Public vs. Private Mental Accounts:

Experimental Evidence from Savings Groups in Colombia*

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Abstract

I study whether modifications to the framing of a *commitment savings product* affects savings accumulations and other poverty-linked outcomes for low-income individuals in newly-formed Village Savings and Loan Associations (VSLAs) in Colombia. The experiment tests whether behavioral responses vary depending on whether subjects are led to label and create 'mental savings accounts' in private versus public ways. Individuals in the *private labeling* treatment stated accumulation targets and earmarked savings for a particular purpose, but this was shared only privately with a member of the research team. Individuals in the *public labeling* treatment received the same intervention but publicly revealed and announced their goals to other members of their savings group. The average treatment effect of the *public-labeling* intervention are very strong and significant. Savings accumulations increased by an average of 35% and savings goals were 8.5% more likely to be reached in comparison to those untreated. Further explorations strongly suggest evidence of differentiated behavioral responses of individuals in the *private-labeling* treatment group: private commitment to a savings goal is more effective for individuals who, after random assignment but prior to the intervention, were less constrained by extant economic circumstances and institutional barriers. The analysis and interpretation of results was enriched by mixed methods for data collection: households' survey data, administrative records and qualitative data from focus groups discussions.

Key words: Behavioral economics, microfinance, randomized controlled trial, savings, mental accounting, labeling, self-control.

JEL Classification: C93, D03, D14, D91, O16

I. INTRODUCTION

Asset accumulation provides poor households an improved ability to generate income and more effectively fight poverty in the future and smooth consumption and investment plans in the face of adverse shocks.[1] Poor households save small amounts of cash flows via informal providers such as ROSCAs and deposit collectors, or save by holding risky assets (livestock, stored grain, durable goods).[2] In all forms of microfinance, high costs of monitoring and transaction relative to the size of the financial amounts involved have often worked to reduce both the supply and the demand for formal financial services or made access costly for clients.[3] For this reason, innovations to bring down costs and improve the terms and usefulness of services offered to the poor are key to expanding service. I design and evaluate two modifications to a well-established methodology of self-help groups in Colombia called Village Savings and Loan Associations (VSLA) in order to understand if private or public commitment through the creation and salience of 'mental savings accounts', affects savings behavior. The results show that public commitment is very effective in increasing savings and private commitment has heterogeneous behavioral responses of treatment effects.

Recent evidence suggests that self-control problems and time-inconsistent preferences prevent individuals from making optimal decisions in everyday scenarios such as waking up early, starting a diet, doing homework, or even saving.[4] As a result, individuals often demand and rely upon commitment mechanisms to mitigate these problems. According to Bryan at el (2010), commitment mechanisms provide individuals with tools to help them stick to a plan that might otherwise be repeatedly postponed because of a disparity between their long- and short-run intentions. Frequently, individuals' preferences for future choices are valued disproportionately lower over current ones.[5,6] As a result, individuals end up anxious to catch the last train and a lecture from the boss, or having to bear that back pain that they have not managed to escape from. These situations are common when making financial choices. Consequently, savings rates and assets accumulation are often low, and this is not just the result of lack of access to formal financial services. A commitment mechanism is something that helps us promise our current selves to behave according to our stated or presumed-known future best interests. In the context of under-savings, a commitment device is an arrangement used by individuals to incentivize higher savings or penalize failure to making deposits. These rewards or penalties could be economic (*hard commitment*) or psychological (*soft commitment*). In this study

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[1] Karlan and Morduch (2009), and Burgees et al (2005).
[2] Duflo and Banerjee (2007), Dupas and Robinson (2010), Collins et al (2009), Karlan and Murdoch (2009).
[3] Karlan and Morduch, 2009; and Dupas and Robinson (2010).
[4] Laibson (1997), Angeletos et al (2001), Shefrin and Thaler (1981).
[5] Angeletos et al (2001), Bryan et al (2010).
[6] Classic examples being situations such as clicking the snooze button of the alarm clock or delaying one's workout by "just one more day".

I use a soft commitment device to evaluate how individuals respond to self-control problems associated to savings decisions.

This study describes the design and implementation of a Randomized Controlled Trial to evaluate if relatively simple modifications to how an existing savings product was framed and labeled creates a commitment mechanism and how it affects savings accumulations and other outcomes of low-income individuals in newly formed Village Savings and Loan Associations (VSLAs) in Colombia.[7] Under the existing VSLA methodology, individuals are encouraged to save but they make no explicit statement of a commitment to reach particular savings goals. The designed experiment explores how *private-* and *public-labeling* alternatives in the ways in which individuals are asked to declare their savings as earmarked for a particular purpose might affect program outcomes hypothesizing that this might work via differences in how mental accounts are created and labeled. Individuals in the *private-labeling* treatment create and label a 'mental savings account' and state a savings goal privately. In the *public-labeling* treatment, label and state savings goals individually but were then asked to publicly reveal and announce their chosen goals to other members of the savings group.[8] In this way individuals in both treatment groups are able to label their 'mental savings account' and create private or public commitments to reaching individual savings goals.

Behavioral economics has been increasingly accepted to be able to make predictions of field phenomena.[9] An important result in behavioral economics is that mental accounting is a commitment mechanism that individuals use in inter-temporal decision-making in order to constrain their own behavior.[10] Mental accounting was originally defined by Thaler (1985) to be the process of mentally coding and categorizing transactions that individuals create to mentally separate the money available to make plans and keep track of their spending. Individuals assign their available income to different expenditure accounts and put labels such as rent, pension, entertainment, etc. This violates the classical principle of fungibility of money in which money should not have labels attached[11] and therefore, individuals should be able to transfer money from one account to other accounts without any (implicit or explicit) costs.

Individuals often rely upon mental accounts as a commitment device to mitigate self-control problems associated with inter-temporal choices.[12] For this reason, by implicitly or explicitly categorizing mental accounts, individuals impose constraints to their behavior and are often better able to achieve initially chosen

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[7] In Colombia, more than 4,500 VSLAs have been formed with over 70,000 beneficiaries up to date. The program targets more than 5 million poor and extremely poor households registered at Red Unidos, the largest anti-poverty intervention in the country. The global outreach of the VSLA clients is of over 8.7 million in the five continents (Source: VSL Associates).
[8] I will use the words VSLA or savings groups interchangeably.
[9] Camerer et al (2004).
[10] Thaler (1985).
[11] Thaler (1985 & 1999), Hastigs and Shapiro (2013).
[12] Bryan et al (2010), Shefrin and Thaler (2004) and Kast and Pomeranz (2009).

savings goals and use financial services more effectively to raise incomes and welfare.[13] As a result, individuals may save more when they save for a declared purpose. Relatively little evidence has been collected from field experiments to indicate how much practical and policy importance such strategies might have on individual behavior.

Thaler (1999) argues that how mental accounts are framed, labeled and evaluated are key components in the decision-making process. If fungibility is violated, the way in which savings choices are framed can have significant impacts on actual savings outcomes. This finding provides a framework for thinking about how individuals evaluate (open and close), frame and label mental accounts in a way to maximize their utility when making financial choices. For this reason, studying these elements further may help us understand better how the process in which mental accounts, as a commitment device, are created actually matter for savings decisions. The contribution of this study is to investigate if opening mental accounts publicly, instead of privately, increases savings through additional constraints imposed to the behavior of individuals as a result of the 'public' nature of commitment. A recent field experiment by Kast et al (2012) shows that commitment is effective at increasing savings. It uses peers as a commitment device, while this study uses mental accounting as a commitment device to constrain savings behavior and investigates if such accounts could be artificially created by labeling their 'savings' account privately or in the presence of their peers.

The RCT that I run in this study randomly assigned 137 newly formed VSLAs, mainly in rural areas from nine municipalities of Colombia into two treatments and a control group. Individuals in the control group were exposed to the standard VSLA model.[14] In the *private labeling* treatment, members received an additional module with a short organized guided conversation aimed at discussing and highlighting the difficulties of committing to a savings path and the potential role and use of mental accounts in strengthening those commitments. I asked participants to voluntarily state in writing a savings purpose and weekly savings goals. This was intended to help guide individuals to form and label their mental 'savings' account to privately commit to achieving that savings purpose. Goals were not stated publicly. The *public labeling* treatment was similar except that the group discussion encouraged members to make commitments to themselves as well as to others in their group in order to explore the possibility that this might lead to different outcomes. As in the *private labeling* treatment members were asked to voluntarily state a savings purpose and weekly savings goals in writing, but in this intervention, those commitments were shared with all members of the group. By doing this, individuals explicitly label their 'mental savings account' and publicly

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[13] Thaler (1985).

[14] Members of the VSLA meet every two weeks to make contributions to a self-managed and self-capitalized savings fund by purchasing shares of the fund. In addition to savings individuals are able to take small loans on terms set by the group at interest rates that are typically much lower than available from other sources. The duration of the savings cycle is from 8 to 9 months at the end of which the funds are distributed according each individual's accumulated shares.

commit to their own decision. Prior to the public announcement, individuals were not informed about the (public) nature of the treatment or their commitment.[15] This module included a trust building game and a guided conversation aimed at highlighting the achievements of group commitments.[16]

With the experimental design I studied how labeling mental accounts in a social environment (publicly) gives rise to higher savings rates and to higher achievement of savings goals in comparison to labeling mental accounts privately. Public commitment of savings goals creates implicit agreements that may affect or even change the behavior of some members of a group.[17] The anticipation of "social punishment", in the form of a shame act or harm to reputation acts as a mechanism to induce individuals to save more and achieve their commitments more often. As a result, higher savings balances and goal achievement rates in the *public-labeling* treatment group support this idea because individuals fear breaking commitments made to other members of the group more than commitments made only to themselves.

If money were fungible or perfectly substitutable, the marginal propensity to consume ought to be the same out of all sources of income and assigning labels to specific expenditures or accounts would not have any impact on how the money is spent. Individuals would just transfer money from, say, the 'rent' account to the 'leisure' account without imposing any psychological or monetary costs.[18] If this were the case, labeling savings accounts (privately or even publicly) would not affect individual's savings decisions in the experimental sample. On the other hand, the classical approach to decision-making under uncertainty assumes a self-interested behavior of individuals. Therefore, choices should not be unaffected by other people's decisions (neglecting any motivation of reciprocity and fairness that induce cooperation and enhances group oriented behavior). If this were the case, social networks wouldn't be relevant for decision-making and individuals in the *public-labeling* treatment will not make any additional effort to achieve their savings goals and therefore, savings rates would be the same as in the other experimental groups.

However, the results demonstrate very significant and strong results for treated individuals in the *public labeling* intervention. Savings increased by an average of 35% (effect size of up to .38 standard deviations) and individuals were 8.5% more likely to achieve the initially established savings goals, when I use OLS regressions. Other estimations suggest similar results. The results for the *private labeling* treatment intervention are heterogeneous. Data indicate that such heterogeneity comes from individuals' intrinsic ability

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[15] Although individuals seemed shy when the experimenter invited to share their commitments, 100% of those in the treatment intervention decided to share it with other members of the group.
[16] The trust building activity played at the beginning of the *public-labeling* treatment is called "Game with balloons". One balloon was distributed to each member of the VSLA and they were challenged to push the balloon up and keep it in the air. Once they were able to hold them up in the air, I added more balloons, so that each participant had to keep an eye not only on their own balloon but also on the balloons of the others. The purpose of the game was to build a cooperative environment within members of the VSLA.
[17] Gächter and Fehr, 1999; Fehr and Gächter, 2000; Fehr and Fischbacher, 2002; Carpenter et al, 2010.
[18] Thaler (1999), Hastigs and Shapiro (2013).

to save and institutional features of the VSLA methodology that impose restrictions on individual savings behavior. In sum, treatment effects are very significant for individuals who initiate purchasing larger number of shares during the beginning of the VSLA savings cycle and insignificant for individuals that were in the beginning less able to save. This uncovers the fact that individuals experience different abilities to respond to the treatment interventions and must be considered in the analysis. The results are robust to different specifications, as described in more detail below.

I used mixed methods for data analysis at different stages of the research project. During July to November of 2011 I administered a baseline survey to 670 individuals from the experimental sample to measure a set of characteristics and choices prior to their exposure to the treatments. The second-stage surveying was administered in the fall of 2012, when I followed-up the same group of individuals interviewed at baseline. I also use administrative records of 1,663 members of the VSLA gathered from two organization which I worked with: IED/Vital and Plan International. Table 1 summarizes the data used in the study. Finally, I collected qualitative data from focus group discussions to gather information about the experience and perspectives of participants in the study.

This intervention translates recent theoretical insights into experimental strategies implemented in the field to both test the theory and possibly improve the impacts of a large-scale public policy program. The experimental design contributes to the understanding of how different strategies used to create mental accounts affect choices and contributes to the growing literature in behavioral economics and microfinance. The methodology represents a new approach to the study of individual behavior and provides valuable insights and information to program administrators and policy makers involved in the design and diffusion of commitment-savings products. The increased availability of these and other products with similar features may serve to increase savings and improve financial literacy amongst poor households, which may contribute to generate income to fight poverty.

II. Microfinance in Colombia

Informal contracting is common in Colombia, predominantly in poor neighborhoods. A recent study of low and middle-income households in Colombia shows that 90% of the surveyed families have borrowed money at least once.[19] Of these, 83% used informal lenders (family, neighbors, friends or informal lenders) and less than 30% have used formal financial institutions (banks, cooperatives).[20] Interest charges and other

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[19] The sample represents approximately 75% of lower income Colombian households. USAID-Econometria S.A (2007).
[20] Duflo and Banerjee, 2007 also find that almost all extremely poor households in their sample of one region of India borrowed money from expensive informal lenders. Only 6.4% of extremely poor households borrowed from a formal lending institution. In contrast, one third of the Indonesian poor population borrows from a bank. In their book, Portfolios of the Poor, Collins et al (2009)

terms of financial access vary greatly. Almost all families reported holding liquid savings (e.g. saved cash at home, purchase of durables or through deposit collector). Yet fewer than 2% saved in a bank.[21] Another study shows that that less than 4% of poor women save in a bank and over 70% save in liquid asset holdings, generally to cover daily, unexpected, expenses.[22]

Although many non-profit and government institutions have designed products to increase the access to microcredit, its beneficiaries are small entrepreneurs and households with income levels higher than the poverty line. The government is shifting the focus from microcredit to providing alternatives for savings and insurance targeted to poor and extremely poor households. However, the design of innovative commitment devices to this population is a relatively new topic in the microfinance agenda.

One component of *Red Unidos*, the largest Colombian governmental anti-poverty initiative, offers households access to specialized financial mechanisms linked to transfer payments including savings, microcredit and micro-insurance.[23] Information collected in the baseline of *Red Unidos* (nearly 600,000 households) suggest that 80% don't know how to use the formal financial services available, and only 1.5% define a savings amount within the household. From these, 90% had a bank account but was not used to deposit savings, but mainly for transactional use. Only 7% save in a savings club or through deposit collectors. *Red Unidos* initiated a pilot operation in 2007 in 37 municipalities, and started its expansion to all the regions of the country in June 2008 to enroll 1.5 million families (1.2 million households classified by the SISBEN index as extremely poor, and 300 thousand displaced from violence).[24]

In the banking and financial inclusion component of *Red Unidos*, Banca de las Oportunidades provides assistance to *Red Unidos* families to get access to both formal and informal forms of microfinance. In 2008, Banca de las Oportunidades pilot the program Village Savings and Loan Associations (VSLA) in 34 municipalities. The VSLA methodology has been implemented in at least 30 developing countries around the globe and has proven to be effective in providing savings and loan services to local communities that have not access to formal services. To date, VSLA has almost reached 9 million clients worldwide.[25] Preliminary results from the pilot in Colombia show that individuals consistently save small amounts of money. It reached almost 7,000 clients with an average savings of USD 78 over an 8 to 9 months period. Loans were on average

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use financial diaries data to document the extensive use of informal lenders (mostly relatives and friends, some at no interest) by households to finance expenditures in South Asia and South Africa.

[21] Duflo and Banerjee, 2007 also found that few extremely poor households have a savings account, and if save, they do it through savings collectors or savings clubs like ROSCA or Self-Help Groups.

[22] Preliminary report of impacts. Mujeres Ahorradoras, Familias en Accion.

[23] *Red Unidos* aims to help 16 million people who live under the national poverty line and 5.3 million under the extreme poverty line. These numbers represent 32.7% and 10.6% of the Colombian population in 2012. Source: Departamento Administrativo Nacional de Estadistica -- DANE.

[24] Red Unidos.

[25] VSLA Global Outreach report. Hugh Allen (October 2013).

of USD 67 each and less that 25% of participants took a loan, at least in the first savings cycle.[26] The successful experience of the pilot program motivated the expansion of this initiative to other regions in the country. In June 2011, the government started the expansion to organize 600 new VSLA in cities and rural areas with high poverty levels and limited access to formal financial services. I used this expansion to carry out the RCT designed in this study. In 2013, the government and other multilateral organizations are funding the promotion of the program in more regions of the country and are planning to form over 2000 new VSLAs.

Village Savings and Loan Associations:

VSLA are community based savings commitment products, built on the ROSCA model and other self-help savings groups as an alternative to formal microfinance that offers access to insurance, savings and small loans to the poor with limited or no access to formal financing.[27] Individuals self-select and participate on a voluntary basis to form a self-managed and self-capitalized fund to save and borrow periodically. Members make small and regular contributions to the savings fund by purchasing up to 5 shares in each meeting. Savings are invested in a fund that is soon used to provide small, short-term loans to participants, used for consumption, making small investments in their businesses, and frequently for emergencies. This is complemented by a social fund that is much smaller but provides insurance to members in the form of grants for fatalities and other unexpected circumstances. All the purchases of shares takes place with all members of the group in biweekly meetings and recorded in each member's passbook. Funds are securely stored in the safe box and kept by one member of the group until the next meeting. In Colombia, VSLA are formed by up to 19 members, usually neighbors, friends or family.

The VSLA has a structured methodology and a set of rules that members establish in the first "training" meeting, before starting making contributions. All members of the group form a General Assembly, which elects a Management Committee consisting of 5 positions (chairperson, record-keeper, box-keeper, and 2 money-counters). The General Assembly also sets the rules and conditions stated in a *constitution* of the fund that every member must agree and sign. The constitution contains information of rules of governance, dispute and resolution, conditions for purchase of shares, uses of the social fund, interest rates and price of the share. There is a limit in the number of share purchased in each meeting. Each member cannot purchase more than 5 shares per meeting. However, occasionally, the group allows extraordinary purchase of shares by all members or sometimes the group purchases additional shares using money from group activities such as selling food at a fair, raffles, etc. The share price, interest rate on loans, value of the contribution to the social fund and other

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[26] Banca de las Oportunidades report, VSLA pilot project, January, 2011. VSLAs are commonly operated by CARE, Oxfam America and Plan International, as well as other local organizations. In Colombia are mostly operated by IED/Vital and Plan International.
[27] Over the last 3 decades, the VSL methodology has been implemented by anti-poverty organizations such as CARE, Oxfam America, Plan International and others, in different countries, namely: India, Bangladesh, many African countries and recently, in some Latin American countries. For more information visit VSL Associates. http://vsla.net/

rules are defined prior to the first purchase of shares and are maintained throughout the first savings cycle. This methodology helps households to manage their cash flows and be able to accumulate larger amounts of money for investment in businesses, education, improving housing conditions, or unexpected expenses. At the end of the savings cycle (8 to 9 months), the fund is closed and the accumulated savings are distributed according to the shareholdings. The VSLA methodology encourages savings and use of loans but does not make any activity to explicitly state savings goals or use of savings.

III. Experimental design

I designed and implemented an RCT to study whether a *commitment savings product,* private or publicly created, may affect savings decisions of low-income individuals that participate in newly formed VSLA in Colombia. Under the existing VSLA methodology, individuals are encouraged to save but make no explicit statement of a commitment to reach particular savings goals. Motivated by hypotheses derived from behavioral economics, the designed experiment explores how *private labeling* and *public labeling* alternatives in the ways in which individuals are asked to declare their savings commitments might affect program outcomes, hypothesizing that this might work via differences in how mental accounts are created. In the *private labeling* treatment, individuals label their mental "savings" account and state a savings goal individually; and in the *public-labeling* treatment, individuals label and state savings goals individually, and then share their goals with all the members of the group. In this way individuals are able to label their mental "savings" account and create private or public commitments to reaching individual savings goals.

Hypotheses:

People often find it valuable and practical to form "mental accounts" [28] as a device to constrain their own behavior. As a result, individuals often save more when they save for a declared purpose. This result indicates a violation of the classical assumption of fungibility of money. If money were fungible or perfectly substitutable, the marginal propensity of consuming all sources of income should be the same and assigning labels to specific expenditures or accounts would not have any impact on how the money is spent. Individuals would just transfer money from, say, "rent" account to "leisure" account without imposing any psychological or monetary costs (Thaler, 1999; Hastigs and Shapiro, 2013). If this were the case, labeling savings accounts (privately or publicly) would not affect savings decisions of individuals in the experimental sample. The standard utility maximization model suggest that accounts are perfectly substitutable, thus the marginal cost of using one dollar to purchase unnecessary or unplanned goods should be the same to the marginal benefit of

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[28] Thaler and Benartzi (2004), Thaler and Sunstein (2009) and Kast and Pomeranz (2009).

one dollar in the established savings goal. As a result, in this experimental setting, labeling mental accounts doesn't matter and savings rates should be the same for control and treated individuals. In addition, achievement of savings goals should be the same for all.

Another classical approach to decision-making under uncertainty is that individuals are self-interested utility maximizers. This indicates that their choices are unaffected by other people's choices, and always choose an optimal action that yields the highest monetary payoff (neglecting any motivation of reciprocity and fairness that induce cooperation and enhances group oriented behavior). If this is the case, social networks don't matter. Consequently, the experimental design predicts that (self-interested) individuals in the *public-labeling* treatment will not make any additional effort to achieve their savings goals and therefore, will not have larger savings rates or achievement of savings goals in comparison with the control group or the *private-labeling* treatment group.

According to these interpretations I evaluate the following null hypotheses: (i) Individuals in the *public-labeling* treatment group have the same savings rates than those in the control group. (ii) Although individuals in the *public-labeling* treatment create their savings goals in a more cooperative environment, they save the same amount than those in the *private-labeling* treatment group. (iii) Conditional on savings commitments (labels), individuals in the *public-labeling* treatment are equally likely to achieve their initially established savings goals than the control and *private-labeling* treatment. Thus, if commitment devices matter, I expect all three of these hypotheses to be rejected.

Assignment to treatment and experimental groups:

I randomly assigned newly formed VSLA to two treatments and one control group in 9 municipalities of Colombia. The assignment of the VSLA was carried out using a simple lottery and the method is called spot-randomization in which the assignment to treatment was random at the time when the VSLA was formed. For example, if 5 new VSLA were formed in a week in Cartagena, I draw the type of intervention that the VSLA would be assigned to (private, public or control). This status is maintained throughout all the experimental period. The unit of randomization is the savings group (VSLA) and the unit of analysis is at the individual level.

Individuals in the control group are exposed to the standard VSLA model which uses a well-scripted model to organize eligible beneficiaries, and allows individuals to save and borrow for any group-approved purpose. Individuals in the *private-labeling* treatment group are subject to an added discussion module aimed at highlighting and discussing the difficulties of committing to a savings path and the potential role of using mental accounts in strengthening those commitments. The session ended by asking members to voluntarily

state in writing a savings purpose and weekly savings goals. In the *private-labeling* treatment individuals create (open) and label their mental "savings" account that may contribute to higher savings rates.

The *public-labeling* module is similar except that it encouraged members to make commitments both to themselves and to others in their group, which may let me explore the possibility that this might lead to different outcomes. This module includes trust-building games and a guided conversation aimed at highlighting the achievements of group commitments. As in the *private-labeling* treatment, members were asked to voluntarily state a savings purpose and a weekly savings goal in writing, but in this intervention those commitments were also shared with all members of the group. By doing this, individuals explicitly label their mental account and publicly commit to their own decision. In the beginning of the session, individuals seemed shy when the experimenter invited to share their commitments. However, 100% of participants agreed to share their goals with other members of the group and in the end became very enthusiastic about sharing their dreams with everyone in the group. The experimenter verified the accuracy of the written commitments. In addition, members of the savings group committed to help each other to reach their goal. For this reason I am able to calculate Average Treatment Effects of the treatment intervention on the outcomes of interest.

The RCT compares the situation of individuals who are statistically equivalent at the baseline but are exposed to different interventions. For this reason, any difference observed across the treatments and control groups is attributable to the intervention. The random assignment allows controlling for selection bias present in the estimates and allows determining causal effects of the interventions on the outcomes of interest. The hypotheses allow investigating how small variations in the information provided, and how it is framed, may affect savings behavior, their ability to commit (privately or publicly) to a savings product and their ability to use financial products more effectively. Other dynamics inside the VSLA may also be evaluated, such as whether individuals punish or reward the behavior of other members of the group, according to their performance throughout the savings cycle.[29]

Experimental subjects:

The target population is comprised of extremely poor individuals with limited access to financial services that participate in the largest Colombian anti-poverty intervention, *Red Unidos*. Using a national system of identification index (SISBEN), families are classified to receive benefits from social programs offered by national and local governments in a preferential basis. Some of the programs and projects offered to this population are: housing subsidies, conditional cash transfers (CCT), training programs, health and

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[29] It may be possible to observe what happens when a group member is always purchasing the maximum number of shares, or when a member is unable to raise their contributions to more than one share. In this sense, social taxation may be implicitly imposed within the group. Despite the interest of evaluating such behaviors, the quantitative data does not provide enough information. For that reason, in the focus groups discussion, I raised this discussion.

nutrition workshops and vaccination, etc. The VSLAs are an important part of the financial inclusion strategy to help extremely poor families to manage and accumulate assets and capital, and improve their well-being.

As data from the baseline survey shows, more than 65% of the sample population belongs to *Red Unidos* and are recipients of the CCT program, Mas Familias en Accion. However, comparing the sampled population with an average individual from *Red Unidos*, I found that participants in the experiment have higher level of education, report having more assets and more experience and use of different types of financial services such as a bank account, loans and savings (although through informal providers).

IV. Data collection and sample size

Sample Size:

To select the sample size I used a (Multi-Site) Cluster Randomized Trial model from the Optimal Design software.[30] I introduced an additional level of randomization, by stratifying the sample of new VSLAs in blocks or different sites in the country and assigning each VSLA to an experimental group (*public-labeling*, *private-labeling* or control). The randomization was performed within blocks in order to reduce heterogeneity in the estimates in each site. Sites or blocks were defined as municipalities (9 in total). The randomization uses a cluster design because of the nature of the savings groups program and the nature of the interventions testes. As a result, treatment assignment is at the group level (VSLA) while the unit of analysis is at the individual level. For this reason, I need to account for the within-group correlation.[31]

The sample needed to achieve a statistical power of 80% in each site is approximately 15 clusters.[32] In total, I selected 137 VSLAs. One third is assigned to each experimental group (control, *public-labeling* and *private-labeling*). From each selected VSLA, all individuals were part of the experimental sample, but I randomly chose 5 members to be surveyed at their house to collect demographic characteristics.[33] In total, the study surveyed 670 experimental subjects at two points in time but uses administrative records of savings balances for all 137 savings groups, which allowed having a much larger sample to measure treatment effects.

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[30] Spybrook et al (2011).

[31] The (standardized) parameters used for this calculations were as follows: Significance level: $\alpha = 0.05$; Intra-cluster correlation in the range of: lower bound $\rho = 0.05$ and an upper bound of $\rho = 0.25$. This parameter was assumed considering an intra-cluster covariance of 0.75 to 0.95 based on information from the pilot. The variance explained by the introduction of the controls not larger than 0.5. An effect size or Minimum Detectable Effect (MDE) of 0.3 standard deviations of savings balances among those in the *public-labeling* treatment versus those in the control group and MDE of 0.2 standard deviations more savings for the *public-labeling* treatment over *private-labeling* treatment group. The number of individuals per cluster to be treated is n = 13; however, only 5 individuals from each cluster were chosen for the household survey. I also carried out power analysis for sample size calculation using the commands Sampsi and Samclus in Stata and the results did not change.

[32] Two other parameters such as the variance explained by the introduction of controls and the variance explained by blocking were 0.5 and 0.25 respectively. I introduced these parameters in the calculations because I include some covariates in the regressions in order to gain some precision in the estimated parameters.

[33] Program officers handed a list of all the members of the VSLA and I selected 5 individuals from each to be surveyed in the baseline, at random.

In total the study sample to measure treatment effects is of 1,663 individuals distributed across the three experimental groups.

Quantitative data:

I use two sources of data in the study. First <u>household surveys</u> collected to the sample of 670 individuals at two points in time:

i. <u>Baseline</u>: Prior to the intervention, I had the list of members of the newly formed VSLA. I randomly chose 5 individuals to be interviewed. The survey took place before the intervention at the place of residence of the individual. I measure a set of individual and household variables in order to evaluate the impact in well-being of the household and other outcomes as a result of participation in the intervention. I collect data on demographic characteristics, use and experience of financial services, housing, poverty, food security, household income and expenditures, social capital, ability to cope with unexpected shocks and time preferences.

ii. <u>Follow-up</u>: I administered a follow-up survey after the first savings cycle was closed and savings were distributed among the members of the VSLA. The follow-up survey allows comparing the situation of participants at two points in time, but given the random assignment of the experimental groups, any difference in the outcomes of interest across treatments and control after the intervention should captures the average treatment effect. Attrition could be a potential bias of the estimates of this study, however, in cases when the VSLA was dissolved before the pre-established period or a member decided to defect, I was able to reach them during the follow-up. Because of this, the loss in sample was very small, less than 5% of the individuals interviewed at baseline did not participate in the follow-up.

I also use <u>administrative records</u> from program officials. To complete the sample I worked along with two practitioner institutions in the study. IED/Vital and Plan International. Two-thirds of the experimental sample was chosen in IED/Vital sites and one-third in Plan International sites. Although the VSLA methodology in the field followed by each organization was exactly the same, the information systems and management of data differ. In particular, the level of detail differs across organizations' records throughout the first savings cycle. But overall the information is very useful to evaluate the hypotheses of the study.

Interventions:

During the interventions, the experimenter collected and recorded the savings goals written by participants in a piece of paper. The principal researcher kept the information in a safe place without access to anybody. In total, I have data on savings purposes and weekly savings goals from 903 individuals that participated in the *public-labeling* and *private-labeling* interventions.

Qualitative data:

I collected qualitative information from 4 focus group discussions with the purpose of exploring further questions related to the understanding of achievement of goals in treatment and control groups. I also explored other behaviors and perceptions of individuals as a result of the interventions. The focus groups discussions took place in two of the nine experimental sites and recruited 30 individuals from the experimental sample. The sample was split between men and women to perform the discussion separately.

V. Results

Baseline survey:

Data gathered at baseline suggest that the outcomes of interest and other covariates related to savings are balanced across experimental groups. Table 3 provides evidence of this statement. It shows no statistical differences across control, *public-* and *private-labeling* in most pre-treatments characteristics. As a result, any difference in outcomes post-treatment can be attributed to the intervention. Table 3 also shows descriptive statistics of the studied sample.

The household size of subjects is between 4.55 for households in the *private-labeling* treatment group and 4.72 for households in the control and *public-labeling* treatment group. Over 60% are married or live with their partner and have less than 2 children of 15 years or less living at home. As shown in Table 3, I find significant differences in the number of children between the *private-labeling* treatment and the other experimental groups. It is also important to note that most VSLA participants are women. The percentage of women varies from 77% to 82% across treatments and control but its difference is not significantly different from zero. Another variable that illustrates a difference between the experimental groups is whether a family is recipient of Conditional Cash Transfers (CCT). The number is significantly lower for the private treatment group. However, the numbers are large. Around 60% of households in the sample receive CCTs of *Mas Familias en Accion*. In contrast, a very small number of households receive in-kind or cash transfers from *Adulto Mayor*, a popular elderly transfers program in Colombia.

The data also reveal that 27% to 33% of the sample held any type of savings before participating in the VSLA program. Their weekly savings rates varied on average from USD 5.4 for the *private-labeling* treatment to USD 7.1 for the control groups. Although the averages are different, they are not statistically different from zero, implying that on average, individuals in the experimental sample saved more or less the same amount prior to the treatment intervention.

An interesting result is that around 50% of all subjects report having a bank account. However, they don't use it for savings, but rather for transactions. Households commonly use bank accounts to receive their CCT payments or to receive their salary. In fact, over 60% of those who have a bank account declared to open

the account exclusively to receive their CCT payments of *Mas Familias en Accion*. I asked individuals in the sample if someone in the household took a loan or made an investment in the past 12 months. 13% to 17% took a loan and 33% to 39% of the sample made an investment in purchase of animals, house improvements, new or existing businesses, etc. Although households use (mostly informal) financial services, they are not familiar with writing a budget. Household incomes vary greatly over time; in fact, only 40% of respondent report having a paid job in the last month. As it is commonly observed in this population, they mainly have informal jobs.

Surprisingly over 60% of individuals own the house where they currently reside. This result is very noteworthy because most savings goals are related to home improvement or acquisition of a new home, suggesting that the conditions of their homes are suboptimal for living. A smaller number of individuals reported participating in community activities such as sports clubs, political party, community organization, women's clubs, etc.

I included in the set of variables the average of an index that measures trust in financial institutions. From 1 to 5, I asked surveyed individuals how much they trusted banks and financial institutions where 1 is untrustworthy and 5 is completely trustworthy. On average, the level of trust is larger than 3. This may inform us that there are other reasons different than trust why these individuals are not using formal financial services. Finally, the average number of members of the VSLA is over 13 across all groups. The second panel of the Table 3 presents the mean difference of a set of variables that measure the perception of individuals about the ability to save of their households. I asked if they are think their households could save "much more", "more", "the same" or "not save at all". I carried out a Pearson's chi-squared test to evaluate the associated values for each possible response for the different experimental groups. The joint test is not significantly different from zero across all experimental groups (the *p-value* is 0.294). However, I observe a difference in the perception that households could save the same. It is much larger for the control than for both the *private-* and *public-labeling* treatment groups. These results indicate that prior to the intervention, households across experimental groups are similar in their perception about their ability to save.

Savings commitments:

After the baseline survey was administered, individuals received the standard training of the VSLA methodology. During one of these sessions I performed the treatment interventions in which individuals set savings goals and label mental accounts privately or publicly. After a short discussion, I asked each individual from the *public-labeling* and *private-labeling* treatments to write down in a piece of paper their individual commitment for their savings. I asked: "What is your plan for the funds saved in this savings group?" The responses varied from home improvements, education expenditures and investment in businesses, to health, travel and unexpected expenditures.

Table 4 disaggregates the responses between *public-labeling* and *private-labeling* treatment groups. The data show that about 34% to 43% of participants plan to save in the VSLA for home improvements or purchase of a new home, around 25% for education for children or other members of the household, and 25% to 28% to invest in an existing or a new business.[34] Other savings purposes are: consumption of non-durables such as clothing, food, celebrations and events (8% to 16%); consumption of durable goods such as computers, appliances or jewelry (7% to 12%); for an unexpected expenditure or for emergencies (3% to 4%) and other expenses accounts for less than 3%.[35] The third column of the table displays the t-statistic resulting from the mean comparison of the responses between the treated individuals. As shown in the table, there are no statistical differences across treated subjects for most of the stated savings goals. Only the average savings goal specific to the purchase of a non-durable good such a bicycle, motorbike, computer, jewelry, etc. is statistically different for the treated groups.

The second set of variables in the table illustrates the amount of savings that individuals plan to contribute every meeting during the first savings cycle and the willingness of taking up loans. Individuals from the *private-labeling* treatment set a biweekly savings goal of purchasing approximately 3.06 shares in the VSLA meeting, while individuals from the *public-labeling* treatment define a savings goal of purchasing approximately 3.1 shares. The difference between the number of shares that individuals from each treatment group plan to save weekly is not statistically significant. In contrast, I observe statistically significant differences between *public-* and *private-labeling* treatments in what individuals express to save in terms of money, prior to the intervention. The discrepancy between the differences in the number of shares and the savings balances can be explained by the difference in shares prices across experimental groups. This may be the result of differences in savings capability across experimental groups.

Treatment effects on savings:

If money were fungible or perfectly substitutable, and if individuals behaved in a selfish manner, savings choices should not be affected by labeling savings accounts or by other people's decisions. However, the results show very significant and strong results for treated individuals in the *public-labeling* intervention. Savings increased by an average of 35% (effect size of up to .38 standard deviations) and individuals were 8.5 more likely to achieve initially established savings goals. For individuals in the *private*-labeling treatment intervention results on savings are heterogeneous. In the following paragraphs I explain in more detail these findings. The quantitative and qualitative data indicate that such heterogeneity comes from intrinsic ability to save of individuals and on institutional features of the VSLA methodology that impose restrictions on individual savings behavior. In sum, treatment effects are very significant for individuals who start saving a large number of shares in the beginning of the savings cycle and insignificant for individuals that are less able

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[34] This category includes also a retirement plan or to continue saving.
[35] Such as traveling, purchase of a gift, insurance, etc.

to save, even prior to the intervention. However, treatment effects are on average insignificant. This uncovers the fact that individuals experience different abilities to respond to the treatment interventions and must be considered in the analysis. The results are robust to different specifications, as described in more detail below.

The random assignment of subjects to experimental groups allows estimating the causal effect of the interventions. Differences in the variables of interest capture the average treatment effect of labeling 'mental savings accounts' (that was opened in the beginning of the VSLA) *privately* or *publicly* on savings and other behaviors of treated subjects. I estimate a simple (OLS) model of differences in savings rates and on the number of shares purchased over the first savings cycle, after exposure to the intervention. The model estimated is:

(1) $$Y_i = \alpha + \beta_R T_{i,R} + \beta_U T_{i,U} + \varepsilon_i$$

where, Y_i is the dependent variable, T_i represents the treatment status for each individual in the sample, R represents private-labeling treatment, U represents public-labeling treatment and ε_i is the disturbance term.

Selection bias is eliminated because of the random assignment of VSLA into different groups (Angrist and Pischke, 2009). The coefficients measure the average treatment effects (ATE) of participating in the *public-labeling* or *private-labeling* intervention. The coefficients β_R and β_U measure the average or mean difference in the outcome for individuals in private-labeling and public-labeling treatment interventions, respectively (or treated), in comparison with individuals in the control group (or untreated). Table 5 presents the treatment effects on savings from estimating equation (1) using administrative records. All regressions are estimated with robust standard errors by clustering at the VSLA level. I estimated the treatment effects on savings accumulations measured with the number of shares purchased by each individual under various scenarios: first (column 1) during the entire savings cycle, second (column 2) during the first 6 meetings[36], lastly (column 3) during the entire savings cycle but for only those with the exact same share price.

The regression results in panel A show that both *private-* and *public commitment* are successful in increasing savings accumulations (measured by the number of shares purchased by participants in each meeting). In the total savings cycle individuals in the *private-labeling* treatment saved on average 6 shares more, representing a 17% (0.3 standard deviations) increase in savings in comparison with the controls. Moreover, the increase in shares purchased by those in the *public-labeling* treatment was 12.4 shares or 35 percentage points (0.61 standard deviations) more than the controls.

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[36] This specification is used in order to check for persistence of the treatment interventions on individual's savings behavior. The average number of meetings in the first savings cycle is 15.8. However, I only have this information for two thirds of the sample. For the remaining, I have detailed information of until the sixth meeting.

The significance level of the coefficients for specifications 2 and 3 is robust. This provides evidence that if I estimate the model for the first 6 meetings only, the treatment effects are very strong for both interventions, although the point estimates are smaller. On the other hand, estimating the ATE for the subsample with the same share price (P = COL 5,000) provides evidence of the effectiveness of the interventions for all individuals under the same conditions. In this case, I isolate the possible heterogeneity in opportunities to save across savings groups. The findings show that private commitment increases savings by 25% while public commitment increases savings by 34%.

The last row shows the Chi-squared statistic corresponding to testing the joint significance of the *private* vs. *public* treatment effect. The effect on savings of publicly announcing savings goals is almost 50% larger than the effect of private commitments. The point estimates are statistically significant at the 1% level in specifications (1) to (3).

Columns 4, 5 and 6 show the treatment effects for the total amount of money saved over the savings cycle. This variable is constructed by multiplying the total number of shares purchased during the savings cycle by the price of the share in the VSLAs where the individual belongs. Interestingly the coefficients representing the ATE for the public and private labeling interventions are insignificant in specifications 4 and 5. But once I isolate the differences in share prices across savings groups (specification in column 6), the coefficients are significant for both interventions. As shown in column 6, the private-labeling treatment increases savings balances during the first savings cycle by USD 22.5 while the public-labeling treatment increases savings balances by USD 31.1. These point estimates represent an increase of 25% and 34% respectively. These results show that the share price is important in predicting savings because it affects the behavior of individuals by imposing constraints on their ability to save in the VSLA.

For some individuals the behavioral response to treatments runs up against institutional features that restrict the optimal savings, such as the maximum of shares allowed to purchase in each meeting, individuals can only buy an integer (0 to 5) number of shares and the rigidity in share prices during the entire savings cycle. These restrictions are observed when an individual is willing to purchase 1.5 instead of 2 shares and ends up purchasing a single one, or in cases in which a participant is capable of purchasing more than 5 shares but is only allowed a maximum of 5. As a result, individuals within a VSLA face different constraints that depend not only on their own capability to raise money to make contributions to the fund, but on the savings capability of other members and the rules set in their own VSLA (mainly price shares).

To be more precise about the first constraint, individuals in VSLAs save exactly 0, 1P, 2P, 3P, 4P or 5P pesos during each meeting. This implies that the data is left, right and 'interval' censored, instead of observing the preferred level of savings, and that OLS parameter estimates are biased, despite randomization of treatment status. For that reason, I estimate a latent variable model, as shown in the following equations:

(3) $\qquad S_{L,i} = \alpha + \beta * T_i + \varepsilon_i$

(4) $\qquad S_{C,i} = \begin{cases} 0, & if & S_{L,i} < P \\ P, & if & P \le S_{L,i} < 2P \\ 2P, & if & 2P \le S_{L,i} < 3P \\ 3P, & if & 3P \le S_{L,i} < 4P \\ 4P, & if & 4P \le S_{L,i} < 5P \\ 5P, & if & S_{L,i} > 5P \end{cases}$

where $S_{L,i}$ corresponds to the underlying savings preference, Ti whether individual i receives treatment or not, ε_i is an iid idiosyncratic disturbance term, $S_{C,i}$ represents the observed level of savings and P the price of shares in each VSLA. Assuming that individuals never exceed their latent savings preference and choose the highest available savings level, the observed savings choices follow a step function such as:

(5) $\qquad S_{C,i} = trunc\left(\frac{S_{L,i}}{P}\right) * P$

For example, suppose that the share price is \$10, and an individual preference to save is \$5, then level of savings observed in the data is \$0 because that individual purchase zero shares. If instead, the latent savings preference is \$27, the maximum number of shares that can be purchased is 2, and the level of savings observed is \$20. Similarly if the latent savings is \$100, the maximum number of shares that can be purchased in a session is 5, therefore, the maximum level of savings per meeting is constrained to \$50. These individuals would be left censored, interval censored and right censored respectively. I use an *interval regression model* to estimate the unbiased parameters that measure the effect of the treatment interventions on saving using equation (6):

(6) $\qquad S_{C,i} = \alpha + \beta_R T_{i,R} + \beta_U T_{i,U} + \eta_i$

where each observation of $S_{C,i}$ is an interval censored data, $T_{i,R}$ is a dummy for *private-labeling* treated individuals and $T_{i,U}$ for *public-labeling* treated individuals. η_i is an idiosyncratic disturbance term. The results are shown in Table 6. The first panel shows the estimations for savings per meeting. The results are consistent with the findings in previous estimations. The effect of *private-labeling* treatment is insignificant, but the *public-labeling* treatment is significant in explaining savings. However, the significance level drops even for the *public-labeling* treatment when I cluster the data by VSLA.

I estimate the model using number of shares purchased as the dependent variable. The coefficients are positive and statistically significant at the 1% level for both treatment interventions, indicating that labeling savings accounts privately and publicly increased the number of shares purchased by an average of 0.65 and

0.75 shares per meeting. See Panel B.[37] The results from these estimations show that the treatment interventions are effective in motivating the purchase of more shares in each meeting, but only *public commitment* is able to explain changes in total savings.

Next, I explore the situation of individuals in all experimental groups pre- and post- treatment intervention. I look at the share prices, the number of shares purchased in the first meeting and the trends of savings over the entire savings cycle. The results show very interesting patterns. Figure 1a depicts trends of the number of shares purchased during the savings cycle by experimental groups. The graph shows that, on average, individuals purchase the same number of shares in the first meeting, regardless of the subsequent treatment status. But after the intervention, this number is increased for both *private-* and *public-labeling* treatment groups relative to the control. The increase is noticeable larger for the *public-labeling* treatment. After approximately meeting 9, public and private get closer together, reaching a similar level to those in the control group. These trends illustrate potential positive treatment effects on the number of shares purchased as a result of the interventions. For total savings, I plotted the cumulative savings for individuals during the savings cycle in Figure 1b. It shows that average total savings are consistently larger for those in the *public-labeling* treatment than those in the control group and *private-labeling* treatment intervention. The latter is the lowest amongst all. This does not necessarily implies that *private commitment* doesn't work, but that there are other characteristics, such as the share price, that should be incorporated in the analysis before making any conclusions about individual savings behavior. For that reason, I need to do further analysis of the differences in share prices across VSLAs, as described below.

Figure 2 plots the distribution of pre-treatment variables for each experimental group. Panel A illustrates the distribution of share prices in all savings groups. Recall that the first purchase of shares was performed prior to the interventions, which allows us to compare the pre-treatment situation across experimental groups. There is a high frequency in the price of shares at P = COP 5,000 (Colombian pesos, equivalent to approx. USD 2.8) across all groups. This provides an opportunity to compare treatment effects at this price, when individuals are more or less equally economically able to save. In the *private-labeling* treatment, there is large concentration of prices below COP 3,000 (approx. USD 1.66) relative to the other experimental groups, suggesting lower average share prices in this treatment group. Panel B presents the frequency of savings groups according to the number of shares bought only in the first meeting. The figure shows that on average individuals purchased up to 2 shares in the first meeting.

To explore further the statistical significance across experimental groups, I calculated the average values of these measures for treatment and control groups. Table 7 shows that the price of shares is statistically

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[37] I also estimated the interval regression model including interactions of number of meetings with treatment status for both savings per meeting and shares purchased per meeting; and also clustering by study site. The treatment effects are robust to those specifications.

different for all experimental groups. The average share prices are USD 2.30, USD 1.90 and USD 2.18 in the control, *private-labeling* treatment and *public-labeling* treatment, respectively. This is consistent with what we observed in Figure 2, where the price of shares of VSLAs in the *private-treatment* intervention was concentrated in lower values. Although these differences seem small, they are statistically different from zero. This creates an imbalance in the pre-treatment variables but does not invalidate the results, because as shown in Table 5, the results are mainly the same when I estimate ATE for individuals under the same conditions (P = COP 5,000). However, identifying these differences in prices helps to explain the sign of the *private-labeling* treatment estimates when I estimate total savings balances. Differences in the price of shares impose a restriction in the total amount of money saved throughout the savings cycle, because no one can purchase more than 5 shares in each meeting. Even if individuals in the *private-labeling* treatment purchase the maximum number of shares permitted in all meetings, total savings would be smaller in comparison to the control and *public-labeling* treatment groups, which have significantly higher share prices.

In terms of average number of shares purchased during the first meeting, I do not observe any statistical difference across experimental groups. This indicates that, conditional on share prices, individuals have a similar ability to save, on average. The last rows of the table show statistically significant differences across experimental groups in the total number of shares purchased and total savings balances under different scenarios.

I also estimated the simple model that includes pre-treatment variables in the model in order to control for imbalances in the sample prior to the intervention. The model estimated is:

$$(7) \qquad Y_i = \alpha + \beta_R T_{i,R} + \beta_U T_{i,U} + \gamma_i X_i + \mu_i$$

where γ_i is a vector of coefficients and X_i a matrix of covariates, which include in the regression the price of shares, dummies for facilitator and the number of shares purchased in the first meeting.[38] Table 8 displays the results of estimating equation (7). Columns 1, 2 and 3 show that estimating the total number of shares purchased in the savings cycle is robust to including different covariates as controls. However, the *private-labeling* treatment effect and the price of shares are insignificant. Columns 4, 5 and 6 show that controlling by the share price, the treatment effect for individuals in the *public-labeling* intervention is significant in explaining higher savings balances of about USD 18 more relative to the controls. Average total savings of individuals in the control group is USD 82.52. Thus, the point estimates predict an increase of 22% in total

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[38] I included dummies for facilitator in order to control whether they have any influence in the determination of share prices. The coefficient for 3 facilitators is significant, but for others, it is not statistically significant.

savings reaching out over USD 100 for the *public-labeling* treatment. Treatment effects for the *private-labeling* intervention are still insignificant but now the sign of the point estimates is positive. The results are similar across specifications.[39]

To provide more evidence of the heterogeneity observed as a result of differences in ability to save and share prices, Figure 3 plots the average number of shares purchased in every meeting by the experimental group and by the number of shares bought in the first meeting. Individuals that, prior to the intervention, were more economically constrained (purchased 0 to 2 shares in the first meeting) exhibited more difficulties to purchase a larger amount of shares during the savings cycle, regardless of the treatment intervention received, whereas those with more ability to save (purchased 3 to 5 shares prior to the intervention) were more responsive to the treatment interventions, indicating differential treatment effects for all types of individuals.[40] As shown in the graphs, the trend in savings is larger for individuals in the *public-labeling* treatment in all meetings. These pictures provide evidence of potential heterogeneous treatment effects that I explore by estimating the following model:

$$(8) \qquad Y_i = \alpha + \beta_R T_{i,R} + \beta_U T_{i,U} + \delta_R T_{i,R} * InitialCondition_i + \delta_U T_{i,U} * InitialCondition_i + \zeta_i$$

where $InitialCondition_i$ represents a set of dummy variables of the number of shares purchased in the first meeting by each individual. Results are reported in Table 9 and in Table 10. Table 9 shows the coefficients of estimating equation (8) for the dependent variables: *total number of shares* and *total savings balance* during the savings cycle. As in the results for the first model the coefficients indicate that *public-labeling* treatment is very strong and effective in increasing savings for all individuals, but are mixed for individuals in the *private-labeling* intervention. In order to see this more clearly, I calculated the joint significance of the total treatment effects for each type of individuals. The computations suggest that the *private-labeling* intervention significantly increases savings accumulations for those who initially bought 3 or 5 shares. For all other types of individuals, the treatment effect is positive but insignificant. This result can be illustrated in a graph. Figure

4 depicts the heterogeneous treatment effects estimated above for the total number of shares purchased (left panel) and total savings in US dollars (right panel). The effect of *public commitment* on total shares bought during the savings cycle is strongly significant and increases monotonically for individuals that purchase 1 or more shares in the first meeting, whereas the impact of *private commitment* fluctuates along the different levels of ability to save; however it's increasing and significant only for those that begin the savings cycle purchasing 3 or 5 shares. On the other hand, the *public* and *private* treatment interventions increase total savings but are significant only for individuals who, pre-intervention, are less economically constrained.

The findings show that setting a *soft commitment* to save is highly effective in increasing savings accumulations for individuals without economic restrictions or with some ability or motivation to save, prior to the intervention. Yet, when adding peer affects to *commitment*, the economic restriction does not prevent all individuals from increasing savings accumulations.

The institutional features of the program, such as the limit in the number of shares purchased in each meeting by each member or the fact that individuals can only purchase discrete amounts of shares, may be underestimating the true effects of the treatment on savings. In order to explore further this idea, I estimated a non-linear model in two steps: In the first step, I estimate the probability of purchasing each discrete number of shares allowed in the first meeting (0 to 5) as a function of group variables (including dummies for city and facilitator). This estimation can be observed as an approximation to model the institutional constraints imposed by the program, and allows estimating the underlying savings of individuals. In the second step, I estimate the average treatment effects on savings accumulations, measured with various outcomes. The results are reported in Tables 11 and 12. The estimations show that the decision of the number of shares to purchase in the first meeting is determined by group variables such as the value of the social fund and the municipality that also describes the facilitator of the VSLA. The facilitator plays an important role in determining the number of shares that participants purchase in the beginning of the savings cycle. Surprisingly, the share price is unrelated to this decision, at least in the first meeting. These results indicate that individuals make their choices following the dynamics of the group rather than their own, financial or behavioral, constraints, at least in the beginning.

In the second step, I use the predicted probability of choosing 0 to 5 shares to purchase in the first meeting as a measure of the institutional constraints that participants may face when making the savings decisions. I estimate average treatment effects on savings accumulations controlling for other explanatory variables (see Table 12). The results are consistent with previous findings (Tables 5 and 8), where *public commitment* strongly affects the total number of shares purchased in the savings cycle with and without including the share prices a control. On the other hand, *private commitment* is positive but insignificant. Interestingly, the variables that measures the institutional constraints faced in the VSLAs are positive and

statistically significant in explaining all different measures of savings accumulations; and support the findings from previous estimations.

Treatment effects on goal achievement:

The last set of calculations exhibit the effects of the interventions on goal achievement. First, I calculated the impact on goal achievement using data from the baseline survey. At that point, I asked all subjects what they wanted to use their savings from the first savings cycle for. I compared this information with the responses from the follow-up survey. The results show that *public* commitment highly affects achievement. See Table 13. However, the impact is not statistical significant in the case of *private* commitment. This result is robust to all specifications, even when I condition the achievement of goals with the initially established goal and I add municipality level control.

In the second estimation I estimate goal achievement, but now I compare the responses in the follow-up survey with the information reported at the intervention sessions. In other words, this specification compares the effect across treatments (excluding the control group). The coefficients are significant at the 5% and indicate an increase of 9.8 percentage points in the likelihood of goal achievement for individuals in the public-labeling treatment relative to those in the private-labeling treatment group.

Qualitative results:

The purpose of the qualitative analysis is to explore further the behavior of treated individuals after exposure of the intervention and to identify whether the interventions contributed somehow to the way participants earmarked transactions and, as a result, change consumption and savings choices. I performed 4 focus groups discussions at two sites of the study and explored the experience of participants in the following: (i) savings as a member of VSLA, (ii) the savings goals, (iii) challenges in reaching goals, (iv) dynamics within VSLAs, and (v) social taxation among members of the group.

Individuals reported that before being part of VSLAs their level of savings was very small, irregular, mostly informal and used frequently in unnecessary spending, despite of understanding that savings is a mechanism to reaching goals, smoothing consumption and facing difficult times in the future. Participants informed us that sharing publicly their savings goals created a competitive environment within the group that motivated each other to making greater effort to save a larger amount of money. In all cases individuals put pressure from one another to buy shares, actively participate in group activities and achieve commitments. In a few situations the group penalized members who were not making regular contributions.

In terms of savings goals, men were more likely to set long run savings goals that required more funds and possibly other sources of funding than the savings from the group. Women set goals that were more

feasible and easier to achieve, but were more like to reach those goals. The most common motivations to reaching goals were personal challenge, pressure from the VLSA members (mainly to purchase the maximum number of shares) and family-related goals (make it less likely to deviate).

During the discussion sessions I found out evidence of social taxes in two contexts: First, individuals penalize VSLA members that failed to contribute to the savings fund or failed to comply with the rules of conduct. Second, at the household level I found that men usually tease their wives and underestimate their ability to save at the beginning of the savings cycle; but once they realize their engagement with savings, husbands reduced the money transferred to their wives for household expenses because women are now able to raise money on their own. These statements provide some insights to study further the dynamic created at the household level as a result of changes in individual's savings behavior.

VI. CONCLUSION

I designed and implemented a Randomized Controlled Trial (RCT) to study how two modifications of a *commitment* savings product*,* in which individuals open *private* or *publicly* a 'mental savings account', affect savings decisions of low-income individuals that participate in newly formed VSLAs in Colombia.

The results show that *labeling* 'mental savings accounts' is effective in increasing savings for different types of individuals. The RCT demonstrates very strong and significant results for treated individuals in the *public-labeling* intervention. Savings increased by more than 30 percentage points (effect size of up to .38 standard deviations) and individuals were at least 8.5% more likely to achieve the initially established savings goals. The results for the *private*-labeling treatment intervention are very interesting also. The effect on savings is heterogeneous and depends on intrinsic characteristics of individuals and on institutional restrictions imposed by the VSLA methodology. However, the impact on goal achievement is statistically irrelevant.

The results also provide evidence that treatment effects are very significant for individuals who save larger amounts of money in the beginning of the VSLA savings cycle; but are insignificant for individuals with lower savings in the beginning of the cycle. Using a model that estimates the underlying savings of individuals using an interval regression model and a 2-step procedure, I show that treatment effects are strong and significant for *public-labeling* treated subjects and insignificant for *private-labeling* subjects. Also, the models shows that the institutional constraints imposed by the methodology and characteristics of the savings groups affect the savings decisions of individuals. For that reason, individuals experience different abilities to respond to the treatment interventions depending not only on their own behavior but also on the institutional features of the VSLA program.

In terms of goal achievement, individuals in the *public-labeling treatment* are more likely to use other sources of income flows, such as investments and loans to achieve savings goals. This could be explained by the fear to be punished by other members of the VSLA if they fail to meet their promises.

Self-help groups such as VSLA are an alternative to poor and extremely poor households to help them manage financial assets and smooth consumption. Creating commitments exogenously enable individuals to think about strategies to improve the use of assets and financial decisions in the future in order to maximize their utility. This intervention translates recent theoretical insights into experimental strategies implemented in the field to both test the theory and possibly improve the impacts of a large-scale public policy program. The methodology represents a new approach to the study of individual behavior and provides valuable insights and information to program administrators and policy makers involved in the design and diffusion of commitment-savings products. The increased availability of these and other products with similar features may serve to increase savings, improve financial literacy amongst poor households, which may contribute to generate income to fight poverty.

The results from the quantitative and qualitative analysis in this study open additional questions that are subject of future research. Some ideas that arise form this study are: to explore how does the change in savings habits affect other behavior at the household and community level. Some potential outcomes to analyze are the bargaining power within the household, the participation in social and community activities, social penalties and rewards to savings behavior, etc. Also, further analysis should be done in terms of heterogeneous treatment effects for men vs. women, young vs. adults, and for long vs. short run commitments.

Table 1. Experimental sample

		Bolivar	Choco	Cundi-Boyaca	Total
Experimental groups (VSLA)	Public	13	15	19	47
	Private	13	15	17	45
	Control	12	14	19	45
	Total	*38*	*44*	*55*	*137*
Treated Individuals	Public	128	131	196	455
	Private	139	135	174	448
	Total	*267*	*266*	*370*	*903*
Experimental subjects (household survey)	Public	65	73	91	229
	Private	67	75	76	218
	Control	57	70	96	223
	Total	*189*	*218*	*263*	*670*
Administrative data	Public	143	179	256	578
	Private	149	183	231	563
	Control	104	166	252	522
	Total	*396*	*528*	*739*	*1663*

Source: survey data, administrative records.
Notes: The first panel contains the number of VSLAs that were subject of the experiment; the second shows the number of individuals exposed to the treatment intervention (recall that the control group is not exposed to the treatment intervention, but only to the VSLA methodology); panel 3 details the number of individuals randomly chosen to be interviewed to administered the households survey; and panel 4 summarizes the total number of individuals in the experimental sample and for which I have administrative records on savings.

Table 2. Power calculations

	Min. Observations	*Min. Clusters per site*
Experiment 1. Public vs. Control, MDE = 0.3 sd		
Rho=0.05	702	6
Rho=0.15	819	7
Rho=0.25	1053	9
Experiment 2. Public vs. Private, MDE = 0.2 sd		
Rho=0.05	1287	11
Rho=0.15	1755	15
Rho=0.25	2223	19

Note: Author's calculations using a Multi-site cluster randomized model at the individual level with Optimal Design software. Results are verified using Stata commands sampsi and sampclus with savings data from the pilot of the VSLA program in Colombia.

Table 3. Baseline comparison of covariates across experimental groups

	Untreated (CTRL)	Public (U)	Private (R)	Mean Diff (CTRL-U)	Mean Diff (CTRL-R)	Mean Diff (R-U)
Household size	4.72	4.72	4.55	0.008	0.174	-0.166
% female	0.77	0.82	0.78	-0.053	-0.014	-0.039
Number of children (<16 yrs of age) at home	1.80	1.76	1.48	0.040	*0.32**￼*	*-0.28**￼*
% married	0.69	0.65	0.64	0.038	0.046	-0.008
% CCT recipient	0.62	0.66	0.58	-0.034	0.043	*-0.077*￼*
% receive cash & in-kind transfers (elderly)	0.04	0.04	0.04	-0.004	0.000	-0.003
% households that hold any type of savings	0.27	0.27	0.33	-0.008	-0.064	0.056
Weekly savings (USD)	7.1	6.4	5.4	0.750	1.750	-1.000
% households with a bank account	0.49	0.50	0.49	-0.014	0.000	-0.014
% open bank account to receive CCT	0.67	0.64	0.61	0.028	0.056	-0.028
% had loan in last year	0.17	0.16	0.13	0.006	0.037	-0.031
% households that write a budget	0.03	0.07	0.03	-0.033	0.000	-0.033
% made an investment last year	0.39	0.35	0.33	0.036	0.055	-0.019
% with paid job	0.4	0.4	0.4	-0.029	0.036	-0.065
% independent	0.53	0.48	0.50	0.047	0.024	0.023
% own home	0.66	0.62	0.63	0.037	0.032	0.005
% participate in community organizations	0.09	0.11	0.14	-0.020	-0.049	0.029
Trust in banks and financial institutions	3.39	3.36	3.49	0.027	-0.106	0.134
Size of the VSLA	13.60	13.57	13.56	0.035	0.039	-0.004
Perception that household could save…:						
Much more	0.36	0.37	0.40	-0.008	-0.035	0.027
More	0.32	0.36	0.32	-0.045	0.001	-0.046
The same	0.16	0.08	0.10	0.073	0.056	0.017
Nothing	0.16	0.18	0.18	-0.020	-0.022	0.002
Pearson's chi-squared	*7.29*	*P-value:*	*0.29*			
Number of observations	**223**	**229**	**218**			
Number of VSLAs	**45**	**47**	**45**			

Source: Baseline household survey. Significance level: * 10%; ** 5%, *** 1%.

Table 4. Savings commitments

Savings goals (percentages)	Private-labeling Mean	Public-labeling Mean	t-statistic
Home	43	34	1.71
Education	25	22	0.53
Investment	28	25	0.64
Consumption of non-durables	8	16	-2.15
Consumption of durables	7	12	-1.47
Pay debt	1	2	-1.34
Unexpected expense	4	3	0.32
Other	2	3	0.33
Biweekly savings (No. of shares)	3.06	3.1	-0.24
Biweekly savings (USD)	5.94	6.93	-2.92
Would like to take a loan	15	10	2.22
Number of participants	448	455	

Source: Treatment interventions. T-statistic calculated from the mean difference of averages between groups

Table 5. Treatment effects on savings

	Number of Shares Bought			Savings Balance		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	Total	*First 6 meetings*	*p=COL $5,000*	*Total*	*First 6 meetings*	*p=COL $5,000*
Private tmt	6.03*	1.99*	8.12*	-5.764	-2.07	22.55*
	(3.45)	(1.01)	(4.5)	(11.72)	(4.05)	(12.51)
Public tmt	12.35***	3.26***	11.19**	14.11	3.12	31.09**
	(3.47)	(1.113)	(4.58)	(11.19)	(3.68)	(12.71)
Constant	35.03***	12.83***	32.72***	82.53***	29.79***	90.9***
	(2.02)	(.722)	(1.9)	(8.702)	(2.96)	(5.27)
Observations	1474	1474	654	1474	1474	654
Chi2 for (Public=Private)	10.76	8.46	7.85	0.18	0.02	7.85

Source: Administrative data. Significance level: * 10%; ** 5%; *** 1%. Robust standard errors in parenthesis.

Table 6. Interval regression results

	A. Savings per meeting (USD)					
	Coef.	Std. Err.		Coef.	Robust SE (VSLA)	
Private	0.02	(0.137)		0.02	(0.980)	
Public	1.14	(0.133)	***	1.14	(0.919)	
Constant	6.75	(0.096)	***	6.75	(0.767)	***
Observations					19,555	
	B. Number of shares bought per meeting					
	Coef.	Std. Err.		Coef.	Robust SE (VSLA)	
Private	0.56	(0.059)	***	0.56	(0.322)	*
Public	0.72	(0.058)	***	0.72	(0.343)	**
Constant	2.27	(0.042)	***	2.27	(0.224)	***
Observations					19,555	

Source: Administrative data. Significance level: * 10%; ** 5%; *** 1%.

Table 7. Pre- and Post-treatment averages for savings outcomes

	Control (C)	Private (R)	Public (U)	C-R	C-U	R-U
Share price (USD)	2.30	1.90	2.18	***	*	***
Shares purchased first meeting	1.78	1.82	1.75			
Total shares						
All	35.03	41.06	47.39	***	***	***
First 6 meetings	12.83	14.83	16.09	***	***	**
P = COL $5,000	32.72	40.84	43.92	***	***	
Total savings (USD)						
All	82.53	76.76	96.64		***	***
First 6 meetings	29.79	27.71	32.91		**	***
P = COL $5,000	90.90	113.45	121.99	***	***	

Source: Administrative data. Significance level: * 10%; ** 5%; *** 1%.

Table 8. Treatment effects with controls

Dependent variable:	Total shares			Total savings balance		
	(1)	(2)	(3)	(4)	(5)	(6)
Private tmt	5.11	4.92	5.08	7.91	8.49	7.85
	(3.49)	(3.37)	(3.33)	(7.05)	(7.11)	(6.68)
Public tmt	12.15***	11.28**	12.39***	18.12*	18.43*	18.64*
	(3.41)	(3.417)	(3.25)	(8.67)	(8.44)	(8.26)
Share price	-2.30	0.19	-1.81	34.23***	37.64***	35.24***
	(1.71)	(1.83)	(1.549)	(6.03)	(7.24)	(5.74)
Shares meeting 1			6.70***			13.78***
			(.58)			(1.418)
Constant	40.32***	32.51***	27.24***	3.92	-6.1	-22.96
	(4.53)	(6.09)	(4.11)	(13.35)	(15.06)	(12.58)
Includes facilitator dummies		X			X	
Observations	1474	1474	1474	1474	1474	1474
Chi2 for (Public=Private)	8.81	8.00	9.45	3.76	4.10	4.08

Source: Administrative data. Significance level: * 10%; ** 5%; *** 1%. Robust standard errors in parenthesis. Specifications (2) and (5) include facilitator dummies as controls. All regressions also include dummies for municipalities or study sites.

Table 9. Heterogeneous treatment effects

Dependent variable	Total shares		Total savings	
	Coeff.	*Std. Err.*	*Coeff.*	*Std. Err.*
Private tmt	-2.231	(3.49)	-4.464	(6.95)
Public tmt	7.689*	(3.22)	10.3	(7.75)
Private tmt*1 share dummy	0.221	(3.37)	-0.485	(7.47)
Private tmt*2 shares dummy	5.892*	(2.9)	12.19*	(5.13)
Private tmt*3 shares dummy	21.44***	(5.95)	30.85***	(8.85)
Private tmt*4 shares dummy	9.652*	(4.09)	13.38	(7.72)
Private tmt*5 shares dummy	32.61***	(5.95)	57.97***	(11.57)
Public tmt*1 share dummy	-6.413*	(2.69)	-16.66**	(6.29)
Public tmt*2 shares dummy	6.498	(3.8)	15.71	(8.78)
Public tmt*3 shares dummy	18.53**	(5.48)	38.04**	(12.18)
Public tmt*4 shares dummy	29.81***	(7.13)	82.17***	(20.64)
Public tmt*5 shares dummy	30.05***	(2.99)	59.14***	(9.8)
Share price (USD)	-1.3806	(1.39)	28.92***	(7.21)
Avge share first meeting	3.262*	(1.44)		
Avge savings (USD)			4.471*	(1.95)
Constant	32.39***	(4.8)	-1.949	(10.31)
Number of observations	1474		1474	

Source: Administrative data. Significance levels: * 10%; ** 5%; *** 1%. Robust standard errors in parenthesis.

Table 10. Total heterogeneous treatment effects

Mean for Control group	Total shares	Total savings (USD)
0 shares in first meeting	30.0	73.8
1 share in first meeting	28.6	66.2
2 shares in first meeting	37.7	90.1
3 shares in first meeting	43.1	105.9
4 shares in first meeting	41.7	112.8
5 shares in first meeting	50.7	101.3

*Total effects^*	Total shares		Total savings (USD)	
	Private	**Public**	**Private**	**Public**
0 shares in first meeting	27.7	37.7 **	69.3	84.1
1 share in first meeting	26.6	29.9 ***	61.2	59.8
2 shares in first meeting	41.4	51.9 ***	97.8	116.1 **
3 shares in first meeting	62.3 ***	69.4 ***	132.3 ***	154.2 ***
4 shares in first meeting	49.1	79.2 ***	121.8	205.3 ***
5 shares in first meeting	81.1 ***	88.4 ***	154.8 ***	170.8 ***

Source: Administrative data. Joint significance level: * 10%; ** 5%; *** 1%.
Notes: ^ These numbers are calculated using the coefficients from estimating the heterogeneous treatment effects from model 2. Robust standard errors are used in the estimation.

Table 11. <u>**Step 1:**</u> Ordered probit to estimate the probability of purchasing a discrete number of shares in the first meeting

Depvar: *Shares bought in first meeting (0 to 5)*	Coefficient		Std. Err.
Size of savings group	0.02		(0.015)
% female in the group	0.07		(0.063)
Share price (USD)	0.00		(0.000)
Value of social fund (USD)	0.00	**	(0.000)
Facilitator 2 (Istmina)	-0.30		(0.208)
Facilitator 3 (Pacho)	-0.20		(0.175)
Facilitator 4 (Quibdo)	-0.44	**	(0.201)
Facilitator 5 (Sogamoso)	-0.63	***	(0.186)
Facilitator 6 (Tado)	-0.23		(0.275)
Facilitator 7 (Ubate)	-0.01		(0.203)
Facilitator 8 (Zipaquira)	-0.19		(0.216)
Facilitator 9 (Cartagena)	-0.06		(0.236)
Facilitator 10 (Cartagena)	-0.29		(0.279)
Facilitator 11 (Cartagena)	-0.48	**	(0.206)
Facilitator 12 (Cartagena)	-0.53	*	(0.283)
Cut 1	-0.70		(0.250)
Cut 2	0.30		(0.244)
Cut 3	0.84		(0.249)
Cut 4	1.06		(0.246)
Cut 5	1.29		(0.249)
Observations			1,474

Notes: * indicates significance level at the 90%, ** at the 95% and *** at the 99%. In total, the experiment was conducted in 9 municipalities with one facilitator per location. However, in Cartagena there were 4 facilitators in total. The table shows the facilitator effect on the probability of purchasing 0 through 5 shares in the first meeting as well as the effect of other group variables. Facilitator 10 is omitted.

Table 12. Step 2: Treatment effects on savings accumulations with and without controls

|	Average shares per meeting	Total shares	Total savings (USD)
Private treatment	0.28	4.96	7.63
	(0.203)	(3.362)	(6.819)
Public treatment	0.54*	12.44***	18.81*
	(0.234)	(3.242)	(8.232)
Residual	-0.20	-2.44	-5.67
	(0.122)	(1.740)	(4.390)
Residual^2	0.06*	0.78*	1.48
	(0.023)	(0.314)	(0.772)
Shares bought in meeting 1	0.53***	7.95***	16.96***
	(0.132)	(1.868)	(4.768)
Share price (USD)	-0.07	-1.58	35.67***
	(0.100)	(1.512)	(5.643)
Constant	1.40***	22.99***	-32.51*
	(0.308)	(4.997)	(13.050)
Observations	1,107	1,474	1,474
Chi2 (private=public)	4.83	9.06	3.89
Chi2 (all)	32.7	38.86	31.25

Notes: * indicates significance level at the 90%, ** at the 95% and *** at the 99%.

Table 13. Treatment effects on goal achievement

| Goal achievement		Robust SE		Robust SE + controls	
depvar:	**Mean control**	*Public*	*Private*	*Public*	*Private*
Achieved savings goal (BL)	0.159	0.085**	0.013	0.098***	0.032
		-(0.04)	-(0.04)	-(0.04)	-(0.04)
Achieved savings goal (TMT)		0.098***		0.064	
		-(0.05)		-(0.05)	
Number of observations	670				

Source: Baseline and end line household survey. Significance levels: * 10%; ** 5%; *** 1%. I use Baseline goals as controls

Figure 1a. Trend of the number of share purchased over the entire savings cycle

Shares purchased in each meeting
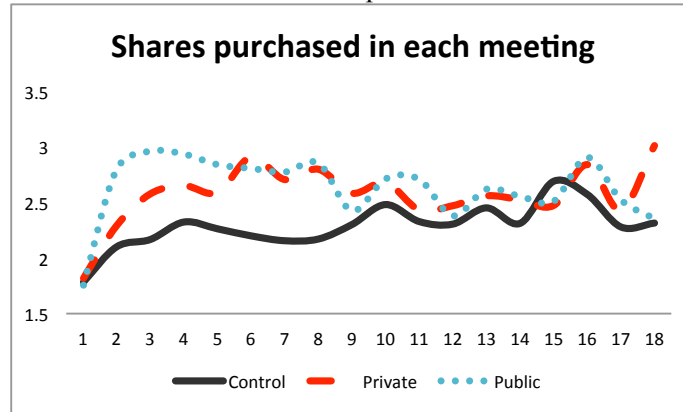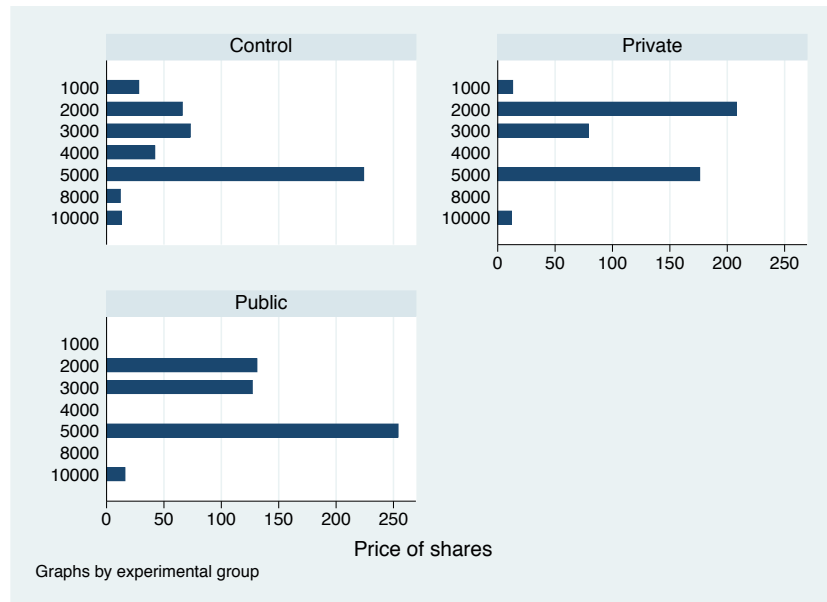
Source: Administrative data.

Figure 1b. Cumulative total savings per meeting by experimental group

Total savings by experimental group
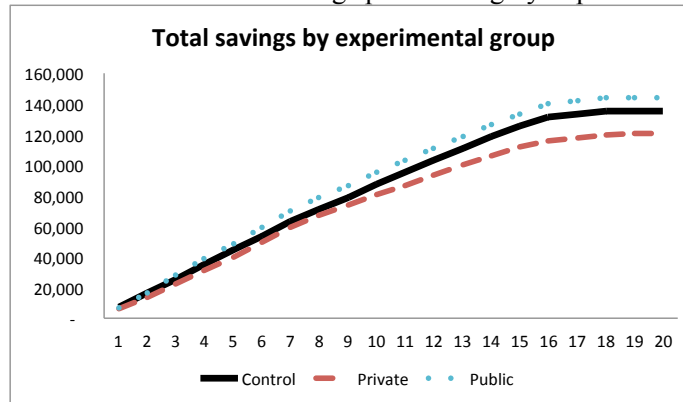
Source: Administrative data.
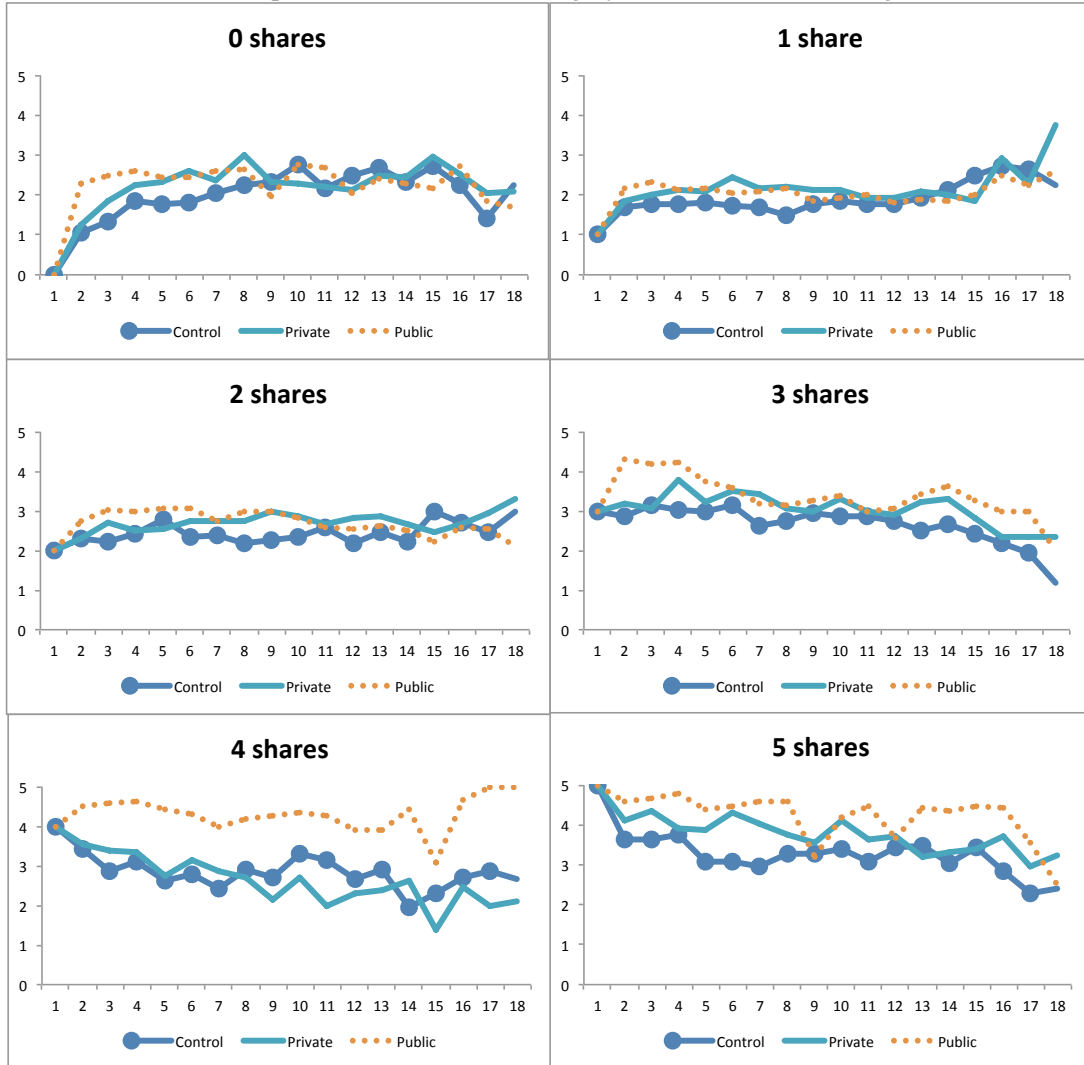
Figure 2. Pre-treatment variables
A. Share prices

Graphs by experimental group

Source: Administrative data.

B. Shares bought in first meeting
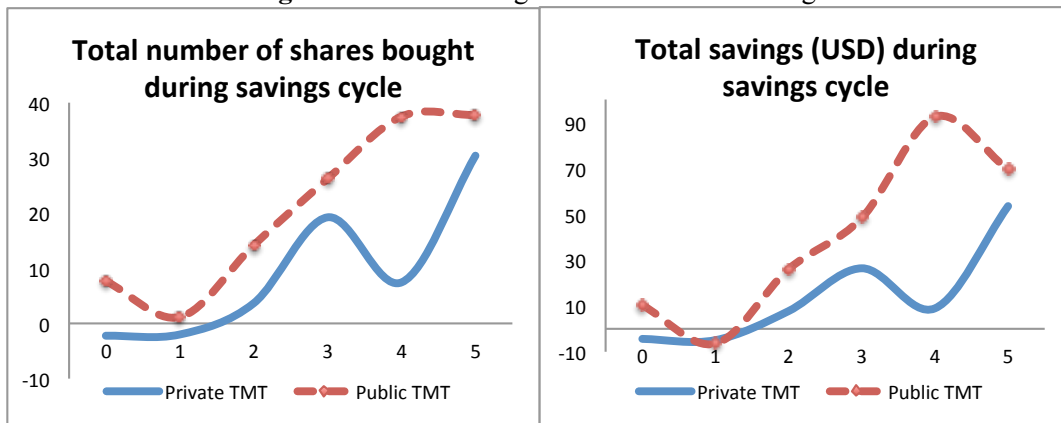

Graphs by experimental group
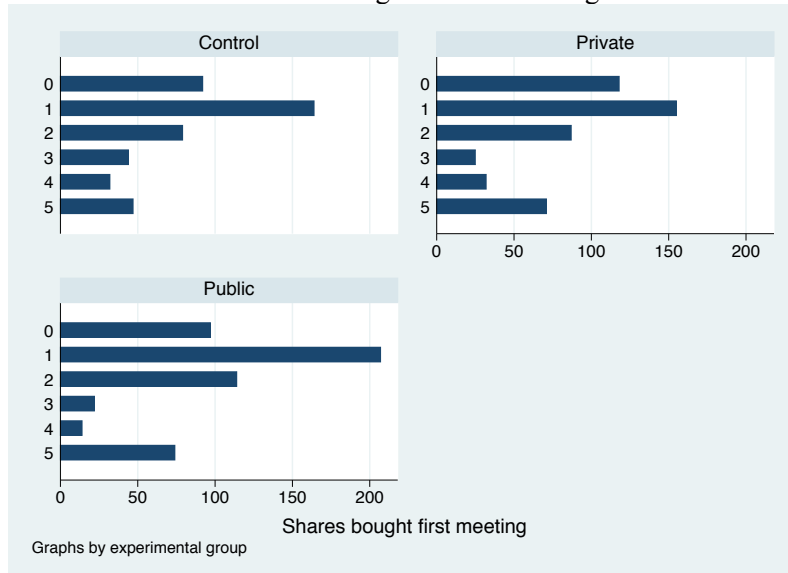
Source: Administrative data.

Figure 3. Trends of shares purchased in each meeting by number of shares bought in the first meeting

Source: Administrative data.

Figure 4. Total heterogeneous effects on savings

Source: Administrative data. The data plotted corresponds to the coefficients of the heterogeneous treatment effects.

Appendix A1. Treatment effects estimated for each meeting

Regression for:	Total shares					Total savings (USD)					Obs.
	Private TMT		Public TMT		Constant		Private TMT	Public TMT	Constant		
Meeting 1	0.034		-0.028		1.784	***	-0.595	-0.361	4.044	***	1474
	(0.18)		(0.18)		(0.13)		(0.58)	(0.49)	(0.38)		
Meeting 2	0.191		0.697	***	2.100	***	-0.724	0.793	4.942	***	1474
	(0.18)		(0.20)		(0.14)		(0.61)	(0.61)	(0.49)		
Meeting 3	0.410	**	0.794	***	2.166	***	-0.235	0.882	5.143	***	1474
	(0.20)		(0.24)		(0.16)		(0.80)	(0.74)	(0.59)		
Meeting 4	0.326		0.611	**	2.323	***	-0.536	0.727	5.295	***	1474
	(0.24)		(0.24)		(0.18)		(0.73)	(0.72)	(0.59)		
Meeting 5	0.326		0.577	**	2.262	***	-0.285	0.443	5.271	***	1474
	(0.21)		(0.22)		(0.14)		(0.84)	(0.72)	(0.59)		
Meeting 6	0.709	***	0.606	***	2.199	***	0.303	0.638	5.092	***	1474
	(0.23)		(0.23)		(0.17)		(0.84)	(0.76)	(0.64)		
Meeting 7	0.550	**	0.618	**	2.155	***	-0.124	0.630	5.218	***	1474
	(0.25)		(0.24)		(0.17)		(0.84)	(0.83)	(0.66)		

Notes: * indicates significance level at the 90%, ** at the 95% and *** at the 99%. Robust standard errors in parenthesis. I estimated each regression until meeting 7 because I only have detailed information about the number of shares bought per person per meeting for the entire sample until meeting 7. After meeting 7, I only have data for one third of the sample.

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