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Do you want to jump on this train?
A field experiment on the impact of financial training on savings behavior in Rwanda

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Abstract

This thesis reports on a field experiment conducted in rural Rwanda, in which farmers were randomly assigned to attend a financial education training and were offered one of two types of savings accounts; a Targeted savings account, or a more binding, Pre-commitment savings account. We conduct a Difference-in-Difference analysis to test whether financial training alone had any effect on savings balances, as well as how the training interacted with farmer characteristics and the new savings accounts. We find that the financial training had no effect on farmers' aggregate savings balances, but that for those who attended the training, savings balances follow the promoted seasonal cycle more clearly. Furthermore, we do not observe that farmer characteristics make financial training attendance more impactful on savings behavior, but that these characteristics on their own, seem important in determining savings balances. Surprisingly, we find no evidence that opening a savings account after attending the training reflected any increased gain from the financial training.

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List of Abbreviations

LWH: Land Husbandry, Water Harvesting and Hillside Irrigation
DIME: Development Impact Evaluation Initiative
MFI: Microfinance Institution
FGD: Focus Group Discussion
DiD: Difference-in-Difference
FE: Fixed Effects
FD: First Difference
OLS: Ordinary Least Squares
RCTs: Randomized Controlled Trials

Introduction

Many economic theories, including contradicting theories, have developed over the years attempting to explain the persistent poverty levels seen in Sub-Saharan Africa.[1][2] Despite the disparity in explanations, there is little dispute about the effects of poverty on the lives of people - they eat less than what is recommended, have severe health deficits, own few to no assets or land,[3] and have little education.[4] According to the 2012 census in Rwanda, 44.9 percent of the population lives below the poverty line and 24.1 percent lives in extreme poverty.[5][6] The numbers are even starker for the rural population, of which 48.7 percent are deemed to be living in poverty, compared to 22.1 percent of the urban population. In Rwanda, the most important source of income is agriculture, contributing 36 percent of GDP in 2001-2008 and employing 80 percent of the population. This leaves the average family owning less than one hectare of land, occupied mostly by food crop, of which 2/3 is used to feed the family.[7]

Given these statistics, it becomes evident that agriculture plays an important role for Rwanda's economy and for the livelihoods of the people. Hence, increasing agricultural income is often viewed as an important tactic to spur growth. However, agriculture offers a seasonal income source, based on harvests, with variability that leaves people particularly vulnerable to income shocks. This highlights the importance of finding methods to consumption smooth – being able to weather shocks with limited change in consumption levels - while still promoting agricultural production.[8] Simple savings technologies, such as creating the possibility to save more safely (e.g. lock boxes), have shown some promising evidence of increasing savings, which in turn helps to smooth consumption and reduce the exposure to income shocks.[9] However, these results must be met with skepticism as the introduction of such new savings products has had varying success across countries and a seemingly decreasing impact over time.[10] This emphasizes the need for further research on what types of savings products and/or savings incentives are most useful for people in developing countries, with the long-term goal of consumption smoothing and increased welfare.

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[1] Bloom, David E., Jeffrey D. Sachs, Paul Collier and Christopher Udry. "Geography, Demography, and Economic Growth in Africa." *Brookings Papers on Economic Activity* 2 (1998): 207-295.

[2] Acemoglu, Daron, Simon Johnson and James A. Robinson. "The Colonial Origins of Comparative Development: An Empirical Investigation." *The American Economic Review.* 91.5 (2001): 1369-1401.

[3] Banerjee, Abhijit V. and Esther Duflo. "The Economic Lives of the Poor." *Journal of Economic Perspectives* (2007): 21, 1, 147-167.

[4] Duflo, Esther, Pascaline Dupas, and Michael Kremer. "School Governance, Teacher Incentives, and Pupil-Teacher Ratios: Experimental Evidence from Kenyan Primary Schools." (NBER Working Paper 17939) *Cambridge, MA: National Bureau of Economic Research* (2012).

[5] Republic of Rwanda National Institute of Statistics of Rwanda. *Statistical Yearbook 2012.* Retrieved from the web on the 15.04.2014. http://www.statistics.gov.rw/publications/statistical-yearbook-2012.

[6] Poverty is defined as living below RwF 64,000 per year and extreme poverty is living below RwF 45,000 per year in 2001 purchasing power.

[7] International Fund for Agricultural Development. *Enabling poor rural people to overcome poverty in Rwanda.* (2011). Retrieved from the web on the 29.01.2014.. http://www.ifad.org/operations/projects/regions/Pf/factsheets/rwanda_e.pdf.

[8] Dupas, Pascaline and Jonathan Robinson. "Why Don't the Poor save more? Evidence from Health Savings Experiments." (NBER Working Paper No. 17255), *Cambridge, MA: National Bureau of Economic Research* (2011).

[9] Dupas and Robinson (2011).

[10] Ashraf, Nava, Dean Karlan, and Wesley Yin. "Household decision making and savings impacts: further evidence from a commitment savings product in the Philippines." (Center discussion paper No. 939) *Economic Growth Center* (2006).

The Land Husbandry, Water Harvesting and Hillside Irrigation (LWH) project was initiated by the Rwandan government with the goal of improving agricultural productivity through the adoption of better farming technologies. In one of the pilot regions, Karongi, the Development Impact Evaluation Initiative (DIME) of the World Bank implemented the Rural Finance Evaluation; striving to increase farmers' savings towards agricultural inputs through financial education training and savings account offerings. Over a period of three months we conducted field work in Karongi, Rwanda, collaborating with the World Bank and the local LWH team to collect transaction data from participating Microfinance Institutions (MFIs), to review the World Bank's household survey implemented in the region, and to conduct focus group discussions (FGDs) with farmers, as well as interview MFI staff.

First of all, this thesis aims to establish whether financial education training has an impact on farmers' savings behavior in rural Rwanda. As we discuss in the literature review, the connection between financial education training and savings behavior has been largely based on intuition rather than a solid foundation of scientific research. We contribute to the research field by using a more rigorous approach, using quantitative analysis supported by qualitative data rather than relying solely on a qualitative study.

Secondly, we seek to determine whether formal savings balances reflect the seasonal cycle as we would expect, falling in the month prior to the planting period due to the purchase of agricultural inputs (e.g. seeds). This is relevant for two reasons, to verify that formal savings balances can be considered representative of the savings behavior of farmers by capturing their major expenditures, and to see if savings patterns matched what would be expected if the financial training's promotion of purchasing agricultural inputs was effective.

Thirdly, we seek to identify particular farmer characteristics that contribute to any impact financial training might have on savings behavior. For instance, are women more likely to respond to training? We hope that this will provide insight into which types of educational programs might be most effective in future projects, especially when catering towards particular target groups. To reach this goal, we investigate what impact the introduced treatment had on account balances controlling for farmer characteristics.

Fourthly, we examine the impact of taking up particular types of savings accounts in combination with the financial training. This allows us to consider the possibility that financial training may only be effective if an extra layer of support is provided to customers, or in the other direction, that financial training alone may be beneficial towards increasing savings, but that savings products that are binding may deter saving.

We add to the depth of our findings by varying the post-treatment period to determine whether the potential impact of financial training changes over time. This is crucial for determining whether financial training (alone or combined with the take-up of the savings account products) seem to be correlated with any sustained changes in savings behavior or if observable changes only exist in the short-term; a question that should be seriously considered before promoting any such intervention on a large scale.

We find no evidence that the financial education training provided to farmers had any influence over their savings behavior, either positively or negatively. The findings held fast when accounting for farmer characteristics that may have interacted with the training. Moreover, even when the financial training was combined with the take-up of either of the savings accounts, we find no significant change in account balances. We do, however, observe seasonality in savings balances that follow the expected cycle with the planting periods.

Our thesis will continue as follows: Section 1 will discuss the relevant literature currently available on savings behavior and financial education training. Section 2 will present the hypotheses investigated. Section 3, will provide background of the thesis project and Section 4 will discuss our data characteristics and collection. Section 5 reviews our method and econometric specifications. Next, Section 6 summarizes our results; Section 7 presents a discussion of the results and their meaning in the Rwandan context. We conclude with Section 8.

1. Motivation and Literature Review

Despite the popularity in declaring the importance of financial literacy in order for consumers to make well-informed financial decisions, surprisingly little research has been done to determine the impact of financial education on financial behavior. What research has been done has been largely focused on consumers in developed countries, attempting to navigate complex financial systems.[11] This is clearly not the same situation faced by rural farmers in developing countries. Statements like "the lack of knowledge on modalities to access semi-formal [financial services] appear widespread in rural Rwanda and justify the need for interventions targeting geographical outreach and financial literacy",[12] demonstrate that the policy push seems to already be going through, before any real evidence of financial trainings' effectiveness has been established. This emphasizes the importance of determining whether financial education is really a worth-while investment through more reliable, scientific methods.

Existing research on the topic often combines financial training with the offering of some type of savings or loan product, thus making it difficult to tease out what aspect of the intervention is responsible for what change.[13] This is a challenge that we also have with our data, but is addressed by including controls for those who took up the savings account product and examining how much additional change to savings behavior could be attributed to take-up, rather than just the financial training. Many other papers on the subject make no such attempt to isolate the impact of financial training specifically. One exception to this is Drexler et al.'s study in the Dominican Republic, where they studied the

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[11] Jacob, Kate, Sharyl Hudson and Malcolm Bush. "Tools for Survival: An Analysis of Financial Literacy Programs For Lower-Income Families." *Woodstock Institute* (2000).

[12] Ali, Daniel Ayalew, Klaus Deininger and Marguerite Duponchel. "Credit Constraints, Agricultural Productivity, and Rural Nonfarm Participation: Evidence from Rwanda." (Policy Research Working Paper Series No. 6769) *The World Bank Development Research Group* (2014): 10.

[13] Willis, Lauren E. "Evidence and ideology in assessing the effectiveness of financial literacy education." *San Diego Law Review* 46 (2009): 415-460.

effectiveness of two different types of financial education training.[14] We approach the question differently, testing rather whether different types of people respond to the same type of training differently.

Nonetheless, the more common research question in the field is either more observational, investigating the pre-existing determinants of savings (e.g. gender, education) or product-specific, i.e. what type of savings products might be most useful in developing countries. To approach those research questions Ordinary Least Squares (OLS) regressions are usually used, sometimes in combination with Instrumental Variables[15] as a robustness check, or a Probit model is used to determine the probability of account take-up.[16] We, on the other hand, are interested in assessing whether financial training, by itself, has a meaningful impact on savings behavior, in which case it is possible that the investment by MFIs and Non-governmental organizations in new savings products may not be necessary. Rather, focusing on financial training may be enough to instigate people to attempt to save in a better way.

Research on this topic also must address the question of how one should determine changes in financial behavior based on the limited surveillance power available to researchers, i.e. how to measure financial behavior. The measureable data available may not be a comprehensive assessment of a person's financial behavior; rather it only gives insight (and often limited insight) into the outcomes of such behavior, e.g. account balances, loan activity, etc. Given the structure of our data we follow the common practice of using account balances as a proxy of financial behavior. However, for future research we believe that using transaction frequency as an additional proxy for savings behavior could bring an interesting angle to the subject. The difficulty in using transaction frequency as a proxy, and why we chose not to delve into it for our thesis, would be the complexity of accurately interpreting any observable changes. Moreover, in our samples one of the financial institutions implemented a fee for automatic salary deposits, which then counted as an additional transaction, limiting the comparability of farmers' transaction frequency across the financial institutions.

An additional weakness to past research on this subject is that it often relies on survey data which is dependent on people's own perception of the education attained from financial training and the reported subjective impact it had on their decision making.[17] This is susceptible to positive response bias or social desirability bias, as people tend to exaggerate the effectiveness of the training that they have received.

We are in a position to contribute to existing literature on financial literacy in developing countries by examining unique transaction data rather than relying on subjective survey responses. As our primary data source is from farmers' MFI accounts we can only draw conclusions on changes in formal savings

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[14] Drexler, Alejandro, Gred Fischer and Antoinette Schoar. "Keeping it simple: financial literacy and the rules of thumb." (Discussion paper, Development economics, 7994) *CERP, London, UK* (2010).

[15] Arestoff, Florence, Touhami Abdelkhalek, Najat El Mekkaoui de Freitas and Sabine Mage. "A microeconometric analysis of households saving determinants in Morocco." (Economics Papers No. 123456789/5550) *University Paris Dauphine* (2009).

[16] Ashraf, Nava, Dean Karlan and Wesley Yin. "Tying Odysseus to the mast: Evidence from a commitment savings product in the Philippines." *The Quarterly Journal of Economics* 121.2 (2006): 635-672.

[17] Willis (2009).

(rather than informal savings) that are correlated with financial training. Nonetheless, we argue that this is still of interest to the field as the formal savings are generally considered safer than informal saving practices[18] and are often a requirement for more advance financial transactions (e.g. loans). To get a wholistic view of the farmers' saving, we account for any type of substitution effect across accounts (i.e. ordinary vs. savings accounts) by aggregating our findings. We then use this to evaluate changes over a two year period in formal savings behavior of both farmers who received and farmers who did not receive financial literacy training. We add another layer to this contribution by identifying characteristics of interest that seem to play a role in the correlation between training and changes in savings.

For developed countries, determinants of financial literacy have been investigated to a limited extent. Results from past studies show that people with low education, and in particular women, have a low level of financial literacy.[19] Other research suggests that family income and gender do not determine financial literacy but race and religion do. Moreover, individual motivation seems to determine how well financial education is understood.[20] Research in developed countries also highlight that low financial literacy is considered to be a problem as people affected by it are more vulnerable to unanticipated economic shocks due to health emergencies or job losses.[21] The importance of consumption smoothing is, even more critical in developing countries. An example of research on the relevance of consumption smoothing is Elaina Rose's article which explains how it can be a matter of life and death for poor children in India.[22] This, again, is why the effect of financial literacy and improved savings behavior in general, is of interest to developing countries and for rural Rwandans specifically.

Given the unique access we have to demographic information of the people participating in the evaluation, we can determine which characteristics are most strongly correlated to financial training success (or lack thereof) in Rwanda. This could contribute to existing literature on poverty evasion through micro-savings in developing countries, particularly Sub-Saharan African countries. There are currently so little consistent findings on the best way to influence savings behavior, whether it be through cooperation methods like Rotating Savings and Cooperative Associations (ROSCAs) or addressing time inconsistencies in peoples' behavior through product features, that it seems apparent that trainings and products need to be more customized not only to the setting in which they are introduced, but to the target group as well.[23] We hope our findings can add to this. It might also be interesting to note if findings in developed countries on the matter are similar to our findings from this different setting.

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[18] Wright, Graham A.N. and Leonard K. Muteesassira. "The relative risks to the savings of poor people." *Small Enterprise Development* 12.3 (2001): 33-45.
[19] Lusa Lusardi, Annamaria. "Financial Literacy: An Essential Tool for Informed Consumer Choice?" (NBER Working Paper No. 14084) *Cambridge, MA: National Bureau of Economic Research* (2008).
[20] Mandell, Lewis and Linda Schmid Klein. "Motivation and financial literacy" *Financial Services Review 16* (2007):105-116.
[21] Jacob et al. (2000).
[22] Rose, Elaina. "Consumption smoothing and excess female mortality in rural India." *Review of Economics and Statistics* 81.1 (1999): 41-49.
[23] Vonderlack, Rebecca M., and Mark Schreiner. "Women, microfinance, and savings: Lessons and proposals." *Development in Practice* 12.5 (2002): 602-612.

Thanks to the set-up of the Rural Finance Evaluation we are also able to add to the current discussion of specific saving products targeted towards poorer populations, a question that has becoming increasing popular over the past twenty years.[24] The question of people's time preferences negatively influencing their savings decisions via hyperbolic discounting[25] and the possibility of financial products that can help counter such inefficiencies are also topics that are appearing more often. This is particularly relevant for our study as the farmers in our sample were found to be present biased.[26] We briefly touch upon this discussion when examining the two products (Targeted savings accounts and Pre-commitment savings accounts) offered to the farmers we study, explained in more detail further on. Similar topics have been addressed in other countries, investigating the outcome of commitment accounts.[27] [28] However, the field is relatively new in Rwanda and more research needs to be done to understand its potential in this particular context. As a country that has developed astoundingly quickly over the past twenty years, overcoming a horrific genocide, it is probable that this setting may present slightly different challenges as compared to countries that are similar in other respects.

2. Hypotheses

Our thesis addresses four main hypotheses starting from the primary question of whether financial training is correlated to a change in savings behavior, to whether savings balances follow the seasonality supported by the financial training, to what characteristics we expect to underscore any correlation between financial training and savings behavior, and finally to the impact on savings behavior if a particular savings account is taken-up after attending training.

1. Financial training has a positive effect on savings behavior manifested in the form of higher aggregate account balances after the training.

This hypothesis is largely inspired by the findings in related literature that there is "a strong association between understanding financial concepts, better financial decisions and household well-being".[29] What we seek to investigate, and what is as yet largely unverified, is whether financial training is successful in increasing financial literacy, and then in turn, increasing savings.[30]

2. Savings will follow the seasonal cycle, meaning that farmers save up to one month prior to the planting period and then in the month prior to planting they purchase agricultural inputs.

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[24] Fischer, Gregory and Maitreesh Ghatak. *Spanning the chasm: uniting theory and empirics in microfinance research*. In: Armendáriz , Beatriz and Labie , Marc, (eds.) The Handbook of Microfinance. World Scientific Publishing, London, UK (2011).

[25] Laibsen, David I. "Hyperbolic Discount Functions, Undersaving and Savings Policy." (NBER Working Paper No. 5635). *Cambridge, MA: National Bureau of Economic Research* (1996).

[26] In the household survey farmers were asked questions indicative of their time discounting. One such questions was: if farmers were offered 2.000RWF today or 2.500RWF in one month vs. 2.000RWF in 6months or 2.500RWF in 7months, 83% of farmers chose the money today but only 77% choose the money in 6 months.

[27] Ashraf, N. et al. (2006a).

[28] Brune, Lasse, Xavier Gine, Jessica Goldberg, and Dean Yang. "Commitments to save: A field experiment in rural Malawi." (Policy Research Working Paper Series No. 5748) *World Bank* (2011).

[29] Drexler et al. (2010): 3.

[30] Willis (2009).

According to Hypothesis 2 we would expect to see a decrease in savings balances in the month prior to planting, compared to the period up to that month. Evidence for when purchases of agricultural inputs occur for the planting period in Karongi comes from World Bank reports, interviews with Tubura[31] staff, and our FGDs. This behavioral pattern was also supported by the financial training, which stressed that farmers should save as much as possible until shortly before the planting season, in order to have the necessary funds to purchase agricultural inputs.

3. The correlation between account balances and financial training will change dependent on client characteristics.
   a. Financial training will be correlated with a larger positive impact on savings for women than for men.[32] [33]
   b. Financial training will be correlated with a larger positive impact on savings as the person increases in educational attainment.[34]
   c. Financial training will be more positively correlated with savings for wealthier clients.[35]
   d. Financial training will be more positively correlated with savings up to a certain age, after which training begins to be negatively correlated with balances.[36]
   e. Financial training will be negatively correlated with savings balances as the household size increases.[37]

Our hypotheses regarding how various characteristics will interact with financial training to ultimately correlate with changed savings behavior is largely influenced by previous research done on pre-existing determinants of saving. In other words, we generally believe that the characteristics which make people more likely to save will also react positively to financial training.

4. Financial training will be correlated with higher aggregate account savings when a savings account was opened. However, this increase in aggregate savings will be more prominent for clients who took-up the pre-commitment account.[38]

We reason that the act of opening a savings account is, in and of itself, a sign of interest in saving at a formal institution. This interest could be because of associated safety with saving at a formal institution as well as the interest rate of 5% applied to the savings accounts. Either way, we argue that the people who are interested enough in the savings process to take the step of opening an account and to pay for

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[31] Tubura is the Rwandan branch of OneAcre Fund and the largest micro-loan organization in the area for agricultural inputs (e.g. fertilizer and seeds).

[32] Dupas, Pascaline, and Jonathan Robinson. "Savings constraints and microenterprise development: Evidence from a field experiment in Kenya." (CEGA Working Papers Series No. WPS-008) *Center for Effective Global Action. University of California, Berkeley* (2010).

[33] Ashraf et al. (2006b).

[34] Bendig, Mirko, Lena Giesbert and Susan Steiner. "Savings, credit and insurance: Household demand for formal financial services in rural Ghana." (Working Paper No. 94) *German Institute of Global and Area Studies* (2009).

[35] Arestoff et al. (2009).

[36] Bendig et al. (2009).

[37] Arestoff et al. (2009).

[38] Brune et al. (2011).

it,[39] are more likely to save than those who did not open an account. However, the opening fee of 3,000 RwF could have been a deterrent to the poorest farmers even if they had initially been interested. This could give rise to a positive correlation between taking up a savings account and a higher aggregate account balance; i.e. the people taking up a savings account were richer to begin with. We expect this positive correlation to be stronger for farmers who took-up Pre-commitment accounts as those who are wealthier, understand the product description better, and might be more willing to tie-up some of their money,[40] while poorer farmers would be hesitant to place any binding constraints on accessing their savings in case they were to incur an unexpected shock. Remembering our first hypothesis however, this would mean that although poorer people are deterred from opening an account due to the fee, they would still benefit from the financial training independent of whether a savings account was taken up or not.

3. Background

3.1. Rwanda- The Rural Finance Evaluation
Financial systems that are available in the Rwandan farming areas we are interested in primarily consist of savings and credit cooperatives (SACCOs) or other Micro Finance Institutions (MFIs) (e.g. institutions which bundle loans with fertilizer purchase, in this case Tubura). In 2011, less than 20% of the rural population saved at a financial institution in the past year.[41]

Our research is based on the LWH project conducted by the government of Rwanda in collaboration with the World Bank's DIME team and the Global Agriculture and Food Security Program (GAFSP). As part of Rwanda Vision 2020, the government views the promotion of agriculture as a means of stimulating the economy and reducing poverty. Thus, the aim of LWH is to improve agriculture methods via 1. Capacity Development and Institutional Strengthening for Hillside Development; 2. Infrastructure for Hillside Intensification; and 3. Implementation through Ministry of Agriculture and Animal Resources (MINAGRI's) SWAP structure.[42] In an effort to transition from subsistence farming to cash crop farming, inputs have been largely subsidized by the government to ease the financial burden for the farmers. However, this will not be sustainable in the long-run and therefore one prong of the research (the Rural Finance Evaluation) being done in the project relates to how to increase farmers' ability to afford inputs on their own, particularly without taking on a loan.

Saving is especially crucial for farmers in Rwanda as the farming seasons are so close together, even partly overlapping. In western Rwanda, in the Karongi district, where our study takes place, season A starts in late August or early September and continues until harvest in late January or early February, while season B starts in early February and the harvest takes place in June (see Graph 4). Moreover,

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[39] There was an opening fee charged at both MFIs.
[40] Hastings, Justine S. and Lydia Tejeda-Ashton. *Financial literacy, information, and demand elasticity: Survey and experimental evidence from Mexico*. (NBER Working Paper No. w14538) *Cambridge, MA: National Bureau of Economic Research* (2008).
[41] Savings rate for rural population aged 15+ in 2011, according to the Global Financial Inclusion Index.
[42] Development Impact Evaluation Initiative (DIME). *LWH Impact Evaluation Concept Note*. Internal document. World Bank (2013).

crops planted in season A often require additional post-harvest treatment, e.g. drying for beans and maize. Consequently, revenue from harvest A is often not available to buy inputs for season B suggesting that the money used for any such purchases stems either from loans or savings. However, as SACCOs and MFIs are relatively new to giving out loans, they still do not have the capacity to resolve this problem on their own, but the market is growing. According to the FinScope 2013 survey the proportion of adults saving with SACCOs and MFIs increased from 3% in 2008, to 25% in 2012.[43] Also, in Karongi, Tubura is available to farmers to receive fertilizer and certain types of minerals on credit. Nonetheless, credit accessibility is limited.[44] Accordingly, the rural finance component of the project promoted increased saving, particularly with a goal of purchasing inputs, by farmers via different approaches which are introduced subsequently.

3.2. Rural Finance Evaluation Set-Up

The LWH project was introduced in Karongi in 2010 and the Rural Finance Extension of the evaluation began in 2012. The evaluation took place in the LWH Karongi sites 12 and 13 which cover four districts and nineteen villages. These sites were chosen because LWH was already established in this region and there were MFIs with a sufficiently large client base with which DIME could collaborate, namely SACCO Mukura and COOPEC Inkunga. The savings products were rolled-out and introduced to the financial institutions in early 2012, and farmers were introduced to the savings accounts in a one-time, interactive lecture during a four-week period starting in mid-June 2012.[45] This should have allowed farmers to save towards agricultural inputs for season A 2013 for approximately 2-3 months. DIME used a Randomized Control Trial (RCT) approach to measure the impact of offering farmers participating in LWH the different types of savings products. Flyers were given to the farmers according to the treatment group they have been assigned prior to the training to encourage participation. Three groups were created as outlined below. Note that our agreement with the World Bank included that we are not permitted to use the data from group 1, as the World Bank's research is focusing on the impact of the introduction of the saving accounts. Therefore, we focus instead on the impact of the financial training provided to group 2 and 3.

1. Training in Agricultural Financial Planning and Savings Awareness Campaign (only)
2. Training in Agricultural Financial Planning and Advanced Savings Awareness Campaign + Targeted Savings Treatment (with account promotion and registration)
3. Training in Agricultural Financial Planning and Advanced Savings Awareness Campaign + Pre-Commitment Savings Treatment (with account promotion and registration)

The training in agricultural financial planning was provided by agronomists to all farmers in the region as they were part of the LWH terracing project. The set-up of the saving awareness campaign was such that instead of a traditional lecture, the participants were encouraged to actively participate in a discussion

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[43] Access to Finance Rwanda. *FinScope Rwanda 2012*. Technical Report. Access to Finance Rwanda, January 2013. Retrieved from the web on the 01.05.2014. http://statistics.gov.rw/publications/finscope-survey-report-2013.
[44] Ali et al. (2014).
[45] Timing of the special accounts introduction as noted in the internal training material for the LWH staff.

that was steered by the presenter. The savings awareness campaign (hereof called 'financial training') differed between the three groups in that control group received a lighter version of the financial training offered to groups 2 and 3. Group 2 and 3 were additionally trained on the benefits of saving for agricultural inputs with an emphasis on how to realize it. The goal of this financial training was to convince the attendants that saving for agricultural inputs is crucial as farming is their main income source. Loans as an alternative source for input purchases were discussed as well, however it was stressed that interest had to be paid on loans and, most importantly, that loans for agricultural inputs are risky as there is always a chance of crop failure, increasing the probability of a default. To give farmers hands-on-examples on how to save, simple calculations on upcoming expenses were combined with practical suggestions on how to save ahead. The examples were made applicable to the farmers' lives as they concerned expenses such as clothing and alcohol. Moreover, simple calculations illustrated how much money is actually spent on seemingly small purchases, namely beer, if accumulated over time. In the last step, the respective account types - Pre-commitment and Targeted saving accounts - and how to use them in the most beneficial way were explained to the farmers. More information on the account specifics and trainings can be found in the appendix under Scripts for savings training, pre-commitment for inputs, and targeted savings for inputs.

Farmer groups were organized by DIME by randomly selecting farmers in each participating village. Each farmer group consists of approximately 20 households, with 4 people per household on average. 80 established farmer groups were registered with a group account with the MFIs and randomly assigned to one of the three treatment arms (stratified by site):[46]

1. Agricultural Financial Planning and Savings Promotion (20 groups)
2. Targeted Savings Treatment + Training in Agricultural Financial Planning and Savings Awareness Campaign (20 groups)
3. Commitment Savings Treatment + Training in Agricultural Financial Planning and Savings Awareness Campaign (40 groups)

Out of this sample that encompassed 1600 people, 728 individuals actually had and/or signed up for an individual "ordinary" account. This aligns fairly well with the statistics of the country where approximately 39.4 percent of households have at least one savings account.[47] Despite Karongi being one of the poorest regions of the country, with 48.4 percent of its population living in poverty,[48] we find the proportion having an ordinary account is slightly higher in our sample, at 45.5 percent of the participants. This higher ratio is likely due to the general promotion of having a formal account via the intervention. Having an individual account was a necessary step to open up one of the savings account offered through the intervention. Farmers were only able to sign-up for the savings product that was randomly assigned to their specific group.

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[46] DIME included more farmer groups in the third treatment arm to increase the statistical power of their study.
[47] Republic of Rwanda National Institute of Statistics of Rwanda. *Statistical Yearbook 2012.*
[48] *Ibid.*

To establish that the randomization had been done correctly, DIME conducted a balance check across the treatment arms. The results are seen in Appendix Table 1. The only variable for which there was a significant difference in the average mean between those that received only financial literacy training and those that were offered savings accounts was pesticide, and that was at the 10% level. Thus, we proceed accepting that the randomization was done correctly.

4. Data

In order to give as much context as possible to our analysis we examined three types of data that will be described in more detail in the following sections:

- Household surveys
- Transaction data from the SACCOs
- Data from Focus Group Discussions (FGDs) and interviews

4.1. Quantitative Data

Household surveys

From June through July 2013, i.e. one year after the intervention, DIME collected 1,019 comprehensive household surveys of participants assigned to a treatment in the LWH project. These household surveys give us detailed information about those participants; including information about their financial flows, land holdings, assets, work practices, education, and nutrition (measured by food consumption). Having access to that detailed data for an array of variables, we had to make a decision about which include in our analysis. As our topic of interest is savings behavior– examined via savings balances -, it could be insightful to look at income and expenditure variables to determine whether there is any connection between pre-existing wealth and savings behavior. However, as surveys rely on self-reported data, there is often a bias in values such as income and expenditures. Especially for those living close to subsistence levels, it has been documented that reported income and expenditures on discretionary goods, e.g. alcohol and tobacco, are often underreported. Moreover, part of the difficulty in reporting income at this level is in valuing agricultural output. That is, how to account for the fact that agricultural output is both produced and consumed within a household, the so called autoconsommation. First, it is difficult to disentangle those two. Additionally, it is sometimes challenging to estimate the value of the goods properly if markets are not well-developed.[49]

An additional consideration when using this data is that the household data was collected regarding a five month period to cover one entire agricultural period (season A 2013). This is problematic for different reasons, first, the recall period for respondents is considered long when asking questions about income and expenditures over a time span longer than a couple of months and as in this case it spans over five months. This increases the risk of misreported value. Second, survey outcomes are sensitive to

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[49] Deaton, Angus. "The analysis of household surveys : a microeconometric approach to development policy" Washington, D.C. : The World Bank (1997). Retrieved from the web on the 15.01.2014. http://documents.worldbank.org/curated/en/1997/07/694690/analysis-household-surveys-microeconometric-approach-development-policy.

the time of year, particularly in an agricultural setting, as responses will depend on what savings and loan mechanisms are available to help people smooth consumption (e.g. borrowing from friends and family); and this fluctuates over time.[50] Given that the people in this sample are all farmers, they are all subject to big seasonal fluctuations which are only partially captured with a five-month period. Because of these factors, extrapolation of the financial data would not be reasonable and as no baseline survey was conducted, we are not able to track changes over time. Therefore, we focus on time-invariant descriptive characteristics of the participants and on the indirect welfare measurement plot area ownership, as this influences the main income source, namely agricultural output. We argue that the latter should not have changed as a result of the treatment itself as land ownership is rather stable over time given the pre-dominant father-to-son inheritance tradition.[51] The household survey data is critical for determining any constant characteristics of farmers who exhibit a particular response to financial training and change savings behavior.

It is also important to note that survey data can be unreliable since there is always the possibility of errors due to measurement and the implementation of the survey. However, we believe that the probability of measurement errors in the data relevant to our analysis is low, since in 94% of the surveys the household head responded about his/her own characteristics which he/she should be perfectly aware of (i.e. the respondent is the account holder of interest) and the other variables are time-invariant data. Nevertheless, implementation errors can still exist due to either unsatisfactory training of the enumerators or poor execution, and unfortunately these possibilities cannot be accounted for.

Table 1: Definition of Explanatory Variables

Variable	Description
After intervention	The post-treatment time period. Run with time periods of 1 month, 2 months, 13 months, and 14 months after the treatment in June 2012
Attended training	Dummy variable, 1 if a person attended the financial training and 0 if they did not attend
DiD coefficient	Interaction between After intervention and Attended training, i.e. After intervention*Attended training
Account holder is male	Dummy variable, 1 if a person is male and 0 if a person is female
MFI is COOPEC	Dummy variable, 1 if the MFI where the person has their account is COOPEC Inkunga and 0 if it is at SACCO Mukura
Site	Dummy variable, 1 if the site where the farmer lives is Karongi 13 and 0 if they live in Karongi 12
Assigned to Targeted treatment	Dummy variable, 1 if farmer was assigned to treatment group 2 – to be offered a Targeted savings account and 0 if they were assigned to treatment group 3 – to be offered a Pre-commitment savings account
Has a savings account	Dummy variable, 1 if the farmer opened a Targeted or a Pre-commitment savings account

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[50] Deaton (1997).

[51] Isaksson, Ann-Sofie. "Unequal Property Rights - A study of Land Right Inequalities in Rwanda." (Working Paper in Economics No. 507) *University of Gothenburg, Dept of Economics* (2011).

Age	Age of the household survey respondent – reflective of the age of the account holder
Age squared	Age of the household survey respondent squared

Highest level of education completed	Educational attainment of the household survey respondent – reflective of the account holder. Takes on the respective values:
	      1 if no education
	      2 some primary
	      3 completed primary
	      4 some secondary
	      5 completed secondary
	      6 some university
	      7 completed university
	      8 vocational training

Number of HH members	Number of household members
Plot area owned in square meters	Owned plot area of the first three plots described in the household survey, measured in square meters.

Savings Account	Definition varies for column:
	      Column (1): Farmer has either a Targeted savings account or a Pre-commitment savings account
	      Column (2): Farmer has a Pre-commitment savings account
	      Column (3): Farmer has a Targeted savings account

Regression 5: DiD coefficient	Interaction between After intervention and Savings Account, i.e. After intervention*Savings Account

Table 2: Summary Statistics of Data

Summary Statistics of Variables Used

Variables	# of observations	Mean	Std. dev.	Min	Max
Savings in RwF	16,857	12,818.26	100,272.10	-28,070.00	4,561,933.50
Attended Training	491	0.78	0.42	0	1
Account Holder Male	491	0.61	0.49	0	1
MFI is COOPEC	556	0.07	0.25	0	1
Site	491	7.61	0.49	0	1
Assigned To Targeted Treatment	491	0.37	0.48	0	1
Has Savings Account	556	0.35	0.48	0	1
Has Commitment Account	311	0.41	0.49	0	1
Has Targeted Account	180	0.27	0.45	0	1
HH Head Age	491	42.97	13.86	21	83
HH Head Education	489	1.99	1.01	1	8
#HH members	491	3.81	1.66	1	10
Plot Area In M^2	556	6,771.37	69,076.11	0.00	1,618,800.00

MFI transaction data

Over a one month period in early 2014, we collected data on all MFI transactions that occurred within 32 months from June 2011-December 2013 on both ordinary and savings accounts for 721 clients (inclusive of account holders from group 1 treatment – not offered a saving account).[52] We digitalized the data from SACCO Mukura as the MFI currently keeps only a hard-copy format of clients' files. We then reconciled this data, using the readreplace command in STATA,[53] with the transaction data that had previously been collected by LWH for the period from June 2011-June 2012. All discrepancies between the two entries were investigated and when necessary we returned to the institution branches to refer to the original hard-copies. Data from COOPEC Inkunga was sent to us via mail as this financial institution already digitalizes its activities.

Using the starting balance and the data on deposits and withdrawals, we calculated the balances on days for each individual. We then calculated the mean of the month's balance by, if applicable, summing up the daily transaction balances and dividing them by the number of days for which there was at least one transaction. This was done for each individual. In the case that no transactions were undertaken during one month, the balance of the last month was duplicated until movement on the account occurred again. In case the accounts were opened later than June 2011, the transaction months leading up to the first transaction show empty values. This results in an unbalanced panel for the transaction data. Moreover, as we have data for both the ordinary and saving accounts, we could distinguish between the three types of accounts (i.e. ordinary, Targeted savings, and Pre-commitment savings). However, it might be that savings under the surveillance period were shifted from one account to the other (substitution effect). Therefore, we decided to look instead at aggregate savings (i.e. savings on both savings accounts and ordinary accounts together) to account for this. Another reason to look at the aggregate savings is that there may have been discrepancies in how the account restrictions were implemented and how they there intended to work, thus blurring the effective difference between the Targeted and the Pre-commitment savings accounts.[54] As the intention of this thesis is to first determine if there is any change in savings behavior from financial training, aggregate savings balances will capture this in more wholistic view.

Because of the thorough verification process that was applied to the transaction data, we feel confident that the balances that appear to be outliers are not caused by errors in the data collection but are rather

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[52] Note that the MFI staff was unable to locate seven account files which is why our data collection includes information on 721 accounts rather than the reported 728 accounts that had an individual account.

[53] The readreplace command is used in combination with the cfout command. These allow two data files to be compared in STATA, all discrepancies over specified variables are exported to an excel file which is manually reconciled. Then the correct values from the excel file are imported into the data file of choice, replacing the previous incorrect values.

[54] The incentive to actually use the savings account was the offered interest rate of 5%. However, as SACCO Mukura calculated balances manually on paper files, the interest rate was not added in a continuous manner but rather once a year. As we discovered in the FGDs, people were not aware of this approach and hence assumed that the interest would not be paid out. Also, the implementation of the Pre-commitment account had additional flaws. As it was a new product, the MFI managers did not want to deter customers from using the account by enforcing the restriction to access their funds until the planting period as intended. Hence, the farmers were allowed to withdraw the savings on the Pre-commitment account earlier than specified but had to face the punishment of not being paid out the accumulated interest.

correct but extreme, otherwise known as genuine outliers.[55] We stipulate that positive outliers are driven by unique individuals whereas negative outliers are due to calculation errors on the part of the MFI itself that were later addressed in their end of the year balance calculation checks. The transaction data is our primary data source and is used to compare savings behavior over time in terms of magnitude of balances.

Note that throughout the analysis, we refer to the level of balances on the account. The currency used are Rwandan Francs (RwF) and currently 1,000 RwF correspond to 1.1 € and 9.6 SEK.[56] Furthermore, accounting for inflation for comparability reasons is not necessary in our sample as the same inflation rate is being applied to everyone in the sample. Thus, we are looking at the nominal value of the balances. Throughout 2011 to 2013, the yearly average inflation was equal to 5.4%.[57]

Merging the household survey with the MFI transaction data
The household survey and transaction data were merged on the unique individual account number since the household survey used the account number as an identifying characteristic. Using this approach, we were able to merge 491 farmers with data from both data sets. This left 65 accounts from the treatment group which could not be attributed to any participant from the household survey. It is important to note that we are only examining clients who were offered a savings account, i.e. farmers who were in treatment groups 2 and 3 in accordance to the World Bank project set-up. As part of our agreement to collaborate with the World Bank and LWH on this project we agreed to limit our research to these groups and therefore do not have access to the data of the most obvious control group (group 1 which only received basic financial training and was not offered any type of savings account). We discuss the data specification and limitations in more detail in the method section, as well as the steps we took to verify that we could, in fact, split the farmers in these groups in such a way to still get a pseudo-control group for our analysis.

Given the outcome of our merge, we will use 556 accounts (all account holders who were assigned to group 2 or 3 treatment, i.e. offered a savings account) for descriptive savings-related purposes but focus on the 491 individuals for whom we could merge the data sets, for the characteristics analysis. As there is the possibility that not being in the survey is due to a systematic reason, we check whether savings, the only common available variable for the merged and non-merged group, are significantly different. Conducting a two-sided T-Test, the difference is not quite statistically significant at a level of 10% (see Appendix Table 2 for the results). Moreover, the standard deviations of both groups are very large compared to their means, suggesting large volatility. Lastly, the 'did not merge' sample has higher mean savings than the group that merged, this means we are studying a poorer sample, which is fundamentally the group that is of interest for such an intervention. Hence, we conclude that we can continue with the merged dataset as a satisfactory sub-sample. Throughout the regressions the number

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[55] Ghosh, Dhiren and Andrew Vogt. "Outliers: An Evaluation of Methodologies." Section on Survey Research Methods. *Joint Statistical Meetings* (2012): 3457-3460.

[56] Exchange Rates. Retrieved from the web on the 24.04.2014. http://www.exchangerates.org.uk/.

[57] International Monetary Fund. *World Economic Outlook April 2014*. Retrieved from the web on the 24.04.2014. http://www.imf.org/external/datamapper/index.php.

of accounts may be further reduced as the household survey does not always report all values for all controls implemented. For a visual overview of the organization of the data and the number of uptakes (in parenthesis below the respective arm), see Graph 1.

Graph 1: Rural Finance Evaluation Set-Up

Source: Authors' Illustration

4.2. Qualitative Data

Focus Group Discussions and interviews

Over a two week period in March 2014 we conducted four FGDs with farmers, two cashier interviews, and two manager interviews. To facilitate the organization of the focus groups, farmers were selected by treatment and/or interesting savings behavior found controlling for the zone in which they lived. [58]

The first FGD took place in Ruhuha, where a high increase in mean savings balances was observable during the two years as compared to the other zones. In Rusasa little changed with regards to the

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[58] The FGD and interview templates as well as protocol can be found in the appendix.

savings balances and hence the second FGD was conducted there. Moreover, as Rusasa also had the highest number of COOPEC clients they were also part of the second FGD.[59] The last two FGDs took place in Kagano and Rugabano and included farmers who were specifically in either in the Pre-commitment or Targeted account treatment whereas in the other zones we invited farmers who could also have been from group 1 in the DIME set-up. The participants for each group were randomly selected from the list of the treatment participants, controlling for these specified characteristics. The goal was to have 8-10 participants in each group, hence 12 were randomly selected as non-attendance was a concern. For a summary of the FGDs' selection criteria and description, see Table 3 below.

Table 3: Overview of Focus Group Discussions

Zone	Interesting characteristic	Financial institution	Attendance (male)	Duration
Ruhuha	High increase in savings balances	SACCO Mukura	5 (3 male)	1:04h
Rusasa	Little change with respect to savings balances	SACCO Mukura COOPEC Inkunga	17 (10 male)	1:19h
Kagano	Only famers assigned to Pre-commitment or Targeted Treatment	SACCO Mukura	8 (1 male)	1:00h
Rugabano	Only famers assigned to Pre-commitment or Targeted Treatment	SACCO Mukura	5 (3 male)	0:49h

Finally, we also conducted interviews with the MFIs' staff for the purpose of providing further context to our quantitative data. The cashier interviews were conducted with one cashier at a time, with the assistance of a translator. The interviews were conducted at the branches of each financial institution where we worked. We also conducted two manager interviews, one with the SACCO Mukura manager and one with the COOPEC Inkunga manager. Each interview took about one hour.

5. Method

5.1. Identifying a Control Group

As mentioned before, when deciding how to approach our analysis we were limited by the set-up of the project and by the data we were given permission to use for our evaluation. Not being able to use the

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[59] Note that the two FGDs in Rusasa were intended to take place separately. However, due to the organization, everyone arrived simultaneously and the discussion took place with everyone included.

natural control group (group 1), we examined the groups that were offered a savings account. Looking at only farmers who had received some sort of treatment does not immediately allow for a control group as one might desire. Nonetheless, we determined that we could tease out a counterfactual group to those who actually took up the treatment (i.e. attended financial training) in those that did not take-up the treatment. In other words, we are examining the actual treatment effect rather than the intention to treat. Of course, this allows for self-selection of the treatment and one has to consider the possibility that only people with similar characteristics might choose to attend the financial training.

To verify that farmers who attended training and farmers who did not attend training are in fact comparable, we look at their characteristics before the financial training intervention in June 2012. Table 4 presents the comparison results of those that attended training and those who did not, by testing the means of time-invariant observable variables from the follow-up survey and the savings balances prior to the intervention (i.e. from June 2011-May 2012). Most variables are defined as dummies taking the value one for the specified variable name and zero otherwise. Exceptions are mean savings, age, education, variables with a pound sign and plot area as these variables are defined in RwF, count, years, categorical values for education level completed, counts and square meters, respectively. As those variables date from pre-treatment and/or are independent of the treatment, any statistically significant difference between the treatment and non-treatment groups would reflect non-random self-selection into the treatment (i.e. self-selection bias). We found no evidence of such a self-selection bias.

Table 4: Summary Statistics According to Treatment Status Prior to Treatment

Summary Statistics Prior to Treatment

Variables	# of obs.	No Mean	Std dev.	# of obs.	Yes Mean	Std dev.	Mean difference	(p-value)
				Attended				
Account Related								
Mean Savings	1,228	8,886.94	34,843.77	4,477	9,906.80	38,080.01	-1,019.86	0.40
Account Holder Male	110	0.10	0.30	381	0.07	0.25	0.03	0.27
MFI: Coopec	110	0.56	0.50	381	0.62	0.48	-0.06	0.25
Household Head Related								
Age	110	42.47	12.16	381	43.12	14.32	-0.65	0.67
Education	109	1.94	1.00	380	2.00	1.01	-0.06	0.62
Family Structure								
#HHmembers	110	3.85	1.69	381	3.80	1.65	0.05	0.79
Wealth Indicator								
Plot Area	110	4,978.78	15,602.57	381	8,444.14	82,991.01	-3,465.35	0.66

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

Training participants have on average 1,019 RwF more on their accounts prior to the training than people not attending the financial training. However, this difference is not statistically significant. The household heads' characteristics do not differ between the treatment and non-treatment group. Moreover, the number of household members and the plot area owned does not differ at a statistically

significant level between the two groups. Since there is no observable variable demonstrating any significant difference between the two groups (attended vs. not attended training), we proceed using the "did not attend training" group as our control group.

Given the small sample size (recall that there are 491 accounts with both transaction and personal characteristics data from the household survey available) our regressions are vulnerable to large standard errors that make precise statistical inference difficult. Furthermore, there is always the concern that there might still be unobservable characteristics that are not captured by our controls and cause a bias in our results, i.e. Omitted Variable Bias (OVB). As explained in more detail in our method section, we apply both individual fixed effects and state fixed effects in our regressions to account for any such unobservable, as well as observable, variables. The statistical method chosen for the analysis also accounts for time-invariant characteristics to reduce the risk of OVB.

5.2. Methodology

It is once again worth discussing, that despite the original RCT design of the project, which supported a counterfactual to measure the impact of being offered one of the new savings accounts, we look at the treatment groups only. RCTs, although arguably the best currently available method for claiming causality when examining treatment interventions, are not flawless. A growing concern is that there may be an overly optimistic outlook of the external validity from many of these experiments. It is therefore important to continue doing research in these fields using different methods as robustness checks before implementing large-scale interventions.

Once we determined that we could use non-attendees as a control group we had to select our econometrical method of analysis. Recall that the MFI transaction data comes in the form of an unbalanced panel; and our selection of variables from the household survey provides time-invariant variables from one point of time (i.e. June/July 2013). Our aim is to see whether financial training is associated with changes in people's saving behavior and which farmers' characteristics are correlated to this success. Note that ideally, we would like to observe all behavioral change in savings after the financial training. However, as this is not possible[60], we proxy the behavioral change using the available data. As the financial training focused on savings at MFIs, we examine aggregate account balances before and after the training to determine if there was indeed a change in savings behavior.

The simplest analysis of panel data would rely on the pooled ordinary least squares (OLS) estimation with regression (I). Note that we use Roman numerals throughout the methodology section to distinguish the equations in the theoretical discussion from the regression specifications used for the analysis.

$$sav_{it} = \beta_0 + \gamma D_i + \beta_1 X_{it} + \varepsilon_{it} \text{ (I)}$$
$$where \ i \ \epsilon \ \{1, 2, \ldots, 491\} \ and \ t \ \epsilon \ \{June_{2011}, July_{2011}, \ldots, December_{2013}\}$$

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[60] Willis (2009).

In this regression, sav_{it} stands for average savings of individual i at time t, β_0 is a constant, D_i is a dummy for training attendance, X_{it} is a vector of control variables, and ε_{it} denotes the error term. The error term can be separated into two components, namely the time-invariant and the time-varying component.

$$\varepsilon_{it} = \alpha_i + u_{it} \text{ (II)}$$

Here, α_i represents an unobservable, time-invariant error term component, which captures all time-invariant omitted variables (i.e. bias that is homogenous across time). Applied to our data, although we find that based on *observable* characteristics training attendance was equivalent to a random selection of attendance (see Table 4), if treatment participation was, in fact, non-random and was determined by time-invariant *unobservable* characteristics, this error term component would capture it.

When this error term component is correlated with the independent variables, unobserved individual level data can bias the regression. Fortunately, this can be accounted for. The pooled OLS assumes that $\alpha_i = \alpha \ for \ all \ i = \{1, 2, ..., 491\}$ i.e. the time-invariant bias is the same for all individuals. If that does not hold and α_i does vary across individuals, *i*, the OLS estimates will be biased and inconsistent. We believe that time-invariant unobservable factors vary across individuals. For example, if a person's general disposition is to be fiscally conservative they are likely to remain fiscally conservative over time, avoiding risky investments and taking care to live within their means; this would not be the same behavior we would expect to see from a person more comfortable with risk. Thus, the simple OLS regression would give us inconsistent and biased estimators.

The panel data structure of the data can help us relax the strong OLS assumption $E(\varepsilon_{it}|X_{it}) = E(\alpha_i + u_{it}|X_{it}) = 0$. In particular, the usage of multiple periods allows us to difference out α_i as is it assumed to be constant over time. This in turn enables us to replace the assumption that $E(\varepsilon_{it}|X_{it}) = E(\alpha_i + u_{it}|X_{it}) = 0$ with the weaker assumption, $E(u_{it}|X_{it}) = 0$. Simultaneously, this step also allows α_i to be correlated with other regressors, e.g. with the training participation. There are two main methods of differencing out α_i, the Fixed Effects method and the First Difference method. Using the Fixed Effect (FE) model, one would account for time-invariant unobservable factors by demeaning all the variables in the regression. The First Difference (FD) model subtracts the respective previous period from the current period to remove the time-invariant error term component. However, as both the FE and the FD model remove time-invariant variables, we cannot use either of these methods as one of our goals is to study the impact of fairly constant individual characteristics (e.g. wealth and education) on the effectiveness of financial training. After reviewing the current literature and methods that are available for examining this kind of data we found that a Difference-in-Difference (DiD) would be the best method. The DiD approach is chosen here as we have two distinct groups that can be compared - attended training and did not attend training -, an intervention particular date, and time-invariant characteristics.
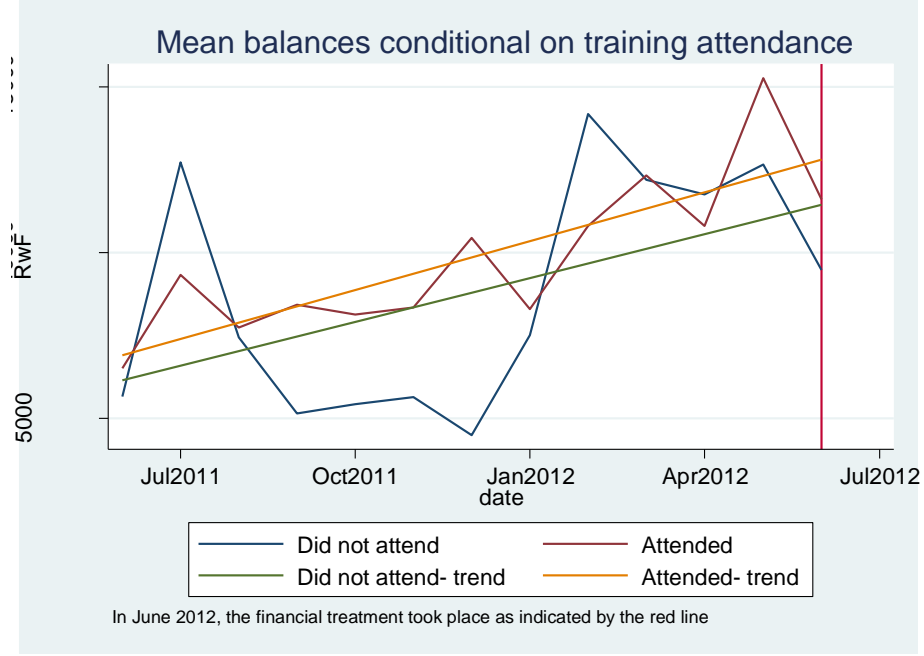
First, a DiD regression controls for non-random selection into treatment, conditional on the discrepancy being time-invariant and constant for individuals within the treatment and non-treatment group. In

other words, the DiD approach accounts for group fixed effects. Non-random selection into treatment would normally be a problem if it was correlated with one of the independent variables, hence introducing an OVB into our regression. One example would be a general interest in financials, which would then be correlated with training attendance. The direction of the bias for the coefficient of training attendance however, would be unclear. One argument is that it would underestimate the effect training has on savings behavior because farmers with little financial interest would not be equally represented and they would be the participants who would probably have been influenced the most – as they have the most potential for improved saving behavior. An opposing argument is that the coefficient is over-estimated. Since financial training inherently relies on people being interested in saving to some extent and those who attended training already seem to have some interest, then it would be impossible to have an impact on farmers who had absolutely no interest (i.e. those who chose not to attend). However, by relying on the DiD design, we allow the omitted variable to be correlated with other repressors as long as it is time-invariant at the group level.

Second, if we were to look at the change in savings for the attended training (from now on called the treatment) group only, we run the risk of only capturing the time-variation in savings. A DiD allows us to subtract out the change in savings that would have occurred solely because of the time-trend and thus capture the change that is correlated with the intervention (financial training) specifically. Using this method one can identify this time-trend by using a control group and stating that the trend seen in the control group after the intervention is what one would have seen in the treatment group had it not been for the intervention. One of the necessary conditions to use DiD is that the treatment is as good as randomly assigned conditional on the group-fixed effect and observable control variables (i.e. the control group and treatment group were the same prior to treatment and are thus comparable afterwards). Table 2 shows that this assumption seems to hold.

The identifying assumption for a DiD estimation is the parallel trend assumption. As explained above, this means that what happens to the control group over time would have happened to the treatment group as well if they had not received the treatment. In other words, our control group is acting as a direct counterfactual to the treatment group. This allows the change for the treatment group with regards to the savings behavior, if existent, to then be attributable to the treatment. Conditional on financial training attendance, we compare the mean savings trends in the treatment and non-treatment group (see Graph 2) prior to the treatment. A visual inspection of mean savings indicates that the mean trend is the same for the treatment and non-treatment group, just on different levels. This confirms visually what was shown in the summary Table 2 above - the difference in mean savings amounts to ca. 1,000RWF. Nevertheless, as the pre-treatment trend is parallel for the two groups, we can proceed using the DiD method.

Graph 2: Pre-Treatment Trend, Identifying Assumption DiD

Mean balances conditional on training attendance

In June 2012, the financial treatment took place as indicated by the red line

5.3. Difference-in-Difference

The DiD estimation accounts for fixed effects using aggregate data on training attendance and non-attendance. Assume that sav_{1ist} are savings of the individual *i* that participated in the financial training at time *t* and that sav_{0ist} are savings of the individual *i* that did not participate in the financial training at time *t*. We assume that for the non-treatment group, there is an additive structure for potential outcomes:

$$E[sav_{0ist}|s,t] = \alpha_s + \beta_t$$

Here, the subscript *s* indicates training attendance status and *t* indicates the pre-treatment or post-treatment time period. Expressed differently, this means that savings for the non-treatment group are determined by the sum of time-invariant effects, α_s, common to all who did not attend the financial training and fixed across time, and a time effect, β_t, that is fixed for the non-participants but can vary across time. Since $E[sav_{1it} - sav_{0it}|s,t]$ is constant and observable, we denote this observed change in savings correlated with training attendance by γ. Putting those building blocks together, we obtain the following regression:

$$sav_{ist} = \alpha_s + \beta_t + \gamma D_t + \varepsilon_{ist} \quad \text{(IIIa)}$$

In this regression, D_t is again a dummy for financial training attendance. Moreover, $E(\varepsilon_{ist}|s,t) = 0$, meaning that the error term is uncorrelated with training attendance and unobservable characteristics over time. We continue our analysis under the assumption that this holds for all fixed unobservable characteristics at the group-level, as they are expected to be captured by the DiD design through the

estimation of α_s. We also assume that the characteristics accounting for any non-random selection to attend are time-invariant, and are therefore captured by this design as well. This gives the following result for the people that did not attend the training:

$$E[sav_{ist}|t = Pre - Treatment, D_t = 0) - E[sav_{ist}| t = Post - Treatment, D_t = 0) \text{ (IV)}$$
$$= \beta_{Pre-Treatment} - \beta_{Post-Treatment}$$

Similarly, for those who did attend the training we get:

$$E[sav_{ist}| t = Pre - Treatment, D_t = 1) - E[sav_{ist}| t = Post - Treatment, D_t = 1) \text{ (V)}$$
$$= \beta_{Pre-Treatment} - \beta_{Post-Treatment} + \gamma$$

The sample DiD is then calculated by subtracting (IV) from (V) to identify the treatment effect:

$$E[sav_{ist}|t = Pre - Treatment, D_t = 1) - E[sav_{ist}| t = Post - Treatment, D_t = 1) \text{ (VI)}$$
$$- E[sav_{ist}| t = Pre - Treatment, D_t = 0) - E[sav_{ist}| t = Post - Treatment, D_t = 0)$$
$$= \beta_{Pre-Treatment} - \beta_{Post-Treatment} + \gamma - (\beta_{Pre-Treatment} - \beta_{Post-Treatment}) = \gamma$$

Ideally, γ would capture the causal effect of the financial treatment, however we expect to identify a correlation from which arguments of interpretation can be drawn. To estimate this model for our empirical study, we re-write regression (IIIa) to the following form:

$$sav_{it} = \beta_0 + \beta_1 Att + \beta_2 Post + \gamma Att * Post + \varepsilon_{it} \quad \text{(IIIb)}$$

Here, β_0 is the constant, Att is a dummy for financial training attendance, Post is a dummy for the post-treatment period and Att*Post is an interaction term between the two former variables. ε_{ist} is the error term. Hence, estimates from regression (IIIa) translate into the following:

$$E[sav_{ist}|t = Pre - Treatment, D_t = 0) = \beta_0$$

$$E[sav_{ist}| t = Post - Treatment, D_t = 0) = \beta_0 + \beta_2$$

$$E[sav_{ist}| t = Pre - Treatment, D_t = 1) = \beta_0 + \beta_1$$

$$E[sav_{ist}| t = Post - Treatment, D_t = 1) = \beta_0 + \beta_1 + \beta_2 + \gamma$$

Regression (VIII) then yields the following result:

Table 5: Difference-in-Difference Estimator

	Attended	Did not attend	Difference
Pre- June 2012	$\beta_0 + \beta_1$	β_0	$\boldsymbol{\beta_1}$
Post- June 2012	$\beta_0 + \beta_1 + \beta_2 + \gamma$	$\beta_0 + \beta_2$	$\boldsymbol{\beta_1 + \gamma}$
Difference	$\boldsymbol{\beta_2 + \gamma}$	$\boldsymbol{\beta_2}$	$\boldsymbol{\gamma}$

Source: Authors' Illustration

Where γ is again the average treatment estimator and unbiased if non-random training attendance is determined by time-invariant characteristics. Visually, this translates into the following graph:

Graph 3: Difference-in-Difference Estimator
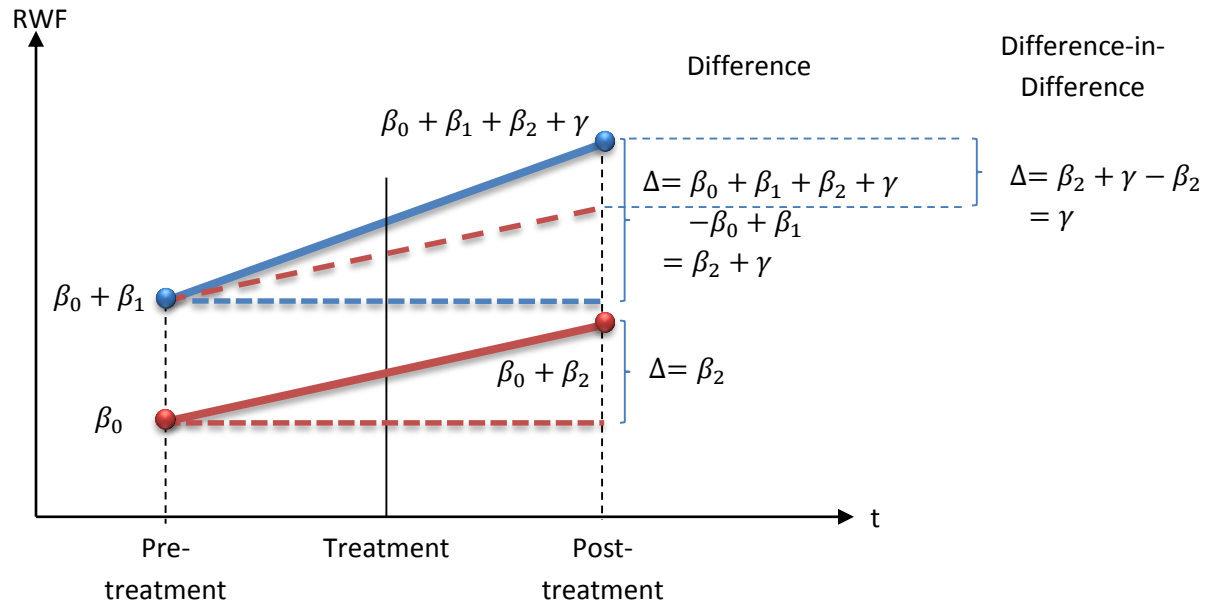
Source: Authors' Illustration

As only the MFI transaction data is panel data, and our data from the household survey is representative of only a fixed period in time, we decide to disregard the time series feature of the transaction data and instead look at two periods only - before and after the treatment. In order to see whether the influence of the financial training changes over time, we vary the post-treatment time frame and compare the results. It is of interest to see if financial training has some sort of immediate impact that may fade over time, no immediate impact but rather is correlated with a slow change over a longer period of time, or no impact at all. Thus, we ran regressions with the post-treatment time frame defined as one month, two months, 13 months, and 14 months after the treatment. We also chose these post-treatment time periods to make them comparable in terms of the seasonal cycle. 1 month and 13 months both represent July which is two months before planting should begin in Karongi. Hence, 2 months and 14 months both represent August, the month before planting usually begins. The pre-treatment time frame is held constant throughout all regressions at the full time period available prior to June 2012 in our data set, i.e. one year, to enable comparability.

By examining the difference in savings activity between the group that attended the financial training and the group that did not, we are able to investigate how savings behavior changed over time, conditional on attendance. This allows us to gain insight into whether financial training is associated with a change in savings behavior, and whether this correlation is positive or negative. Moreover, as we are interested in what personal characteristics influence the effectiveness of receiving financial training, we interact training attendance and the post-treatment period with the control variables we used for individual's specific traits. Again, although no causal relationship can be established here, this approach enables us to identify the characteristics of the people responsive to financial training, as well as to see which characteristics reinforce the effectiveness of this financial training. Note that we define savings as the mean-of-the-period balance, and the period refers to the post-treatment time frame of the respective DiD regression.

5.4. Specification
In the previous section, the DiD regression was introduced in regression (VIII), which will be denoted as regression (1) from now on as it is the basic regression we are examining.

$$sav_{it} = \beta_0 + \beta_1 Att + \beta_2 Post + \gamma Att * Post + \varepsilon_{it} \ (1)$$
$$where \ i \ \epsilon \ \{1, 2, \dots, 491\} \ and \ t \ \epsilon \ \{Pre - treatment, Post - treatment\}$$

To make our inferences as accurate as possible we cluster the standard errors at the village level. By clustering our standard errors we are accounting for the possibility of heterogeneity in our standard errors which, if not addressed, could lead to overly significant results. By clustering at the village level we are allowing for standard errors to have intra-cluster correlation (i.e. they are correlated at the village level). We argue that this is an important consideration as certain villages may have farmers that are particularly prone to save (perhaps there is a MFI branch in the village) while other villages will have farmers that are particularly hesitant to save (e.g. they have a cooperative group leader who is not organized and does not promote saving amongst the farmers).

In the second regression, dummies for individual accounts and villages are added to the baseline regression. The former shall capture individual fixed effects (i.e. unobservable, as well as observable, characteristics that are specific to an individual) whereas the latter accounts for village fixed effects by introducing dummies for each individual and each village. Village fixed effects are important to consider as crops are chosen at the village level and could be influential in harvest outcomes, but also contain geographical indicators that might be important for the regression.

$$sav_{it} = \beta_0 + \beta_1 Att + \beta_2 Post + \gamma Att * Post + \delta_i Account_i + \theta_v Village_v + \varepsilon_{it} \ (2)$$
$$where \ i \ \epsilon \ \{1, 2, \dots, 491\}, t \ \epsilon \ \{Pre - treatment, Post - treatment\} and \ v \ \epsilon \ \{i, 2, \dots, 19\}$$

Next, we add the vector X_i which includes time-invariant controls on an individual level. Following previous research, we include time-invariant characteristics as controls in the main regression to prevent those variables from driving the results.[61] Furthermore, including explicit controls allows us to clearly estimate variation on an individual level, increasing the precision of the causal effect.[62] The controls used are standard for existing literature related to savings determinants. They include account-related controls namely gender of account holder, financial institution, site, which treatment group they were assigned to, and if the farmer took up a savings account, household-head related controls such as age, age squared (to account for a concave shape in savings associated with age) and education, as well as household size and plot area, the latter being a proxy for wealth. Interestingly, according to a study in Morocco, household size was shown to only significantly influence savings in urban setting.[63] Being critical of the external validity of those results, we include household size as a control variable. With regards to our wealth proxy, we refer to Filmer and Pritchett[64] who explored various methods of creating wealth indices. However, with so little information on the market prices for dwelling materials in Rwanda, we chose to abstain from developing an index and rather included a variable for plot area owned[65]. As plot size is largely time-invariant and hence probably not influenced by the treatment, as well as strongly correlated with wealth - determining the agricultural output we use this as our proxy. It should also be recognized that ownership of land, by definition, can also be considered a form of saving.[66] However, this variable had to be adjusted somewhat from the plot area as reported in the HH survey. For several of the household survey respondents, the question regarding how many plots of land owned was positive but the amount of land owned in square meters was left blank or was zero. As it seems highly unlikely that so many farmers would not own land, we replaced these blanks and zeroes with the mean plot size for that plot number. Of course, there are cases of farmers who only work as hired hands on other's land, but this was not prevalent within our sample.

We added the controls in categorical blocks as specified above to see how the estimators change as more controls were added, this can be found in Appendix Table 4 and for winsorized data in Appendix Table 6.[67] To determine whether all the controls should be included we tested the joint significance of the added variables in the categorical blocks and found that we could reject the null hypothesis that they are jointly insignificant.[68] For the sake of brevity we do not show the control coefficients explicitly in our regression outputs in the paper, but we do indicate when the full set of controls are included. This yields the following regression (3):

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[61] Brune et al. (2011).
[62] Angrist, Joshua D. and Jörn-Steffen Pischke. "Mostly harmless econometrics: An empiricist's companion." *Princeton University Press* (2008).
[63] Arestoff et al. (2009).
[64] Filmer, Deon, and Lant H. Pritchett. "Estimating wealth effects without expenditure Data—Or tears: An application to educational enrollments in states of India." *Demography* 38.1 (2001): 115-132.
[65] Plot size was measured in square meters and referred only to the first three plots described in the household survey.
[66] Arestoff et al. (2009).
[67] To account for outliers, we applied the Winsor method, which is explained in more details in the following section 5.5 on unusual large and influential data.
[68] See Appendix Tables 5 and 7 for test outcomes.

$$sav_{it} = \beta_0 + \beta_1 Att + \beta_2 Post + \gamma Att * Post + \beta_3 X_i + \delta_i Account_i + \theta_v Village_v + \varepsilon_{it} \quad (3)$$

All three specifications above are increasingly moving from an unconditional DiD regression to a conditional one and are used to test Hypothesis 1, namely whether financial training increases savings, and Hypothesis 2, whether savings follows the promoted seasonality. To test Hypothesis 3, we start with regression (3) and add interactions between financial training attendance, the post-treatment period and all the control variables to see whether particular characteristics of the account holder/household in question seem to influence the effectiveness of the financial training, resulting in regression (4).

$$sav_{it} = \beta_0 + \beta_1 Att + \beta_2 Post + \gamma Att * Post + \beta_3 X_i + \beta_4 X_i * Att * Post + \delta_i Account_i + \theta_v Village_v + \varepsilon_{it} \quad (4)$$

To test Hypothesis 4 we use a similar regression design to (4) to examine more closely the correlation between account balances and the take-up of a savings account and then further, Targeted savings accounts versus Pre-commitment savings accounts. Note that the covariate *SavingsAcct* thus takes on the definition "has a savings account", "has a Targeted account", and "has a Pre-commitment account" depending on what we are analyzing.[69]

$$sav_{it} = \beta_0 + \beta_1 SavingsAcct + \beta_2 Post + \gamma SavingsAcct * Post + \beta_3 X_i + \beta_4 X_i * SavingsAcct * Post + \delta_i Account_i + \theta_v Village_v + \varepsilon_{it} \quad (5)$$

Furthermore, as mentioned before, we will also vary the time-frame of the post-treatment period to see how financial training effects show over time while keeping the pre-treatment time period constant Recall, the financial awareness campaign targeted savings towards agricultural inputs; hence we expect savings to be higher prior to the planting period. To check whether this holds, we investigate whether the mean savings balances is lower in the month just before the planting, compared to the mean savings balances from the period up to two months prior to planting. If we are correct, the coefficient on one month before the planting should be smaller than the coefficient on up to one month before. The data allows us to check for this pattern for two subsequent agricultural seasons of season A, as well as short- and long-term effects of the treatment. Considering the agricultural calendar in Karongi, this gives us the following time spans to evaluate (see Table 6). For a graphical illustration of the data available, the seasons and the time frames to look at, see Graph 4.

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[69] For comparability reasons, we use the people that did not attend training as the baseline group with the exclusion of seven people who despite not attending the financial training opened up a savings account.

Table 6: Regressions Extensions

Expected savings behavior	Post- treatment period	Post-treatment time frame
Increasing savings towards season A 2013	1 month	July 2012
Less savings due to purchasing inputs for season A 2013	2 months	July & August 2012
Increasing savings towards season A 2014	13 months	July 2012 – July 2013
Less savings due to purchasing inputs for season A 2014	14 months	July 2012 – August 2013

Graph 4: Overview of Time Spans Used in Regressions
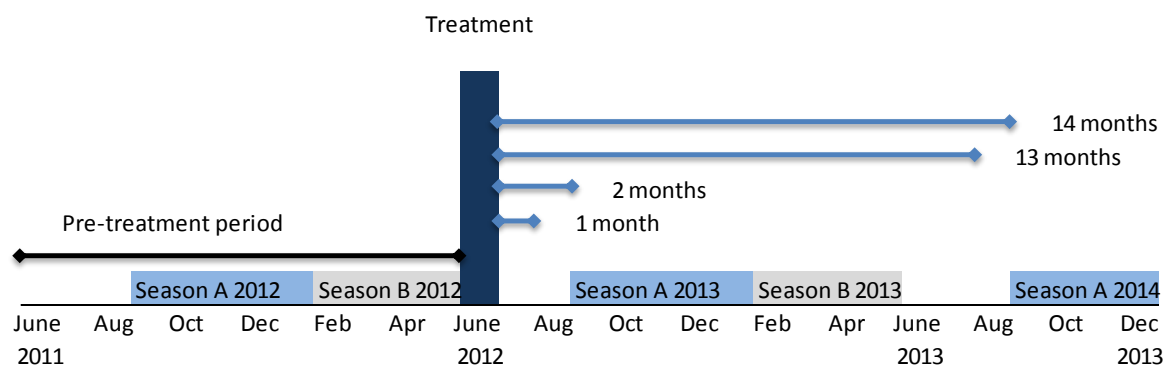
Source: Authors' Illustration

Given our identification strategy, however, there are also several concerns that have to be considered as they affect standard errors and accordingly statistical inference. Those will be discussed in the next sections.

5.5. Unusual and Influential Data

First, one possible problem for the regression estimates are outliers as they could potentially inflate standard errors and distort the coefficients of the regression[70], especially with the underlying limited size of the data set. The savings data suggests that there are obvious extreme positive, as well as negative, outliers that have to be accounted for (see Graph 5 below). As we have determined that the probability of outliers occurring from clerical errors is very low, we believe that positive outliers most probably represent people who are different from the rest of the population. In the case of negative outliers, we expect them to be caused by errors in calculation by the MFI cashiers. Hence, these extreme observations might, on the one hand, provide additional relevant information about the sample but, on the other hand, could bias the coefficients when we are considering the behavior of an average farmer – upwards in the case of positive and downwards in the case of negative extremes.

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[70] Osborne, Jason W. and Amy Overbay. "The power of outliers (and why researchers should always check for them)." *Practical Assessment, Research & Evaluation* 9.6 (2004).

In order to determine how much of an influence the observed positive outliers had on our outcomes, next to the regressions with the actual data, we also conducted a robustness check with data that we manipulated to be somewhat normalized. We considered this necessary because the most extreme outliers are found on the right side of the data, i.e. in the later time periods after the treatment. However, as we believe that some of these extreme figures stem from loans being paid out via the ordinary account rather than high balances only, we control for the outliers using the Winsor method.[71] When applying the Winsor method we are replacing the top 1% with the 99th percentile and the bottom 1% with the 1st percentile. It is common to apply this method with top 5% and bottom 5% keeping the data within the mean 90%[72] range but considering the relatively few outliers in the sample as well as the limited sample size we wanted to adjust as little data as possible. Furthermore, we did not want to entirely mitigate any information that the positive outliers might contribute to our findings. The results from our regression using the winsorized data can be found in Appendix Tables 3, 8 and 9. The results are very similar to that of our regression outputs using all of the available data, indicating that the outliers do not strongly pull our coefficients in one way or the other.

Graph 5: Visual Inspection of Outliers
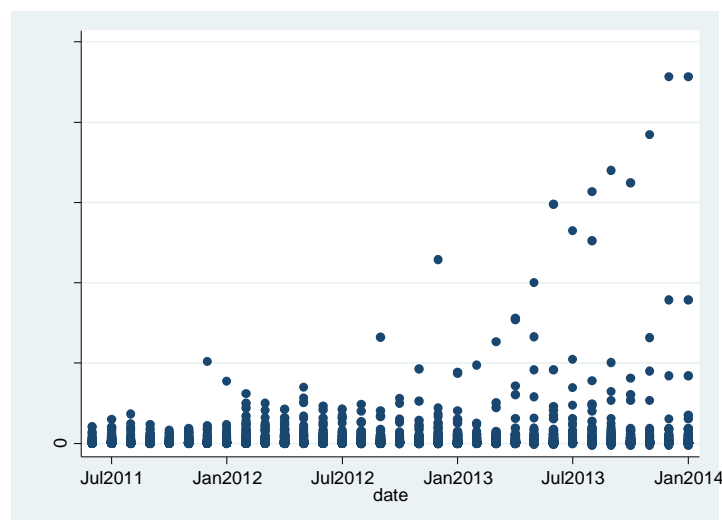
5.6. Statistical Inference
5.6.1. Correlation Across Groups
When interpreting our results we were concerned with the possibility of correlation at the LWH group level. The LWH groups are close to one another in geographical proximity and received the same training. However, we were also concerned with correlation at the village level, caused both by spillover

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[71] Ghosh and Vogt (2012).
[72] *Ibid.*

effects from the LWH groups and exogenous factors that could influence farmers' savings that impact them at the village level (e.g. crop failure caused by pests contained to a certain area). To correct for this intra-cluster-correlation, we clustered our standard errors at the village level.

5.6.2. *Small Sample Size*

With the already relatively small sub-sample size of 491 accounts, this number is further reduced due to clustering on the village level (n=19). The effective sample size is rather the number of clusters than the number of individual observations, rendering inference more problematic.[73] To check the robustness of our results, we ran the regressions clustering our standard errors at the LWH group level (n=58) rather than the village level and determined that the significance levels were the same. Consequently, we are not concerned that statistical inference has been threatened by our choice of clusters.

6. Results

In Table 7 we highlight the results from regressions (1) the unconditional DiD, (2) the DiD accounting for fixed effects, and (3) the conditional DiD with the full set of controls and accounting for fixed effects, to investigate Hypotheses 1 and 2. Here the three regressions are run, side by side, for each post-treatment time period as defined in Table 6. This way the differences across not only the short-term and the long-term can be easily examined but also fluctuations indicating seasonality. To identify the latter, differences from the period up to one month before planting (post-treatment periods 1 month and 13 months) are compared the periods including the month before planting (post-treatment periods 2 months and 14 months).

To answer Hypothesis 1, we find that the DiD coefficient is never significant across the three regressions, regardless of post-treatment time period. This suggests that financial training does not have a significant impact on aggregate savings. However, it is interesting that in regression (3) the "Attended training" variable is highly and positively significant for all of the time periods. This would indicate that although our summary statistics for the pre-treatment period showed no significant difference between farmers who attended the training and those who did not, those who save more are more likely to attend the training. Recall that regression (3) includes a full set of controls while our summary statistics were unconditional. Moreover, it becomes apparent that the first regression, the unconditional DiD regression, hardly holds any explanatory power with an R-squared of less than 1%.

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[73] Angrist and Pischke (2009).

Table 7: Regression Output to Test Hypothesis 1 and Hypothesis 2

Regression 1-3: Varying Post-Treatment Periods

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	One Month Mean			Two Month Mean			Thirteen Month Mean			Fourteen Month Mean		
After intervention	4,759	4,401	4,409	3,332	3,332	3,321	10,987	10,987	11,096	12,602	12,602	12,726
	(3,059)	(4,336)	(4,377)	(2,559)	(3,620)	(3,658)	(8,646)	(12,227)	(12,310)	(9,899)	(13,999)	(14,093)
Attended training	1,244	-1,609	46,019***	1,244	-2,100	-38,699***	1,244	-19.71	34,027***	1,244	272.5	34,552***
	(1,362)	(2,744)	(2,595)	(1,362)	(2,511)	(2,530)	(1,362)	(6,135)	(6,177)	(1,362)	(7,115)	(7,163)
DiD coefficent	-2,675	-2,553	-2,555	-1,988	-1,988	-1,973	-9,853	-9,853	-9,959	-10,778	-10,778	-10,897
	(3,874)	(5,489)	(5,531)	(3,551)	(5,022)	(5,059)	(8,677)	(12,271)	(12,354)	(10,061)	(14,229)	(14,326)
Observations	967	967	963	982	982	978	982	982	978	982	982	978
R-squared	0.002	0.733	0.733	0.001	0.734	0.734	0.005	0.677	0.677	0.005	0.656	0.656
Village dummies	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Individual dummies	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Full set of controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Robust	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Hypothesis 2, on the other hand, receives preliminary support from our findings in Table 7. Differences from the period up to one month before planting and the period including the month before planting are clear. There is a prominent dip in aggregate savings in post-treatment 2 months compared to post-treatment 1 month. To be able to compare whether the seasonally adopted behavior is more pronounced in either of the two groups - attended training and did not attend training -, we look at how much money is spent between the up to one month prior and one-month prior to planting in these two groups. We were curious to see if the seasonal behavior would be more prominent amongst those who attended the training. This would suggest that their savings behavior was more sophisticated than the non-attendee group's, not only in terms of magnitude, but also in intention. To determine this, we tested, with a full set of controls and conditional on training attendance, whether the difference in mean balances up to one month prior and one month prior to planting was statistically significant. To prevent our results from being solely driven by differences in magnitudes, we standardized the means by dividing them by the mean value of the variable we were examining. The results can be found in the Appendix Table 10. We found that there was no significant difference in volatility between the two groups for the first season (i.e. the drop in savings between July and August 2012) but that there was a more pronounced seasonally adapted behavior for those who attended training one year later (i.e. between July and August 2013).

When examining Hypothesis 3 in Table 8, we are considering regression (4) from the specification section; which includes the core DiD, dummies accounting for fixed effects, the full set of control variables, and interaction terms for all of those control variables with attended training and the post-treatment time period. Again the regression was run for all four post-treatment time periods. It is the interaction terms that allow us to identify how personal characteristics are correlated with the effectiveness of financial training. However, it is also of interest to examine the coefficients of the control variables themselves. Although these do not explain the effect of a certain characteristic combined with financial training, they do give us insight into what characteristics are independently correlated with higher savings.

Table 8 shows many interesting results with regards to the influence of individual characteristics on savings but the main take-away in terms of our hypothesis is that only two of the interaction terms, the interaction term with plot area and assignment to targeted treatment, were significant. Again, this means that apart from those two interaction terms, personal characteristics were not found to be significantly correlated with any change in savings behavior after the training. In the discussion we examine more closely possible explanations for the highly significant results for the controls. Moreover, we also argue why those with larger plots of land, who attended training, may save less than those who own smaller plots of land and attended training. Similarly, we examine why those who attended training and were assigned to the Targeted treatment group might save more in the month immediately following the training.

Table 8: Regression Output to Test Hypothesis 3

Regression 4: Varying Post-Treatment Period

VARIABLES	(1) One Month Mean	(2) Two Month Mean	(3) Thirteen Month Mean	(4) Fourteen Month Mean
After intervention	4,409	3,321	11,096	12,726
	(4,425)	(3,696)	(12,439)	(14,240)
Attended training	50,584***	-37,697***	35,430***	33,284**
	(5,669)	(9,926)	(12,271)	(15,166)
DiD coefficient	-18,985	-17,387	-14,544	-22,167
	(26,882)	(23,533)	(11,296)	(17,521)
Account holder is male	133,458***	-129,207***	88,245***	78,487**
	(14,184)	(29,523)	(25,910)	(34,574)
Interaction: account holder is male, attended training and post treatment	3,658	2,454	1,766	3,189
	(3,992)	(3,732)	(2,825)	(3,719)
MFI is COOPEC	-518,172***	376,045***	-522,300***	-494,403***
	(44,982)	(105,710)	(94,721)	(124,573)
Interaction: COOPEC, attended training and post treatment	-531.0	65.97	2,820	2,907
	(4,087)	(3,610)	(2,389)	(2,761)
Site	-45,228***	51,862***	-18,063*	-13,705
	(4,639)	(11,019)	(9,770)	(12,875)
Interaction: Karongi 13, attended training and post treatment	-3,200	-2,632	955.3	134.4
	(4,404)	(4,244)	(4,894)	(5,812)
Assigned to Targeted Treatment	51,858***	4,363	95,080***	87,369***
	(2,162)	(8,133)	(6,624)	(9,249)
Interaction: treatment=1 if assigned to targeted treatment, attended training an	7,043*	4,842	2,026	3,646
	(3,510)	(3,505)	(2,724)	(3,865)
Has a savings account	-117,468***	134,399***	-65,757**	-54,785
	(11,466)	(30,697)	(25,579)	(34,698)
Interaction: has a savings account, attended training and post treatment	-77.42	-40.25	-1,742	-2,625
	(3,464)	(3,081)	(3,559)	(3,628)
Age	43,298***	-39,455***	35,907***	32,882**
	(3,582)	(10,031)	(8,798)	(11,705)
Interaction: age, attended training and post treatment	502.2	486.1	192.6	539.5
	(1,215)	(1,061)	(625.8)	(976.2)
Age squared	-433.6***	372.8***	-394.4***	-365.9***
	(35.40)	(97.91)	(86.13)	(114.6)
Interaction: age squared, attended training and post treatment	-4.751	-4.585	-2.495	-5.950
	(11.67)	(10.48)	(6.443)	(9.857)
Highest level of education completed	26,983***	-19,342***	25,505***	22,941***
	(1,845)	(5,740)	(4,881)	(6,520)

Interaction: education, attended training and post treatment	1,995	2,049	-2.554	394.2
	(3,552)	(3,026)	(1,345)	(1,913)
Number of HH members	-32,426***	36,498***	-18,614**	-15,579
	(2,880)	(8,547)	(7,199)	(9,670)
Interaction: number of HH members, attended training and post treatment	-599.5	-495.9	-78.22	-754.3
	(2,837)	(2,370)	(1,364)	(2,103)
Plot area owned in square meters	0.530***	-3.540***	-3.425***	-3.747***
	(0.134)	(0.459)	(0.358)	(0.484)
Interaction: plot size, attended training and post treatment	-0.0178***	-0.0160**	-0.0136***	-0.0155**
	(0.00567)	(0.00565)	(0.00422)	(0.00582)
Observations	963	978	978	978
R-squared	0.737	0.737	0.677	0.657
Village dummies	Yes	Yes	Yes	Yes
Individual dummies	Yes	Yes	Yes	Yes
Robust	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9 delves into Hypothesis 4 to determine whether or not there is a difference in amount saved correlated with the take-up of a savings account and further, if there is a difference in amount saved correlated with either of the specific savings accounts (Targeted or Pre-commitment). In this table only regression (5) from the specification section is run, the three types of savings accounts definitions (has either of the types of savings accounts, has a Targeted savings account, or has a Pre-commitment savings account) are side by side and all four post-treatment time periods are tested. As predicted in Hypothesis 4, we find that owning a savings account is correlated with higher savings. The DiD coefficient and the post-treatment dummy, however, are again insignificant. When looking at the savings accounts separately, we find that the only significant result is that people with higher than average savings take-up a Pre-commitment account; nothing is significant in the regression for the Targeted account. These results are consistent throughout all time periods.

We include the results of all our regressions run with the winsorized data in Appendix Tables 3, 8 and 9 as a robustness check. We find that our results are similar when running the regressions with all the available data and when run with winsorized data, although having a Targeted savings account becomes positively significant in the later time periods.

Table 9: Regression Output to Test Hypothesis 4

Regression 5: Varying Post-Treatment Period

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	One Month Mean			Two Month Mean			Thirteen Month Mean			Fourteen Month Mean		
	Savings Account	Targeted Account	Commit-ment Account	Savings Account	Targeted Account	Commit-ment Account	Savings Account	Targeted Account	Commit-ment Account	Savings Account	Targeted Account	Commit-ment Account
After intervention	4,983	7,719	3,758	3,767	6,999	2,221	11,783	31,282	2,458	13,540	35,807	2,891
	(4,718)	(13,279)	(4,567)	(3,919)	(11,308)	(2,556)	(13,162)	(39,911)	(1,769)	(15,094)	(45,877)	(1,948)
Has savings account	34,960***	-2,920	31,799***	36,698***	1,296	30,994***	70,357***	35,034	60,433***	73,986***	36,689	63,095***
	(2,707)	(2,754)	(3,045)	(2,627)	(5,689)	(1,973)	(7,117)	(20,247)	(1,849)	(8,056)	(23,216)	(1,945)
DiD coefficent	-4,003	-1,376	-4,745	-2,965	-2,595	-2,777	-11,760	-27,987	-3,669	-13,555	-32,669	-4,094
	(5,413)	(11,130)	(5,437)	(5,254)	(11,377)	(3,945)	(14,235)	(40,495)	(3,699)	(16,113)	(46,432)	(3,890)
Observations	531	153	378	540	158	382	540	158	382	540	158	382
R-squared	0.780	0.777	0.787	0.802	0.795	0.811	0.660	0.651	0.745	0.643	0.635	0.740
Village dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Full set of controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Robust	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses

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

7. Discussion

Our research has provided some interesting results about the effectiveness of financial education training in Rwanda. We find no evidence to support our Hypothesis 1, that financial training has a positive impact on savings behavior in the form of aggregate account balances. None of our DiD interaction terms were significant as seen in Table 7, which should read as the effect of attending training on balances after the treatment intervention. As no correlation between attending the financial training and savings balances was discovered, this suggests that the financial training did not have the effect it was intended to. However, the conclusiveness of this finding needs to be read in the full context, taking consideration of both the project specifics, including implementation, as well as regional factors.

When examining Table 7, it is evident that the "Attended training" coefficient becomes highly significant in regression (3), when fixed effects are accounted for and the full set of controls is implemented. The magnitude of the coefficient, 35-46K RwF, is very large. The mean monthly balance over the full time period that we have data for (June 2011-December 2013), is less than 13,000 RwF (see Table 2). That means that the amount that people who attended training save is nearly an additional three times that. It also remains very high, despite a slight decrease, over the entire post-treatment time period examined. This means that those who attended the training continue to save significantly more than those who did not attend the training throughout the 14 months. Consequently, it is apparent that those who attend the training differ from those who did not in savings levels. As was shown in the method section in Table 4, when looking at the summary statistics of participants and non-participants, there was no indication that the two groups were statistically different based on the observable variables available. However, regression (3) suggests that conditional on the included control variables, training participants were, in fact, different from non-participants. A first thought might be that those who chose to participate in the training share some unobservable characteristic like sophistication or a general interest in saving, whereas those who chose not to attend do not share this characteristic. Fortunately, as we described in the method section, the DiD method allows for different intercept points of the two groups (the higher level of savings of the attended training group) as long as the pre-intervention trend is parallel. Similarly, panel data, when utilizing two or more time periods, is robust to time-invariant unobserved effects. Hence, we are not concerned that the "Attended training" coefficient is being biased because of a time-invariant OVB like training participants being more sophisticated than their counterparts. Rather, we maintain that our findings mean first, a specific group selected itself into treatment and second, the financial training received by those farmers did not change their formal savings behavior.

Turning to Hypothesis 2, we find that savings in formal accounts do, in fact, follow the seasonality we expected. Mean savings in the time periods up to one month before planting (1 and 13) are higher than the mean savings balances for the periods that include the month before planting (2 and 14) which means that there must be a large drop in savings balances in the month prior to planting compared to the months before that. The drop seems much more prominent in month 2 than in month 14 only because it is the mean calculated over a much shorter time period. If the financial training had been

effective, it would have promoted this seasonality; as farmers would be saving in order to purchase agricultural inputs during the month prior to planting. We find that this could very well be the case. Through our FGDs we understood that it is during the month prior to planting that farmers purchase the necessary inputs for their crops (e.g. seeds, spuds, fertilizer). However, from these discussions it also became clear that many of the inputs are still largely subsidized, or in the case of fertilizer, usually received as a loan at the time of planting and then paid back over time. This draws the question, if this drop in savings balances is not caused by the purchase of agricultural inputs what could cause such a seasonal drop? One possible explanation is that it could be reflective of farmers taking out less loans to purchase goods such as fertilizer, as they now have enough funds to make the purchases on their own in lump-sum.

As mentioned in the Results section, to better understand the seasonality seen from our results we compared the extent of the drop between the up to one month prior to planting period and the one-month prior to planting period across those who attended training and those who did not..[74] We observe that even though there is statistically no difference in the seasonal spending in 2012, the people who attended the training do manage their money better in terms of seasonality in 2013. This could be an indicator of the sophistication and ability of those who attended the training to better manage their money than those who did not attend. Considering this result, it would have been interesting to see if the training could have been effective for those less sophisticated[75].

Turning to Hypothesis 3, we observe that training does not seem to be more effective for people with any particular observable characteristics. This is determined through the interaction terms of the respective control variables, attended training and post treatment period; as they are not significant but for two exceptions, we determine that most characteristics do not add any significant value to the training.

Turning to the two characteristics that do interact with the financial training, first, there seems to be a positive effect (significant at the 10% level) from attending training on balances for those who were assigned to the Targeted treatment when looking at the first month post-treatment. One way to interpret this result is that the financial training was successful for the Targeted group for one month as it incited people to save more, at least for this short period. Note that the coefficient for simply being assigned to the Targeted group is large, at 50-95K RwF, positive and significant throughout the post-treatment period at a 1% and 5% level. So, as explained above, even though the assignment to treatment groups was initially random on a group account level, the actual population we look at includes a Targeted group that is, on average, richer than the Pre-commitment group.[76] With this in mind we look again at the interaction term. The positive sign and significance indicates that those who attended training and were assigned to the Targeted treatment increased their saving more in the one

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[74] The mean balances were standardized in order to make the volatility comparable across the two groups.

[75] Hastings et al. (2008).

[76] As a reminder- the random assignment to treatment groups was done on a group account level, but the individuals we have information on are only the ones that actually opened up an individual account. Therefore, even though the assignment to treatment groups was initially random, the final sample might be non-random with respect to the treatment groups.

month post-treatment period than those who attended training and were assigned to the Pre-commitment treatment. This suggests that the training was more influential for those assigned to the Targeted treatment, i.e. those who already saved more. This should be considered when trying to deduce who education programs like this might work for. If we continue with the belief that those assigned to the Targeted treatment are in general more sophisticated in how they manage their money then this finding gives an indication that education programs may be more beneficial for people who already show some inclination to be sophisticated. However, since this difference is only significant for the 1 month period, the meaningfulness of the finding is questionable.

The second set of interactions that show any significance are the interaction terms with the control variable "Plot area measured in square meters". The results indicate that farmers who attended the training save less the larger their plots were, as compared to farmers who did not attend the training. However, given the small magnitude of the coefficients and taking the average plots' size into account, this amounts to saving approximately 200RwF less for an average farmer, not a very meaningful amount when compared to the other coefficients. Moreover, given the method we used to clean the plot area data for this variable, we are even more cautious when drawing conclusions from this result.

Interestingly, we observe that nearly all of the included controls are not only highly significant but maintain their significance over all the time spans. When discussing the results for our controls note that unless otherwise stated, the coefficients exhibit a behavior fitting the agricultural cycle (i.e. higher in post-treatment periods 1 month and 13 months compared to post-treatment periods 2 months and 14 months respectively). Therefore, for simplicity, the following discussion focuses mainly on coefficients of months one and thirteen.

Our first control, "Account holder is male", shows that male account holders save, on average, 90-130K RwF more than women at a 1% significance level. This is supported by existing literature on the determinants of saving.[77]

The "MFI is COOPEC" variable shows that customers of COOPEC Inkunga have, at a significance level of 1%, much lower balances than SACCO Mukura customers. This highlights the importance of accounting for the financial institution itself when estimating how people will save. However, we found the result surprising, as we understood COOPEC Inkunga to be the better organized institution and its cashiers to be more knowledgeable. Therefore, we predicted that farmers who save at COOPEC Inkunga would save more. One explanation for the opposing finding might be that COOPEC Inkunga's branch is relatively further away from its customers; hence it might very well be that its customers use their bank accounts less frequently due to the longer travel distance.

Next, the coefficient on "Site" is negative, which coincides with the observation that Karongi 13 was the poorer region. We also observe that people in Karongi 13 do not seem to follow the savings seasonality closely, generally indicated by spending more in the month prior to planting. Moreover, the magnitude

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[77] Bendig et al. (2009).

and significance of the "Site" coefficient decreases over time. We included this dummy primarily as the sample was stratified by site, and to account for any systematic differences across the sites. We strengthen this control by accounting for village fixed effects which should also capture any geographical differences.

As saw through the different starting levels of our groups, and later discussed when examining the significant interaction terms, people assigned to the Targeted group by DIME (i.e. "Assigned to Targeted Treatment" control) save, on average, significantly more than those assigned to the Pre-commitment group. Recall that the pre-condition to treatment was being a part-owner of a group account or having an individual ordinary account at one of the MFIs. Thus, not everyone that was randomly selected had an individual account. Hence this coefficient seems indicative of a non-randomness in those that had and/or signed up for an *individual* ordinary account – the transaction data of which we are examining. This is accounted for in our DiD, but it is of interest because it gives us insight into what kind participation is realistic to expect from similar voluntary educational programs.

Controlling for the other observable characteristics, people who took-up either one of the savings accounts save on average less than people who did not take-up the product offered. The significance of this coefficient dissipates over time and is insignificant by the 14th month post-treatment. As Hypothesis 4 addresses this question in more detail we will leave the discussion of this variable for later.

We find that the effect of age on savings is in line with previous research on the subject.[78] Consistent across all regressions, we find that the farmers save more for each additional year of their age, but with decreasing marginal savings. Additional savings associated to age peaks when farmers are approximately 50 years old, amounting to ca. 1.1mln RwF. After that, savings decreases with age.

With regards to education, we observe that higher education is correlated with higher savings which is also in line with results from the literature. Educational attainment is measured in categorical values (refer to Table 1), implying that for each additional advancement in schooling, an additional 25-27K Rwf are saved. This translates into farmers with some primary education saving ca. 25K RwF more than those with no education, and farmers who completed their primary education, saving on average 50K RwF than those without any formal education.

Interestingly, we find that household size is significant for our data, diverging from what was found in the Moroccan study where household size was found to only be significant in an urban setting.[79] The negative sign of our coefficients however, is aligned with their findings. The larger the household size, the less is saved. This makes sense as more people usually implies that there are more children in a household, who in turn, cannot contribute as much as adults to covering expenses. Even though children work, either exclusively or in addition to going to school, we observed via the household survey that child labor is mainly unpaid labor either at home or on family-owned plots.

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[78] Arestoff et al. (2009).
[79] *Ibid.*

Finally, we find that increasing plot area (i.e. land owned) correlates with significant lower savings, which was an unexpected result. As explained when discussing the interaction term, we multiplied the coefficient with the average plot size for a clearer interpretation. Ultimately, this control implies that an average farmer (owning 0.7 hectares) saves 21K RwF less than if they had had no land. Although this amount may seem large, the magnitude is not as influential as many of the other controls. Nonetheless, it seemed counterintuitive that our wealth proxy would suggest that those who we claim are wealthier save less.[80] As a robustness check, we ran the same regressions with other wealth proxies such as the materials of dwelling structure. Using these wealth proxies we also found the same results, i.e. that more stable dwelling structures were associated with lower savings balances. One explanation could be offered through a behavioral approach - people who own more, might be more prone to spending. In the case of dwelling structures, the mere fact that people have better dwellings indicates that they do spend more on housing, and correspondingly probably on other things. In the case of plot area, lower saving balances could be correlated with higher agricultural expenses throughout the season, but higher income at harvesting time. To explore either hypothesis in more depth, however, more data would be needed.

Our resounding result from this investigation is that the financial training does not seem to have an impact on financial behavior. Instead, we find that the coefficients of the controls generally coincide with findings in previous research regarding the pre-existing determinants of savings. In particular, we find that gender, age, education and household size impact savings to a large extent.

Lastly, turning to Table 9, we investigate the outcome of Hypothesis 4. We find that attending training in combination with the actual uptake of either type of savings account does not yield significant results either. That is, we do not find that attending training and having a savings account increases savings as compared to the people who did not attend training and did not take-up a savings account. Contrary to the actual outcome, we expected to see a positive effect on balances, because not only should farmers who opened such a specific savings account be saving more consciously, we suspected that the interaction with the MFI staff may have been more savings-facilitating, thanks to the staff training. However, a limitation of the latter statement is that from the MFI staff interviews we understood that the staff was not always knowledgeable about the products and the respective product features. This was not only made apparent by their incomplete descriptions of the different savings products, but also from their struggle to correctly calculate interest on a continuous basis in one of the MFIs. Therefore, our initial prediction that savings account owners received additional assistance from staff did not hold in practice. Thus, it is feasible that the actual implementation practice of the products may have attributed to why the savings accounts did not translate into higher savings.

Instead, we see that people who generally save more, take-up a savings account. This result is significant at a 1% level and for all time periods examined. The sign contradicts the sign on the "Has a savings account" control in regression (4) (discussed under Hypothesis 3 above), this is likely in-part because we

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[80] Bendig et al. (2009).

do not explicitly control for attended training in regression (5). We did not include an explicit control for attended training because we compare the people who did not attend training and do not have a special account to those who did attend training and actually took up an account. Consequently, attended training and has a savings account are equivalent in this case and correspondingly, the coefficient on "Has a savings account" captures both effects.

Distinguishing between the two types of savings accounts, we find that there is still no effect of attending the training in combination with the uptake of a respective product type. This means that both savings accounts consistently failed to incite higher savings even though people believed in their added value and were willing to pay the opening fee.

However, we do find that the account types seem to appeal to different kinds of people. First, remember that people assigned to the Targeted group are richer on average; however those assigned to the Targeted group who actually took up the savings account are conditionally not different from those in the baseline group. This leads to the conclusion that from the relatively richer Targeted group, only poorer people took up the Targeted account. This in turn implies that people who needed support in saving from this group were inclined to take-up this form of account. Contrary to that, we observe that people taking up the Pre-commitment account are relatively richer than others from the baseline group. This indicates that only the ones who were generally richer, and perhaps had more disposable money were willing to lock it away. This result is interesting as it indicates which people can be targeted with which type of account.

8. Conclusion

From this field experiment, we find that neither attending financial education training, nor taking up a savings account, correlated with higher savings balances. What we do find is that different people show interest in the offered treatment, namely that people with more savings in formal accounts attended the offered financial training. We also discover that there is a difference in people taking up the respective savings accounts - relatively richer people sign-up for the Pre-commitment account whereas people with mean savings take-up the Targeted account. These results indicate a few things. First, certain types of people are more inclined to actively take steps to educate themselves about savings by attending trainings, thus if there is a particular group that a project is targeted towards one must think strategically in order to incentivize the right people to attend. Second, there are many people who are interested in improving their savings behavior, as indicated by them opening an account and paying the opening fee. Thirdly, different features of the savings accounts appeal do different types of people, again indicating that implementation must be done strategically according to who the target group is.

We suspect that part of the reason the intervention did not translate into higher savings was that the financial training was a one-time, fairly short training. This might not have been sufficient to cause a meaningful behavioral change. Perhaps if the training was more extensive like in Drexler et al's

experiment on different types of trainings[81] we would find a more significant change in savings behavior. The findings from that experiment suggest that a financial training similar to the set-up of this intervention - intuitive, with examples people can easily relate to - but conducted on a repetitive basis, yields positive results for financial management and correspondingly for financial outcomes. Similarly, we also expect that more professional assistance from the MFI staff could help enlarge the effect of the financial training and savings accounts. However, this has to be set in perspective as well - the MFI managers reported that they would have wished for more support from the project team in order to launch the new products successfully and the training received by the MFI staff about the products was also fairly limited.

Based on these findings, we suggest that future research needs to be done to determine whether financial education training is effective in changing savings behavior. Furthermore, future financial training interventions should expand on the financial education component, preferably through several courses, and examine it as a primary research question rather than as a secondary component to research on savings products. Additionally, before implementation it should be verified that the involved MFI staff have the required capacity to realize such a project. The results from our thesis are indicative of what kinds of products are appealing to which people – more binding accounts are acceptable to those who are relatively richer, whereas those who are poorer are more interested in taking up savings accounts that do not limit accessibility to one's funds. This insight should be used in the process of creating savings products. Moreover, it is clear from our findings that the people who might need the largest support are also the hardest to incentivize to voluntarily take-up new treatments.

The external validity of these findings should be applied cautiously; we cannot claim that financial training in this form would be ineffective everywhere. However, it is worth noting that Rwanda is similar to many other Sub-Saharan countries in climate, income per capita, and its agricultural base. We determine that the effectiveness of financial training should not be taken for granted and that ignorance of financial systems may not be a major obstacle to saving in this setting. Rather there may be other, equally important factors at play. Thus, we conclude that more research needs to be done in the field to determine whether financial training would be an effective method of improving savings behavior before further policy action should be taken.

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[81] Drexler et al (2010).

Appendix

Appendix Table 1: Balance Check Produced by DIME

	Offered Savings Account	Financial Literacy Training Only	Total Groups	Financial Literacy Mean	Financial Literacy SD	Offered Savings Mean	Offered Savings SD	Mean difference	P Value	
Input Purchases (per person in Rwf)										
Seeds	57	18	75	78185.27	40694.23	97049.31	71246.29	-18864.05	0.1631133	
NPK	57	18	75	15916.29	8284.182	19756.47	14503.71	-3840.181	0.1631133	
Pesticide	57	18	75	3449.023	2314.225	4648.051	2920.359	-1199.029	0.0765683	*
Labor	57	18	75	27923.31	14533.65	34660.47	25445.1	-6737.161	0.1631133	
Total Inputs	57	18	75	125473.9	64487.04	156114.3	112227.1	-30640.42	0.1518244	
Other Characteristics										
Total Income	57	18	75	178471.4	154682.1	251415	285608.5	-72943.52	0.1669516	
Yield (tons/ha)	57	18	75	0.9502932	2.391151	0.6361148	0.4667773	0.3141783	0.582857	
P value is from a T test of equality of means between the two study arms. * = significant at 10% level										

*75 of the 80 groups were included in the balance test as it was based on pre-existing LWH information that was not attainable for 5 of the groups.

Scripts for savings training, pre-commitment for inputs, and targeted savings for inputs

(Scripts will be administered in a group meeting immediately following the administration of the group-level baseline survey by the trainer.)

Replace [RFO] (Rural Finance Organization) with either SACCO Mukura or COOPEC Inkunga.

COMMON MODULE (ALL GROUPS)

I.1. GENERAL SAVINGS

Point 1: Saving money in an individual bank account is a very smart way to protect your money and improve your wellbeing. As you know, your local [RFO] has saving accounts that are easy and affordable to *use.*

Counterpoint 1: *I already* have my own savings arrangements and even savings account within my group. So why should I have an individual savings account as well?

First ask the group *to list things that are good about individual savings account. When the group has* come up with several suggestions, move on to the next line:

Point 2: An individual savings account is yours alone. Though a group account is beneficial, you might have some financial needs which concern only you or your immediate family and not your entire group. The individual account can help you to address those needs.

Counterpoint 2: But I can just keep money at home. What are some of the benefits of saving my money in a [RFO] account instead of at home?

Ask the group *to make suggestions. After several things have been suggested, agree with the group and then move on to the next line.*

Point 3: Money is safer in a [RFO] account than at home. If you keep your money at home, it could be stolen or lost in a fire or could be destroyed by mice. If you keep it at the [RFO], it is protected.

Counterpoint 3: But I do not think that banks are safe. We have heard about a lot of Micro Finance institutions collapsing in 2006 in which many people lost their money.

Point 4: That is true but what happened in 2006 served as a learning lesson. Now, the Central bank has regulations in place which requires that all SACCOs and financial institutions are audited on a regular basis. There is also a District official representing RACA (Rwanda Cooperative Agency) and BNR (Central Bank) who conduct frequent inspections and gather monthly reports to ensure proper functioning. In case, a microfinance institution collapses, there is 100% government backing which ensures that people do not lose their money.

Counterpoint 4: Even if it is safer to keep money at the bank, it is more time-consuming to go to the bank.

Point 5: You do not need to go to the bank every time you need to save money. You can subscribe for automatic deductions at the bank. For example: If you receive money in your group account from the sale of harvest in season B 2012, you can sign up for a part of it being automatically transferred to your individual savings account. This will not only help you in saving valuable time but in saving money without having to constantly remind yourself.

Counterpoint 5: But we don't feel very happy about the customer services at the bank.

Point 6: The Government already has a program in place which trains the SACCO and COOPEC to provide quick and timely services to customers. If you feel dissatisfied with the services offered, you can discuss it with the SACCO & COOPEC during the General Assembly and together find the solution to the problem. If you feel that the SACCO & COOPEC are unresponsive, then you can call up the toll free number set up by the Government to tackle complains against the Bank.

Counterpoint 6: So what are the some of the other advantages of keeping money at the bank?

Point 7: If you keep money at the bank, you will not be tempted to incur unplanned expenditures. You will think twice about taking out money to spend. You could leave it in the bank to save for important purchases like school fees or buying fertilizer or accumulating the deposit for a new loan. Also, you can be sure to put away money in case you have an emergency in the future, like someone gets sick and needs to go to the hospital.

Also very importantly, as your money is saved at the [RFO], you will earn more money—that is called interest rates. The same way you have to pay an extra amount when you borrow, you get extra money when you save. This can help you buy more things in the future.

FOR TREATMENT GROUPS ONLY

I.2. SAVINGS FOR THE FUTURE

Counterpoint 1: It would be good to save for the future. But we have many needs now. How can we afford to save?

Point 1: In order to save, you have to first classify expenditures as necessary or unnecessary. For example: paying school fees, health insurance and inputs are necessary expenditures. But if your child already has 3 clothes, buying the 4th pair of clothes right now may not be necessary. Drinking beer every day may not be necessary.

Ask Group: Discuss how much money the farmers spend on beer per day and calculate the monthly expenditure on beer.

Once they calculate the amount of money spent on beer, the trainer points that this money could instead have been spent more fruitfully on purchasing agricultural inputs. Give example: If a farmer drinks 2 beers every night which cost 500 Rwf and he drinks for 20 days in a month. It means that the farmer spends about 10000 Rwf on beer every month. This money could have been used to buy 20 kg of DAP instead which would be enough to fertilize 0.2 hectare of wheat.

Once you have made a decision regarding what the necessary expenditures are, you then need to make a plan for how to spend your money on these. One way to do this is to divide the money you will have after selling your harvest and paying your loans into two amounts:

One amount is to use now, and

The other amount is to use for the future. Then, you can commit to keeping money for the future safe, and not touching it now or before you really need it.

Counterpoint 2: But in practice, how would I do that?

Point 2: Think about how much money you will have after you sell your harvest and repay your loan to [RFO]/Agribusinesses/TUBURA/other. Then, think about expenses you have immediately and until the next time you will receive some income.

Ask the group *to list things they need to spend money on immediately. Get a list of 5-6 things, then move on.*

Counterpoint 3: Yes, I will need money to buy books for my children, health insurance for my family and pay for electricity *[quote 2-3 examples from farmers' responses]*.

Point 3: Yes, these are the kinds of things you need to spend money on right away, when you get cash from your harvest. But now think of things you will need to spend money on in the future. What do you want to be absolutely sure you can afford in the future? How about agricultural inputs in season B?

Ask group *to list things they want to save for in the future. Make sure they are thinking of long-term things or expenses that will happen in a few months. Get the group to list 5-6 things, including agricultural inputs for the next two seasons, then move on.*

You can all think of many things. You will need to pay school fees. Also, you want to make sure you can buy seeds, fertilizer and pesticides for your crops in the next 2 seasons. And you want to have money for food next year during the dry season.

 These are important expenses. You should plan to protect some of your money so that it is available for those expenses. It may not be possible for you to save for all these expenses. ***Ask group:*** If you had to save for only one of these expenses, which one would it be?

Help them conclude that it is the expenditure on agricultural inputs. This is because if they do not have agricultural inputs, they will not be able to produce or sell any crops next season. This means that the income from their main source of livelihood i.e. agriculture will be 0 and thus, they will find it extremely difficult to fulfill any of the other expenditures in the following seasons.

If the group is growing beans in season A 2013, Mention the problem of the lack of gap between Season A and Season B and the need for cash for inputs. Is there time to dispose of crops and get cash between the 2 seasons? Make sure they conclude that no, there is not much time.

Counterpoint 4: Even if agricultural expenses are important, we can always take loans to purchase the inputs.

Point 4: Yes, you could take loans but you should remember that loans for agricultural inputs are risky. This is because agricultural production depends on several external factors which are outside one's control like rainfall and crop disease. For example if you take a loan and your crop does not do as well as expected like in last season when Irish potatoes were infested by pests, then you might not even generate enough cash to repay the loan. If you default on the loan, it will affect your ability to take loans in the future.

Ask Group to discuss reasons why the loan repayment rate was low in the last season and how this will affect their ability to take loans this season.

In addition, there is a limit to the number of loans that one can take. Thus, even if your production is bounty, if you take loans for inputs, it might reduce your chances of getting loans for other expenditures like starting a non-farm enterprise or emergencies like funeral. Saving for inputs will help you free up collateral for other loans.

Ask group to discuss things which are higher in value than inputs for which they wish to take loans

There are other difficulties associated with loans as well. There is extra cost associated with loan in the form of interest rate. If you take a loan from a formal institution, there will be loan collectors visiting your home frequently, interrupting your day-to-day life. And if you take a loan from your family or friends and are unable to repay it, it might cause problems in your personal relations.

Ask Group to discuss their experience with Loan Collectors and previous loans taken from friends/family.

CONCLUSION: Thus, you should take a loan for agriculture inputs only if necessary. You might think it is difficult to save for inputs but it is possible if you plan ahead of time. If you save for agricultural inputs, it will free up your collateral and increase your likelihood to get loans for other purposes like starting your own non-farm enterprise. If you start a non-farm enterprise like construction, then the profit that you earn from it will not depend on external factors like rainfall but will be highly correlated to your performance. This is unlike agriculture where even if you put in maximum effort, the production depends largely on external factors. Thus, you are most likely to default on a loan for agricultural inputs than for any other purpose. Thus, you should try and save as much as you can for inputs. Also, if you save for inputs, you will spend less for the same goods. Inputs cost less if you buy them without taking a loan.

Counterpoint 5: How do we decide how much we need to save for inputs for the future?

Point 5: You have worked with the zone agronomist to find out how much money you will need to buy inputs, - seeds , fertilizers and pesticides for the coming seasons A and B [*Refer to Farmers Record Book, FRB*]. These are your input requirements to secure high yield in the coming year. Think of a target for your savings that will allow you to meet these requirements.

It is particularly important to remember that during the previous season, Irish potatoes in the region got infected by pests which led to destruction of a large amount of the crop. So it is especially important if you save for pesticides and spray them on the crops in a timely manner so that it prevents diseases from attacking the crops and you do not lose your production.

Take the specific example of a farmer. Look at the farmer record booklet. Suppose farmer has 1 hectare of land on which he has decided to grow maize in season A and Irish Potatoes in Season B. Say his input requirements – seeds, fertilizers and pesticides for season A will cost 85000 RwF and for season B will cost 150,000 RwF.

First focus on SEASON A only. Ask farmer what expenditures he needs to incur in season A besides the purchase of inputs. Suppose he says that he needs to pay school fees for his son, buy new clothes for himself and purchase a cow.

Probe him to make sure that the expense on the new clothes is actually necessary. If he avoids it, he might be able to save for more productive reasons like purchase of inputs. Suppose he decides to forego the expenses on clothes.

Next ask him how much the school fees and cow will cost. Suppose he says the school fees will cost 20,000 RwF and the cow will cost 50,000 RwF.

After, he has listed all the expenditures he needs to incur in season A and you have helped him decide which are necessary and which are not, ask him which expenses need to be incurred immediately and for which he will need money in the future.

Maybe school fees is due immediately and he wants to purchase the cow in a few months.

Next, ask him to list all his sources of income. Make sure he includes not only the money that he will get from the harvest of season B but any other income like from land husbandry works, non-farm enterprise or any other source till the beginning of season A.

Help him calculate the expected income based on his production and past experience. Suppose you conclude that he would earn a total income of 120,000 Rwf.

Out of this income, he will pay the school fees as it is due immediately. He will then be left with 100,000 RwF as savings. This can be used to purchase inputs or the cow but not both.

If he decides to use the savings to buy the cow, then he will be left with 50,000 RwF which could be his target saving for inputs for season A. He would take a loan for the remaining amount of inputs.

On the other hand, the trainer can try and convince him that taking loans for agricultural inputs is very risky. Thus he might want to set his target as the entire amount of input costs for season A i.e. 85000 RwF and take a loan to purchase the cow instead.

OFFER OF TARGETED SAVING ACCOUNT: ZIGAMIRINTEGO ACCOUNT

Point 1: We have talked a lot about how to make a budget that gives you enough money for immediate needs and commits you to saving money for the future. Also, we've discussed why saving at the [RFO] is useful.

Counterpoint 1: Fine, at harvest I can make a plan about the amount of money I need for the short term and the amount I want to be sure to save for future inputs. But how am I to use the [RFO] to do this?

Point 2: Usually when you sell your harvest, you receive cash either in hand or in your current account. If you receive cash in hand, it is better not to keep it at home – as we discussed it is not safe there. It is better to save your money on an account. In addition, as we have discussed, saving money for the future is the smart way to go. Inputs will cost you less, you will have higher access to credit for other things. The problem is that it is easy to forget what target you have set for your input savings and not to withdraw that money for other needs.

Ask the group. *Ask the group if they have ever tried to save but failed to. Get a few examples and then move on to the next point.*

This season SACCO Mukura and COOPEC Inkunga are offering a new account called the ZIGAMIRINTEGO account. The ZIGAMIRINTEGO account is an account where you set a target for your input savings for season A 2013 and season B 2013 and we remind you of the target directly in your pass books.

Counterpoint 2: How would [RFO] know my target? How would I remember it better with ZIGAMIRINTEGO than without?

Ask the group. *Does the group find it easy to remember their targets? Do they sometimes regret spending money on not-so-useful things after the harvest instead of securing inputs?*

Point 3: You would have to set your own target using the required amount of inputs you have worked out with the agronomist. You can set up to 2 different targets:

  - A target to save for seeds, fertilizers and pesticides in season A
  - A target to save for seeds, fertilizers and pesticides in season B

For each target, you will have separate reminders in your Pass Book. Thus, you will remember to save!

Counterpoint 3: So if I agree to this, what do I have to do?

Point 4: The first thing to do is to open an ZIGAMIRINTEGO account at your [RFO]. I can help with filling out the forms today.

Once you open the ZIGAMIRINTEGO account, you can do any of the following:

1. You can sign up to have the money you currently have in your [RFO] account transferred directly into the ZIGAMIRINTEGO account. You could transfer either the entire target amount or a portion of it.

2. You can sign up to have the money that will receive from the harvest of season B 2012 transferred directly into the ZIGAMIRINTEGO account. You could transfer either the entire target amount or a portion of it.

3. You can sign up to have a certain percentage of your future earnings transferred directly into the ZIGAMIRINTEGO account. You could decide the percentage yourself. (MAY NOT BE FEASIBLE OPERATIONALLY)

4. You can deposit cash in the ZIGAMIRINTEGO account whenever you visit the SACCO/COOPEC.

The first 3 options will help you in achieving your target more easily as you will not have to constantly remind yourself unlike in the fourth option.

You could also register for any combination of the above options.

Counterpoint 4: You'll ask – is that really all I have to do?

Point 5: Yes. It is very easy. If you open an account or already have one at the [RFO], and fill out the form for the ZIGAMIRINTEGO account, then you can save towards your target every time you visit the [RFO] or you can subscribe to any of the automatic deductions

Ask the group *if there are any questions about how to sign up for ZIGAMIRINTEGO.*

Counterpoint 5: What if I decide I don't want to just save for inputs? What if I have other savings needs?

Point 6: You can still open a ZIGAMIRINTEGO account. Saving for inputs will help you improve the way you manage your money. As you learn to save for inputs, suddenly you will find it easier to save for many more other things you want to buy.

Counterpoint 6: Okay, so if I sign up for the ZIGAMIRINTEGO account, how do I get my cash?

Point 7: You can withdraw cash from the [RFO]. You can use your Pass Book and make the withdrawal by talking to a cashier, just the same way to talk to the cashier to deposit money or repay a loan. We encourage you to save up to your target and only take the cash out at planting time, when you need the inputs. We can work with you to suggest two dates by which you can try and achieve the target for season A and B respectively. However, you can always take the cash out before, whether or not you have met the target. This product is to help you save smart—but there is only so much it can do!

Counterpoint 7: So does that mean you can take money out whenever you want?

Point 8: Yes, you can, but you should remember the commitment you thought about to save money for a date in the future.

Counterpoint 8: Do I earn interest on money in this special account?

Point9: Yes. You earn the 5% interest rate a year on money in the commitment account as in most ordinary savings account. The only difference is that the money in the ZIGAMIRINTEGO commitment

account is set against a target you have chosen, helping you meet your future needs for inputs without credit.

Counterpoint 9: What if I earn more or less money than I thought I would have?

Point 10: It works just like the bottle caps we talked about earlier. After you start earning money from the harvest or other work you do, you can keep the amount you said you needed now to have available immediately in a normal current account. Then, the money you want to save for inputs goes into the ZIGAMIRINTEGO savings account to be safe for the future. When you have reached your target for saving for the future seasons, extra money you get from the interest rates will be added to your savings balance—money brings money!

In sum, the registration form and the Pass Book will simply state the target amount you want to save for input purchase in the two coming seasons A and B. That is, how much you need to save for inputs for the coming year.

You can choose any amount you want, and [RFO] will help you get ahead in the next season by keeping these savings for you—so that you remember to stick to the plan. I will help you now fill out the form if you would like to use this special ZIGAMIRINTEGO account and I will place stickers in your Pass Book to show you have registered for this account and remind you of your targets.

OFFER OF PRE-COMMITMENT ACCOUNT: HIGA UZIGAME ACCOUNT

Point 1: We have talked a lot about how to make a budget that gives you enough money for immediate needs and commits you to saving money for the future. Also, we've discussed why saving at the [RFO] is useful.

Counterpoint 1: Fine, at harvest I can make a plan about the amount of money I need for the short term, an amount I want to be sure to save for future inputs. But how am I to use the [RFO] to do this?

Point 2: Usually, when you sell your harvest, you receive cash either in hand or in your current account. If you receive cash in hand, it is better not to keep cash at home – as we discussed it is not safe there. It is better to save your money on an account. In addition, as we have discussed, saving money for the future is the smart way to go. Inputs will cost you less, you will have higher access to credit for other things. The problem is that it is easy to forget what target you have set for your input savings and not to withdraw that money for other needs.

Ask the group. *Ask the group if they have ever tried to save but failed to. Get a few examples and then move on to the next point.*

This season, we are offering you a new option: the HIGA UZIGAME account. You set a target for input savings and a date when you will need that money. We remind you of that target directly in your Pass Book. We make sure you don't withdraw before that date by locking the money away the money.

Counterpoint 2: How would [RFO] know my target? How would I remember it better with HIGA UZIGAME than without?

Ask the group. *Does the group find it easy to remember their targets? And to avoid spending money before they really need it? Do they sometimes regret spending money on not-so-useful things after the harvest instead of securing inputs?*

Point 3: You would have to set your own target using the required amount of inputs you have worked out with the agronomist. You can set up to 2 different targets:

- A target to save for seeds, fertilizers and pesticides in season A
- A target to save for seeds, fertilizers and pesticides in season B

For each target, you will have separate reminders in your Pass Book. Thus, you will remember to save!

In addition, for each target you will set a date at which you will most likely need the money to buy the inputs you require. Hence, you set up two dates:

- A withdrawal date when you will need money for seeds and fertilizer in season A. You could withdraw the money either on this date or anytime after this date till the end of season A. But you can withdraw only ONCE in season A.
- A withdrawal date when you will need money for seeds and fertilizer in season B. You could withdraw the money either on this date or anytime after this date till the end of season B. But you can withdraw only ONCE in season B.

Counterpoint 4: Withdrawal only once a season? But it is my money. Why should I not be allowed to withdraw it whenever I need? That seems really unfair.

Point 5: The withdrawal only once a season is to help you protect some of your money so that it is available for purchasing inputs in the future. By committing to locking away the money until a date in the future, you ensure that you do not end up spending it impulsively on the not-so-useful things.

Ask Group: Do they sometimes regret spending money on not-so-useful things after the harvest instead of securing inputs?

Counterpoint 5: But what if I need the money for a medical emergency or any other contingency in the future?

Point 6: Before deciding the amount of money that you want to pre-commit for inputs, you must think critically about various needs that you have immediately or any other needs that you might have in the future.

As discussed earlier, you should divide the money that get after sale of harvest and paying loans into two parts

- The amount you need to use now

- The amount you will need in the future. When deciding this you should not only think about expected expenses but also unexpected expenditures.

For example: If you receive 50,000 RwF after sale of harvest and paying loans. Say you need 10,000 RwF to spend immediately. Suppose if your only expected need in the future is inputs. You might decide to pre-commit the entire 40,000 RwF. But you should think about this carefully. You might want to keep a certain portion of this income for emergencies say 5000 RwF and pre-commit only 35,000 instead.

Counterpoint 6: How do I choose the withdrawal date?

Point 7: You should ideally choose a withdrawal date right before the planting season of season A and season B. The time of planting in the 2 seasons will obviously depend on the crop-mix that you select. So suppose that you are growing maize in season A 2013 and you decide to start planting on 15th September, you probably don't want to set the withdrawal date before 1st of September because you might be tempted to spend the money on other not-so-useful things. But also you don't want to set a very late date like 14th September because you will most likely need to purchase your inputs one day before planting. Even if you set the date as 1st September, if you don't purchase inputs till the 14th, you don't have to withdraw till the 14th. As mentioned before, you can withdraw the money either on the withdrawal date or anytime after that till the end of season A. But you can withdraw only ONCE in one season.

Counterpoint 7: So if I agree to this, what do I have to do?

Point 8: The first thing to do is to open a HIGA UZIGAME account at your [RFO]. I can help with filling out the forms today.

Once you open the HIGA UZIGAME account, you can do any of the following:

1. You can sign up to have the money you currently have in your [RFO] account transferred directly into the HIGA UZIGAME account. You could transfer either the entire target amount or a portion of it.
2. You can sign up to have the money that will receive from the harvest of season B 2012 transferred directly into the HIGA UZIGAME account. You could transfer either the entire target amount or a portion of it.
3. You can sign up to have a certain percentage of your future earnings transferred directly into the HIGA UZIGAME account. You could decide the percentage yourself. (MAY NOT BE FEASIBLE OPERATIONALLY)
4. You can deposit cash in the HIGA UZIGAME account whenever you visit the SACCO/COOPEC.

The first 3 options will help you in achieving your target more easily as you will not have to constantly remind yourself unlike in the fourth option.

You could also register for any combination of the above options.

Counterpoint 8: You'll ask – is that really all I have to do?

Point 9: Yes. It is very easy. If you open an account or already have one at the [RFO], and fill out the form for the HIGA UZIGAME account, then you can save towards your target every time you visit the [RFO] or you can subscribe to any of the automatic deductions.

Counterpoint 9: What if I decide I don't want to just save for inputs? What if I have other savings needs?

Point 10: You can still open a HIGA UZIGAME account. Saving for inputs will help you improve the way you manage your money. As you learn to save for inputs, suddenly you will find it easier to save for many more other things you want to buy. The HIGA UZIGAME account will help you save for the future and you will discover the value of keeping your money safe until you really need it. That will be useful for you in the future.

Counterpoint 10: Okay, so you think you want to try out the HIGA UZIGAME account, but you wonder— if I sign up for that, how do I get my cash?

Point 11: You will be able to withdraw the cash you locked away at the [RFO] in the HIGA UZIGAME on or after the withdrawal dates you are setting for yourself for inputs in seasons A and B. You will use use your Pass Book and make the withdrawal by talking to a cashier, just the same way to talk to the cashier to deposit money or repay a loan. We encourage you to save up to your target! Then at planting time you will have that cash on hand as you need the inputs—you will get ahead of the game! This product is to help you save smart—but you need to be smart and meet your target for it to really be helpful!

Counterpoint 12: Do I earn interest on money in this special account?

Point 13: Yes. You earn the 5% interest rate a year on money in the commitment account as in most ordinary savings account. The only difference is that the money in the HIGA UZIGAME commitment account is locked away until the date you have chosen.

Counterpoint 13: What if I earn more or less money than I thought I would have?

Point 14: It works just like the bottle caps we talked about earlier. After you start earning money from the harvest or other work you do, you can keep the amount you said you needed now to have available immediately in a normal current account. Then, the money you want to save for inputs goes into the HIGA UZIGAME savings account to be locked away and safe for the future. When you have reached your target for saving for the future seasons, extra money you get from the interest rates will be added to your savings balance—money brings money!

In sum, the registration form and the Pass Book will simply state the target amount and withdrawal date of the commitment you want to make to input purchase for the two coming seasons A and B. That is, how much you need to save for inputs, and when you will access this money to buy the inputs you require.

 You can choose any amount and date you want, and [RFO] will help you get ahead in the next season by holding the savings for you—so that you stick to the plan. I will help you now fill out the form if you would like to use this special HIGA UZIGAME account and I will place stickers in your Pass Book to show you have registered for this account and remind you of your targets and the dates you need the money.

Focus Group Discussions and Interviews Protocol

Objective

We would like to conduct Focus Group Discussions and interviews with farmers and MFI staff and managers who participated in the DIME rural finance project in the Karongi district, Rwanda. The interviews will act to supplement the quantitative data that has been collected from household surveys and transaction history. The objective is twofold:

- Give more context to the inputs market
- Better understand the functioning and shortcomings of ordinary accounts and special accounts

Who will conduct discussions/interviews?

A translator will conduct the discussions/interviews in Kinyarwanda unless participants are conversational English. We believe that we will be able to conduct the manager interview in English with the manager at SACCO Mukura.

How will the data be collected?

As the discussions are underway there will be simultaneous translation so that Elena and Emma can take notes and suggest ways for the translator to guide the discussion if there is anything that should be discussed in more depth.

Sample construction

Focus Group Discussions (FGDs)

There will be six FGDs based on the frequency of account use and/or treatment type and each will be within one zone.

Four FGDs will be with people based on the frequency of their ordinary account use / changes in transaction values, regardless of what type of treatment they received. High frequency users were drawn from the top 25% of users from Kagano, which had on average the highest transactions across all zones. In Ruhuha, a relatively high increase in savings balances was observable over time which makes it interesting to talk to this group. In Rusasa little happened with regards to the savings balances and hence the third FGD will take happen there. As Rusasa also has the highest number of COOPEC clients, the fourth FGD will take place there as well.

- 1 FGD of high frequency ordinary account users: SACCO Mukura in Kagano
- 1 FGD of high increase in savings balances for ordinary account users: SACCO Mukura in Ruhuha
- 1 FGD of little change in savings balances for ordinary account users: SACCO Mukura in Rusasa
- 1 FGD of COOPEC Inkunga clients: Rusasa

Two FGDs will be with people who were in one of the treatment groups and opened a special account. They were randomly selected from all special account holders, controlling for the SACCO Mukura and zone Kagano. The focus is on one branch to establish comparability between the two special account discussions. The COOPEC merged the special accounts with the ordinary accounts

as the contract wasn't renewed- this might bias the data and hence has been excluded from the random selection.

- 1 FGD targeted account holders: SACCO Mukura in Kagano
- 1 FGD pre-commitment account holders: SACCO Mukura in Kagano

Each group consists of 12 randomly selected participants with the goal of having 8-12 farmers finally participating in the FGD as non- attendance might be an issue.

Staff Interviews
Staff interviews will be conducted with one to two cashiers from each branch. If there are two available cashiers the interview would be conducted with them together.

Managers
An interview will be conducted with the managers from SACCO Mukura and COOPEC Inkunga. As mentioned above, we hope to conduct the interview with the manager from SACCO Mukura in English.

Timeline for data collection
Data collection is expected to take four work days, ideally between 11-22 March, 2014. The staff and manager interviews are probably going to take place between 12-14 March and the FGDs in the 17-21 March, 2014 workweek. In agreement with the LWH staff, we are most probably going to spit up to conduct the FGDs parallel.

Focus group discussion- Farmer group

Instructions for the translator:
The instructions to lead the discussion can be found in italics; before a section as an introduction of what is to come and after a question, in brackets, to tell you how to handle the question. The introduction should be read out loud, the instructions in brackets are a reminder for you to guide the participants through the question.

Ordinary & special accounts
Part I
Welcome everyone, we work with LWH and are very happy to have you all here! We want to collect data on agricultural activities and savings towards agricultural inputs. Since you are all part of LWH and members of Sacco Mukura/ Coopec Inkunga, we are interested in knowing more about how you save money and your experience with the SACCO are curious about your opinion.

In the following, there are some questions on agricultural behavior and we invite everyone to actively contribute to the discussion. If questions are unclear, you are welcome to clarify them immediately to make sure that the discussion can work properly.

1. Do you have a plot in a consolidated land site?
2. Do you have private plots that are not on land consolidation sites?
3. What crops did you grow on consolidated plots??
4. How is it decided what crop is grown on the consolidated land?
   [Trying to understand how the group picks a crop. Is it the group together or an individual?]
5. What do you think is the most profitable crop?
6. What inputs do you use for *[pick the top crop from question 4]*?
   a. What inputs do you use for *[pick second most common crop from question 4]*?
   b. What inputs do you use for *[pick third most common crop from question 4]*?
7. Did you plan what types/quantities of inputs you would need with an agronomist?
   a. Did most group members reach that target? Why or why not?
   b. Do you think that it was a realistic target?
8. Where do you get your *[input 1]* from?
   [Refer to answers in 6]
   a. Does the distributor come to your house or do you go somewhere?
   b. Are the inputs delivered to your village or do you collect them yourself?
   c. If you collect them, how long does it take you with which transport?
9. Where do you get your *[input 2]* from?
   [Refer to answers in 6]
   a. Does the distributor come to your house or do you go somewhere?
   b. Are the inputs delivered to your village or do you collect them yourself?
   c. If you collect them, how long does it take you with which transport?
10. Where do you get your *[input 3]* from?
    [Refer to answers in 6, make sure that fertilizer, seeds and compost have been mentioned.]
    a. Does the distributor come to your house or do you go somewhere?

  b. Are the inputs delivered to your village or do you collect them yourself?

  c. If you collect them, how long does it take you with which transport?

11. In which month do you get your inputs for *[crop 1]*?

 [Refer to answers in 6, make sure that fertilizer, seeds and maybe compost are mentioned]

  a. In which month do you get your inputs for *[crop 2]*?

  b. In which month do you get your inputs for *[crop 3]*?

12. If you buy seeds, how do you fund the purchase of them?

 [If there is any unclarity about the question, list the suggestions below.]

  a. Savings

  b. Formal group loans (from whom?)

  c. Formal individual loans (from whom?)

  d. Borrow from family (in kind?)

  e. Borrow from friends (in kind?)

  f. Borrow from village chief (in kind?)

  g. Borrow from shop owner (in kind?)

  h. Borrow from religious leader (in kind?)

  i. Borrow from money lender (in kind?)

13. If you use fertilizer, how do you fund the purchase of it?

  a. Savings

  b. Formal group loans (from whom?)

  c. Formal individual loans (from whom?)

  d. Borrow from family (in kind?)

  e. Borrow from friends (in kind?)

  f. Borrow from village chief (in kind?)

  g. Borrow from shop owner (in kind?)

  h. Borrow from religious leader (in kind?)

  i. Borrow from money lender (in kind?)

14. How do you fund the purchase of any other inputs?

  a. Savings

  b. Formal group loans (from whom?)

  c. Formal individual loans (from whom?)

  d. Borrow from family (in kind?)

  e. Borrow from friends (in kind?)

  f. Borrow from village chief (in kind?)

  g. Borrow from shop owner (in kind?)

  h. Borrow from religious leader (in kind?)

  i. Borrow from money lender (in kind?)

15. Were there any instances of major crop failure during the last four harvests? If so, what were they caused by? *[Examples: Early rains, drought, crop disease, unproductive seeds, etc.]*

16. Were there any problems with sales of your crops during the last four harvests? *[Examples: Price decrease, low demand, etc.]*

Now we would like to talk to you about farmers' cooperatives. We know that most of you are a member of at least 1 cooperative and would like to better understand how they function.

17. When do you pay the fees for the cooperative?
18. What services do the cooperative fees cover?
19. What crops do you sell through the cooperative?
20. How are sales of the crops structured *[i.e. does the coop handle the entire sale? To whom does the coop sell? Does the coop keep a percentage of the revenue from sale?]*?
21. Did you reduce the amount of livestock during the last year?

Part II

[Throughout the discussion, pay attention to how people feel about the Sacco/ Coopec, in particular whether they seem to trust the financial institutions or not.]

Now, we are going to talk about saving behavior. We would like to know more about the experience you have had with the Sacco where you have your account.

Okay, let's start with the first question [read it out loud].

1. Do you feel welcome at the SACCO Mukura / Coopec Inkunga?
2. What are the main challenges you face in banking at the SACCO?
3. Do you think you benefit from having an account at the SACCO?
    a. Why, why not?
4. Are the staff helpful and do they explain the products clearly?
5. What would you change about the SACCO/ Coopec if you could?
   [Select financial institution according to discussion group. If people are hesitant to answer, suggest topics like 'Long waiting lines'.]

Part III

Special accounts only

1. What is the difference between the ordinary and special *[either pre-commitment or targeted]* account?

We see that on average you made few deposits last year. We are curious to know why you used it so little even though you paid the fee to open up the account.

2. Did you make any deposits into the special account?
    a. Why, why not?
   [If they have troubles getting the discussion started, suggest topics like 'I do not want to lock my money away as I don't know whether I will need it later' or 'I can as well save on my normal account if I want to'.]
3. Did you make any withdrawals from your special account? Why or why not?

Staff Interview

Instructions for the translator:
The instructions to lead the discussion can be found in italics; before a section as an introduction of what is to come and after a question, in brackets, to tell you how to handle the question. The introduction should be read out loud, the instructions in brackets are a reminder for you to guide the participants through the question.

Ordinary & special accounts
First of all we want to thank you for all of your help in collecting the data for LWH and the World Bank's research. Without you we would never have been able to do it! Our findings will be used to continue improving financial outreach here in Rwanda.

Today we would like to ask you a little about your work and the financial products that the SACCO/COOPEC offer. Particularly, we would like to talk about people's normal accounts and the special (pre-commitment and target) accounts some were offered in conjuncture with the LWH project.

1. How long have you worked at SACCO Mukura/COOPEC Inkunga?
2. What is your position?
3. Please describe a normal checking account.
   a. How much is the fee for opening an account?
   b. Are there transaction fees? *[Fees for automatic deposits?]*
   c. Are there end of year fees? Is everyone charged the exact same fees or does it depend on particular services offered?
4. How well do you think most clients understand how their ordinary accounts?
   a. How to open an account
   b. Fees (registration, end of year, deposit)
   c. When to make withdrawals
5. How do you think that clients could be made to better understand their ordinary accounts?
6. Do you think that clients use their ordinary accounts enough?
   [Skip question a) if the answer is yes]
   a. If not, how do you think clients could be incentivized to use their ordinary accounts more?
7. Please describe the pre-commitment account that some clients were offered.
   a. How much is the fee for opening an account?
   b. Are there transaction fees? *[Fees for automatic deposits?]*
   c. Are there end of year fees? Is everyone charged the exact same fees or does it depend on particular services offered?
   d. Are there limitations on withdrawals?
8. Please describe the targeted account that some clients were offered.
   a. How much is the fee for opening an account?
   b. Are there transaction fees? *[Fees for automatic deposits?]*

      c.   Are there end of year fees? Is everyone charged the exact same fees or does it depend on particular services offered?

      d.   Are there limitations on withdrawals?

9. When a client came in to make a deposit, how did you know which account to deposit the money in if they had both current and special accounts?

10. Where are the special account files normally stored?

   [Are they all stored in the same place?]

11. We have noticed that people did not use their special accounts very much. Why do you think that might be? What are possible constraints to using their accounts?

12. Do you think that clients who were offered the special account use it for something different than what their normal account was used for? If so, what?

13. What do you think most clients use their normal account for?

   [Are the accounts being used to save or are they used because of automatic deposits when LWH pays them?]

14. How well do you think most clients understand their special accounts?

      a.   How to open an account

      b.   Fees (registration)

      c.   When to make withdrawals

      d.   Interest rate

15. How do you think that clients could be made to better understand their special accounts?

16. How do you think clients could be incentivized to use their special accounts more?

17. Did clients ever complain about any features of the special accounts? If so, what features?

18. Did any clients try to make withdrawals from the special accounts during the restricted-withdrawal period? If so, how did you handle the situation?

19. What kind of new product do you think your clients would like and use?

   [If they are having trouble coming up with something perhaps suggest temporarily lowered account opening fees, savings accounts with interest, etc.]

Instructions for the translator:
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Ordinary & special accounts

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Today we would like to ask you a little about your work and the financial products that the SACCO/COOPEC offer. Particularly, we would like to talk about people's normal accounts and the special (pre-commitment) accounts some were offered in conjuncture with the LWH project.

1. What do you think most clients use their normal account for?
2. How well do you think most clients understand their ordinary accounts?
   a. How to open an account
   b. Fees (registration, end of the year, deposit)
   c. When to withdraw money (do they just withdraw when there is money put into the account or do they withdraw when there is an expense coming up?)
3. How do you think that clients could be made to better understand their ordinary accounts?
4. What do you think are the constraints to people opening an ordinary account?
5. What do you think are the constraints to people using their ordinary account?
6. How do you think clients could be incentivized to use their ordinary accounts more?

7. Please describe the two special accounts that some clients were offered and how they have been introduced to the staff.*[If the manager does not seem to know enough about the special accounts to describe them, focus on the second part of the question.]*
8. We have noticed that people did not use their special accounts very much. Why do you think people who were offered a special account did not use it more often?
9. Do you think that clients who have a special account use it in a different way than they use their normal account? If so, how?
10. How well do you think most clients understand their special accounts?
    a. How to open an account
    b. Fees (registration)
    c. When to withdraw money (do they just withdraw when there is money put into the account or do they withdraw when there is an expense coming up?)
    d. Interest rates?

11. How do you think that clients could have been made to better understand their special accounts?
12. What do you think were the constraints to people opening a special account?
13. What do you think were the constraints to people using their special accounts? How do you think clients could be incentivized to use their special accounts more?
14. What kind of new product do you think your clients would like and use? [If they are having trouble coming up with something perhaps suggest temporarily lowered account opening fees, savings accounts with interest, etc.]
15. What challenges did the SACCO/COOPEC face in administering the special accounts?
16. What would you do differently if you were going to launch the special accounts this year, knowing your past experience?
17. Do you think that you had enough support when introducing the special accounts?
    a. Would you want more assistance and if so, in what?
18. What are the main challenges in developing (and/or administering) new financial products?

Appendix Table 2: Summary Statistics Conditional on Merging of Datasets

Summary Statistics Conditional on Merging of Datasets

Variable	Merged				Mean difference	(p-value)
	No Mean	Std dev.	Yes Mean	Std dev.		
Mean savings	18,921.91	89,292.21	9,649.71	32,883.98	9,272.20	0.10

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

Appendix Table 3: Regression Output to Test Hypothesis 1 and Hypothesis 2, Winsorized Data

Regression 1-3: Varying Post-Treatment Periods, Winsorized Data

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	One Month Mean			Two Month Mean			Thirteen Month Mean			Fourteen Month Mean		
After intervention	4,766	4,408	4,416	3,321	3,321	3,309	5,464	5,464	5,523	5,854*	5,854	5,917
	(3,060)	(4,337)	(4,378)	(2,558)	(3,618)	(3,657)	(3,265)	(4,618)	(4,644)	(3,313)	(4,685)	(4,710)
Attended training	1,244	-1,613	45,986***	1,008	-2,364	-39,122***	1,008	-2,894	30,768***	1,008	-3,026	31,053***
	(1,362)	(2,745)	(2,603)	(1,368)	(2,442)	(2,461)	(1,368)	(2,397)	(2,410)	(1,368)	(2,509)	(2,522)
DiD coefficent	-2,667	-2,545	-2,547	-1,734	-1,734	-1,717	-4,378	-4,378	-4,433	-4,454	-4,454	-4,513
	(3,875)	(5,490)	(5,532)	(3,454)	(4,884)	(4,921)	(3,390)	(4,795)	(4,821)	(3,548)	(5,018)	(5,044)
Observations	967	967	963	982	982	978	982	982	978	982	982	978
R-squared	0.002	0.733	0.733	0.001	0.732	0.732	0.003	0.778	0.778	0.003	0.773	0.773
Village dummies	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Individual dummies	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Full set of controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Robust	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses

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

Regression 3 Sensitivity Analysis- Varying Post-Treatment Periods

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	One Month Mean			Two Month Mean			Thirteen Month Mean			Fourteen Month Mean		
After intervention	4,401	4,409	4,409	3,332	3,321	3,321	10,987	11,096	11,096	12,602	12,726	12,726
	(4,336)	(4,377)	(4,377)	(3,620)	(3,658)	(3,658)	(12,227)	(12,310)	(12,310)	(13,999)	(14,093)	(14,093)
Attended training	16,867***	6,954**	46,019***	16,537***	-208,722***	-38,699***	18,070***	-81,051***	34,027***	18,338**	-88,337***	34,552***
	(2,744)	(2,843)	(2,556)	(2,511)	(2,530)	(2,530)	(6,135)	(6,177)	(6,177)	(7,115)	(7,163)	(7,163)
DiD coefficent	-2,553	-2,555	-2,555	-1,988	-1,973	-1,973	-9,853	-9,959	-9,959	-10,778	-10,897	-10,897
	(5,489)	(5,531)	(5,531)	(5,022)	(5,059)	(5,059)	(12,271)	(12,354)	(12,354)	(14,229)	(14,326)	(14,326)
=1 if account holder is male	8,825***	4,809***	124,303***	8,844***	-372,017***	-129,274***	5,953***	-156,471***	84,867***	5,779***	-170,239***	84,577***
	(5.03e-07)	(145.9)	(645.8)	(6.40e-07)	(0.211)	(0.0126)	(9.80e-07)	(0.0799)	(0.000545)	(9.64e-07)	(0.0881)	(0.000369)
=1 when MFI is COOPEC	-14,761***	-19,311***	-480,213***	-14,637***	.479e+06**	382,300***	-4,376***	585,446***	-505,079***	-4,472***	641,736***	-509,695***
	(9.06e-07)	(580.8)	(2,202)	(8.13e-07)	(0.863)	(0.0444)	(1.17e-06)	(0.327)	(0.00191)	(1.16e-06)	(0.361)	(0.00136)
Site	-15,800***	-4,579**	-42,231***	-13,551***	-166,370***	51,356***	-11,801***	-70,824***	-16,240***	-11,678***	-77,011***	-15,225***
	(2,168)	(2,125)	(1,963)	(1.11e-06)	(0.0930)	(0.00452)	(1.54e-06)	(0.0353)	(0.000192)	(1.54e-06)	(0.0389)	(0.000127)
=1 if Assigned to Targeted Treatment	-12,846***	24,763***	54,265***	-12,817***	.986e+06**	7,577***	-8,348***	.275e+06**	95,115***	-8,234***	.382e+06**	90,173***
	(1.32e-06)	(1,147)	(119.5)	(1.37e-06)	(1.654)	(0.00232)	(2.09e-06)	(0.627)	(9.33e-05)	(2.06e-06)	(0.691)	(5.47e-05)
Has savings account	9,651***	3,753***	-107,196***	9,794***	165,403***	135,645***	12,137***	75,386***	-62,120***	12,287***	81,570***	-60,848***
	(7.39e-07)	(62.54)	(617.9)	(4.69e-07)	(0.0910)	(0.0122)	(5.90e-07)	(0.0345)	(0.000516)	(6.04e-07)	(0.0380)	(0.000345)
Age		416.7***	40,095***		-126,429***	-39,683***		-51,688***	34,556***		-56,380***	34,680***
		(49.14)	(203.4)		(0.0719)	(0.00402)		(0.0273)	(0.000172)		(0.0301)	(0.000117)
Age squared		-4.078***	-402.0***		1,277***	375.5***		514.7***	-381.5***		562.5***	-383.9***
		(0.496)	(1.983)		(0.000731)	(3.95e-05)		(0.000277)	(1.69e-06)		(0.000305)	(1.17e-06)

Highest level of education completed		3,946***	25,627***		398,750***	-19,065***		170,359***	24,764***		184,590***	23,943***
		(152.9)	(113.9)		(0.221)	(0.00223)		(0.0836)	(9.44e-05)		(0.0921)	(6.23e-05)
Number of HH members			-29,813***			36,634***			-17,538***			-17,134***
			(169.2)			(0.00334)			(0.000136)			(8.68e-05)
Plot area owned in square meters			0.393***			-3.516***			-3.495***			-3.690***
			(0.00990)			(2.41e-07)			(7.41e-09)			(6.78e-09)
Observations	967	963	963	982	978	978	982	978	978	982	978	978
R-squared	0.733	0.733	0.733	0.734	0.734	0.734	0.677	0.677	0.677	0.656	0.656	0.656
Village dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Robust	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses

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

Testing Joint Significance for Added Controls

Post- treatment period	Control blocks tested	Degrees of freedom	Remaining degrees of freedom	F statistics	(p-value)
One Month Mean	Account related	5.00	18.00	1.79e+22	0.00
	Household head related	3.00	18.00	6.32e+13	0.00
	Household size and wealth proxy	2.00	18.00	9.95e+16	0.00
Two Month Mean	Account related	5.00	18.00	3.27e+22	0.00
	Household head related	1.00	18.00	13,159.40	0.00
	Household size and wealth proxy	2.00	18.00	7.40e+16	0.00
Thirteen Month Mean	Account related	5.00	18.00	2.44e+23	0.00
	Household head related	1.00	18.00	11,838.42	0.00
	Household size and wealth proxy	2.00	18.00	9.12e+17	0.00
Foourteen Month Mean	Account related	5.00	18.00	2.49e+23	0.00
	Household head related	1.00	18.00	4,341.20	0.00
	Household size and wealth proxy	2.00	18.00	9.49e+17	0.00

Regression 3 Sensitivity Analysis- Varying Post-Treatment Periods, Winsorized Data

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	One Month Mean			Two Month Mean			Thirteen Month Mean			Fourteen Month Mean		
After intervention	4,408	4,416	4,416	3,321	3,309	3,309	5,464	5,523	5,523	5,854	5,917	5,917
	(4,337)	(4,378)	(4,378)	(3,618)	(3,657)	(3,657)	(4,618)	(4,644)	(4,644)	(4,685)	(4,710)	(4,710)
Attended training	16,863***	6,979**	45,986***	16,273***	-209,661***	-39,122***	15,195***	-83,748***	30,768***	15,040***	-90,710***	31,053***
	(2,745)	(2,844)	(2,591)	(2,442)	(2,461)	(2,461)	(2,397)	(2,410)	(2,410)	(2,509)	(2,522)	(2,522)
DiD coefficent	-2,545	-2,547	-2,547	-1,734	-1,717	-1,717	-4,378	-4,433	-4,433	-4,454	-4,513	-4,513
	(5,490)	(5,532)	(5,532)	(4,884)	(4,921)	(4,921)	(4,795)	(4,821)	(4,821)	(5,018)	(5,044)	(5,044)
=1 if account holder is male	8,825***	4,858***	124,216***	8,844***	-373,312***	-129,955***	5,953***	-156,081***	83,519***	5,779***	-168,405***	84,036***
	(4.89e-07)	(146.7)	(552.6)	(6.20e-07)	(0.211)	(0.0126)	(9.77e-07)	(0.0798)	(0.000519)	(9.52e-07)	(0.0871)	(0.000379)
=1 when MFI is COOPEC	-14,761***	-19,508***	-479,915***	-14,637***	.485e+06**	384,863***	-4,376***	583,840***	-498,832***	-4,472***	634,181***	-506,517***
	(8.91e-07)	(583.6)	(1,884)	(7.83e-07)	(0.866)	(0.0447)	(1.17e-06)	(0.327)	(0.00182)	(1.14e-06)	(0.357)	(0.00138)
Site	-15,803***	-4,561**	-42,204***	-13,414***	-166,908***	51,628***	-11,664***	-70,485***	-15,494***	-11,542***	-75,918***	-14,651***
	(2,168)	(2,125)	(1,995)	(1.06e-06)	(0.0933)	(0.00455)	(1.54e-06)	(0.0352)	(0.000182)	(1.51e-06)	(0.0384)	(0.000131)
=1 if Assigned to Targeted Treatment	-12,846***	24,374***	54,249***	-12,680***	.996e+06**	7,447***	-8,211***	.272e+06**	91,922***	-8,098***	.368e+06**	87,381***
	(1.30e-06)	(1,152)	(102.2)	(1.32e-06)	(1.659)	(0.00233)	(2.09e-06)	(0.626)	(8.81e-05)	(2.04e-06)	(0.683)	(5.71e-05)
Has savings account	9,651***	3,732***	-107,113***	9,794***	166,023***	136,363***	12,137***	75,200***	-60,785***	12,287***	80,692***	-60,360***
	(7.29e-07)	(62.85)	(528.7)	(4.45e-07)	(0.0912)	(0.0123)	(5.91e-07)	(0.0344)	(0.000492)	(5.85e-07)	(0.0376)	(0.000354)
Age		433.3***	40,067***		-126,880***	-39,915***		-51,552***	34,071***		-55,741***	34,471***
		(49.38)	(174.0)		(0.0721)	(0.00404)		(0.0272)	(0.000164)		(0.0297)	(0.000120)
Age squared		-4.246***	-401.8***		1,281***	377.7***		513.4***	-376.5***		556.0***	-381.5***
		(0.499)	(1.697)		(0.000733)	(3.97e-05)		(0.000277)	(1.61e-06)		(0.000302)	(1.20e-06)

Highest level of education completed		3,895***	25,611***		400,109***	-19,195***		169,950***	24,073***		182,665***	23,420***
		(153.6)	(97.43)		(0.221)	(0.00224)		(0.0835)	(8.98e-05)		(0.0911)	(6.41e-05)
Number of HH members			-29,790***			36,813***			-17,146***			-16,955***
			(144.7)			(0.00336)			(0.000129)			(8.95e-05)
Plot area owned in square meters			0.392***			-3.524***			-3.470***			-3.656***
			(0.00847)			(2.42e-07)			(7.03e-09)			(6.56e-09)
Observations	967	963	963	982	978	978	982	978	978	982	978	978
R-squared	0.733	0.733	0.733	0.732	0.732	0.732	0.778	0.778	0.778	0.773	0.773	0.773
Village dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Robust	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses

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

Appendix Table 7: Testing Joint Significance for Added Controls, Winsorized Data

Testing Joint Significance for Added Controls, Winsorized Data

Post- treatment period	Control blocks tested	Degrees of freedom	Remaining degrees of freedom	F statistics	(p-value)
One Month Mean	Account related	5.00	18.00	1.78e+22	0.00
	Household head related	3.00	18.00	6.32e+13	0.00
	Household size and wealth proxy	2.00	18.00	9.96e+16	0.00
Two Month Mean	Account related	5.00	18.00	3.01e+22	0.00
	Household head related	1.00	18.00	3,373.05	0.00
	Household size and wealth proxy	2.00	18.00	7.25e+16	0.00
Thirteen Month Mean	Account related	5.00	18.00	2.81e+23	0.00
	Household head related	1.00	18.00	3.13e+10	0.00
	Household size and wealth proxy	2.00	18.00	9.13e+17	0.00
Foourteen Month Mean	Account related	5.00	18.00	2.46e+23	0.00
	Household head related	1.00	18.00	33,492.32	0.00
	Household size and wealth proxy	2.00	18.00	9.44e+17	0.00

Regression 4- Varying Post-Treatment Period, Winsorized Data

VARIABLES	(1) One Month Mean	(2) Two Month Mean	(3) Thirteen Month Mean	(4) Fourteen Month Mean
After intervention	4,416	3,309	5,523	5,917
	(4,426)	(3,695)	(4,693)	(4,759)
Attended training	50,600***	-38,184***	31,893***	31,775***
	(5,668)	(9,712)	(8,805)	(9,208)
DiD coefficient	-18,997	-17,659	-9,526	-11,153
	(26,902)	(23,672)	(8,751)	(8,788)
=1 if account holder is male	133,505***	-130,141***	86,173***	85,033***
	(14,210)	(29,122)	(23,956)	(25,205)
Interaction: account holder is male, attended training and post treatment	3,618	2,800	1,587	2,154
	(3,978)	(3,534)	(2,392)	(2,593)
=1 when MFI is COOPEC	-518,425***	379,123***	-513,103***	-515,611***
	(45,107)	(104,518)	(87,674)	(91,906)
Interaction: COOPEC, attended training and post treatment	-385.3	76.17	2,943	3,138
	(3,924)	(3,417)	(2,271)	(2,357)
Site	-45,241***	52,238***	-16,818*	-15,502
	(4,649)	(10,903)	(9,111)	(9,487)
Interaction: site=1 if Karongi 13, attended training and post treatment	-3,197	-3,079	557.7	370.4
	(4,402)	(4,042)	(4,497)	(4,834)
=1 if Assigned to Targeted Treatment	51,850***	4,335	91,737***	86,720***
	(2,151)	(8,021)	(6,225)	(6,557)
Interaction: treatment=1 if assigned to targeted treatment, attended training an	7,068*	4,945	1,941	2,535
	(3,519)	(3,455)	(2,217)	(2,496)
Has a savings account	-117,521***	135,093***	-63,907**	-61,811**
	(11,534)	(30,468)	(23,834)	(25,137)
Interaction: has special account, attended training and post treatment	-68.18	143.9	-1,048	-1,512
	(3,480)	(3,078)	(3,119)	(3,111)
Age	43,314***	-39,722***	35,154***	35,040***
	(3,595)	(9,934)	(8,188)	(8,584)
Interaction: age, attended training and post treatment	501.5	492.4	177.3	262.0
	(1,215)	(1,062)	(615.1)	(650.6)
Age squared	-433.8***	375.4***	-386.9***	-386.8***
	(35.53)	(96.96)	(80.12)	(84.02)
Interaction: age squared, attended training and post treatment	-4.750	-4.513	-2.094	-2.969
	(11.66)	(10.44)	(6.242)	(6.668)

Highest level of education completed	26,992***	-19,484***	24,639***	23,729***
	(1,849)	(5,688)	(4,572)	(4,782)
Interaction: education, attended training and post treatment	1,998	1,878	71.14	-24.30
	(3,555)	(3,018)	(1,495)	(1,485)
Number of HH members	-32,440***	36,656***	-18,041**	-17,435**
	(2,895)	(8,487)	(6,736)	(7,070)
Interaction: number of HH members, attended training and post treatment	-588.0	-397.3	-13.06	-114.3
	(2,840)	(2,377)	(1,364)	(1,448)
Plot area owned in square meters	0.530***	-3.542***	-3.412***	-3.612***
	(0.134)	(0.458)	(0.344)	(0.357)
Interaction: plot size, attended training and post treatment	-0.0178***	-0.0164***	-0.0133***	-0.0141***
	(0.00567)	(0.00568)	(0.00385)	(0.00422)
Observations	963	978	978	978
R-squared	0.737	0.735	0.779	0.774
Village dummies	Yes	Yes	Yes	Yes
Individual dummies	Yes	Yes	Yes	Yes
Robust	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table 9: Regression Output to Test Hypothesis 4, Winsorized Data

Regression 5: Varying Post-Treatment Period, Winsorized Data

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	One Month Mean			Two Month Mean			Thirteen Month Mean			Fourteen Month Mean		
	Savings Account	Targeted Account	Commit-ment Account	Savings Account	Targeted Account	Commit-ment Account	Savings Account	Targeted Account	Commit-ment Account	Savings Account	Targeted Account	Commit-ment Account
After intervention	4,988	7,719	3,765	3,757	6,969	2,221	5,830	12,876	2,461	6,267	13,310	2,898
	(4,717)	(13,279)	(4,564)	(3,917)	(11,311)	(2,554)	(4,890)	(15,043)	(1,773)	(4,968)	(15,418)	(1,956)
Has savings account	34,952***	-2,954	31,803***	36,531***	1,199	30,812***	66,980***	25,762***	60,026***	69,758***	25,373**	62,520***
	(2,711)	(2,793)	(3,044)	(2,461)	(5,715)	(1,827)	(2,931)	(7,854)	(1,803)	(2,968)	(8,011)	(1,917)
DiD coefficent	-3,987	-1,296	-4,752	-2,631	-2,401	-2,393	-5,440	-9,444	-3,218	-5,905	-10,035	-3,635
	(5,422)	(11,183)	(5,435)	(4,922)	(11,430)	(3,655)	(5,862)	(15,707)	(3,606)	(5,935)	(16,022)	(3,833)
Observations	531	153	378	540	158	382	540	158	382	540	158	382
R-squared	0.781	0.777	0.787	0.806	0.796	0.821	0.777	0.787	0.767	0.772	0.783	0.761
Village dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Full set of controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Robust	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses

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

Appendix Table 10: Seasonality of Savings Conditional on Training Attendance

Difference between Mean Balances in July and August Conditional on Training Attendance

Variables	# of observations	No Mean	Std dev.	# of observations	Yes Mean	Std dev.	Mean difference	(p-value)
		Attended						
Difference 2012	478	0.06	0.37	478	0.04	0.28	0.03	0.21
Difference 2013	478	-0.12	0.41	478	-0.08	0.21	-0.04	0.09*

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