5.

3. THE INTER-TEMPORAL SUPPLY OF LABOR

A substantial literature in macroeconomics examines the role of interest rates in governing the intertemporal allocation of labor (Lucas and Rapping, 1969). Central to these models is the idea that increasing the real interest rate raises the relative return to working in the present relative to working in the future. In this section, we provide a standard model to develop this point.

Consider an agent who consumes c_t and works h_t in period t with utility

(1) $$u(c_t, l_t) = \frac{1}{(1+\rho)^t}\left[\ln(c_t) - h_t^{\gamma}\right].$$

In period t the agent has wealth A_t, which is a function of the prior period's assets and wage income.

(2) $$A_t = (1 + r_t)A_{t-1} + w_t h_t - c_t.$$

Several simplifying assumptions are worth noting. First, We ignore the issue of risk in the income stream. Increasing risk would lead to precautionary savings, but as our experiment changes the interest on savings in isolation, any effect on the experiment on the level of precautionary savings is likely to be second order. Second, we assume that utility from consumption and disutility of effort are additively separable, implying that the marginal utility of consumption is independent of the disutility of effort. We also assume a unitary intertemporal elasticity of substitution in consumption. We do this mainly to simplify the algebra, though there is some evidence that the intertemporal elasticity of substitution in consumption is close to one. (See, for example, Beaudry and Van Wincoop 1996.) Finally, we assume that wage rates are not affected by the experiment. Because the treatment group is a small part of the regional population, we take the increase in the interest rate as isolated from any effect on wages or other prices. The Euler equation governing the intertemporal allocation of labor is:

(3) $$\frac{h_t}{h_{t+1}} = \left[(1 + r_{t+1})\frac{w_t}{w_{t+1}}\frac{1}{(1+\rho)}\right]^{\frac{1}{\gamma-1}}.$$[11]

[11] The corresponding Euler equation for the intertemporal allocation of consumption is $\frac{c_{t+1}}{c_t} = \frac{(1+r_{t+1})}{(1+\rho)}$.

If the POS savings treatment provides an improved savings option,[12] then for the experimental treatment group, the increase in r, holding other parameters fixed, represents an increase in lifetime wealth for those in the treatment group with positive savings balances. The treatment thus implies an income effect in addition to the substitution effect. In this formulation, the elasticity of the labor ratio with respect to the gross interest rate $1 + r_t$ is $\frac{1}{\gamma-1}$ and so is a function of the curvature of the disutility of effort function. The Euler equation communicates a standard feature of these models: if the cost of effort function is convex ($\gamma > 1$) then the substitution effect will dominate over the income effect such that an increase in the future interest rate should lead to relatively more work in the current period relative to the future period.[13] That is, holding the period-specific wage rates constant, the increase in r will lead to an increase in hours in period t relative to period t+1.

4. RESULTS.

The changes in labor supply and intertemporal consumption that come from the model are driven by an increase in the interest rate on savings. If the collection services did increase the effective interest rate realized by program participants, then we should observe both a robust take-up of the product, and an increase in total savings. These outcomes are the analogous to the first stage in the experiment. We begin, then, by looking at these preliminary outcomes.

4.1. Uptake and Usage.

Appendix Table A4 uses pre-treatment averages (data from rounds 1-5) to predict subsequent uptake and usage of the accounts among the 389 individuals offered the treatment. Account usage appears progressive in that those most likely to make any deposit are low-income females who score poorly on the digitspan test. At the same time, a strong predictor of use of our accounts is a history of formal savings prior to the initiation of the experiment. Low-expenditure households use the accounts most frequently for both deposits and withdrawals, but none of our determinants are significant in explaining the total number of transactions in program accounts. Among those who make deposits the two significant determinants of the log of deposits are having more education and

[12] The NSB accounts paid an interest rate of 5 percent per year. Plausibly, the effective interest rate on cash held at home – a common alternative in our sample – is negative, because money might be stolen or used by someone other than the respondent.

[13] There are, similarly, income and substitution effects in consumption. The increase in r favors consuming tomorrow over today, but the increase in wealth increases aggregate lifetime consumption.

having higher formal savings at baseline. Those with high baseline savings also make significantly more withdrawals from program accounts, however, leading to final savings balances that are not significantly higher than those who had no formal savings at baseline. Final savings balances are strongly progressive in expenditures (meaning that poor households save significantly more through the program than wealthier households even in absolute terms), and baseline *seetu* participation is also predictive of lower final savings balances. The progressivity of final savings balances is encouraging, indicating that weekly home visits with the POS terminals are particularly effective at generating deposits from low-income households.

4.2. Savings impacts.

The weekly visits to households generate a large increase in the frequency of transactions with formal financial institutions; Table 1 indicates that these quadruple from a control average of .5 transactions per month to a treatment average of over 2 per month. The dramatic nature of this effect is depicted in Figure 2. Despite a large increase over the control group, the data show that treatment households only make deposits through the collectors about half of the time. The institutional data (Column 3) indicate that net savings in the NSB accounts linked to the project increase by 425 LKR per month. The survey data (Column 5) indicate that total bank savings increase by roughly 690 LKR per month. On the surface, this implies that the treatment also leads to an increase in deposits in other formal accounts. While we would be cautious about that conclusion – not least because the difference of 265 LKR per month is not significantly different from zero, but also because the survey data may be noisier – the data at least suggest that the NSB savings are not coming directly from other formal savings accounts. Indeed, the remaining columns on Table 1 indicate that informal savings increase by a nominal (and insignificant) amount, and that total savings through all vehicles increase by 883 LKR per month as a result of the treatment. The increase in total savings is around 4 percent of the mean personal income in the sample and about 16 percent of control-group mean savings.

This preliminary look at savings therefore indicates that treatment leads to a statistically significant and economically meaningful increase in total savings by the individual. There is no evidence that the savings deposited in the NSB account is crowding out either other formal savings or informal savings. The increase in savings suggests that participants viewed the collection services as a substantial change in their portfolio of financial services, opening the possibility that the treatment led to more fundamental changes in their behavior.

4.3. Headwaters.

Having verified that the experiment had the intended effect of increasing the aggregate savings of the participants, we can now ask the central question for which the experiment was designed: where does the increase in savings come from? The three immediate possibilities are: 1) a reduction in expenditures; 2) an increase in earned income; 3) and increase in net transfers from others inside or outside the household. Previous research gives some support for at least the first two possibilities. Banerjee et al (2013) find that microfinance clients investing in microenterprises reduce expenditures on 'temptation goods' and increase the purchase of durable goods. And both Dupas and Robinson (2012) and Prina (2013) find that expenditures increase with the introduction of savings products which are similar to the one we implement. Though neither of these papers reports data on income, the implication is that incomes increased for those offered the accounts.

Our high frequency data allow us to present a very detailed answer to this question. We take a "sources and uses" accounting approach to analyzing the effect of the POS treatment on outcomes, beginning by examining the sources and uses of funds at an aggregate household level. The first column of Table 2 shows the treatment effect on total household income from all sources, including earnings, remittances from domestic and foreign sources, and other flows (including loans, government program payments, and the like). Panel A shows the effect of treatment using the raw data and Panel B shows the treatment effects using data winsorized at the 99th percentile to ensure that the results are not driven by outliers. Treatment has a substantial positive effect, increasing total household income by roughly 10 percent of the mean in the control group using both the untrimmed and trimmed data, though the effect is statistically significant only with the trimmed data.

Columns 2, 5 and 6 disaggregate the household income into three sources which come directly from survey responses: earned income (Column 2), remittances from domestic and foreign sources (Column 5) and other household inflows, including loan receipts, *seetu* payments, government program payments, and the like (Column 6). In the untrimmed data, just over 90% of the increase in household income comes from earned sources. The trimming reduces the effect on earned income somewhat. Combined with a slightly larger treatment effect, this implies a larger role to other household inflows. However, even though the measured flow from other sources is higher in the trimmed data (Column 6, Panel B), the treatment effect on these flows remains insignificant.

Columns 3 and 4 split the increase in household earnings into changes in income reported by the respondent and a residual, which we label changes in earnings from other household members.[14] Using either the untrimmed or trimmed data, the change in the respondent's income is about twice that of other household members. We return to a further analysis of the respondent's income below.

The right-hand side of Table 2 shows the treatment effect on uses of funds in the household. Cash flows are either consumed, saved or transferred outside the household. There are fairly large measured effects on both consumption and savings in both the untrimmed and trimmed data, but only the savings effect is statistically significant, and then only when using the trimmed data. The trimmed data indicate an increase in consumption, but one that is highly insignificant. Splitting transfers outside the household into loan repayments and transfers to individuals, we find a statistically significant increase in loan payments following treatment, but no change in transfers to individuals. In aggregate, the data on the sources and uses of funds balance reasonably well, with the uses increasing by 70 percent of the increase in sources of funds in both the trimmed and untrimmed data. One possible explanation for this "leakage" is that there are uses which are not categorized by respondents in any of these bins- for example, investment in business inventories.

Panel C of Table 2 allows the treatment effect to vary across time. The save / invest / earn channel implies that income should increase little if at all initially, with larger increases occurring later after treatment. The labor reallocation effect should have the opposite pattern, with income rising immediately as households increase effort today. In the time-varying regressions, we include the last two survey rounds, 18 and 24 moths after treatment. The data show a pattern more consistent with the labor-adjustment story, with very large immediate increases that do not grow over time and may diminish by the last survey round. This pattern of treatment effects across time is the first piece of evidence in favor of the labor reallocation effects.

Given that increases in income appear to be the main source of funds invested in the savings accounts, we turn in Table 3 to examining the income data in more detail. We shift our focus to the respondent's behavior because that is where we have the more detailed income data. In any case, the data on Table 3 indicate that the majority of the increase in earned income is due to the respondent.

Panel A of Table 3 shows the disaggregation of income for the full sample, with all income data winzorized at the 99th percentile. We begin by examining the effect of treatment on occupational

[14] The baseline survey included a household roster for which the respondent reported the occupation, hours worked and income for other household members. We did not include the roster in later rounds to limit the length of the survey.

choice. We find no evidence that the savings accounts operate by relaxing credit constraints and increasing entry into self employment. On the contrary, we find that self employment falls by 3.4 percentage points in the treatment group, relative to control. Engagement in wage work increases by two percentage points, though the effect is not statistically significant. However, earnings from wage work (Column 4) and average hours in wage work (Column 7) both increase by significant amounts. Earnings from self employment increases insignificantly even though hours working in self employment are unchanged. Note that all of the measures capture both intensive and extensive effects. Self employment and wage income and hours are coded as zeros for those not self employed, and hence the treatment effects are impacted by the entry into wage work and exit from self-employment reflected in Columns 1 and 3.

We can also calculate an hourly treatment effect by comparing the increase in hours with the increase in earnings. Hours worked is reported on a weekly basis, and the income on a monthly basis. Thus, the increase of 1.5 hours per week in wage work is thus approximately 6.4 hours per month. This implies a wage a the margin of 49.4 LKR per hour (315 LKR / 6.4 hours), an amount very similar to the average hourly wage in the control group, which is 50.5 LKR per hour. This is reassuring with regard to the consistency of the data.

In sum, we find that earned income increases immediately after treatment and the magnitude of the treatment effect is either steady or decreasing across time. Treatment also leads to a shift out of self-employment and into wage work. These results are not consistent with the save-invest-earn channel of savings. They are consistent with the reallocation of labor effort across time. To explore this channel a bit further, we step outside the experiment and examine effects in three subsamples of the overall sample.[15] We first split the sample into those who are self-employed and those who are not. We do this using the activities reported in the four baseline rounds, before treatment status was known to the participants. This initial split is justified by the fact that the save-invest-earn channel can work through either the intensive or extensive (e.g., lower exit rates) channel among those self employed at baseline, but only through the extensive margin among those not self employed. There are 419 respondents active in self employment in at least one baseline round, and 360 not active in

[15] The project started before the RCT registry was established. Hence, we have no public randomization plan. However, the stratification indicates that our initial analysis was built around the informal – formal savings channel. The data show little evidence for this channel in aggregate, though we return to this issue below. Though we balanced on expenditures, indicating that we saw this as a key outcome, we did not expect to find an immediate increase in income and a shift out of self-employment. The three subsamples we construct are hence non-experimental and the analysis should be interpreted with this in mind.

self employment. We then split the self-employment sample further into those active in manufacturing during baseline, and those active only in trade / services. Given the very rural nature of our sample, we expect those in trade and services may have less opportunity to increase demand, while those in manufacturing are more likely to be able to sell output in nearby towns, and hence less likely to be constrained by the very local market.[16]

Panels B through D of Table 3 report the treatment effects on activities of participants in the self-employed manufacturing, self-employed non-manufacturing, and non self-employed samples. Among those active in self-employed manufacturing in baseline (Panel B), we see no shift toward wage work, but instead a very large increase in reported business earnings. This is accompanied by a modest (and statistically insignificant) increase in hours worked. Together, these imply marginal earnings of 219 LKR per additional hour, about three times the sample mean of 72 LKR / hour among all self-employed. These data are thus consistent with an increase in both work intensity and work hours, as should be expected if the effective returns to labor increase.[17] Researchers have struggled to measure effort effects in self employment activities, and we lack good measures as well. So we can only say that while the reported increase in earnings is high relative to the reported increase in hours worked, and speculate that this may reflect an increase in the intensity of effort.

In contrast, among those who are self-employed in trade / services, we see a sharp exit effect (column 3), accompanied by an increase in reporting wage work (column 1), in wage earnings (column 4) and in the hours worked in wage work (column 7). Again, the incremental earnings in wage work per incremental hour worked – 54.9 LKR – is close to the sample average. Now, however, the same calculation on self-employment – measured as a decrease in earnings per decreased hour worked – is also much closer to the sample mean, at 88.2 LKR per hour. Note that this decrease is a LATE of those remaining in self-employment in the control group who would have exited with treatment. Thus, we should not expect to find any effect of effort, since the self-employed over whom this effect is measured are in the control group. While the trade / services self-employed are induced to make large movements to wage work, these movements are ultimately ineffective in increasing income: Wage earnings rise, but business earnings fall by an almost identical amount. Finally, we see no effects on

[16] As it turns out, those active in manufacturing are also much more likely to report income from more than one activity during the baseline period. Two-thirds of those in manufacturing are also active either in wage labor or agriculture, compared with less than 5 percent of those in trade and services. The differences in the subsamples may be due to these multiple activities or some other characteristic correlated with involvement in manufacturing.

[17] See, for example, the estimated intensity effects found by Lazear (2000) when workers were switch from wage to piece rates.

either activities or income among those not self-employed at baseline (Panel D). There are neither significant shifts in activities nor in income earned.

The cash-flow analysis reported in Table 2 indicated that the income effects were immediate. In Table 4, we examine the time-varying effects using the more detailed income data, beginning (Panel A) with the full sample. We see that the movement out of self-employment is immediate, but the increase in wage earnings is significant only beginning 5 months after treatment. Wage earnings in the treatment group increase steadily and the treatment effect on business earnings – positive but not significant initially – becomes negative in the last survey round. [18]

Panels B and C show the time-varying effects for the two self-employment subsamples. We add columns for purchases of business assets and the change (net investment) in inventories. Among the self-employed in manufacturing (Panel B), we see the immediate jump in business earnings, which fades very slightly over time until the last survey round, when it falls markedly and becomes statistically insignificant. Looking at investment, there is a modest and insignificant increase in inventories in the first post-treatment period, and there is a significant increase in business asset purchases in the last survey round, giving some suggestion of the save/invest/earn channel among this sub-sample.

In Panel C, we find that the exit from self-employment among those initially in trade and services increases throughout the post-treatment period. The treatment effect on reporting self-employment activity is -5.4 percent and significant in the initial post-treatment period. But this grows so that by the final two survey rounds, fully one in six of those in the treatment group who were initially self-employed has exited self-employment. Both reporting any wage earnings (Column 1) and the level of wage earnings (Column 4) increase steadily across time, but business earnings decrease by a comparable amount in the first 13 months, and by substantially more than the wage increase in the last two surveys.

The results in Tables 3 and 4 indicate that both subsamples of the self employed show significant labor responses following introduction of the savings services. However, only those self-employed in manufacturing have success in increasing income. Neither the self-employed in trade / services nor those not self-employed show any increase in earned income. It is reasonable to ask whether the differences in the earnings response across the subsamples is mirrored in differences in

[18] Note that unlike us, both Dupas and Robinson (2013a) and Schaner (2014) *do* find that savings programs lead to higher capital investments in different samples of Kenyan microenterprises. Two findings in these studies contrast with our results. First, in a sample of 249 market vendors and bicycle taxi drivers, Dupas and Robinson (2013a) find no increase in hours worked. Second, Schaner (2014) finds that a six month savings subsidy provided substantial increases in savings, business investment, profits, and expenditure after 2.5 years.

savings behavior. We examine this question in Table 5, where we return to the sources-and-uses analysis, this time by subsample. Panel A repeats the results for the full sample from the trimmed sample in Table 4. Panels B, C, and D then show the self-employed manufacturing, self-employed trade / services, and not self-employed subsamples. We see that there is a significant increase in savings (column 8) and loan repayments (column 10) only among the self-employed in manufacturing, where individuals were successful in increasing earnings. In this subsample, there is a small and insignificant change in consumption (column 7). Among the self-employed in trade/services, the only category showing a significant treatment effect is a decrease in transfers outside the household, which while statistically significant, is relatively small in magnitude. Among those not self-employed, there is a marginally significant increase in other household inflows (column 6) and non-trivial, though highly insignificant, increases in savings and consumption.

5. SELF-CONTROL AND OTHER-CONTROL.

While our primary results indicate that a neo-classical model of intertemporal time preference may be important in explaining the response to changes in savings options, other mechanisms may also be operative. We now seek to understand the extent to which self-control and other-control may explain the treatment effects we observe.

5.1. Self-Control

Much of the recent interest in the theory underlying the use of savings products is due to behavioral economics' focus on the difficulty that people have putting aside money from daily cash flows to meet savings goals. Several distinct motivations for under-saving have emerged, but each suggests that a high-frequency deposit collection service can alter consumption in important ways. Individuals with self-control problems will achieve a higher savings trajectory if the long-term self can lock the short-term self into a commitment (Laibson 1997). Frequent deposit collecting allows people to get loose cash away from themselves and may permit them to control their own consumption of 'temptation goods' (Banerjee and Mullainathan 2009). Our savings product provides an interesting window on these behavioral dimensions: it features convenient deposit and inconvenient withdrawal (individuals had to travel to town to withdraw money from the accounts) and therefore has a commitment dimension. Accounts are in the name of the core respondent, providing a potentially confidential environment for savings, potentially changing intrahousehold bargaining.

The fact that we find no evidence that treatment decreases expenditures suggests that none of these arguments about controlling consumption can be playing a driving role in the overall effects of the program. Moreover, when household and personal income go up by more than 10%, controlling consumption may become less important, not more so. Nonetheless, the improved control offered by the deposit collecting could generate heterogeneity in the changes in consumption and savings that occur as the program is introduced. If self-control is a dominant explanation for behavioral changes, then we would expect to see that income increases more, consumption increases less, and consequently savings balances are built up faster for those who were time inconsistent at baseline.

We measured time inconsistency in the standard way; our baseline survey asked the respondent "Suppose someone was going to pay you Rs. 1500 1 month from now. He/she offers to pay you a lower amount today. What amount today would make you just as happy as receiving Rs. 1500 in 1 month?" The answer to this question gives the total current discount factor, or the product of beta and delta if intertemporal preferences take the quasi-hyperbolic form $U = E_0\left[u(c_0) + \beta\sum_{t=1}^{T}\delta^t u(c_t)\right]$. We then ask the same question with the time scale moved out by one year (12 months from now versus 13 months), and the beta parameter can be calculated by the ratio of the former to the latter answer. We then classify as time inconsistent anyone with a beta parameter of less than one.

Because of the special role of deposit collecting in potentially controlling the consumption of 'sin' or 'temptation' goods, we conducted a panel survey experiment to induce truthful revelation of goods whose use may be stigmatized. The survey experiment asked respondents to pick (and not reveal to the enumerator) cards numbered from 1 to 10 prior to each question about consumption of different sin goods. If the number was between 4 and 10 the respondent was to answer 'blue' if they consumed that sin good in the past month and 'red' if not, but if the number was between 1 and 3 they were to answer 'blue' regardless of the correct response. In this way enumerators can never infer the consumption status of a specific individual, but the sample prevalence of sin good consumption can be readily calculated. The same property of aggregation holds across sin goods for an individual; consequently the panel dependent variable used in the analysis is $\frac{\sum_j I_j(blue_j)\Big/J - .3}{.7}$, where $I_j(blue_j)$ is an indicator for a response of 'blue' for each of the *J* sin goods in the survey experiment. We asked about purchase of alcohol, purchase of tobacco, and about gambling on horse races and on cards. These questions were asked in pre-treatment wave 3 and in post-treatment waves 10 and 18.

Table 6 examines the heterogeneity of treatment effects by the extent of time inconsistency. There is no evidence of heterogeneous program effects on the measure of total household income, the likelihood to engage in wage work or self-employment, or in business income (Columns 1 and 4), meaning that the surge in income in the treatment group is not driven by time inconsistent individuals. Turning to consumption, there is indeed a negative coefficient on the interaction term, indicating that the increase in consumption is about half as large for time inconsistent individuals, but the difference is far from being significant. There appears to be no special differential role of 'sin' goods in consumption, whether we use the direct survey measures of consumption or the three rounds of list experimental measurement. In terms of savings there is similarly no significant heterogeneity of impacts on bank, non-bank, or total savings, but the magnitudes of the point estimates are not small.[19] These results indicate that the impact of the treatment on total savings are insignificant in the time consistent population (611 rupees per month), and the impact of the treatment among the time inconsistent is roughly twice this large (an additional 666 rupees). Hence, while there is evidence of mild differential effects in the expected directions for time consistent individuals, these differences are never significant and so behavioral issues of self-control do not appear to the primary drivers of our overall treatment effects.

5.2. Other Control

We investigate two mechanisms through which issues of interpersonal conflict may drive the impacts of new, private savings options. First, when members of a household disagree on the optimal savings trajectory, private commercial savings may allow the pro-savings individual in the household to control the household savings rate (Ashraf et al. 2006). To investigate this, we conduct heterogeneity analysis to examine how the baseline income share of the individual offered the account drives savings behavior. Andersen & Baland (2002) argue that other-controlling savings options (in their case ROSCAs) are less valuable for individuals who have full household control (because they do not need them) and for those who have no household control (because even with such an option they still cannot sufficiently control income). Hence, their prediction is that such savings options will have the largest effect for those who have an intermediate degree of financial control. Following their methodology we calculate the ratio of the earned income of the individual offered the savings account

[19] We have also tested to see whether the treatment effects on labor hours are heterogeneous by time inconsistency; they are not.

to total household income, and use this to proxy for financial control. We divide this variable into terciles and look for evidence of heterogeneity in impacts by the respondent income share.

Table 7 includes individual and round fixed effects, the panel dummy for treatment, and the interaction between this dummy and indicators for the second and third terciles of the income share distribution. The first row therefore gives the impact of the program among individuals who have the lowest 1/3rd of income share, and the second and third rows provide a test for the extent to which the treatment effect in the higher income share terciles differ from the impact in the lowest tercile. The results are suggestive of considerable heterogeneity, with the largest impact on household income and the *sole* impact on earned individual income arising from the lowest tercile, which is the most economically disempowered group. The treatment causes the lowest tercile to decrease transfers to spouses and increase transfers from spouses, although these differences are not significant. While these results indeed indicate that treatment effects differ by pre-treatment income share, the savings results are the opposite of those predicted by Anderson & Baland; the savings effects are largest for the top and bottom terciles and smallest for the middle tercile. The results are consistent with the fact that our treatment was most effective for individuals who started the intervention in a relatively disempowered position.

Our second approach to understanding the importance of other-control follows from the inherently fragile nature ROSCAs which are dependent on social coercion to enforce contributions (Anderson et al. 2009) and are particularly vulnerable to outside formal financial options of their members (Besley et al. 1993). The design of our randomized saturation experiment provides an experimental shock to the formal savings options of the other members of ROSCAs. As an increasing share of these other members have access to private personal savings accounts, they may become more likely to renege on ROSCA contributions, and less likely to exert effort monitoring the behavior of group members. Thus, if formal savings are a substitute for informal savings groups, we expect to see the attractiveness of ROSCA savings fall as an increasing share of the ROSCA members renege on their savings commitments and on their attempts to control the behavior of fellow group members. At the extreme this could lead to group collapse, and on the intensive margin it would lead to a decline in the number of ROSCAs an individual chooses to participate in and the quantity of savings achieved through ROSCAs.

To understand the impact of *seetu* saturations on informal insurance, we use the monthly flows of transfers into the household and out of the household (we screened out cases in which a second household member was in the same *seetu* as the respondent, and so the interpersonal flows of

informal insurance should be primarily inter-household). We also examine the effect of the *seetu* saturation experiment on overall savings. The sample for this analysis consists of the sampled 20% of the membership of each of the 84 close *seetus* drawn into the survey at baseline, and the experimental variation arises from whether an additional random 0%, 20%, or 40% of their ROSCA was offered the POS weekly home savings product.

We use two empirical specifications for each outcome. The first examines the overall intention-to-treat effect within the close *seetu* membership. The second includes a dummy for treatment and further includes the randomly chosen saturation within treatment *seetus*. In this specification the dummy for treatment gives the impact in the 20% treatment saturation *seetus*, and the slope term on the saturation gives the marginal effect on outcomes of treating (but not studying) an additional fraction of the membership beyond this. Because we have no individuals that are studied but not treated in treatment *seetus*, we are only able to estimate the Spillover on the Treated, and not the Spillover on the Not Treated (Baird et al., 2015). The effects of the saturation should therefore be interpreted as the impact of increasing the fraction of the *seetu* given the deposit collection treatment, conditional upon the fact that the studied individual is treated.

Before presenting the results of this analysis, Appendix Table A6 shows the balance of the *seetu* saturation experiment. This randomization was conducted in a very small number of units; the saturation experiment is particularly subject to potential imbalance as each saturation cell contains only 13 *seetus*. We show balance over 15 tests and find only one of them to be significant at the 10% level, but nonetheless there are some quantitatively large differences, particularly across the saturation amounts within the treatment. Overall savings is imbalanced across the distribution of the saturation. We therefore proceed to the analysis of the saturation experiment with some caution as the number of assignment units is small and the balance not perfect.

The results of this analysis are given in Table 8. Contrary to the hypothesis that private individualized savings poses a threat to *seetu* participation, the treatment and the treatment saturation appear to lead to superior outcomes for the *seetu*. Treatment on average increases the number of *seetus* in which a respondent participates by 0.293 over a base of 2, an increase of more than 10 percent. Allowing for heterogeneity across the saturation rate indicates that where more individuals are treated, membership in *seetus* increases more rapidly. The coefficient of 1.435, significant at the 1% level, indicates that each 20% increase in the fraction of the *seetu* treated increases the number of *seetus* participated in by its members by a further 0.3. The monthly amount saved through *seetus* is higher in

treatment than control zones, and the saturation effect is again positive and large in absolute magnitude although not significant.

Finally, looking at the informal insurance issue, we find that the saturation experiment has no significant effects – and, indeed, the point estimates on the saturation experiment are positive – on transfers into and out of the household. The transfers coming into the household are perhaps most meaningful given the nature of the experiment: all of the studied individuals in the saturation experiment are themselves treated, and so if the increase in the treatment saturation were having a detrimental effect on the quality of social insurance then we would expect transfers to these treated individuals to be declining. This does not appear to be the case, and so we find no evidence of a deterioration in informal insurance as a result of the intensity of access to formalized individual savings products. The total amount saved is 940 LKR per month higher in treatment than control *seetus*, not quite significant at the 10% level, and the coefficient on the saturation is again positive. Hence, the result of the randomized saturation experiment indicates that far from posing a threat to them, the bank-driven deposit collecting appears to be fortifying informal savings groups.

6. CONCLUSION.

We conducted an experiment with a powerful inducement to save: weekly visits by a deposit collector equipped with a wireless POS device capable of printing out account balance receipts on the spot. The experiment was implemented in Sri Lanka, a country with strong informal savings institutions and a tradition of using formal banks for 'children's accounts' into which parents often begin to save at the birth of a child. Despite this, we find strong effects of the treatment not just on savings into program accounts, but into bank savings and overall savings as a whole. The data therefore suggest that, in spite of the sample having access to a wide range of formal and informal savings mechanisms, the simple low-transactions cost account represented a significantly better option. We tracked the sample with monthly surveys including five pre-treatment waves, and a careful cash-flow accounting instrument designed to answer two questions. First, what are the headwaters of formal savings? Second, what are the channels through which formal savings affects income and expenditures?

Our results confirm recent experimental findings that access to formal savings products can produce surprisingly large benefits for unbanked households, even when these accounts bear low real interest rates. Our detailed high-frequency surveys help to shed light on the mechanisms behind these savings increases. We find a large and immediate increase in income following treatment, allowing

savings to increase without any accompanying decrease in consumption. The surveys reveal that this newfound income is not generated by increased investment in entrepreneurial activities, and indeed the savings product triggers *exit* from self-employment. In aggregate, households enter the labor market and work more hours at wage work. An increase in earned income explains just over 90 percent of the increase in income in the untrimmed data, and just under 60 percent in the trimmed data.

In the final monthly survey, we asked participants in the treatment group several direct questions about how the savings treatment had affected their behavior. Their responses are quite consistent with the outcomes and channels we identify in the data. 78% of those opening accounts reported that the intervention helped them to increase savings, and only 5% said they felt pressured to save by the door-to-door collection. Among those who said they had saved more, 38% said this was achieved primarily through increased income, 34% through decreased consumption, and 22% through changes in intra-household transfers. By far the most common source of increased income was said to be "more effort to increase sales" in self-employment or agriculture. This was reported by 50% of those saying that income had increased, with 13% reporting doing more work at a previously engaged wage-paying job, 5% doing new wage work, and 18% reporting increased income from re-investing in self-employment or agriculture. Although we find some evidence of increased investment in enterprises in the final survey rounds these self-reported narratives reinforce the idea that our intervention increased savings by raising incomes, principally through more labor effort.

The novelty of our results, then, is that we show substantial increases in savings and income without any foregone consumption, and without the save-invest-earn channel that is suggested by the literature on non-convexities in production. Earnings increase immediately upon advent of the treatment, and they increase in the face of exit from self-employment. In contrast, the self-employed in retail / services do not grow their businesses, and instead move out of self-employment into wage work. The treatment effect is large in this group – one in six individuals have exited self-employment by the end of the study – but ultimately ineffective. Wage earnings increase, but self-employment earnings decrease by an almost identical amount. We note that while we believe this subsample analysis is illuminating on the mechanisms, our main conclusions are driven by the full sample analysis on which the experiment is based.

In one sense it is surprising that a financial service offering nothing more than a low-cost means to save in a bank could increase household income. However, as Blanchard and Fischer showed long ago, the responses we observe in the data are consistent with those we would expect from

individuals who experience a large increase in the interest they receive on savings. Such an increase in the interest rate drives up the benefit of working today to save for tomorrow, and causes individuals to increase labor effort in the initial period in order to build up savings. We read the full-sample results, supported by the subsample analysis, as suggesting that the labor channel, rather than the investment channel, deserves more attention in our thinking on the mechanisms for the benefits of savings products.

There are some features of our setting and sample that are unusual and suggest that these mechanisms may not be replicated in every setting. First, many of our wage workers work as daily paid workers. They may have more flexibility to adjust hours worked than wage workers in longer-term work relationships. Daily paid wage work is not uncommon in low-income countries, but neither is it the most common form of wage work everywhere. Second, our impression from conversations with various participants in the program is that Sri Lankans are quite sophisticated financially, and quite numerate. Education levels are high, given the rural nature of the sample, averaging 10 years. Perhaps the response to these changes is more robust than it would be in populations with lower formal schooling and numeracy skills. Similarly, issues of self- and other-control may not be as salient in this context as they would be where savings options are more limited and intrahousehold power are more asymmetric.

We do not want to claim that the labor/effort channel is the only way that savings may affect outcomes related to expenditures and outcomes. But in our data, it appears to be the main channel. We find some evidence supporting a role for enabling capital investments in the last survey rounds, but only a weak association with time inconsistency – often a trigger for behavioral explanations of savings program effects. Moreover, the increase in income over the course of the study is directed towards formal savings, expenditures, and also towards informal savings. Indeed, this individualized formal financial product appears to promote participation in informal savings vehicles in a number of interesting ways. Within the sample participating in *seetus* at baseline, treatment results in a sharp increase in the number of *seetus* they use over time, and a weak increase in the amount of money they save through *seetus*. An increase in the randomized fraction of members offered the treatment appears to further increase *seetu* usage, indicating that the treatment not only enables personal participation in informal savings groups but confers a positive network externality on this participation.

Our study is subject to a number of limitations. First, while the sample size for the study is relatively large, the number of units over which the randomization was conducted is more modest, and the study suffers from some sub-group imbalance problems, particularly with respect to gender.

We have discussed the imbalance explicitly where it is visible and attempted to stay away from imbalanced subgroup analysis. Also, while many comparable papers study the extension of marketable savings products, we intentionally used a very intense impetus to savings in order to study the headwaters question. We show in a companion paper that when the deposit collection technology is modified to be more cost-effective (and hence commercially viable) this does not diminish savings rates at NSB, but the program no longer increases overall savings (de Mel et al 2013). In this sense we are studying an impetus to save that may be considered unnaturally strong, and the near-term expansion of more financially viable pro-poor savings products (such as community deposit boxes or mobile money) is unlikely to provoke as strong a response in income and overall savings as observed here.

We conclude by reflecting on the fact that a treatment offering nothing but an additional inducement to generate liquidity led to large enough increases in income that savings increased without any fall in expenditures. This suggests that financial service innovation can have a major effect on the incentives for the poor to escape poverty.

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TABLES.

Table 1. Savings Impacts

	From NSB Institutional Data			From Survey Data			
	# of Program Transactions	Program Deposits	Program Deposits - Withdrawals	Total # of Trans-actions	Bank Savings	Informal and Cash Savings	Total Savings
Panel A: Pooled							
Treated	1.728***	1,143***	424.9***	1.570***	689.6***	42.16	883.0**
	(0.08)	(138.90)	(58.17)	(0.11)	(202.40)	(303.10)	(380.70)
Panel B: By Period							
Treated months 1-4	1.744***	1,112***	660.2***	1.516***	660.6***	217.10	964.3**
	(0.07)	(138.50)	(82.51)	(0.09)	(197.70)	(346.00)	(443.40)
Treated months 5-8	1.745***	1,061***	269.6**	1.653***	616.4**	-184.20	634.80
	(0.09)	(161.80)	(108.70)	(0.13)	(241.00)	(405.20)	(500.90)
Treated months 9-13	1.686***	1,283***	279.2**	1.545***	818.5***	35.87	1,037**
	(0.11)	(231.30)	(122.50)	(0.13)	(276.20)	(394.90)	(474.10)
18 Months Post-treatment	0.422***	247.8*	-527.8**	0.320**	436.00	80.48	926.20
	(0.08)	(143.40)	(232.50)	(0.13)	(369.50)	(630.60)	(741.30)
24 Months Post-treatment	0.360***	806.7***	-386.3	0.19	10.86	-204.00	-220.40
	(0.07)	(276.40)	(269.60)	(0.14)	(293.40)	(562.60)	(681.30)
Baseline Control group mean	0.01	1.84	0.93	0.51	761.64	4,350.60	5,266.15
Observations	10,055	10,055	10,055	10,055	10,055	10,055	10,055
R-squared	0.411	0.016	0.016	0.02	0.007	0.012	0.012
Number of HHs	783	783	783	783	783	783	783

Outcomes are monthly totals at the individual level. Individual-level Fixed Effects regression with SEs clustered at both the individual and zone level; regression includes month dummies and uses data for rounds 2-20. Top 1% tail of outcome truncated.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 2. The Headwaters of Formal Savings

Panel A: Full Sample, Untrimmed Data (n=783)

	Sources of income						Uses of income			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
		Earned Income			Transfers and loans		Consumption and savings		Transfers and loans	
VARIABLES	Household Income	Earned by household	Earned by respondent	Earned by other household members	Transfers from outside household (remittance)	Other household inflows (e.g., loan receipts)	Household consumption	Total savings	Transfers to outside household	Loan payments
Treated months 1-13	2,634.9	2,372.0**	1,646.9*	725.1	103.0	158.7	272.8	865.3	-92.5	804.7**
	(1,801.7)	(1,163.0)	(932.7)	(974.2)	(386.1)	(1,334.2)	(1,319.3)	(689.9)	(137.4)	(409.9)
Observations	9,172	9,172	9,172	9,172	9,172	9,172	9,172	9,172	9,172	9,172
R-squared	0.241	0.271	0.302	0.295	0.180	0.157	0.167	0.223	0.072	0.142
Panel B: Full Sample, Data Winsorized at 1st and 99th percentiles (n=783)										
Treated months 1-13	3185.3**	1853.5**	989.0	392.3	68.6	858.2	1587.2	883.0**	3.31	362.5
	(1299.9)	(842.1)	(712.7)	(664.7)	(179.3)	(800.0)	(1069.4)	(380.7)	(52.7)	(257.9)
Observations	9,172	9,172	9,172	9,172	9,172	9,172	9,172	9,172	9172	9,172
R-squared	0.360	0.425	0.378	0.518	0.243	0.202	0.290	0.325	0.255	0.198
Panel C: Time-varying effects (Winsorized data, n=783)										
Treated months 1-4	3465.8**	2018.8**	1436.7*	49.3	93.4	1025.9	2108.1*	964.3**	-16.5	558.1*
	(1571.2)	(979.3)	(864.8)	(751.1)	(177.2)	(959.1)	(1267.8)	(443.4)	(55.8)	(306.9)
Treated months 5-8	3270.6**	1823.4*	541.9	800.6	72.2	930.2	1183.0	634.8	11.2	277.8
	(1532.6)	(1055.7)	(893.5)	(797.2)	(226.8)	(1114.8)	(1241.3)	(500.9)	(76.4)	(352.9)
Treated months 9-13	2495.1	1493.8	862.6	279.3	11.9	553.6	1290.7	1036.8**	22.8	188.5
	(1677.7)	(1221.4)	(942.0)	(958.4)	(236.3)	(950.2)	(1242.5)	(474.1)	(63.9)	(295.4)
18 months post-treatment	3038.2	1854.9	1190.0	743.4	57.2	606.4	2402.3	926.2	-131.9	735.8
	(2117.5)	(1914.2)	(1783.0)	(1312.9)	(300.5)	(1178.7)	(1522.6)	(741.3)	(98.4)	(521.1)
24 months post-treatment	82.5	-1654.3	-659.1	-808.6	-96.9	1242.4	937.4	-220.4	-43.1	86.5
	(2181.7)	(1553.5)	(1880.8)	(1552.3)	(302.9)	(1231.9)	(1472.3)	(681.3)	(68.9)	(424.6)
Observations	10,055	10,055	10,055	10,055	10,096	10,096	10,055	10,055	10,055	10,055
R-squared	0.355	0.415	0.365	0.513	0.221	0.196	0.284	0.312	0.243	0.191

Notes: Data for Rounds 2 through 18 - the full period of door-to-door deposit collection - in Panels A and B, rounds 2 through 20 in Panel C. All data from direct survey responses except for those in Column 1, which is the aggregation of Columns 2, 5, and 6, and Column 4, which is the difference between reported household income and the income the respondent reports in each of three economic activities - agriculture, wage work and self employment. All standard errors adjusted for clustering at both the individual and zone level.

Table 3: Respondent Earnings Details

Panel A Full Sample (n=783)

VARIABLES	(1) Y/N: Wage Employment?	(2) Y/N: Ag employment?	(3) Y/N: Self-employment?	(4) Wages earned	(5) Ag income earned	(6) Business income	(7) Wage hours	(8) SE hours
Treated months 1-13	0.020	-0.000	-0.034**	314.9*	-15.4	573.5	1.52**	-0.22
	(0.014)	(0.014)	(0.017)	(162.8)	(544.0)	(490.2)	(0.65)	(1.08)
Observations	9,172	9,172	9,172	9,172	9,172	9,172	9,172	9,172
R-squared	0.858	0.915	0.923	0.759	0.209	0.676	0.831	0.848
Panel B: Some SE and some manufacturing during baseline (N=259)								
Treated months 1-13	-0.019	-0.016	-0.009	-204.1	-210.6	2,808.5**	-0.04	3.05
	(0.025)	(0.024)	(0.024)	(258.9)	(1,043.2)	(1,152.1)	(1.07)	(2.15)
Observations	3,085	3,085	3,085	3,085	3,085	3,085	3,085	3,085
R-squared	0.798	0.887	0.652	0.624	0.134	0.556	0.778	0.679
Panel C: Some SE but no manufacturing during baseline (N=160)								
Treated months 1-13	0.100***	0.030	-0.078**	1,014.6***	293.9*	-1,141.3	4.40***	-3.08
	(0.029)	(0.031)	(0.037)	(391.3)	(165.6)	(1,052.0)	(1.36)	(2.73)
Observations	1,821	1,821	1,821	1,821	1,821	1,821	1,821	1,821
R-squared	0.742	0.634	0.514	0.659	0.105	0.615	0.708	0.627
Panel D: No SE during baseline (N=364)								
Treated months 1-13	0.012	-0.008	-0.026	339.7	-257.0	-228.2	1.30	-0.77
	(0.022)	(0.023)	(0.023)	(213.6)	(824.9)	(355.9)	(0.88)	(0.84)
Observations	4,266	4,266	4,266	4,266	4,266	4,266	4,266	4,266
R-squared	0.888	0.898	0.588	0.810	0.239	0.526	0.866	0.486

Notes: Data for Rounds 2 through 18. All data from direct survey responses. All standard errors adjusted for clustering at both the individual and zone level.

TABLE 4: Time-varying respondent income

Panel A Full Sample (n=783)

VARIABLES	(1) Y/N: Wage Employment?	(2) Y/N: Ag employment?	(3) Y/N: Self-employment?	(4) Wages earned	(5) Ag income earned	(6) Business income	(7) Wage hours	(8) SE hours
Treated months 1-4	0.013	-0.011	-0.034***	19.9	449.9	825.7	1.14**	-0.13
	(0.010)	(0.012)	(0.012)	(124.6)	(745.7)	(506.1)	(0.45)	(1.03)
Treated months 5-8	0.024	0.013	-0.028	446.1**	-528.6	578.8	1.44*	-0.058
	(0.018)	(0.018)	(0.020)	(204.2)	(649.3)	(565.0)	(0.78)	(1.17)
Treated months 9-13	0.027	-0.002	-0.040	583.8**	-101.5	210.3	2.21**	-0.63
	(0.024)	(0.020)	(0.026)	(274.3)	(681.0)	(646.7)	(1.08)	(1.73)
18 months post-treatment	0.031	-0.012	-0.045	884.0**	465.7	-473.4	1.72	-4.05*
	(0.027)	(0.025)	(0.034)	(403.4)	(1,300.1)	(892.8)	(1.26)	(2.09)
24 months post-treatment	0.055*	-0.008	-0.028	1,165.9**	-283.3	-1,692.9*	2.95*	-1.17
	(0.030)	(0.028)	(0.036)	(554.6)	(1,620.8)	(922.5)	(1.51)	(2.24)
Observations	10,055	10,055	10,055	10,055	10,055	10,055	10,055	10,055
R-squared	0.852	0.909	0.914	0.743	0.203	0.667	0.817	0.839

Panel B: Some SE and some manufacturing during baseline (N=259)

VARIABLES	Y/N: Wage Employment?	Y/N: Ag employment?	Y/N: Self-employment?	Wages earned	Ag income earned	Business income	Wage hours	SE hours
Treated months 1-4	0.0096	-0.042**	-0.034	-298.6	-241.4	3106.1***	0.69	1.41
	(0.018)	(0.021)	(0.021)	(242.5)	(1381.0)	(1130.1)	(0.82)	(2.30)
Treated months 5-8	-0.035	0.0053	0.010	-185.7	-266.7	2348.4*	-1.03	4.37**
	(0.032)	(0.027)	(0.028)	(316.9)	(1080.1)	(1237.6)	(1.31)	(2.10)
Treated months 9-13	-0.041	-0.0087	-0.00052	-79.9	-154.8	2867.6*	0.15	3.46
	(0.034)	(0.036)	(0.037)	(379.3)	(1424.5)	(1718.1)	(1.56)	(3.07)

18 months post-treatment	-0.047	0.0066	-0.050	309.8	-1368.8	2747.3*	-0.71	-1.01
	(0.043)	(0.048)	(0.052)	(708.7)	(2526.6)	(1612.7)	(2.17)	(3.72)
24 months post-treatment	-0.050	0.019	-0.012	56.3	1291.0	-1572.3	-1.21	0.49
	(0.043)	(0.051)	(0.056)	(872.9)	(3809.0)	(1951.0)	(2.17)	(3.77)
Observations	3,388	3,388	3,388	3,388	3,388	3,388	3,388	3,388
R-squared	0.790	0.881	0.634	0.587	0.130	0.542	0.755	0.664

Panel C: Some SE but no manufacturing during baseline (N=160)

VARIABLES	Y/N: Wage Employment?	Y/N: Ag employment?	Y/N: Self-employment?	Wages earned	Ag income earned	Business income	Wage hours	SE hours
Treated months 1-4	0.063***	0.015	-0.054*	596.7**	495.7	-660.9	2.66***	-0.88
	(0.021)	(0.022)	(0.029)	(249.6)	(316.6)	(1,334.2)	(0.94)	(2.97)
Treated months 5-8	0.120***	0.045	-0.079*	1,274.1***	129.7	-899.8	5.29***	-3.52
	(0.033)	(0.039)	(0.040)	(487.1)	(188.9)	(1,280.6)	(1.61)	(3.02)
Treated months 9-13	0.137***	0.032	-0.111*	1,382.3**	193.3	-2,130.3	6.08***	-5.89
	(0.044)	(0.044)	(0.062)	(593.0)	(136.3)	(1,404.1)	(2.14)	(4.30)
18 months post-treatment	0.147***	-0.016	-0.162**	1,785.6***	-7.8	-6,772.4***	6.11***	-17.29***
	(0.047)	(0.034)	(0.073)	(692.0)	(121.8)	(2,067.5)	(2.29)	(5.35)
24 months post-treatment	0.172***	-0.042	-0.161**	2,903.1***	720.4	-6,098.3*	8.35***	-9.79
	(0.053)	(0.041)	(0.082)	(1,000.0)	(556.3)	(3,466.7)	(2.70)	(6.50)
Observations	1,979	1,979	1,979	1,979	1,979	1,979	1,979	1,979
R-squared	0.756	0.637	0.529	0.672	0.106	0.616	0.718	0.623

Panel D: No SE during baseline (N=364)

VARIABLES	Y/N: Wage Employment?	Y/N: Ag employment?	Y/N: Self-employment?	Wages earned	Ag income earned	Business income	Wage hours	SE hours
Treated months 1-4	-0.0089	-0.0015	-0.020	-37.7	562.1	-156.4	0.70	-0.53

	(0.017)	(0.018)	(0.015)	(189.7)	(1043.5)	(210.3)	(0.71)	(0.36)
Treated months 5-8	0.022	-0.0035	-0.027	491.8*	-1152.6	-131.9	1.42	-1.01
	(0.025)	(0.031)	(0.028)	(274.2)	(1177.4)	(487.0)	(1.01)	(1.03)
Treated months 9-13	0.028	-0.022	-0.033	665.7*	-367.9	-442.8	1.99	-0.86
	(0.037)	(0.032)	(0.037)	(388.4)	(989.1)	(450.3)	(1.53)	(1.66)
18 months post-treatment	0.038	-0.036	0.0071	958.1*	1692.0	-392.5	1.76	-0.96
	(0.037)	(0.041)	(0.043)	(498.3)	(1918.5)	(1037.7)	(1.49)	(1.94)
24 months post-treatment	0.079*	-0.027	0.016	1236.1*	-2523.4	-328.5	3.53*	1.02
	(0.042)	(0.039)	(0.041)	(711.0)	(2119.5)	(651.6)	(2.05)	(1.82)
Observations	4688	4688	4688	4688	4688	4688	4688	4688
R-squared	0.88	0.89	0.60	0.80	0.24	0.50	0.85	0.49

TABLE 5: Savings Impacts by Sub-group.

Panel A Full Sample (n=783)

	Sources of income						Uses of income			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	Household Income	Household earned income	Respondent earned income	Earned income: other household members	Transfers from outside household (remittance)	Other household inflows (e.g., loan receipts)	Household consumption	Total savings	Transfers to outside household	Loan payments
treated	3185.3**	1853.5**	989.0	392.3	68.6	858.2	1587.2	883.0**	3.31	362.5
	(1299.9)	(842.1)	(712.7)	(664.7)	(179.3)	(800.0)	(1069.4)	(380.7)	(52.7)	(257.9)
Observations	9,172	9,172	9,172	9,172	9,172	9,172	9,172	9,172	9172	9,172
R-squared	0.360	0.425	0.378	0.518	0.180	0.202	0.290	0.325	0.255	0.198
Panel B: Some SE and some manufacturing during baseline (N=259)										
treated	5711.3***	4316.0***	2329.7*	1765.0	84.4	19.0	1760.7	1898.8***	33.0	796.9**
	(2139.7)	(1446.4)	(1376.5)	(1179.5)	(277.3)	(1311.3)	(1765.5)	(607.5)	(71.3)	(382.5)
Observations	3388	3388	3388	3388	3388	3388	3388	3388	3388	3388
R-squared	0.39	0.42	0.32	0.47	0.22	0.20	0.28	0.34	0.25	0.16
Panel C: Some SE but no manufacturing during baseline (N=160)										
treated	594.3	738.7	-86.2	370.6	-97.8	-111.9	852.9	-159.6	-223.7**	370.4
	(2652.4)	(1563.6)	(1132.9)	(1268.1)	(299.6)	(1825.6)	(1947.8)	(716.7)	(88.3)	(506.7)
Observations	1979	1979	1979	1979	1979	1979	1979	1979	1979	1979
R-squared	0.39	0.57	0.57	0.63	0.18	0.16	0.32	0.35	0.27	0.24
Panel D: No SE during baseline (N=364)										
treated	1984.1	12.8	39.2	-497.9	102.7	2019.7*	1767.5	445.9	63.6	22.5
	(1748.2)	(1236.7)	(897.6)	(1044.0)	(297.7)	(1059.2)	(1436.4)	(615.4)	(80.5)	(341.8)
Observations	4688	4688	4688	4688	4688	4688	4688	4688	4688	4688
R-squared	0.32	0.35	0.31	0.48	0.23	0.21	0.26	0.28	0.23	0.20

Notes: Data for Rounds 2 through 18. All data from direct survey responses. All standard errors adjusted for clustering at both the individual and zone level.

Table 6. Consumption Impacts

VARIABLES	Total Household Income	Wage Employment	Self Employed	Business Income	Consumption	Consumption of 'Sin' Goods	Survey Experiment 'Sin' Good Consumption	Bank Savings	Non-Bank Savings	Total Savings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Time Inconsistent * Treated	1454.6	0.033	-0.016	49.7	-615.7	54.8	-0.010	394.0	306.8	666.4
	(2122.8)	(0.020)	(0.025)	(583.0)	(1400.8)	(46.1)	(0.065)	(270.7)	(454.0)	(594.2)
Treated (not Time Inconsistent)	2592.1*	0.0069	-0.027	433.5	1144.3	-18.0	-0.0028	528.9**	-83.0	611.3
	(1386.9)	(0.017)	(0.017)	(481.6)	(910.2)	(36.4)	(0.048)	(246.4)	(358.7)	(454.8)
Observations	9172	9172	9172	9172	9172	9172	1986	9172	9172	9172
R-squared	0.28	0.20	0.29	0.53	0.44	0.58	0.30	0.25	0.32	0.33

Outcomes are monthly totals at the individual level unless otherwise indicated. Regressions include individual-level fixed effects and SEs are clustered at both the individual and zone level, regression includes month dummies and uses data for rounds 2-20. Top 1% tail of outcome truncated.

*** p<0.01, ** p<0.05, * p<0.1

Table 7. Heterogeneity by Respondent Income Share.

VARIABLES	Total Household Income	Individual Earned Income	Household Consumption	Transfers to Spouse	Transfers from Spouse	Net Transfers	Bank Savings	Non-Bank Savings	Total Savings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated (Impact in Lowest Tercile)	4,375**	3,249***	1609	-190.8	229.1	687	655.6**	209.7	1,050*
	(2112.0)	(820.4)	(1577.0)	(296.3)	(606.5)	(1328.0)	(303.2)	(457.6)	(586.7)
Treated * Middle 2 of Income Share	-3739	-3,455***	-2570	-6.166	117	-541.2	-23.79	-419.1	-569.1
	(2296.0)	(1013.0)	(1778.0)	(394.8)	(608.9)	(1450.0)	(363.0)	(568.2)	(775.8)
Treated * Highest 3 of Income Share	-399.4	-3,358***	8.9	790.3*	238.2	-384.2	125.4	-113.8	36.35
	(2093.0)	(1095.0)	(1596.0)	(458.4)	(626.8)	(1590.0)	(385.4)	(528.1)	(766.2)
Observations	9172	9172	9172	9172	9172	9172	9172	9172	9172
R-squared	0.702	0.558	0.644	0.443	0.581	0.289	0.305	0.521	0.523

Outcomes are monthly totals at the individual level unless otherwise indicated. Regressions include individual-level fixed effects and SEs are clustered at both the individual and zone level, regression includes month dummies and uses data for rounds 2-20. Top 1% tail of outcome truncated.

*** p<0.01, ** p<0.05, * p<0.1

Table 8. The Randomized Saturation Experiment within ROSCAs.

	Number of Seetus currently Participating in		Monthly amount Saved through Seetus		Monthly Transfers FROM Outside the Household		Monthly Transfers TO Outside the Household		Total Monthly Savings	
Treated	0.293**	0.182	286.9	254.7	912.1	845.6	-25.34	-32.5	945.8	844.7
	(0.14)	(0.12)	(185.4)	(178.9)	(958.2)	(1049.0)	(105.2)	(116.1)	(597.8)	(635.3)
Seetu Treatment Saturation		1.435***		415		858		92		1305
		(0.53)		(586.3)		(3232.0)		(326.3)		(2159.0)
Constant	1.263***	1.273***	2,167***	2,170***	1,266*	1,272*	445.5***	446.1***	2,818***	2,827***
	(0.08)	(0.08)	(146.40)	(147.60)	(737.20)	(744.30)	(77.37)	(77.23)	(441.50)	(443.10)
Observations	3,255	3,255	3,255	3,255	3,255	3,255	3,255	3,255	3,255	3,255
R-squared	0.032	0.043	0.03	0.03	0.012	0.012	0.067	0.067	0.026	0.026
Number of HHs	278	278	278	278	278	278	278	278	278	278

Outcomes are monthly averages at the individual level unless otherwise indicated. Regression with fixed effects at the individual and survey wave level with standard errors clustered at both the individual and the zone level. Table includes data from round 1-18 on households that were members of ROSCAs within with the saturation experiment was conducted (no more than 24 ROSCA members all of whom lived in the same zone); treatment of non-study ROSCA members began in round 6.

* significant at 10%; ** significant at 5%; *** significant at 1%

FIGURES:
Figure 1.

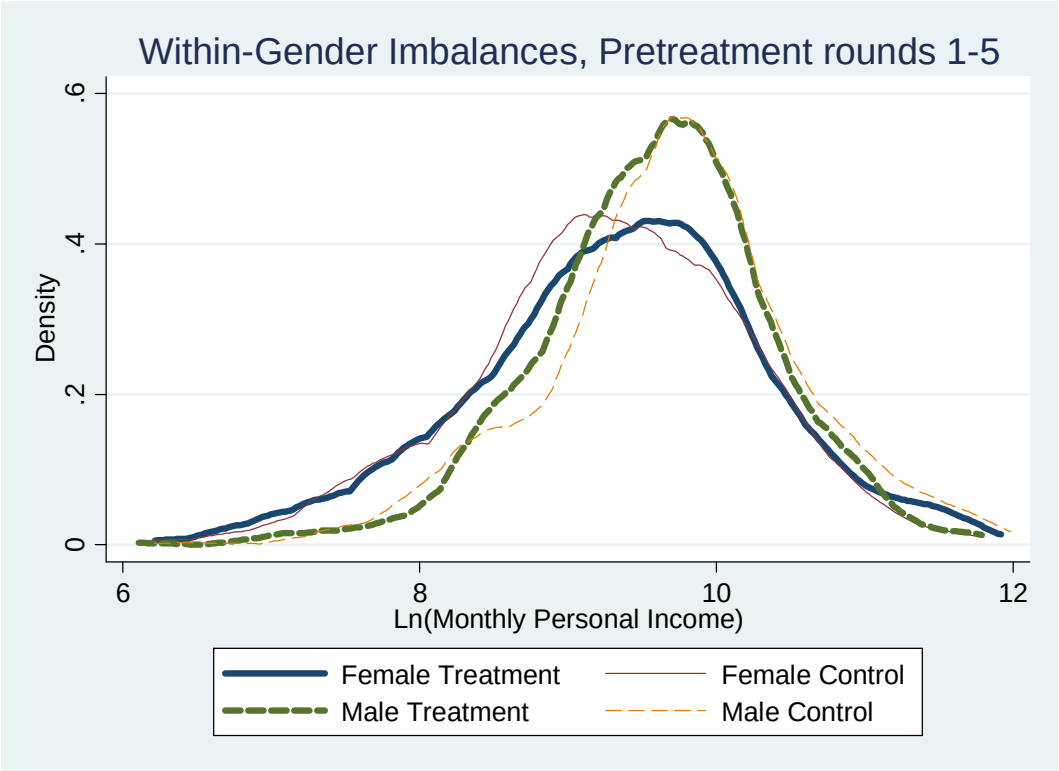

Figure 2.

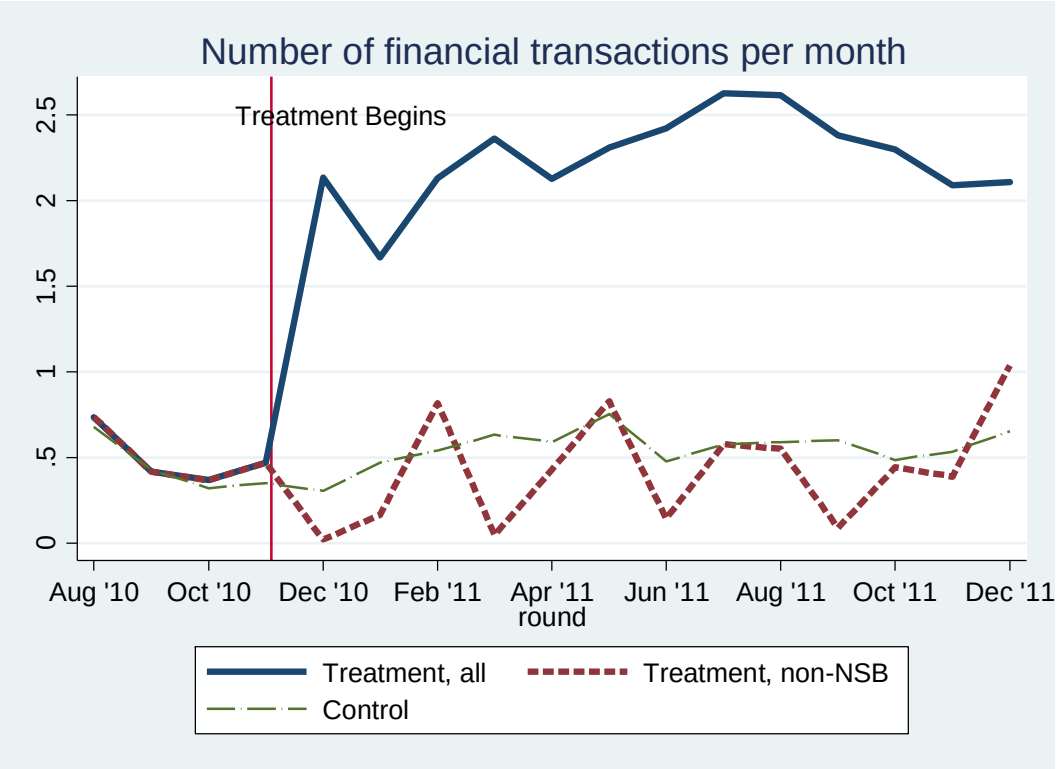
Number of financial transactions per month
Treatment Begins
0
.5
1
1.5
2
2.5
Aug '10
Oct '10
Dec '10
Feb '11
Apr '11
Jun '11
Aug '11
Oct '11
Dec '11
round
Treatment, all
Treatment, non-NSB
Control

Figure 3.

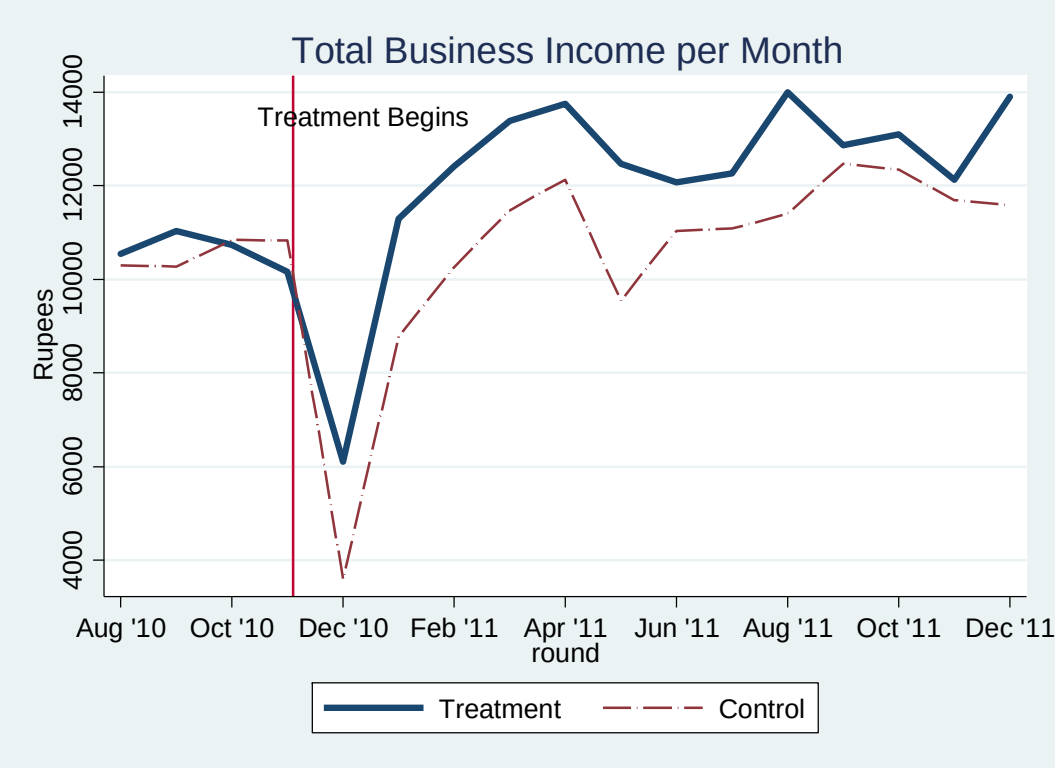
Total Business Income per Month
Treatment Begins
Rupees
4000
6000
8000
10000
12000
14000
Aug '10
Oct '10
Dec '10
Feb '11
Apr '11
Jun '11
Aug '11
Oct '11
Dec '11
round
Treatment
Control

Table A1. Survey Design.

Round	Treatment Month	Month	Survey Waves	Pure Control 52 zones, 256 hhs	Control to Weekly Box 13 zones, 89 hhs	Control to Biweekly Box 13 zones, 61 hhs	Weekly Home visits 40 zones, 197 hhs	Weekly to Biweekly Home visits 19 zones, 85 hhs	Weekly Home to Weekly Box 19 zones, 107 hhs
1		August, 2010	Baseline Survey						
2		September, 2010	Full Wave						
3		October, 2010	Full Wave						
4		November, 2010	Full Wave						
5		December, 2010	Monthly Only						
6	1	January, 2011	Monthly Only						
7	2	February, 2011	Full Wave						
8	3	March, 2011	Monthly Only						
9	4	April, 2011	Monthly Only						
10	5	May, 2011	Full Wave						
11	6	June, 2011	Monthly Only						
12	7	July, 2011	Monthly Only						
13	8	August, 2011	Full Wave						
14	9	September, 2011	Monthly Only						
15	10	October, 2011	Monthly Only						
16	11	November, 2011	Full Wave						
17	12	December, 2011	Full Wave						
18	13	January, 2012	Full Wave						
19		July, 2012	Full Wave						
20		January, 2013	Full Wave						

Yellow boxes indicate survey waves used in the study of Weekly Home Visits

Blue boxes indicate Treatment with Weekly Home visits, as well as inclusion in the study of Weekly Home Visits

Green boxes indicates samples for which surveys are not included in the sample because of a substantial change in the treatment protocol

Table A2. Determinants of Attrition.

	Attrited from Panel analysis		Attrited from Survey by Round 18	
Baseline Characteristics:				
Treated	-0.0094	-0.010	-0.019	-0.018
	(0.014)	(0.015)	(0.014)	(0.014)
Female		-0.0095		0.0055
		(0.0091)		(0.014)
Years of Education		0.0012		0.0021
		(0.0018)		(0.0019)
Score on digitspan test		0.00077		0.0049
		(0.0044)		(0.0048)
Beta (Time inconsistency parameter)		-0.013		-0.0099
		(0.024)		(0.044)
Delta (Discounting parameter)		-0.0055		-0.19**
		(0.033)		(0.080)
Self Employed		-0.017*		-0.0056
		(0.0094)		(0.0093)
Self Employed in Agriculture		-0.020		-0.015
		(0.015)		(0.014)
Household Consumption (000,000 Rs.)		0.0085		-0.0100
		(0.016)		(0.021)
# of Seetus participating in		-0.0052*		-0.0066
		(0.0031)		(0.0045)
Bank savings (000,000 Rs.)		-0.051		-0.036
		(0.035)		(0.035)
Constant	0.020	0.054	0.034***	0.21**
	(0.013)	(0.057)	(0.012)	(0.10)
Observations	795	795	795	795
R-squared	0.0015	0.013	0.0037	0.027
Mean of Dep Var	0.015		0.025	

Regression uses pretreatment data (rounds 1-5) to explain subsequent attrition from the household survey.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table A3. Balance.

Variable:	Mean in control	Mean in treatment	Treatment-control differential
Number of transactions with a formal financial institution during previous month	0.45	0.50	0.044
Female	0.52	0.54	0.016
Discount Factor (delta)	0.92	0.93	0.0090
Time Consistency Factor (beta)	0.97	0.99	0.019
Total number of seetus in which respondent participated in previous month	1.23	1.17	-0.067
Total payouts from seetus in previous month	1292.7	1186.7	-105.4
Respondent reports being self-employed in non-ag enterprise during previous month	0.51	0.53	0.019
Total asset purchases over previous month	1034.8	2589.0	1536.3
Total value of business inventory	37264.3	49070.3	11800.0
Total business expenses over previous month	22065.0	23800.8	1702.5
Total business sales over previous month	33468.3	34374.4	972.6
Total business income over previous month	11009.9	10974.9	7.61
Number of transactions with a financial institution other than NSB over previous month	0.45	0.50	0.044
Total amount saved through ROSCAs over previous month	1587.2	1435.7	-152.3
Total amount withdrawn from ROSCAs over previous month	1471.4	1383.9	-90.1
Total amount put into informal savings over previous month	3318.9	2814.8	-514.2
Total amount withdrawn from informal savings over previous month	15463.8	14655.9	-830.3
Total change in household cash balance over previous month	1334.6	540.9	-802.7
Total amount received in loans over previous month	4154.3	4131.6	-37.3
Aggregate income over previous month	23906.0	22642.4	-1250.9
Aggregate consumption over previous month	19403.5	18327.7	-1074.8
Earned household income over previous month	23313.4	21510.7	-1786.6
Total household income over previous month	30626.7	28754.3	-1836.2
Personal income taken from informal savings over previous month	14039.9	12269.7	-1734.5
Transfer from spouse over previous month	4149.1	4435.7	281.3
Transfer from outside the household over previous month	3902.1	3853.0	-50.2
Total personal income over previous month	22091.0	20558.3	-1503.5
Transfers to spouse over previous month	2116.9	2443.2	322.8
Transfers to children over previous month	349.3	293.0	-57.8
Transfers to anyone over previous month	3486.7	3620.3	118.5
Monthly savings into banks	738.1	896.9	184.2
Month savings in cash	3187.2	2359.7	-807.2*
Monthly savings through ROSCAs	1552.8	1532.9	-18.9
Monthly savings through other means	562.6	655.9	83.7
Total Monthly savings over previous month	6040.6	5445.3	-558.3

NOTES: Regressions include the 2815 observations from the pretreatment rounds 2-5 (treatment began in round 6). Fixed effects for month are included, and standard errors are clustered at the zone level to reflect the design effect.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table A4. Determinants of Uptake and Product Usage

Baseline Characteristics:	Made Any Deposit	Made Any Withdrawal	Total Number of Transactions	Log of Monthly Deposits (if any)	Log of Monthly Withdrawals (if any)	Log of Final Balance (if any)
Female	0.058*	-0.054	1.56	-0.18	-0.096	0.29
	(0.032)	(0.059)	(1.62)	(0.21)	(0.26)	(0.20)
Years of Education	-0.00095	0.011	0.15	0.054*	0.084**	0.0019
	(0.0049)	(0.0081)	(0.19)	(0.029)	(0.041)	(0.031)
Self Employed	0.060	-0.021	1.64	0.30	0.15	0.50**
	(0.039)	(0.061)	(1.72)	(0.24)	(0.35)	(0.24)
Employed in Agriculture	0.0070	-0.036	-0.35	-0.063	0.040	0.10
	(0.043)	(0.062)	(1.67)	(0.25)	(0.35)	(0.22)
Household Expenditure (000,000 Rs.)	-0.75***	-0.67**	-15.9***	0.47	18.9**	-2.88**
	(0.17)	(0.26)	(4.80)	(2.24)	(8.95)	(1.40)
Beta (Time inconsistency parameter)	0.11	0.050	0.58	-0.16	-0.18	0.20
	(0.12)	(0.22)	(5.13)	(0.77)	(1.19)	(0.77)
Delta (Discounting parameter)	-0.024	-0.22	2.83	-0.046	0.56	-0.31
	(0.19)	(0.32)	(6.94)	(1.08)	(1.60)	(1.29)
Score on digitspan test	-0.030**	0.015	0.13	0.023	-0.050	-0.084
	(0.014)	(0.020)	(0.58)	(0.083)	(0.100)	(0.078)
Participates in Seetu	-0.036	-0.013	-2.51	-0.28	0.23	-0.69***
	(0.034)	(0.059)	(1.59)	(0.21)	(0.27)	(0.20)
Has a formal account	0.087**	0.024	1.19	0.17	-0.035	0.41*
	(0.039)	(0.057)	(1.56)	(0.22)	(0.24)	(0.21)
Formal Savings Balance (000,000 Rs.)	-0.067	-0.44	2.61	5.90	11.0	4.89
	(0.59)	(0.94)	(22.3)	(4.94)	(6.96)	(4.50)
Observations	389	389	389	347	150	331
Mean of Dep Var in sample	0.89	0.39	13.7	8.02	9.04	7.31
R-squared	0.065	0.023	0.022	0.053	0.13	0.090

Outcomes are monthly totals at the individual level. Regression is run at the individual level, using pretreatment averages of
* significant at 10%; ** significant at 5%; *** significant at 1%

Table A5. Detailed Analysis of Headwaters.

		Regression Coefficients		Liquidity Balance (positive number indicates incoming liquidity for core respondent)			
		Full year of study:			By Period:		
Category:	**Variable:**	**Coeff**	**SE**	**Full Year**	**Months 1-4**	**Months 5-8**	**Months 9-13**
Informal Savings:	Saved through ROSCAs	80.2	(90.9)				
	Saved in gold	-81.5	(83.3)				
	Saved in durables	193.2	(258.5)				
	Saved in land	303.0	(186.4)				
	Informal loans repaid	-53.5	(222.6)				
	Saved through other means	155.8	(111.8)				
	Total informal savings	**651.2**	**(449.8)**	-651.2	-523.4	-940.8	-462.4
Informal Withdrawals:	Withdrawn from ROSCAs	-241.8	(424.1)				
	Withdrawn from gold	97.0	(242.5)				
	Withdrawn from durables	8.49	(34.0)				
	Withdrawn from land	26.4	(49.1)				
	Withdrawn informal loans	84.6	(87.2)				
	Withdrawn from other	-23.1	(35.8)				
	Total informal withdrawals	**-48.4**	**(475.6)**	-48.4	513.8	-163	-348.8
Loans Received:	Government loans received	58.2	(238.2)				
	MFI loans received	-26.2	(29.1)				
	Bank loans received	25.5	(86.0)				
	Total loans received	**-13.6**	**(433.8)**	**-13.6**	**-183.3**	**175.5**	**-37.5**
Loans Paid Back:	Total loan repayment	73.6	(207.1)	-73.6	-229.2	-56.9	137.2
Individual Consumption:	Spending on tobacco	-18.3	(14.2)				
	Spending on alcohol	-6.51	(12.4)				
	Spending on parties	-0.57	(12.6)				
	Spending on gambling	-0.32	(7.39)				
	Total individual consumption	**867.8**	**(698.4)**	-867.8	-1095.6	-826.1	-514.6
Transfers:	Transfers to others (spouse, children, other)	106.0	(248.7)	-106	-86.2	-126.8	-113.6
	Transfers from spouse	366.2	(311.5)	366.2	447.0	465.9	98.7
	Transfers from outside HH	280.4	(521.7)	280.4	180.7	307.7	371.7
	Informal loans given	-51.2	(62.8)	51.2	95.4	38.1	5.85
	Gifts Given	4.26	(38.8)	-4.26	-3.87	9.92	-23.8
	Informal loans received	-157.3	(178.6)	-157.3	-331.6	-54.7	-60.9
	Gifts Received	24.1	(24.2)	24.1	11.8	21.7	42.3
	Total cash balance sum of headwaters coefficients:			**-1,700.06**	**-1,329.07**	**-2,065.58**	**-1,320.15**
	Number of observations: 9,168						
	Number of households: 782						

Table A6. Randomized Saturation Balance Tests.

	Number of Seetus currently Participating in		Monthly amount Saved through Seetus		Monthly Transfers FROM Outside the Household		Monthly Transfers TO Outside the Household		Total Monthly Savings	
Treated	-0.0653	0.122	35.86	361.2	161.8	94	-14.65	4.964	85.46	984.9
	(0.19)	(0.27)	(323.9)	(441.1)	(828.7)	(960.3)	(96.4)	(104.1)	(673.0)	(806.6)
Seetu Treatment Saturation		-0.93		-1,619		337.40		-97.60		-4,476*
		(0.83)		(1358.0)		(2706.0)		(560.4)		(2422.0)
Constant	2.066***	2.065***	2,244***	2,244***	4,432***	4,432***	502.6***	502.6***	5,464***	5,462***
	(0.11)	(0.11)	(213.30)	(213.40)	(751.70)	(752.10)	(77.98)	(78.01)	(502.40)	(503.00)
Observations	996	996	996	996	996	996	996	996	996	996
R-squared	0.001	0.006	0.002	0.008	0.004	0.004	0.003	0.003	0.005	0.008

OLS with round fixed effects and standard errors clustered at both the individual and the zone level. Table includes data from round 1-5 on households that were members of ROSCAs within with the saturation experiment was conducted (no more than 24 ROSCA members all of whom lived in the same zone).

* significant at 10%; ** significant at 5%; *** significant at 1%

APPENDIX B. Cash Flow Survey Module.

SECTION 11: HOUSEHOLD EXPENDITURE

Interviewer: Ask this Section from all respondents.

11.1. In the past month of December 2011 , how much in total did your household (including yourself) spend on each of the following?
Interviewer: Include only the household expenses. Business expenses are not included)

	Expenditure Category	Expense (Rs)
1.	Food consumed at home	
2.	Food consumed outside the home (eg. restaurants, tea-shops, bought on street)	
3.	Non-durable household goods (eg. personal care products, soaps)	
4.	Recreation and entertainment (eg. movies, CD/VCD/DVD, trips)	
5.	Housing (rent, taxes, maintenance)	
6.	Fuel, water and light (eg. electricity, gas, firewood, kerosene, candles, matches)	
7.	Telephone (including land line and mobile)	
8.	Other household services (laundry, grinding, domestic servants)	
9.	Transport (including fuel for car/motor cycle, bus fares)	
10.	Schooling (include fees, books and other materials, uniforms, etc.)	
11.	Health expenditures (medicine, doctor/consultant fees)	
12.	Clothing (including clothes, footwear)	
13.	Jewelry and watches (eg. beads, bangles, bracelets and necklaces)	
14.	Household furnishings (eg. furniture, curtains)	
15.	Electronic goods and household appliances	
16.	Repairs to house and land (including garden related)	
17.	Travel to visit friends or family	
18	Expenses on cigarette, beedi	
19.	Consumption of alcoholic drinks (eg. liquor, beer)	
20.	Hair Cuts / Hair dressing	
21.	Religious activities	
22.	Horse race betting	
23.	Ceremonies related to family members and relatives (eg. weddings, funerals)	
24.	Social activities	
25.	Repayment of loans/interests	
26.	Consumption of betel	

27. Card playing for money	
28. Expenses on lotteries	
29. Expenses on books, newspapers and magazines	
30. Other expenses (Specify)……..	
31. Total expenses of last month	

Can you tell me about the total income earned by your household including yourself in the past month of December 2011?

11.2. Can you please tell me how much money you and your family members received from each of the following sources in the previous month (December 2011)?

Source	Income (Rs)
1. In the past month of December 2011, how much income did your house hold earn from economic activity? (eg. wages from work, earnings from business, income earned from sales of agricultural crops)	
2. In the past month of December 2011, how much money did you receive as remittances from your relatives living in other areas of Sri Lanka (eg. children/siblings) and from your friends?	
3. In the past month of December 2011, how much money did you receive as remittances from your relatives (eg. children/siblings) and from your friends residing in abroad countries	
4. In the past month of December 2011, how much money did you and your house hold members receive from other sources (eg. government payment schemes such as Samurdhi, seetus/loans, etc.)	
5. Total	

Now, I want to ask you about income earned/money received and expenditure incurred by you personally in the past month of December 2011. Do not consider the income earned or expenses made by other members of the family

11.3. Can you tell me about income you received during the month of December 2011 from each of the following sources?

Source	Amount (Rs)
1a. In the past month of December 2011, how much income did you earn from economic activity? (eg. wages from work, earnings from your business)	
1b. In the past month (December 2011) how much of your institutional savings did you withdraw in the form of money?(eg: bank accounts)	

1c. During the past month (December 2011) how much did you withdraw from your own /house hold non institutional savings? (eg: tills, money balances maintained to face emergencies)	
2. In the past month of December r 2011, how much money did you receive from your spouse or other household members? (eg. money given by HH members to purchase things for the household, loans given by HH members, other remittances from foreign countries)	
3. In the past month of December 2011, how much money did you receive from persons living outside the household or institutions? (eg. gifts or loans given by family members outside the home, money received from seetus, other transfers etc)	
4. Total	

11.3a This means that you received a total amount of Rs. **(mention total amount in 11.3.4)** during the past month of December 2011. Does this sound correct?

1. Yes **Go to Qn. 11.4**
2. No **Go to Qn. 11.3b**

11.3b If not, then what might be the correct amount of money received after including any other sources as well?

Rs _____

Now I would like to ask you about money given to other people in the past month of December 2011

11.4. Can you tell me how much money you gave to each of the following in the past month of November 2011?

Recipient	Amount (Rs)
1a. Money given to spouse for expenses	
1b. Money given to spouse for savings	
2a. Money given to children for expenses (for transport to school, food etc)	
2b. Money given to children for savings or money directly deposited in children's savings accounts	
3. Money given to other members of the household	
4. Money given to members outside the household (e.g. as gifts etc)	
5. Other (specify) _____	
6. Total	

11.4a. This means that you gave a total amount of Rs **(mention total amount in 11.4.6)** during the past month of December 2011. Does this sound correct?

1. Yes **Go to Qn. 11.5**
2. No **Go to Qn. 11.4b**

11.4b If not, then what might be the correct amount of money given after including any other recipients as well?

Rs _____

11.5 **Interviewer: Subtract the total in 11.4.6 (if incorrect, then 11.4b) from the total in 11.3.4 (if incorrect, 11.3b) and write the answer here.**

Rs _____

11.6 Then, after deducting the total amount of money given to others from the total amount of money received, you had Rs **(mention total amount in 11.5)** remaining for saving or spending in the past month of December 2011. Can you tell me how much money you saved using the following methods?

Saving Method	Amount (Rs)
1a. Money saved in National Savings Bank (NSB)	
1b. Money saved in bank accounts in your name of banks other than NSB	
2. Retained as money	
3. Invested in a Seetu	
4. Savings in another form (specify) _____	
5. Total	

11.7. **Interviewer: Subtract the total in 11.6.5 from 11.5 and write the answer here.** Rs _____

11.8 Then, after deducting the total amount of money saved by you, you personally had Rs ***(mention total amount in 11.7)*** remaining for spending in the past month of December 2011. Can you tell me how much money you personally spent on each of the following?
Interviewer: Include only the expenses the respondent personally spent. Do not include day-to-day business expenses

	Expenditure Category	Expense (Rs)
1.	Food consumed at home	
2.	Light food items consumed outside the home (e.g. short eats, snacks, tea etc.)	
3.	Other food consumed outside the home (at restaurants, hotels etc)	
4.	Non-alcoholic beverages consumed outside the home (e.g. soft drinks such as sprite, coca cola etc)	
5.	Bus fares	

6.	Three wheelers / taxi fares	
7.	Clothing (including clothes, footwear)	
8.	Jewelry and watches (e.g. beads, bangles, bracelets and necklaces)	
9.	Telephone (including land line and mobile)	
10.	Cigarettes and Tobacco (including Beedi)	
11.	Beer and other alcoholic beverages	
12.	Hair Cuts / Hair dressing	
13.	Personal care products (e.g. perfumes, creams, shampoo etc.)	
14.	Religious activities (e.g. visits to temple)	
15.	Parties and other celebrations	
16.	Betting at Horse Races / Turf Accountants	
17.	Travel to visit friends and family	
18.	Expenditure on weddings / funerals and other such family events	
19.	Social activities / festivals / ceremonies	
20.	Housing (rent, taxes, maintenance)	
21.	Fuel, water and light (eg. electricity, gas, firewood, kerosene, candles, matches)	
22.	Other household services (laundry, grinding, domestic servants)	
23.	Schooling (include fees, books and other materials, uniforms, etc.)	
24.	Health expenditures (medicine, doctor/consultant fees)	
25.	Household furnishings (eg. furniture, curtains)	
26.	Electronic goods and household appliances	
27.	Repairs to house and land (including garden related	
28.	Repayment of loans/interests	
29.	Consumption of betel	
30.	Card playing for money	
31.	Expenses on lotteries	
32.	Expenses on books, newspapers and magazines	
33.	Other (Specify)_____	
34.	Total expenses of last months	