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Save to Win: Using Contests to Promote Savings

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Abstract

We ask whether linking savings accounts to contests can promote saving. We do this by offering contest-linked accounts to individuals in rural Uganda where poverty is a serious problem. Our design builds off of results in experimental economics documenting excessive competitiveness in contests, with the goal of harnessing this behavior for the good. We find that, properly designed, we encourage savings beyond both pre-treatment levels and the control group. We explore reasonable heterogeneous treatment effects and document long lasting impacts on wealth.

Keywords: microsavings; poverty; Tullock Contest; Uganda

JEL Codes: O16; D14; G21; I32; D91

1 Introduction

In developing countries, the poor face numerous uncertainties and must develop costly coping strategies to deal with them (Morduch, 1995). Governmental agencies, non-governmental organizations, and academic researchers have devised numerous interventions to help the poor. A central component to these interventions is a focus on financial markets. Financial markets provide the opportunity to self-insure through savings, smooth consumption via borrowing, and obtain the capital necessary for entrepreneurship. Since the popularization by Mohammed Yunus and his Grameen Bank (Pitt and Khandker, 1998; Yunus, 1999), microfinance activities, particularly microcredit, has been a useful tool to cope with the uncertainties of economic life in poor communities.

What has received considerably less attention are interventions designed to promote savings amongst the poor. As emphasized by Besley, Coate, and Loury (1998), this is an important omission. While income constrained, the poor do have the ability to save. Demand for durable goods and a desire to smooth adverse income shocks are high. As has been well documented, credit instruments come at extremely high interest rates (Morduch, 2000). Thus, savings, even at negative real interest rates, should be of considerable value.

Early microfinance initiatives did not offer savings products, as many institutions targeted entrepreneurs, offering them microcredit. As institutions begin to incorporate more formal savings products into their services, we see a shift in the focus of microfinance from entrepreneurship to risk-coping mechanisms for households (Churchill, 2002).

The practical question becomes how to design financial institutions, mechanisms, and products to promote savings by the poor. Financial sustainability (Louis, Sere, and Baesens, 2013) is a problem faced by many microfinance institutions. Lack of subsidies (D'Espallier, Hudon, and Szafarz, 2013) and poor governance (Servin, Lensink, and van den Berg, 2012) make it difficult for these institutions to operate efficiently within communities. The addition of savings products provides institutions with the means to address sustainability, but questions arise regarding how to structure savings products that meet the demands of households. Time preferences, such as time inconsistency and impatience (Ashraf, Karlan, and Yin, 2006), are hindrances. Financial literacy (Lusardi and Tufano, 2009) can be expected to be minimal. Further, salience in appreciating the future benefits to the savings, relative to the more-salient, immediate opportunity costs of forgone consumption, can be expected to be a barrier.

We design a novel mechanism which we believe can promote savings. We call it "Save to Win" accounts. Our intuition comes from the extensive behavioral and experimental economics literature on contests. Building off Tullock's (1980) seminal theoretical contribution, contests ask participants to make non-refundable expenditures to obtain a prize. The prize is assigned probabilistically to one contestant, and the probability of winning is driven by a participant's relative share of total expenditures made. Extensive literatures have applied this framework to rent-seeking, R&D expenditures, election spending, and employee effort just to name a few. Central to our intuition, laboratory experiments have documented consistently an over-spending where subjects compete excessively, relative to Nash equilibrium predictions, which are themselves excessive relative to social optimums (Dechenaux, Sheremeta, and Kovenock, 2015). Numerous explanations have been investigated including, prominently, a joy of winning preference (Sheremeta, 2010) and impulsiveness (Sheremeta, 2018).

Typically, these contests are viewed through the lens of destructive competition. The excessive competition is socially wasteful. For example, consider rent seeking. If a government agency is to award a procurement contract to one supplier, for example, firms in that market are incentivized to promote their bids, which can include lobbying expenditures, perks to the decision makers, and even corrupt activities such as bribes (Tullock, 1967). Our innovation is to ask whether the excessive competitiveness contests generate can be harnessed for the good. Rather, can linking contests to savings activities produce the positive spillover effect of building up wealth in a poverty stricken community not currently utilizing savings opportunities?

To answer this question, we partner with a small, nonprofit organization operating in rural, southern Uganda. The organization had previously made microloans in the community as part of its development strategy. It had just expanded by constructing a microfinance bank that now also accepts deposits. Seeing little uptake, we created contest-linked savings accounts and made them available in two six-month phases.

Participation eligibility for our control and treatment mechanisms was done randomly to establish our interventions' causal effects.

In our Save to Win accounts, subjects are assigned to a treatment. In it, we provide the necessary opening account balance (2500 Ugandan Schilling) and provide the funds to cover the interest on the deposits (2% per month). In the control group, subjects are free to make deposits into the account. The high interest rate compensates them for the illiquidity, as the money could not be withdrawn until the end of the six-month trial phase. In our treatments, a fixed proportion of a subject's deposit each month is entered into a contest. The remaining is deposited into their savings account. Subjects are put into five person groups and a prize winner is determined probabalistically, as in Tullock Contests. The prize is deposited into the winner's account. Treatments differ in the proportion of the deposit that goes towards the contest and by the determination of the size of the prize. Those in the control are given the high interest rate and beginning account balance as well, but do not have the opportunity to engage in the contests.

In the first phase, two treatments were implemented where 40% and 20% of the deposit, respectively, went into the contest (leaving 60% and 80%, respectively, for the savings account). For both, the prize was set equal to the total expenditure of the five group members. Hence, our Save to Win intervention was zero sum. In the second phase, the treatment maintained the 20/80 distribution, but introduced a guaranteed minimum prize.

By comparing depositing behavior of those in the treatments to those in the control, we can assess the causal impact of contests on savings. By comparing subject savings choices to their own choices prior to the intervention and to those not in the study, we can evaluate the overall impact of our intervention on savings and wealth. Finally, by tracking the account balances in the year after the intervention, when their accounts become liquid and the high monthly interest rate is gone, we can appreciate the longer term impact on wealth.

We document a number of important findings. First, during our intervention those in our study deposit substantially more than they did prior to our intervention and by others in the community not currently participating in the study. Average account balances in the month prior to our intervention were 3757 Ugandan Schillings (hereafter, UGX) per subject. By the end of our intervention it is 21,027 UGX. Savings grew by a factor of 5.6. Thus, we promoted savings.

Second, the Save to Win treatments had higher deposits than the control group, with the effect greatest in our treatment with the lower proportion going into the contest and the guaranteed minimum prize. Here, average monthly deposits are almost 2000 UGX greater than the control group, and the probability of making any deposit in a month is almost 8 percentage points higher. Therefore, properly constructed, introducing contests into savings account, can promote savings.

Third, we administer decision-making assessments and collect background information on our subjects. We document important heterogeneous treatment effects. For one, we show that our Save to Win intervention is valued primarily by those who are recorded (in an incentivized, price-menu instrument) as not being risk

averse. This is reasonable since the intervention introduces uncertainty. We also show that the increased deposits are concentrated in those who self report as being a patient person. Hence, we find evidence that both risk and time preferences matter for the intervention's success. Additionally, surveying our subjects' past experiences with financial markets, we find evidence that those who have previously received microloans do not utilize the Save to Win accounts as much. This effect is sensitive to the inclusion of other background characteristics though. Finally, we document a relationship between self-reported participation in gambling on sports and behavior in our mechanism. Gamblers "gamble" more when their savings is linked to contests. In fact, the effect is enhanced when there is not a minimum guarantee on the prize. Thus, we have suggestive evidence that participation in our Save to Win program correlates with other, related personal finance activities.

Fourth, we track the account balances of our subjects after the intervention. We document, contrary to our initial worries, that subjects do not liquidate after their account balances roll over into standard accounts without withdrawal restrictions or interest payments. While 88% of our subjects did not have an account prior to the intervention, only two closed their accounts within six months of the end of our trial (1.4%). For most subjects, balances remain relatively stable and for some treatment cohorts, average balances actually grow! Even at twelve months after the Save to Win accounts ended, the average savings account balance is 28,076 UGX for our subjects (regardless of treatment) when it was 4020 UGX prior.[1] This is almost a doubling of the average account balance since the end of our intervention.

We are obviously concerned about where the funds came from and what consumption opportunities were given up to make the deposits we observe. Tracking use of informal savings and borrowing mechanisms (such as family loans) and correctly identifying forgone consumption is notoriously difficult. Even more problematic is identifying the normative implications of our intervention. To that end, we survey subjects twelve months (six months) after our intervention ended for participants in the first (second) phase. We find that the majority of our subjects report that they would have saved their money at home absent our intervention. Thus, we mitigate risk. Approximately one-third of our subjects say that they gave up consumption. Regarding anticipated future behavior, only 10% expect to close their account in the future, while 60% plan to further increase their balance.

In Section 2 we briefly survey the literature on microsavings efforts as part of poverty mitigation, outlining how our mechanism compares to others attempted. It also samples the behavioral literature on contests. In Section 3 we provide a straightforward theoretical model, which generates testable hypotheses. We describe our intervention's setting in Section 4. In Section 5, the methods and protocols employed are outlined. We analyze the data in Section 6 establishing both the average and heterogeneous treatment effects. Section 7 presents the post-intervention quantitative and qualitative results. In Section 8 we provide our concluding discussion.

[1] As will be described in the methods section, these last two figures are based on the subjects who participated in the first phase of the invention, so that they have a full twelve months after the invention.

2 Prior Research on Microsavings and Contests

Prior research on microfinance is vast and focuses primarily on microcredit. Mostly due to the influence of Muhammad Yunus and the Grameen Bank, microcredit was quickly adopted by many governments and non-profit organizations as part of large-scale economic development strategies that target poverty alleviation among low-income households (Morduch, 1999). Results from these studies identify two channels through which microfinance functions, as entrepreneurship or consumption smoothing.

Many of the first microcredit initiatives targeted small scale entrepreneurs as their clientele. Evaluations of profits and incomes (Banerjee *et al.*, 2015), employment (Augsburg *et al.*, 2015), investment (Crépon *et al.*, 2015), and return on capital (de Mel, McKenzie, and Woodruff, 2008) offer compelling evidence with regards to microfinance and local economic growth and institutional efficiency. Studies that assess the effects on social welfare characterize microfinance as being used as a consumption smoothing mechanism. Studies of food consumption (Pitt and Khandkar, 1998; Attanasio *et al.*, 2015), health outcomes (Hamad, Fernald, and Karlan, 2011), and subjective well-being (Karlan and Zinman, 2011) provide evidence for important effects of microfinance on low-income households.

Given the identified benefits of microfinance to households, a large portion of the academic literature focuses on the various structures of microcredit and evaluate its delivery and use by individuals and groups. The institutions studied aim to address possible shortcomings of traditional credit-lending mechanism and design financial products to meet the environment and demand of local communities. Attempts to experiment with repayment frequency (Field and Pande, 2008) and grace periods (Field *et al.*, 2013) provides information to institutions that help determine the most effective menu of financial products, with regards to both financial sustainability and customer retention (Tedeschi, 2006). Moral hazard and adverse selection make it difficult for microfinance institutions to operate effectively in rural communities. Group lending has traditionally been a way for these institutions to mitigate the monitoring costs associated with credit. Other studies have looked at dynamics of group-lending like the regularity of group meetings (Feigenberg, Field, and Pande, 2013; Feigenberg *et al.*, 2014), group size (Abbink, Irlenbusch, and Renner, 2006), and incumbent lending (McIntosh, De Janvry, and Sadoulet, 2005).

Ultimately, the economic impact is mixed and the general consensus is that the positive effects of micro-credit is limited and temporary (Banerjee, Karlan, and Zinman, 2015).[2] This has led researchers to evaluate alternative financial market interventions.

Some have considered conditional cash transfers (Baird, McIntosh, and Ozler, 2011) and microinsurance (Banerjee, Dufalo, and Horbeck, 2014). A few, like us, have explored ways to promote savings among those in poverty. Savings products were not offered by many early microfinance initiatives, as many assumed low-income households were too poor to save (Morduch, 2000). Yet, empirical evidence on savings finds the opposite to be the case. Positive effects to savings mobilization like increased account balances and

[2]The reader is encouraged to consult the special issue in *American Economic Journal: Applied Economics* which provides a special issue bringing together numerous papers attempting to measure microfinance's impact.

savings activity (Asharf, Karlan, and Yin, 2008; Dupas, Keat, and Robinson, 2018), as well as investments in household public goods (Schaner, 2016), suggests significant welfare effects for low-income households. Increased empowerment of women (Anderson and Baland, 2002; Duflo, 2012), lower education expenditures (Karlan and Linden, 2014), and reduced risky sexual behavior (Witte, *et al.*, 2015) represent more important benefits of savings to communities, at large.

Like the research on microcredit, microfinance institutions work to design different products to encourage savings among low-income households. Studies explore the effects of no-fee savings accounts (Dupas and Robinson, 2013), fixed-deposit savings, and bank-insured savings groups (El-Gamle *et al.*, 2014) on households' willingness to save. *Karlan et al. (2016) investigates the effectiveness of reminders to save.* Each study provides insight into the demand for savings, and we add to this literature by introducing contests as a mechanism to promote savings activity.

The design of our savings mechanism most closely resembles a prize-linked savings account. Kearney *et al.* (2010) provides a survey of the literature on prize-linked savings products and their prevalence in banking institutions around the world. A large portion of these products target medium to low-income households, given the high demand for gambling and lotteries among these groups. These accounts pool the interest accrued on the deposits of account holders and uses it as the prize in a monthly lottery. The probability of winning the lottery becomes a function of how frequently you make deposits, thus encouraging savings among account holders. Experimental evidence shows that this deposit behavior can be driven by the expected value of winning or a joy of winning behavior, where individuals gain utility from participating in competitions, even more so than the expected value of a prize. Contests represent an environment where the joy from winning is particularly prevalent. Sheremeta (2013) provides an extensive survey of the experimental economics literature on the overbidding, which raises concerns about the the utility gained from the joy of winning in contests. Wasteful spending is destructive to communities in any context, but especially so for medium and low-income households. Banking institutions that understand this overbidding behavior can offer a savings product to reduce its consequences and harness potentially destructive behavior for a positive spillover for an individual.

We contribute to the literature on field experiments and savings mobilization in rural communities by designing and implementing a contest-linked savings product that promotes savings behavior in southern Uganda. Our experiment is the first to combine a contest and savings mechanism in the real world, where deposits directly affect your probability of winning a prize. The effects of prize-linked savings accounts have been studied in laboratory settings, where studies explore preferences for prize-linked products (Dizon and Lybbert, 2019) and how this type of account affects wealth generation (Filiz-Ozbay *et al.*, 2015). Two field studies have explored prize-linked savings accounts, but are not designed to harness the overcompetitiveness of contests. Linardi and Tanaka (2013) attempt to induce savings in a U.S. homeless shelter. A $100 prize was given to the individual with the greatest savings each month. Thus, their intervention is akin to an all-pay auction. While effective in the first month, they explore why it is unable to incentivize savings in

the following months of their study. Cole *et al.* (2016) explore data from a South African bank that offered prize-linked savings accounts. Like a typical contest, savers were given tickets into a lottery. The number of tickets received corresponded to the savings account balance. Importantly, the tickets were not awarded based on additional deposits, but the size of the balance each month. Further, unlike contests studied in the laboratory, there is no chance of incurring a loss if the participant does not win the prize. Thus, each of these efforts have designs that constrain the competitive environment. Gertler *et al.* (2018) partner with a Mexican bank and, like the scenario studied by Cole *et al.* (2016), issue lottery tickets for saving. They issue tickets based on deposits, rather than accumulated savings, and show that they are able to induce individuals to open accounts. Again, there is not 'skin in the game' by the participants in that downside losses are not possible. Our contest environment allows participants to choose their monthly 'effort' level to win the prize, and conforms closely with the behavioral literature on contests which lead to excessive competition. Finding important treatment effects in our field experiment, our results are also the first to indicate positive, long-term effects of contest-linked savings accounts. Our results align with experimental literature regarding competitive behavior in contests and provide a novel contribution to the field of development economics and a broad literature on the positive effects of savings mobilization in growing, rural communities.

3 Theory

To create our hypothesis regarding the intervention's effects, we build a straightforward theoretical environment. We consider an individual making the decision between current consumption and future consumption in a two-period model. Savings allows for both consumption smoothing and participation in a contest, which is enjoyable. Parameterizing a joy of winning, risk preference, and the contest treatment, we derive testable predictions.

3.1 Model

An individual plans consumption over two time periods, labeled 1 and 2. Let c_t and w_t denote the amount consumed and earned in period t. Income is exogenous. Along with choosing how much to consume, the individual has the opportunity to save, with the amount denoted by s. We consider an environment where the individual can save but not borrow; i.e., $s \geq 0$. This, we believe, accurately represents the typical opportunities for an individual in a developing part of the world. Informal savings mechanisms are available, such as at-home savings, but poverty is correlated with inaccessible financial markets.

Let the instantaneous utility from consumption be denoted $u(c_t; \theta)$ and let time be discounted at the rate $\delta \in (0, 1)$. The parameter θ captures the subject's risk preference. Larger values of θ correspond to a more risk averse individual.[3] Assume the payoff function is continuously differentiable, strictly increasing, and strictly concave in c_t to ensure the full exhaustion of resources.

[3] We have in mind θ as the coefficient of relative risk aversion that would arise in the CRRA utility function $u(c; \theta) = \frac{1}{1-\theta} c^{1-\theta}$. We choose to not limit ourselves to a specific functional form though.

Also, research in behavioral economics has provided evidence that a "joy of winning" exists. Subjects tend to enjoy winning, above and beyond the additional expected consumption that can arise with the prize. We use the term joy of winning loosely so as to represent any motivation that leads to the excessive competitiveness observed in the laboratory. To keep the analysis general, we let $J(s;\theta,\tau)$ denote the expected joy experienced from the contest. This benefit term is driven by the amount saved as that can be expected to increase the size of the prize and the probability the subject wins it. Hence, assume $J(s;\theta,\tau)$ is continuously differentiable, strictly increasing, and strictly concave in s.

Thus, the expected, discounted utility function to be maximized is

$$U(c_1, c_2, s) = u(c_1;\theta) + \delta u(c_2;\theta) + J(s;\theta). \tag{1}$$

The contest we implement organizes individuals into cohorts (in the field, we set the cohort size to 5). Each subject is assigned a treatment and the cohort s/he is grouped into includes individuals of the same treatment (where both the treatment and cohort are random assignments). We parameterize the treatment by $\tau \in [0,1]$. If the individual chooses to save s, then τs is deposited into his/her savings account and $(1-\tau)s$ is entered into a contest. Following a traditional Tullock Contest framework (Tullock, 1975; 1980; Dechenaux, Sheremeta, and Kovenock, 2015), the probability the individual wins the prize of size Π is $\rho = \frac{(1-\tau)s}{(1-\tau)\Sigma s_j}$ where Σs_j is the total amount saved by all subjects in the contest. If the individual wins the contest, the prize is deposited into his/her savings account and, therefore, is subject to interest accumulation. Along with varying the treatments, we will differentiate the determination of the prize.

In the field, we consider three values of τ. In the "Low" treatment for every 500 UGX deposited, 300 UGX goes into the savings account and 200 UGX are entered into the contest. Thus, $\tau_L = 0.6$. In the "High" treatment less is entered into the contest and more is deposited into the savings account. In it, for every 500 UGX deposited, 400 UGX are saved and 100 UGX are entered into the contest. Hence, $\tau_H = 0.8$. In the "Control" all money deposited goes into the savings account. Thus, $\tau_C = 1$.

A budget constraint exists for each time period. The constraints on the individual's utility maximization problem are $c_1 + s \leq w_1$ for $t = 1$ and $c_2 \leq w_2 + (1+i)\tau s + (1+i)E(s;\tau)$ for $t = 2$ where $E(s;\tau)$ is the expected gain from the prize when s is saved and the treatment is τ.[4] The term i is the interest rate earned on the savings.[5]

Finally, we consider two mechanisms to determine the size of the prize. In the first, the prize received by the contest winner is simply the sum of the expenditures made by the subjects in the cohort. This is done so that the intervention is zero-sum fostering savings in the community without external subsidization. Hence, $\Pi^z = (1-\tau)\Sigma s_j$. In the second mechanism, we ensure a guaranteed minimum prize. That is, if the total expenditures on the contest falls below a set threshold, denoted Π^g, then the contest winner receives Π^g. If

[4] We are considering a very simple problem where the expected prize is used in the budget constraint. A fuller model would allow second period consumption to be a function of the realization of the contest. Our goal here is to present the simplest model possible to create testable hypotheses for our field experiment.

[5] While it is natural to presume i is positive, it can be zero (as many bank accounts in the developing world do not provide non-zero (nominal) interest rates) or even negative (with inflation causing negative real interest rates). We only require $1+i > 0$. In the field, we provide a 2% interest rate per month.

the total expenditures exceed this threshold, then $\Pi = (1-\tau)\Sigma s_j (= \Pi^z)$, as before. Traditional contests in the lab utilize a fixed prize that does not vary with expenditures. Thus, our guaranteed minimum attempts to foster the over-competitiveness identified there.

3.2 Save to Win Mechanism

The Lagrangian to be maximized is

$$L = u(c_1;\theta) + \delta u(c_2;\theta) + J(s;\theta,\tau) + \lambda_1(w_1 - c_1 - s) + \lambda_2(w_2 + (1+i)\tau s + (1+i)E(s;\tau) - c_2). \tag{2}$$

First, it follows immediately that the joy of winning component is necessary for our intervention's success. If $J(s;\theta,\tau) = 0$ and $\Pi = \Pi^z$ (so that the second-period budget constraint is simply $w_2 + (1+i)s \geq c_2$), then a standard utility maximization problem arises and our treatments do not have an effect on savings choices made.

Hypothesis 0: *If subjects derive utility only from consumption, then: [1] the Save to Win mechanism will not change the likelihood that they will save a non-zero amount, [2] the amount saved (if a non-zero amount is chosen) will not change if offered the Save to Win intervention, and [3] savings behavior will be the same across the treatments.*

Hence, a joy of winning is needed if our intervention is to change behavior.

Our intervention is designed to leverage individual's willingness to "over-compete" in contests. To illustrate this, when a subject selects a non-zero level of saving it follows from complementary slackness that $\frac{\partial L}{\partial s} = 0$. Rather,

$$\frac{\partial L}{\partial s} = \frac{\partial J}{\partial s} - \lambda_1 + \lambda_2 \left[(1+i)\tau + (1+i)\frac{\partial E}{\partial s} \right] = 0. \tag{3}$$

Consequently, the optimal level of saving for the individual will be driven in large part by the marginal benefit to the contest, $\frac{\partial J}{\partial s}$. We make the following assumption regarding the individual's joy of winning:

Assumption 1: $\frac{\partial^2 J}{\partial s \partial \tau} < 0$

The implication is that the marginal joy from saving is less if little is entered into the contest (higher values of τ). If most of the deposit goes into savings, rather than the contest, then the joy from anticipating victory is small. The more our intervention allows entries into the contest, the more happiness is generated in our subjects from competing.

Applying this assumption, it is straightforward to verify the following result.

Hypothesis 1: *If subjects experience a joy to winning and Assumption 1 holds, then in the no-guarantee, zero-sum treatment ($\Pi = \Pi^z$): [1] the Save to Win mechanism will increase the likelihood that they will save a non-zero amount, [2] the amount saved (if a non-zero amount is chosen) will be greater if offered the Save to Win intervention, and [3] savings will be greater in treatments where more is entered into the contest.*

Hypothesis 1 is our main result and motivation for conducting the field experiment. Fixing the enjoyment of contests and risk preferences, the Save to Win intervention is expected to increase the amount deposited, relative to the environment where all of the deposit goes directly into a savings account.

This result, though, is driven by Assumption 1. Creating treatments where more of the deposit goes into the contest leads to more savings if the marginal utility to competing grows. If this assumption does not hold, then the Save to Win mechanism will not be effective.

With the zero-sum prize, the expected monetary gains are simply the amount put into the contest by the individual, $E(s;\tau) = (1-\tau)s$. Thus, an individual expects to get his/her expenditures back. Consequently, it is not obvious that a person's nonmonetary enjoyment from the contest is really affected by the treatment parameter here. Thus, in the zero-sum prize without the guarantee, Assumption 1 may not hold. As a result, our intervention may not be effective.

With a fixed, guaranteed prize, on the other hand, the expected return depends on how much other individuals put into the contest. In laboratory experiments, the over-competitiveness arises with fixed prizes. Hence, we install the guaranteed minimum prize to ensure the property defined in the assumption holds.

More precisely, we make the following additional assumption regarding the individual's joy of winning:

Assumption 2: $\frac{\partial^2 J}{\partial s \partial \tau}|_g < \frac{\partial^2 J}{\partial s \partial \tau}|_z < 0$

That is, the marginal effect of savings on the joy of winning is affected more by the treatment when in the guaranteed prize contests as compared to the zero-sum, no-guarantee prize contests. The guaranteed prize treatment ($\Pi = \Pi^g$) provides the possibility for the Save to Win mechanism to promote savings even if Assumption 1 does not hold for the no-guarantee prize.

Hypothesis 2: *If subjects experience a joy to winning and Assumption 2 holds, then [1] the likelihood of saving a non-zero amount will be greater in the Save to Win mechanism with the guaranteed minimum prize than when there is no guarantee and [2] the amount saved (if a non-zero amount is chosen) will be greater with the guaranteed minimum prize than when there is no guarantee.*

Hypothesis 2 further refines our predictions when we go to the experimentally-generated field data. While our Save to win intervention is expected to improve the extensive margin and intensive margin of savings (Hypothesis 1), we expect these effects to be larger when the guaranteed minimum prize is used (Hypothesis 2).

3.3 Risk and Time Preferences

Not every person can be expected to respond the same to the Save to Win intervention. The theoretical model incorporates a discount factor to capture time preferences, and a risk aversion term to parameterize risk preferences. Here, we explore how our intervention interacts with these important preference dimensions.

First, it is straightforward to verify that a more patient individual, who has a greater value of δ, is more likely to save.

Hypothesis 3: *A more patient individual [1] is more likely to save a non-zero amount and [2] saves more (if a non-zero amount is chosen).*

An individual with a greater value of δ places more weight on future consumption and is, consequently, more interesting in savings. Our intervention is designed to facilitate savings and, thus, it is reasonable to expect that this will be especially well received by patient individuals.

Regarding risk preferences, though, our intervention introduces uncertainty. A portion of the individual's deposit is entered into the contest. With a high probability, the individual will not win the contest. For example, since we organize subjects into five person cohorts for the contests, in a symmetric Nash equilibrium the chance of winning the prize is only 20%.

What is unclear is how the joy of winning component responds to this uncertainty. We proceed with the assumption that the marginal benefit from competing declines the more risk averse an individual is. Assumption 3 formalizes.

Assumption 3: $\frac{\partial^2 J}{\partial s \partial \theta} < 0$.

With this assumption, the more risk averse individuals are less willing to save with our Save to Win intervention.

Hypothesis 4: *If subjects experience a joy to winning and Assumption 3 holds, then a more risk averse individual [1] is less likely to save a non-zero amount and [2] saves less (if a non-zero amount is chosen).*

Hypothesis 4 suggests that our intervention can create a conflict. While our Save to Win mechanism is designed to encourage savings by those who enjoy competition, it can be off-putting to those who dislike uncertainty. Therefore, we would expect our intervention to be successful with those who are not too risk averse.

Notice that the comparative statics predictions laid out in Hypotheses 1 through 4 are not conditioned on the behavior of the others in the cohort. The amount saved is driven by others' expenditures (through affecting ρ and, hence, J and E). Thus, Hypotheses 1-4 apply to the Nash equilibrium of the game as well.

4 Setting

Embrace It Africa is a 501(c)3 nonprofit organization operating in the town of Bethlehem in the Rakai district. The Rakai district is in southern Uganda bordering Tanzania. Embrace It Africa has been functioning in the region since 2008 providing access to education for orphans and microfinance to entrepreneurs.[6] In January

[6]See McCannon and Rodriguez (2019a) for an analysis of Embrace It Africa's microfinance activities in the community and McCannon and Rodriguez (2019b) for an evaluation of orphanhood there.

2018 the organization opened a bank facility, named Mikwano[7], to provide savings opportunities to those in the community.

We partnered with the organization to design a savings product, which we describe in the next section. Our intervention occurred in two six-month phases. Phase 1 occurred between July and December 2018. Phase 2 occurred between January and June 2019.

We utilized the networks established by the nonprofit organization to recruit. Embrace It Africa provides funding for the school in town and has a long history of providing microloans. Community leaders and region-wide advertising were utilized to let individuals in the community know that a savings product will be made available at the beginning of each phase.

During the first week of July 2018, 103 adults showed up to participate in our intervention. These individuals were randomly assigned into one of three cohorts, again described in the next section. All members of the community were eligible to participate, including those who had previously opened savings accounts at the bank and those who had not. During the first week of January 2019, 43 additional individuals showed up to participated in our second phase. These 43 did not participated in Phase 1, and those who participates in Phase 1 were not eligible for Phase 2. Those in Phase 2 were randomly selected to be in one of two cohorts. The Phase 2 intervention ended in June 2019.

Mikwano started opening savings accounts and accepting deposits in January 2018. The accounts offered were standard for the country. To open an account, an individual is required to have a 2500 UGX minimum account balance.[8] Deposits can be withdrawn at any time that the bank was open. No interest is paid on the balances.

Over the first six months of the bank's existence, few in the community utilized the opportunity. Those who did open an account maintained only small balances. The upcoming section provides descriptive data on these accounts. Embrace It Africa had been making microloans in the community for over a decade, so it has a strong, positive image in the community and should not suffer from lack of institutional trust. Therefore, we worked with Embrace It Africa to promote the new savings opportunity they provide to the community.

5 Method

We first describe the Save to Win accounts provided. Then, we lay out the protocols used in the field. Finally, the data collected is described.

[7]The word 'mikwano' is the Lugandan word for friendship.

[8]The exchange rate is approximately 3500 UGX per 1 USD. From interviews with locals, a typical daily wage for a laborer is 2500 to 4000 UGX *per day*. Further, Uganda has a minimum wage law requiring 6000 UGX *per month*. Thus, the opening balance is a nontrivial amount.

5.1 Save to Win

Subjects in the treatment can make any size deposit any time within the month. A proportion τ of the deposit went into the individual's savings account and $1-\tau$ goes into a contest. Subjects may make more than one deposit during the month, or may choose not to make a deposit.

On the last business day of the month, subjects within a treatment who made a non-zero deposit during the month were grouped randomly into five person cohorts. The amounts entered into the contest for the five are pooled together. One person is selected at random and wins the pooled amount. The probability of winning is equal to that person's share of the total expenditures for the five-subject group for the month.

The winner's prize is publicly announced and deposited into that subject's account. Interest is then paid on each account at the nominal rate of 2% per month. This procedure is redone each month for six months. Thus, each subject has six observations − his/her monthly deposits for the six months of the intervention.

In Phase 1, subjects are randomly assigned to one of three protocols. One is the control who may not enter the contest. For them, 100% of each month's deposit goes into the savings account. In a second, which we call *Treatment 4-1*, four of every five Ugandan Schillings deposited go into the savings account. Thus, if a subject deposits 5000 UGX, then 4000 UGX go into savings and 1000 UGX are entered into a contest (rather, $\tau = \tau_H = 0.8$). In the third, which we call *Treatment 3-2*, three of every five UGX deposited go to savings. Thus, a person in this cohort who deposits 5000 UGX, for example, would have 3000 UGX added to savings and 2000 UGX entered into a contest (rather, $\tau = \tau_L = 0.6$). Phase 2 subjects were randomly assigned to one of two protocols. One is a replication of the control group from before. The other cohort engages in Treatment 4-1 again.

The difference between Phase 1 and Phase 2 is the size of the prize gained by those in Treatment 4-1. In Phase 1, as described, the prize is equal to the total expenditures of the five in the group. Rather, it is zero sum ($\Pi = \Pi^z$). Our motivation is that this design requires the least amount of subsidization. We do cover the interest payments, to reduce the burden of our intervention on the community's bank, and provide the opening account balances as a "show-up" fee for our experimental subjects. After that, no additional funds are needed from the bank or the researchers to offer Save to Win accounts.

In Phase 2, we instituted a minimum prize guarantee. We ensured that if the total prize was less than 20,000 UGX, then we would make up the difference to give the winner 20,000 UGX. If total expenditures exceeds this lower bound, then the prize is equal to the full pot.

The minimum prize was set to be near, but exceed, the upper bound of the prize sizes from Phase 1. We did this to see if we can increase deposits from what we see in Phase 1. It was designed to further promote savings. Also, our intention was to consider a design that more closely resembles Tullock Contests, which typically involved pre-determined, fixed prizes. The five person group size was selected to promote anonymity, given our modest sample sizes. The group size is held constant across all treatments. Finally, we decided not to replicate Treatment 3-2 with a minimum prize guarantee in Phase 2. As will be shown in the upcoming section, this treatment did not have an effect on savings relative to the control. Fearing that

it may be dis-incentivizing savings by putting too much into the contest, we discontinued it for the second phase.

5.2 Protocol

As described, our intervention was widely advertised throughout the community. On the opening day of our intervention, we provided food for a community meal, which is an expected cultural practice. Adults willing to participate came into Mikwano's bank building and completed the paperwork necessary to open the account.

During this process, subjects pulled a piece of colored paper out of a cup. Three colors were used in equal proportions. The colors corresponded to the three cohorts. Thus, each subject was equally likely to be assigned to the Control, Treatment 4-1, and Treatment 3-2.

A total of 103 individuals agreed to participate during the first week of July 2018. This makes up the Phase 1 subject pool. There were 34 selected for Treatment 3-2, 43 for Treatment 4-1, and 26 for Control.

Included in the application paperwork was a survey collecting basic background information. Subject reported their gender, age, marital status, number of children, education obtained, occupation, and experience with financial markets.

Deposits were taken continuously over the month. On the last business day of the month the contests were scored. Subjects often attended the contest drawings, but this was not mandatory. The winner's prize was deposited into his/her account. The procedure for determining the winner was repeated each month for six months. The groupings were randomly determined and anonymous.[9] Each group consisted only of those within the same treatment.

Individuals who did not open an account the first week of the study, were recruited to engage in Phase 2. This was introduced in January 2019, corresponding to the end of Phase 1. The same procedures were utilized. With equal likelihood subjects were assigned to Treatment 4-1 and Control. As before, the treatment stayed the same over the six month trial.

A total of 43 adults participated in Phase 2. Of them, 33 were assigned to Treatment 4-1 and 10 were assigned to Control. Thus, we have a total of 146 subjects engaged in our intervention monthly over six months spread out over two phases.

During the six months of the intervention, withdrawals from the Save to Win accounts were not allowed. Subjects were free to have standard savings accounts as well. A few subjects had opened accounts before the intervention. Subjects who had these standard accounts can deposit and withdraw from them freely. The standard accounts did not pay interest. To compensate individuals for our intervention's illiquidity, we paid each subject a 2% monthly interest.[10]

[9] It was anonymous except for the identities of the winners, who were publicly announced. Also, it was common for the number of depositors in a treatment for a month to not be perfectly divisible by five. The remainders (randomly selected) were paired with depositors in the next month (of the same treatment) to determine the prize winner.

[10] Therefore, our (external) compensation to the participants consists of the community meals (before the start of Phase 1), the 2500 UGX opening balance for each of the 146 subjects, the difference between the pot size and our 20,000 UGX guaranteed minimum for those groups below the cutoff in Phase 2, and the 2% monthly interest on deposits made.

At the end of the six month phase of our intervention, the account balances were rolled over into standard savings accounts. Thus, they were liquid and earned no interest.

5.3 Data

First, while we use a randomization device to assign subjects to treatments, it is appropriate to check the similarity of the partitions of the subject pool. Table 1 presents a balance table comparing the subsample averages on the background characteristics measured.

Table 1: Balance Table

	Control	Treatment 4-1	Treatment 3-2
Age	32.11	33.12	28.53
Number of years of education	7.60	7.30	7.18
Number of children	3.26	3.61	2.44
Are you married? (Yes = 1)	0.514	0.697 *	0.529
Are you single?	0.343	0.184 *	0.412
Are you divorced?	0.057	0.053	$\emptyset$
Are you widowed?	0.086	0.066	0.059
Do you grow crops?	0.829	0.933	0.824
Do you own livestock?	0.686	0.684	0.853
Do you hold an elected position?	0.114	0.408 ***	0.235
Is your house made of mud?	0.057	0.066	0.030
Is the floor of your house concrete/solid?	0.771	0.803	0.735
Does your house have electricity?	0.457	0.263 *	0.324
N	35	76	34

The individual-level data set is considered. A difference-in-means t-test (allowing for unequal variances) compares the treatment's mean to the control group's mean; *** denotes 1% level of significance, while ** and * denote 5% and 10%, respectively. Data from both phases pooled.

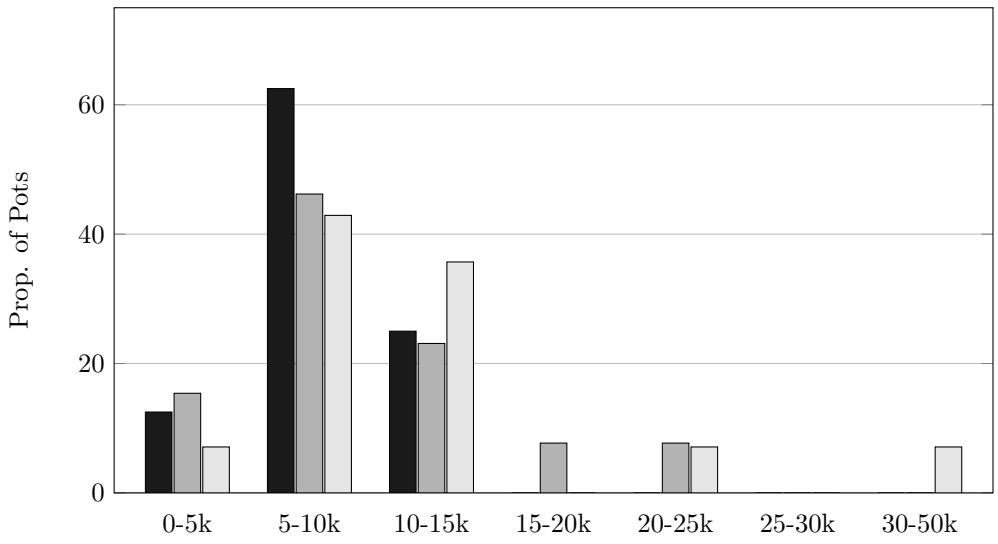
The asterisks in the table are the result of a difference-in-means t-test between that particular treatment and the control group. For the most part, the samples are indistinguishable − age, family size, education, and occupations look similar. Those in Treatment 4-1 are slightly more likely to be married and may have less access to electricity. The one anomaly is that they are more likely to have an elected position in the community. Overall, the subject pools are similar in these observables.

Regarding deposits made, participation varied. Only 18 had a savings account prior to the intervention (12%). During the intervention, 98 made no additional deposits beyond what they contributed the first day of the trial (68%). Thus, in the upcoming analysis, we will explore both the extensive and intensive margins to saving. For those deposits after the initial month of each phase, the average monthly deposit made was just less than 9000 UGX. In the next section, though, we evaluate differences in depositing behavior across the treatments. By the end of our intervention, total wealth in the accounts is 16,260,359 UGX[11], where it was only 544,700 UGX prior − an increase by a factor of 30!

[11]This figure includes (compounded) interest earned and the opening account balance, along with the deposits made and prizes won.

Finally, consider the pot sizes won in the actual contests. The distribution of the pots is depicted in Figure 1. For Treatment 4-1, Phase 1 (darker gray) is separated from Phase 2 (lighter gray). We separate the prizes received in Treatment 4-1 since the phases differ in the use of the guaranteed minimum. The distribution of prizes in Treatment 3-2 is provided in black.

Figure 1: Distribution of Contest 'Pots'



Each column depicts the proportion of scored contests that have a pot size within each specified range. The black bars are the distribution in Treatment 3-2. For Treatment 4-1, the dark gray depicts the distribution from Phase 1 and the light gray provides it for Phase 2.

As one can see, the pots are concentrated in the 5000 UGX to 15,000 UGX range in both phases. Overall, the average size of the pot is 11,339 for the five-subject groupings (combining both treatments and both phases). Thus, the 20,000 UGX minimum guarantee represents a nontrivial increase in the expected prize's size. In fact, for those subjects in Phase 2, the minimum was a binding constraint for 85% of the cohorts. The distribution of pots in Treatment 3-2 sits to the left of those in Treatment 4-1. For those in Treatment 4-1, the distribution shifts to the right for Phase 2 where the minimum prize guarantee was added. These two observations will be explored further in the upcoming section.

6 Results

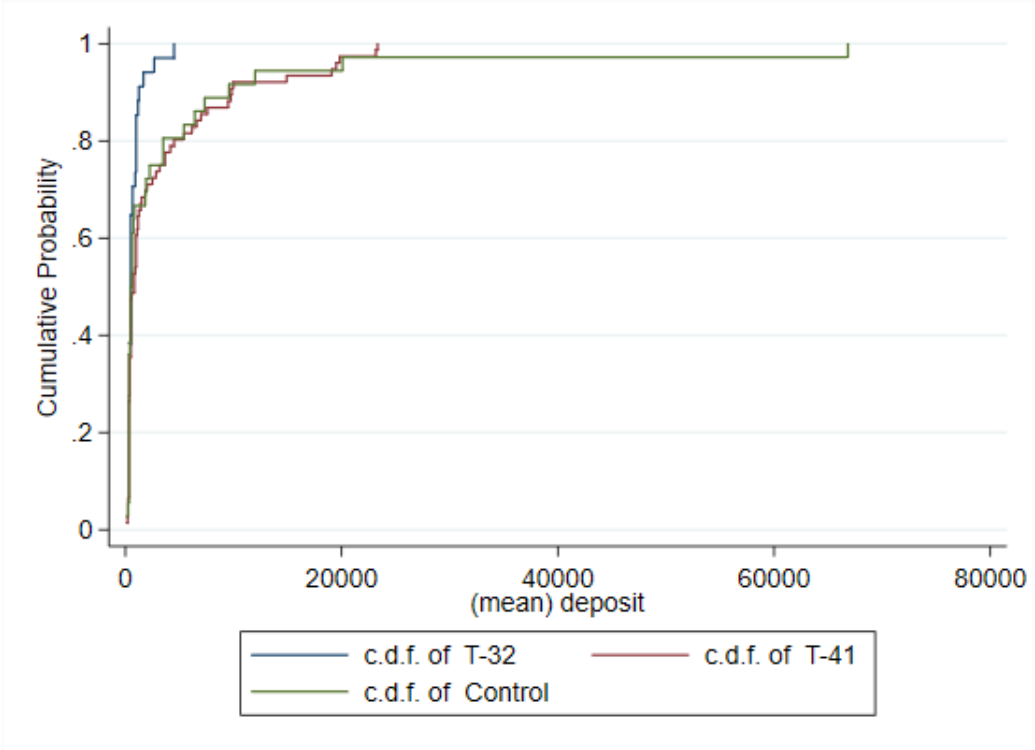
Our analysis of the results is done in three steps. First, we conduct an outlier test. Due to the small, rural community as our targeted population, our sample sizes are modest. As a consequence, one can reasonably be concerned that extreme behavior out of one subject can have meaningful distortions on the sample averages. Second, we establish the treatment effects statistically. This provides our main result. In the final subsection, we evaluate heterogeneous treatment effects to assess which subject characteristics correspond to the success of the treatments.

6.1 Outlier Test

As with any individual-level data set with a modest number of observations, one should be concerned about an outlier observation affecting the estimated average treatment effect.

To illustrate, Figure 2 depicts the distribution of the average, per month deposit made by subjects in each of the three treatments.

Figure 2: Distribution of Average Deposits Across Treatment



The data set is collapsed to the subject level calculating the average per month deposit for each subject. The plotted cumulative distribution functions separate those subjects in Treatment 3-2 (labeled 'T-32'), Treatment 4-1 (labeled 'T-41'), and the control.

As one can see, the cumulative distribution function of those in the control group experiences a substantially different right tail to the distribution. One subject deposited substantially more than any other individual in the cohort. His/her deposits are almost 3.4 times as large as the second largest depositor in the control. The savings behavior of this one subject sits far beyond the rest of the subjects in the pool.

Behavior is more consistent within the pool of treated subjects. For example, in *Treatment 4-1, Guarantee* the person with the highest average deposit has a per month value only 19.7% higher than the second highest depositor and is 24.3 times as great as the median individual, as compared to 133.7 times as great for the outlier in the control cohort. Similarly, in the *Treatment 4-1, No Guarantee* sample, the person with the highest average deposit is only 16.8% higher than the second highest depositor. The amount is 46.3 times as great as the median.

The control subject pool has one outlier individual who engaged in substantially more savings than

others in the experiment. The concern, then, is that the presence of this one individual with extreme behavior can sufficiently distort the average treatment effects. Therefore, in the upcoming analysis, we exclude this individual from the data set. Also, depictions of the sensitivity of our results to outliers are presented in the appendix. There, the primary result (the estimated average treatment effect) is re-estimated by systematically dropping each of the other subjects in the control. It illustrates that there is little change in the estimated average treatment effect when any other subject is eliminated from consideration. Thus, our results are not sensitive to the subjects selected in our control sample, except the single outlier shown in Figure 2.

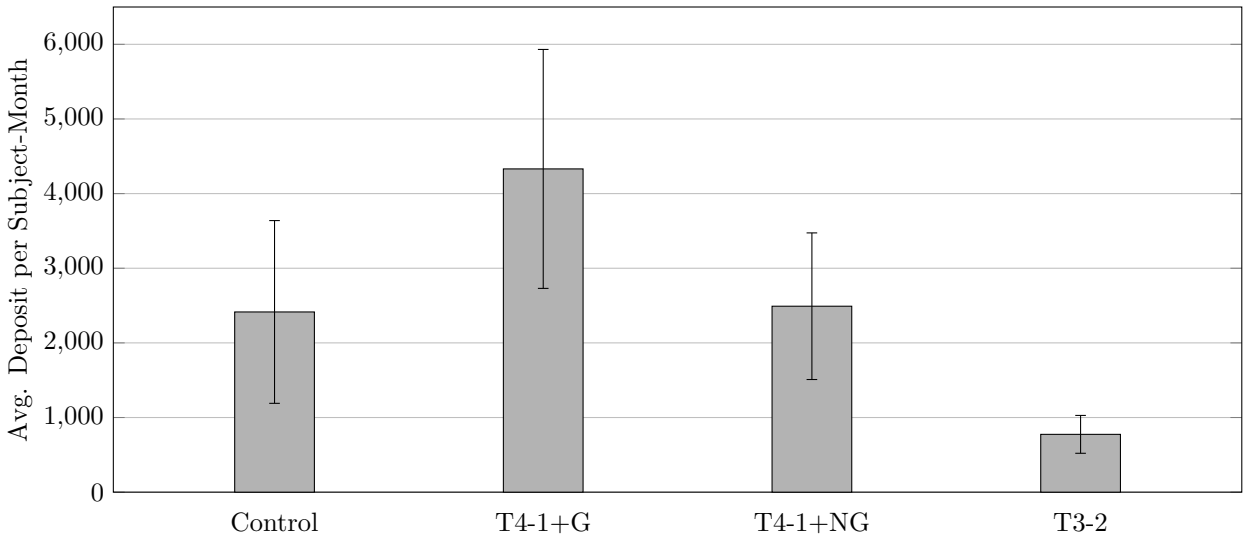
Second, both Phase 1 and Phase 2 included a control group. Behavior in the two cohorts is statistically indistinguishable. The difference in the mean deposit for the control group across the two phases has $t = 0.42$ from a difference in means test ($p > 0.67$). Since the two distributions are statistically indistinguishable, we will pool them into one sample for the upcoming analysis.

6.2 Treatment Effect

We now turn to the primary question − whether the Save to Win intervention promoted savings. We evaluate it using two metrics. First, is the savings level observed (statistically) greater than zero? Since our subject pool is made up of those who do not engage in savings (for the most part), any savings can be considered a victory. Prior to the beginning of the two phases, only 11.6% of the subjects had a savings account at the community bank. Our intervention consists of two parts − an enhanced monthly interest rate well above market rates and a contest. Therefore, the second, higher hurdle to leap over is to establish that the Save to Win accounts outperform the control group's savings behavior. As stated, our control group gains from the high interest rates, but does not have the contest.

Figure 3 first compares the cohort average per month deposit sizes. We chose to separate those who engaged in Treatment 4-1 into those with the guarantee, labeled "T4-1+G", and those who did not, labeled "T4-1+NG". We include average per month per subject deposits of those assigned to Treatment 3-2, labeled "T3-2", and the control.

Figure 3: Treatment Effect: Deposit Size



Comparing only the *Control* to the *Treatment 4-1, Guarantee* group (labeled "T4-1+G" in the figure), a two-tailed, difference-in-means t-test (allowing for unequal variances) has $t = 1.88$ ($p = 0.062$). A Wilcoxon Ranksum test has $z = 2.07$ ($p = 0.038$).

As one can see, all cohorts experience savings behavior far exceeding zero levels. Thus, the intervention leads to more formal savings. In addition, the average per month deposit into traditional savings accounts prior to the intervention is less than 500 UGX. Each confidence interval lies above this amount. Hence, deposits during our intervention are all substantially greater than what is observed using traditional savings accounts. The difference is highly statistically significant. Thus, we achieved our first goal.

Result 1: *The Save to Win intervention lead to more formal saving than traditional savings accounts.*

Regarding the second hurdle, first note that the Treatment 3-2 has a substantially lower average deposit than any other cohort including, importantly, the control group. Thus, it seems that too much of the amount deposited went to the contest. Presumably, subjects were uninterested (for the most part) in participating.

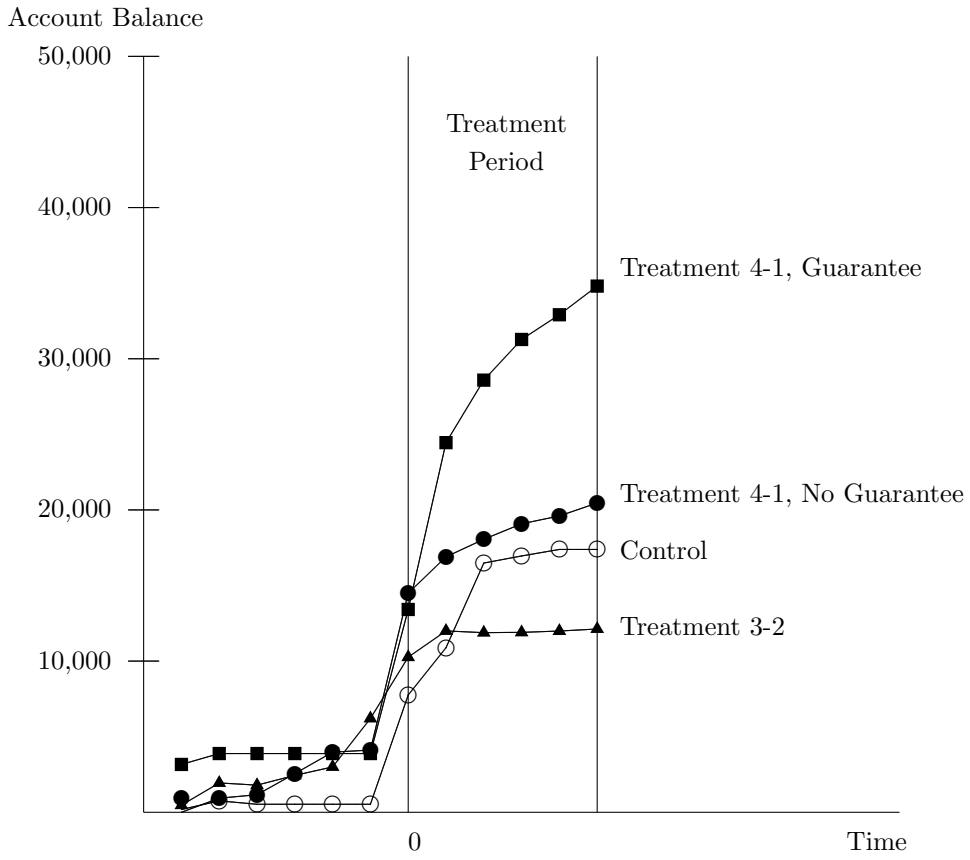
Treatment 4-1, without the guarantee, sees greater depositing than Treatment 3-2 (which also does not have the guarantee), but is statistically indistinguishable from the control group's savings behavior.

The noticeable treatment effect arises when the guaranteed minimum prize is introduced. The average, per month deposit is 79.4% greater than in the control. Using a Wilcoxon Ranksum test, the difference in the distributions is statistically significant ($z = 2.07$; $p = 0.038$). Therefore, a treatment effect exists.

Another way to assess our intervention and the difference between the treatments is to consider the time series of account balances. Figure 4 presents the average account balance for our subjects in each month separating the individuals into the four cohorts. Hence, Phase 1 and Phase 2 are pooled together. Time is re-centered around the beginning of the treatment. That is, rather than consider the calendar months, we consider $Time = 0$ as the first month of the intervention. Thus, the Save to Win contest occurred in

$Time \in [0, 5]$. We also report in Figure 4 data from account balances prior to the intervention. Since the microfinance bank had been open for six months prior to our intervention for participants in Phase 1, we include $Time \in [-6, -1]$ as well.

Figure 4: Account Balances Centered on the Treatment

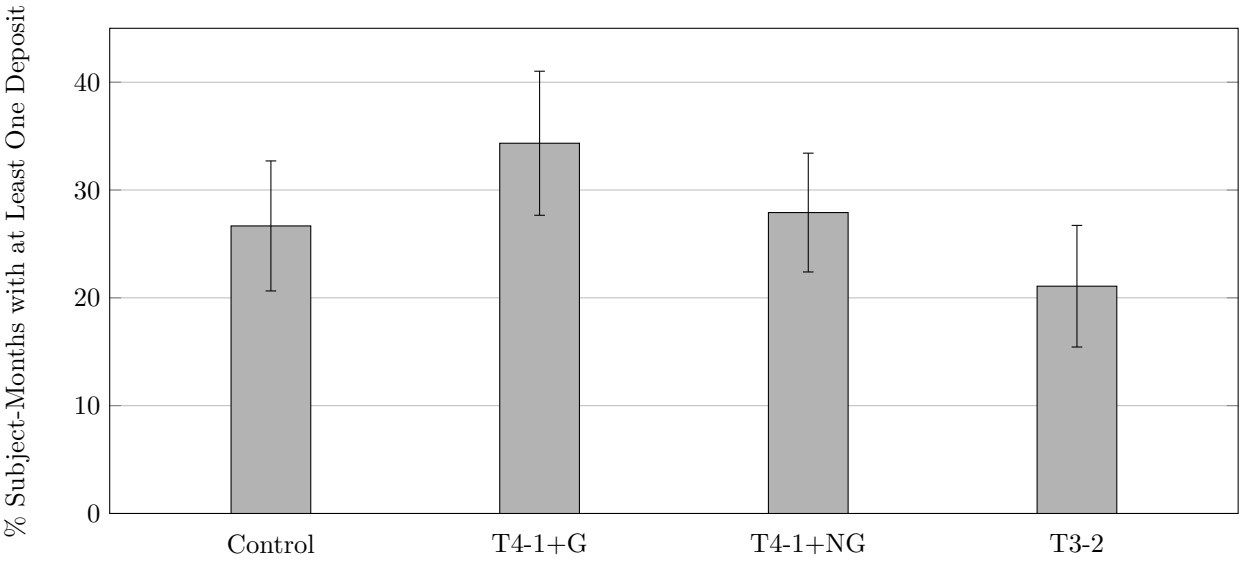


Data is centered on the beginning of the treatment. Open circles denote the average account balance of those within the *Control* group. Closed circles denote the average account balance of those within the *Treatment 4-1, No Guarantee* group. The squares denote the average account balance of those within the *Treatment 4-1, Guarantee* group. The triangles denote the average account balance of those within the *Treatment 3-2* group. The y-axis measures the average (per subject) account balance for those within each cohort each month.

As one can see, average account balances were quite modest prior to our intervention. Regardless of which cohort a subject is assigned to, a substantial improvement in savings occurs. Our Treatment 4-1, where 80% of one's deposit goes into the savings account, especially with the prize guarantee, sees the largest response.

Figure 5 also considers the prevalence of making a deposit. Specifically, it considers the proportion of subjects in a month who make at least one deposit, regardless of its size. In other words, it investigates saving's extensive margin.

Figure 5: Treatment Effect: Activity



Comparing only the *Control* to the *Treatment 4-1, Guarantee* group (labeled "T4-1+G" in the figure), a two-tailed, difference-in-means t-test (allowing for unequal variances) has $t = 1.68$ ($p = 0.093$). A Wilcoxon Ranksum test has $z = 1.68$ ($p = 0.092$).

Similar findings arise. Treatment 3-2 has relatively less savings activity. Treatment 4-1 without the guarantee is, again, statistically indistinguishable from the control. Treatment 4-1 with the guarantee records a noticeably higher proportion of the subjects who make a deposit in a given month than the control.

While Figures 3, 4, and 5 provide our main result, they do not account for differences in the background characteristics and experiences of the individuals who make up each cohort. The balance table previously presented shows some slight differences in the subject pools. Thus, it is appropriate to establish that the cohort differences identified are not sensitive to controlling for differences in the subject pools. Table 2 presents the results.

Table 2: Treatment Effect: Deposit Size

Model:	OLS (1)	OLS (2)	OLS (3)	Tobit (4)
Treatment 3-2	-163.78 ***	-1639.78 **	-930.81	-5240.65
	(819.05)	(816.94)	(804.44)	(2719.83)
Treatment 4-1	875.74			
	(694.83)			
Treatment 4-1, Guarantee		1916.52 **	1836.34 **	6299.18 ***
		(823.19)	(838.71)	(2493.37)
Treatment 4-1, No Guarantee		76.99	786.25	3703.05
		(772.36)	(773.61)	(2453.81)
Background Controls	No	No	Yes	Yes
Treatment Month Controls	No	No	Yes	Yes
R^2	0.015	0.021	0.158	0.062
AIC	18180	18177	17673	5423

The dependent variable is the size of the subject's total deposit for a month (in Ugandan Shillings); $\mu = 2488.79$ UGX. A constant is included in each specification, but not reported. Standard errors are presented in parentheses; *** 1%; ** 5%; * 10% level of significance. $N = 870$ for (1) and (2). For (3) and (4), it is $N = 852$. There is missing information from one subject on *Age*, one subject on *Crops*, and one subject on *Mud*. In (4), there are 622 left-censored observations and 230 uncensored observations.

As suggested in Figure 3, Treatment 3-2 coincides with lower average monthly deposits. Treatment 4-1 has larger deposits than the control, but the difference is statistically indistinguishable from zero, column (1). When this treatment is separated into those who had the guarantee and those who did not, the effect is identified, column (2).

The average treatment effect is robust to the inclusion of subject specific controls and month fixed effects, column (3). Also, we estimate a Tobit model recognizing the large number of zeroes that exist in the data.[12] The treatment effect persists in this alternative estimation, column (4).

While Table 2 establishes the volume of savings, Table 3 uses an indicator variable equal to one if a deposit of any size is made in a month by the subject as the dependent variable. This table tests the robustness of the observations made in Figure 5.

[12]Here, 72.0% of our subject-month observations are zero.

Table 3: Treatment Effect: Activity

	(1)	(2)	(3)
Treatment 3-2	-0.0559	-0.0559	-0.0460
	(0.0438)	(0.0438)	(0.0290)
Treatment 4-1	0.0404		
	(0.0371)		
Treatment 4-1, Guarantee		0.0768 *	0.0757 **
		(0.0441)	(0.0302)
Treatment 4-1, No Guarantee		0.0124	0.0484 *
		(0.0414)	(0.0279)
Background Controls	No	No	Yes
Treatment Month Controls	No	No	Yes
R^2	0.008	0.010	0.626
AIC	1065	1064	239

The dependent variable is an indicator variable equal to one if the subject made at least one deposit; $\mu = 0.2747$. A constant is included in each specification, but not reported. Linear probability model estimated. Standard errors are presented in parentheses; *** 1%; ** 5%; * 10% level of significance. $N = 870$ for (1) and (2). For (3) and (4), it is $N = 852$.

Again, the Treatment 4-1, especially with those within the cohort that were given the guarantee on the prize's size, respond by increasing the rate at which they participate in the savings program. Therefore, we establish our second result.

Result 2: *The Save to Win intervention with contests for a guaranteed prize lead to more formal savings than the control group with only the high interest rate.*

Thus, Hypothesis 2 receives strong empirical support.

Finally, while not the focus on the analysis, we also identify an effect of winning a contest on subsequent deposits. Table 11 in the appendix provides the estimates. If a subject won the contest in the past month or had won a contest in any previous month, the size of the deposit made in the next month grows. The former grows the average deposit made by more than 8000 UGX, increasing the probability of making a deposit by 30%, while the latter increases the average deposit by approximately 3000 UGX increasing the probability of making a deposit in the month by about 10%. Thus, the act of winning a contest creates inertia.

6.3 Heterogeneous Treatment Effect

The previous subsection established that the Save to Win accounts, buttressed with a minimum prize guarantee, can promote savings by those who otherwise would not be saving. We next ask who it is who responds positively to the intervention. In this subsection, we will explore four dimensions to the background and characteristics of the subject pool.

First, we consider two important dimensions to preferences, namely risk aversion and patience. Hypotheses 3 and 4 predict they should be important for our intervention's success. We had each subject complete an incentivized price menu risk assessment. Similar to Eckel and Grossman (2002), subjects make a series of binary choices between a 50-50 lottery and a certain amount. The lottery resulted in either 0 or 1000 UGX. The decision problems differ by the size of the certain amount (ranging from 50 to 1000 UGX). A reproduction of the risk assessment is provided in Table 10 of the appendix.

We record a subject as being risk averse if the number of choices where s/he selected the certain outcome is more than what a risk neutral decision maker would select.[13] In our sample, 79.3% of our subjects who completed the risk assessment are recorded as being risk averse.

In addition, in the background questionnaire a simple survey question asks subjects to rate on a one to five Likert scale how patient of a person s/he views him/herself. The mean value is 3.95. In total, 34.2% score themselves as a '5' on the scale, while 36.8% report a '4'. This leaves 29.0% of the subjects viewing themselves as having some degree of impatience. These two assessments allow us to evaluate two important dimensions to savings behavior: risk and time preferences.

Third, we will compare those subjects with differing financial market activities. In our questionnaire, we ask about experience with microfinance loans. This will potentially be important as the implementation of Save to Win accounts by others will likely be done through already established microfinance institutions. Overall, 44.1% of our subjects have received a microloan in the past.

Also, we compare those subjects who enjoy gambling to those who do not. Our Save to Win accounts introduce uncertainty which may be enjoyed by those who enjoy gambling. We record a subject as being a gambler if they report that they had placed a wager in the previous month; 5.6% of our subjects had.

6.3.1 Risk Preference

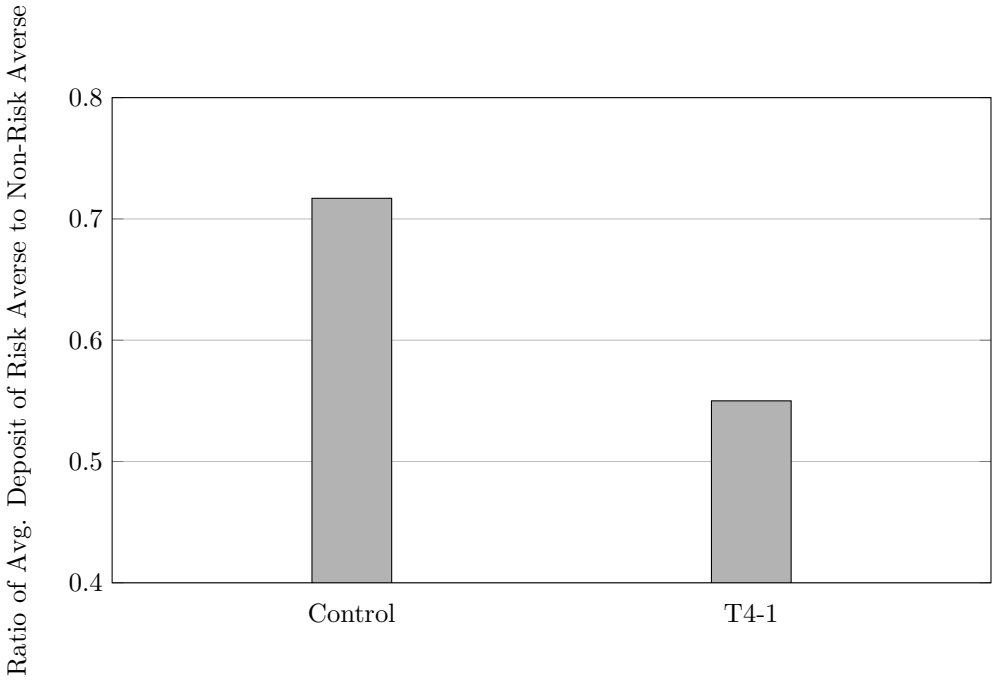
First, the Save to Win account's design was done to take advantage of subject's competitiveness and utility received from engaging in lotteries. Of course, not every subject will prefer to have uncertainty interjected into his/her savings decisions. As articulated in Hypothesis 3, risk averse individuals can be expected to be less interested in participating in the Save to Win program.

Therefore, we use the risk assessment to identify those subjects who are risk averse. To first evaluate differences in behavior of risk averse and non-risk averse subjects, we consider the ratio of average deposit size (per month, per subject) for those who are identified as being risk averse, over the average deposit size for those who are not identified as being risk averse. If this ratio is less than 1.0, then the risk averse subjects deposit relatively less, which is what we anticipate will happen. Lower values to this ratio, though, denote a stronger relative effect of risk aversion. Figure 6 presents this ratio for the those in the Control and those

[13] In our assessment, ten of the twenty decision problems have the certain amount strictly greater than the expected value of the lottery. The eleventh has the two equal. Hence, a risk averse subject will select the certain outcome at least ten times. In addition, due to incomplete participation in the assessments, data on risk preferences exist for 111 subjects (76.6% of the subject population). An indicator variable for missing information is created and included in the upcoming regression analysis. Thus, the risk aversion metric should be interpreted as being conditioned on the completion of the assessment.

in Treatment 4-1.

Figure 6: Difference Between Risk Averse and Non-Risk Averse Subjects



Each column depicts the average per subject deposit for those who are risk averse divided by the average per subject deposit for those who are not risk averse (for that cohort). Thus, values less than one indicate that the risk averse make smaller deposits than the non-risk averse.

For both cohorts, risk averse individuals deposit less than non-risk averse subjects. The difference is more pronounced for those in Treatment 4-1, which includes the lottery. The risk averse only deposit approximately 55% as much as others within the same treatment. Thus, Figure 6 provides suggestive evidence that risk preferences matter for the success of the Save to Win program.

To evaluate econometrically risk's heterogeneous treatment effect, we extend the previous tables to include the subjects' risk preference metric. We include this characteristic as an explanatory variable for the deposit sizes made by subjects in the treatments. We also interact it with the treatment indicators to evaluate heterogeneous treatment effects. Table 4 provides the results.

Table 4: Risk Preference & Deposit Size

Model:	OLS (1)	Tobit (2)
Risk Averse	-891.28 (1508.46)	-1809.62 (4172.56)
Treatment 4-1, Guarantee	7847.22 *** (2171.04)	17296.38 *** (5981.10)
Treatment 4-1, No Guarantee	77.99 (1659.98)	5511.42 (4465.09)
Risk Averse x Treatment 4-1, Guarantee	-6697.80 *** (2343.84)	-11955.63 * (6357.52)
Risk Averse x Treatment 4-1, No Guarantee	-168.65 (1876.88)	-2299.26 (5140.14)
Background Controls	No	Yes
Treatment Month Controls	No	Yes
R^2	0.042	0.064
AIC	18166	5421

The dependent variable is the size of the subject's deposit for a month (in Ugandan Shillings); $\mu = 2488.79$ UGX. A constant is included in each specification, but not reported. Also, the Treatment 3-2 indicator, along with its interaction with risk aversion, are included as well. Standard errors are presented in parentheses; *** 1%; ** 5%; * 10% level of significance. $N = 870$ for (1) and $N = 852$ for (2). In (2), there are 622 left-censored observations and 230 uncensored observations.

Within the control group, the deposits of risk averse individuals are slightly smaller than the non-risk averse. This difference, though, is not statistically significant. This is reasonable since those randomly selected to be in the control group do not have the uncertainty created by the contest. Without this uncertainty, the difference in behavior is not pronounced.

Regarding those individuals selected randomly to be in the treatment, a noticeable difference arises. Relative to those in the control group (the omitted category), the non-risk averse increase their deposits when the contests are included in the intervention. Once again, this effect is pronounced for those who also have the minimum guaranteed amount on the prize. Using the coefficient estimate in column (1) of 7847.22, being in Treatment 4-1 with the guarantee for a non-risk averse subject results in almost a full standard deviation increase in the amount deposited each month (0.94 standard deviations). For those in this treatment, the risk averse subjects deposit significantly less than the non-risk averse subjects. Using the estimated coefficient in column (1) of -6687.80 and the summary statistics for only those subjects assigned to Treatment 4-1, the risk averse deposit more than two-thirds of a standard deviation less than the non-risk averse subjects.[14] Hence, risk preferences are an important consideration in the implementation of Save to

[14] For the sample of those randomly assigned to Treatment 4-1, the standard deviation to the deposits is 9673.16. Hence, $\frac{6697.80}{9673.16} = 0.69$.

Win.

As before, column (2) adds the background control variables and treatment month fixed effects, and estimates a Tobit model acknowledging the clumping of the data at zero. The results identified in column (1) persist with these alterations.

The specifications presented in Table 4 include an indicator variable for being assigned to Treatment 3-2, along with an interaction between it and a subject being risk averse. They are added to maintain subjects in the control as the omitted, reference category. We do not present the results because, as already established, behavior by those in this treatment are not statistically different from the control and the differentiation between guaranteed and non-guaranteed prizes were not done.[15] Also, while not presented here, the risk averse are less likely to make a deposit and those within the treatment with the guarantee are less likely to make a deposit than those who are not risk averse. These differences are small and, for the most part, statistically insignificant. Thus, they are not presented separately here. The margin that adjusts is the size of the deposit, as illustrated in Table 4.

6.3.2 Time Preference

A second dimension to individual's traits is time preferences. Hypothesis 4 predicts that patient individuals are more likely to save, and save more when a non-zero amount is chosen. Here, we use the survey question asking subjects to rank, on a Likert scale, the degree to which they view themselves as a patient person.[16] Table 5 explores the relationship between patience and savings.

Regarding those individuals selected randomly to be in the treatment, the more patient a subject is the greater the monthly deposit becomes. For example, comparing a self-identified 'somewhat patient' person to a 'very patient person' (i.e., comparing a "agree" to "strongly agree" response) is associated, using the estimate in column (1), with a increase in deposits per month of 4149.24 UGX. Again, using the distribution of deposits made by those assigned to Treatment 4-1, this corresponds to an increase of almost one-half of a standard deviation (0.43 standard deviations). Thus, within the group selected to participate in the Save to Win accounts, the effectiveness of the intervention in encouraging savings is stronger the more future orientated the person is.

Interestingly, this effect also exists for those assigned to Treatment 4-1 without the guaranteed minimum prize. Here, comparing a 'somewhat' to 'very' patient subject, deposits per month are 0.22 standard deviations greater. Thus, both Save to Win designs are effective for patient individuals.

As before, column (2) adds the background control variables and treatment month fixed effects, and estimates a Tobit model acknowledging the censoring of the data at zero. For the most part, the results identified in column (1) persist.

Again, while not presented, patient individuals in the treatment are more likely to make a deposit in a

[15] The results presented do not change when these subjects are instead omitted from the data set.

[16] The survey question simply states, "I consider myself a patient person." and provides a five-point Likert scale ranging from 'Strongly Disagree' (=1) to 'Strongly Agree' (=5).

Table 5: Patience & Deposit Size

Model:	OLS	Tobit
	(1)	(2)
Patient	-3261.75 ***	-6271.80 *
	(983.30)	(3365.73)
Treatment 4-1, Guarantee	-14167.22 ***	-23400.87
	(4557.23)	(16134.00)
Treatment 4-1, No Guarantee	-13240.55 ***	-21586.55
	(-13240.55)	(18852.00)
Patient x Treatment 4-1, Guarantee	4139.24 ***	8579.90 **
	(1190.43)	(4191.71)
Patient x Treatment 4-1, No Guarantee	2089.13 **	6194.51
	(1429.77)	(4831.98)
Background Controls	No	Yes
Treatment Month Controls	No	Yes
R^2	0.052	0.063
AIC	9656	3014

The dependent variable is the size of the subject's deposit for a month (in Ugandan Shillings); $\mu = 2585.20$ UGX (for those observations without missing values). A constant is included in each specification, but not reported. Also, the Treatment 3-2 indicator, along with its interaction with patience, are included as well. Standard errors are presented in parentheses; *** 1%; ** 5%; * 10% level of significance. $N = 870$ for (1) and $N = 852$ for (2). In (2), there are 324 left-censored observations and 126 uncensored observations.

month than less patient individuals in the treatment. Hence, time preferences matter for both the intensive and extensive margin of savings with our Save to Win intervention.

Putting together the results from these last two tables, predictable relationships between individual preferences and savings behavior arises. The Save to Win program, which was shown to be effective on average, is especially effective amongst those subjects who are not risk averse and who are patient. It may not be a useful tool for risk averse and impatient individuals.

Result 3: *The Save to Win intervention is more effective for those who are not risk averse and not impatient.*

Thus, Hypotheses 3 and 4 receive empirical support.

6.3.3 Microfinance Experience

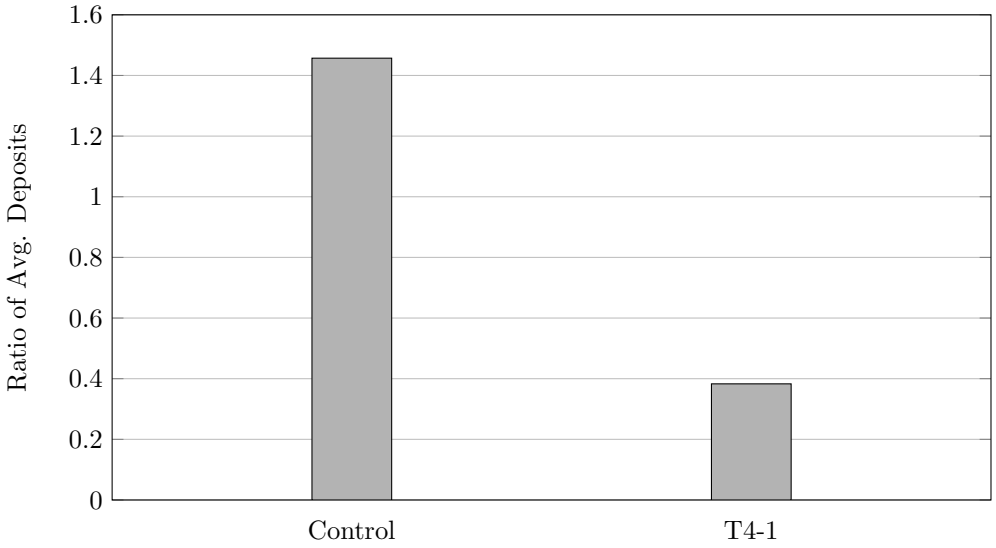
Along with considering a subject's assessed preferences directly, we can also consider observable choices in financial markets that can reasonably correlate with savings behavior. This is relevant for application. Financial institutions promoting local development will be unlikely to directly measure preferences. Implementation through existing microfinance organizations is likely.

First, we explore the correlation between experience in financial markets and savings behavior. Our subject pool is made primarily of those who for the most part do not use formal savings accounts. Many have experience with financial markets through microcredit (more than 40%). We separate our subjects by those who have received a microfinance loan in the past from those who have not. In Figure 7 we consider the ratio of the average per month deposit of those who have received a microloan to those who have not, similar to Figure 6's analysis of risk preferences.

The relative size of the deposit amounts of those with microfinance experience are substantially higher than those without the experience in the control group. Those who have received microloans in the past take advantage of the high monthly interest rate provided in the baseline intervention. This difference reverses for those subjects assigned to the Save to Win treatment. Those who have previously received microloans do not participate. This suggests that individuals with experience in financial markets respond poorly to the intervention, relative to those with less experience.

Next, we consider the econometric results controlling for differences in the subject population. Table 6 presents the results.

Figure 7: Difference Between Those Who Have Received Microloans and Those Who Have Not



Each column depicts the average per subject deposit for those who who have received a microfinance loan divided by the average per subject deposit for those who have not. Thus, values less than one indicate that those who have received a microloan make smaller deposits than those who have not received a microloan.

Table 6: Microfinance Experience & Deposit Size

Model:	OLS	Tobit
	(1)	(2)
Microfinance	997.79	-5658.47
	(1227.80)	(3784.03)
Treatment 4-1, Guarantee	3863.59 ***	6992.64 **
	(1007.60)	(3020.64)
Treatment 4-1, No Guarantee	771.99	3467.33
	(1051.42)	(3373.61)
Microfinance x Treatment 4-1, Guarantee	-5489.85 ***	-4799.02
	(1731.05)	(5430.48)
Microfinance x Treatment 4-1, No Guarantee	-1653.81	1897.79
	(1610.60)	(4890.53)
Background Controls	No	Yes
Treatment Month Controls	No	Yes
R^2	0.037	0.065
AIC	18176	5419

The dependent variable is the size of the subject's deposit for a month (in Ugandan Shillings); $\mu = 2488.79$ UGX. A constant is included in each specification, but not reported. Also, the Treatment 3-2 indicator, along with its interaction with microfinance, are included as well. Standard errors are presented in parentheses; *** 1%; ** 5%; * 10% level of significance. $N = 870$ for (1) and $N = 852$ for (2). In (2), there are 622 left-censored observations and 230 uncensored observations.

Again, within those assigned to the control group, past microloan recipients behave (statistically) similar to those without that experience. Within the treatment group, especially those with the guaranteed prize, microfinance recipients make smaller deposits. Using the estimated coefficient in column (1), this corresponds to 0.60 standard deviation decrease.

In column (2), though, the statistical significance of this effect is gone. Investigating this change further, if (2) is re-estimated using OLS, the coefficient on *Microfinance* x *Treatment 4-1, Guarantee* remains statistically insignificant. Significance is regained if either the wealth-related controls (i.e., composition of the floor and walls of a subject's house and the availability of electricity) or the occupation-related indicators (whether the subject owns livestock or farms crops) are excluded. Thus, microfinance experiences coincides with economic well-being of the subjects. Relatively better off individuals tend to be the one's who receive microfinance loans in the past. It is this group that does not respond positively when the Save to Win intervention is offered.

Once again, while not presented here, similar findings arise if the extensive margin to savings is considered (rather, whether a deposit was made in a month). Taken together, those types of people who chose to seek out microloans are also the ones who save and prefer not to participate in the Save to Win program. Our intervention is effective for those without experience with financial markets.

6.3.4 Gambling

Finally, the Save to Win program introduces uncertainty into the savings decision by having contests. The winner of the contest is driven by "luck", akin to gambling. Further, related research suggests that sports gambling can be a strategy used to provide the funds needed to acquire indivisible, lumpy expenditures (Herskowitz, 2021). Therefore, we consider a subject's self-reported interaction in gambling markets and use of Save to Win in Table 7.

The increase in the average, per month deposit is concentrated in those who gamble leisurely. What is noteworthy about the findings in Table 7 is that this heterogeneous effect occurs in the Treatment 4-1 cohort who did not have the guarantee. This suggests that "gamblers" prefer not to have the protection of the minimum prize. They escalate their savings when risks exist. In the treatment with the guaranteed minimum, it is the non-gamblers who respond by depositing relatively more.

Result 4: *Savings behavior of those in the Save to Win intervention is correlated with microfinance experience and gambling participation.*

7 Long-Term Effects

While we have documented an effect of our treatment, it is prudent to ask whether we have a long-term impact. In this section, we evaluate two additional data. First, we track account balances over time after

Table 7: Gambling Participation & Deposit Size

Model:	OLS	Tobit
	(1)	(2)
Gambler	-2142.16	-7522.62
	(3410.61)	(12529.66)
Treatment 4-1, Guarantee	1964.61 **	6344.46 ***
	(827.93)	(2465.41)
Treatment 4-1, No Guarantee	-472.44	1816.84
	(779.72)	(2477.77)
Gambler x Treatment 4-1, Guarantee	-1464.61	-9497.94
	(4825.48)	(17458.78)
Gambler x Treatment 4-1, No Guarantee	12639.11 ***	29152.28 **
	(4190.20)	(13820.38)
Background Controls	No	Yes
Treatment Month Controls	No	Yes
R^2	0.043	0.065
AIC	18165	5416

The dependent variable is the size of the subject's deposit for a month (in Ugandan Shillings); $\mu = 2488.79$ UGX. A constant is included in each specification, but not reported. Also, the Treatment 3-2 indicator, along with its interaction with gambling, are included as well. Standard errors are presented in parentheses; *** 1%; ** 5%; * 10% level of significance. $N = 870$ for (1) and $N = 852$ for (2). In (2), there are 622 left-censored observations and 230 uncensored observations.

our intervention ended. Second, we survey subjects one year after Phase 1 ended (six months after Phase 2 ended) to gain insight into their post-intervention behavior.

7.1 Wealth Accumulation

A concern is that the wealth accumulation documented is transitory. As stated, the intervention provided both a higher monthly interest rate and a contest. After the six month intervention ended, all accounts automatically rolled over into a 0% interest savings account, but without withdrawal constraints. It is quite possible that a subject would rather not engage in the savings contest, prefers to not save when the interest rate is zero, but is willing to save and even take on the contest's uncertainty when the interest rate is 2% per month. Such a person would be expected to participate but liquidate his or her holdings at the end of the trial's phase. Consumption could have simply been delayed. Alternatively, if the wealth accumulated during the intervention promoted savings, and more importantly promoted saving behavior, then the account balances will stay above zero.

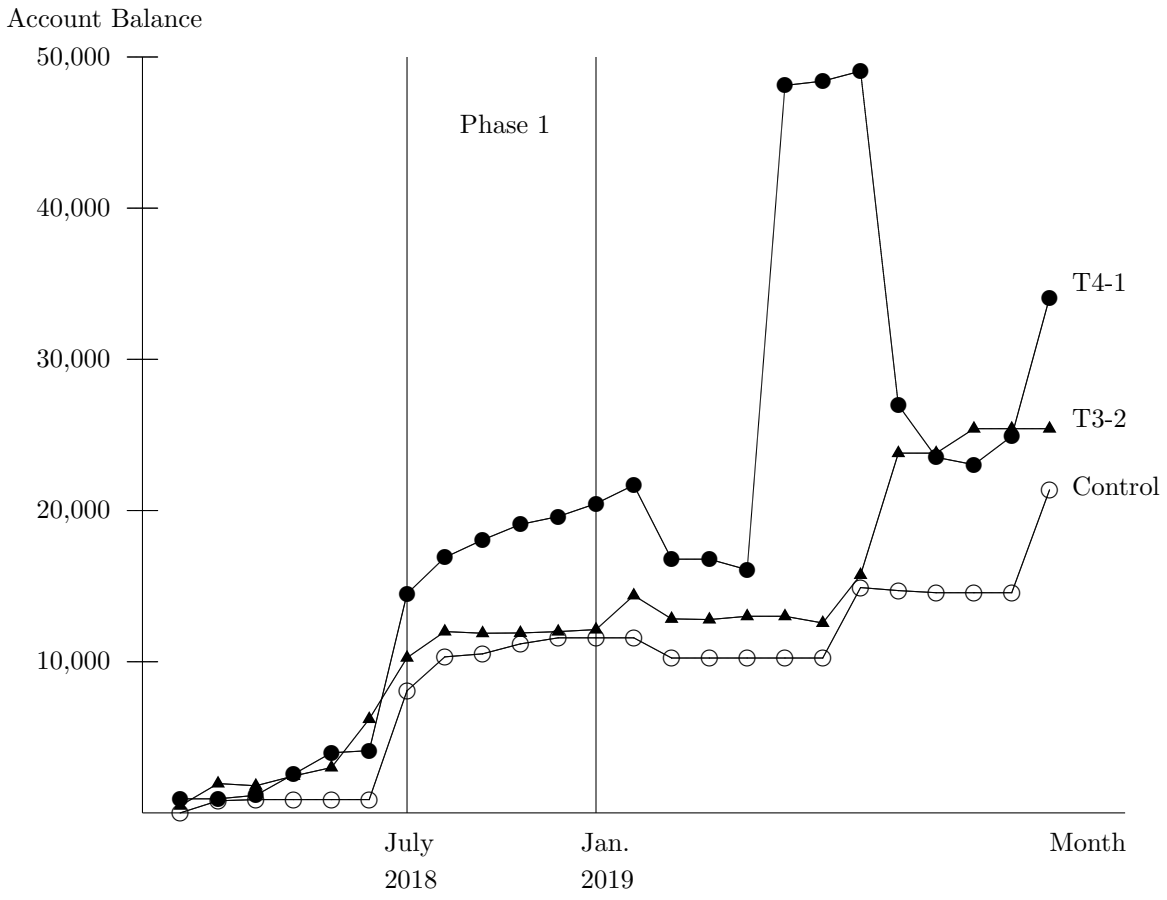
Therefore, we track the subjects after the intervention. Figure 8 depicts the average savings account balances of the Phase 1 subjects in each cohort. It includes data since the opening of the bank in the community, through our intervention, and for the twelve months after Phase 1 of the Save to Win mechanism ended. This means that we have twelve months of post-study balance information for those in Phase 1 and six months for those in Phase 2. It also means that we have six months of pre-study information for those in Phase 1 and twelve months for those in Phase 2. We do not include interest accumulation in these balances so that we isolate saving choices.

During Phase 1, the balances of the subjects in all cohorts grow rapidly. Clearly, there is an initial jump in the intervention's first month, which is to be expected given that a deposit must be made to be eligible to be a part of our intervention. Recall that if a subject came to the advertised meeting to be a part of the study, they received an account with a starting balance of 2500 UGX, which is the minimum account balance allowed by the bank. The per subject balance, though, is substantially higher than this minimum. For example, the per subject account balance for those in the control is 14.4 times as great after the first month of our intervention than the month prior. If subjects had only taken the opening account balances as charity, the per subject balance would have only increased by a factor of 6.2.

Balances grow for subjects in all three cohorts over the intervention's time period, as established previously. Interestingly and importantly, the balances after the intervention do not zero out. As one can see in Figure 8, the balances of those in Treatment 3-2 and the control stay relatively stable afterwards. Over time they begin to grow. Those assigned to Treatment 4-1 experience an escalated post-intervention growth. There are periods of partial liquidation. A subset of this subject pool starts actively using their savings account building up their balances substantially. These observations suggest that the intervention promoted savings behavior.

To evaluate post-intervention behavior in Phase 2, we recenter the data around the timing of the treatment

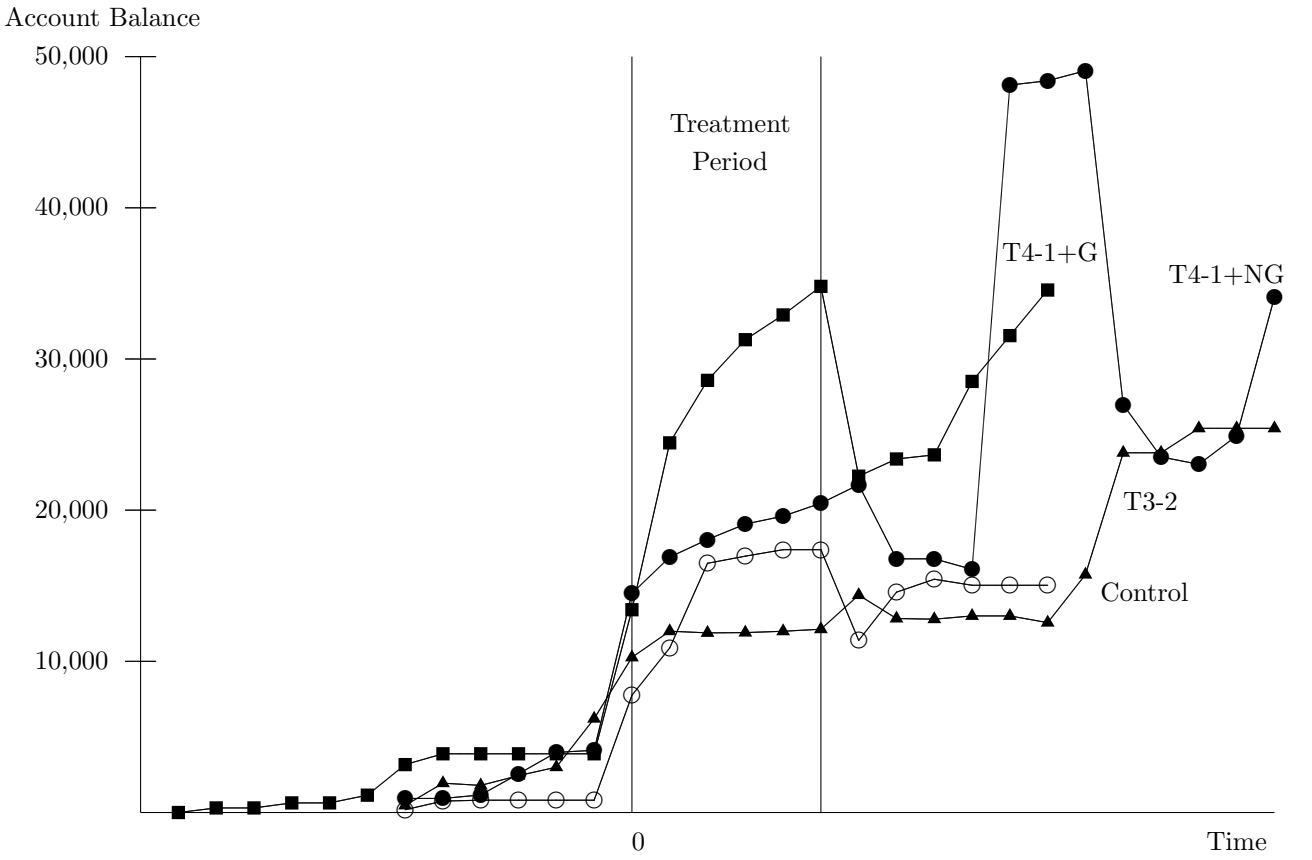
Figure 8: Account Balances for Phase 1 Participants



Open circles denote the average account balance of those within the *Control* group. Closed circles denote the average account balance of those within the *Treatment 4-1* group. The triangles denote the average account balance of those within the *Treatment 3-2* group. The y-axis measures the average (per subject) account balance for those within each cohort each month.

so that $Time = 0$ is the first month of the intervention for both phases and the post intervention behavior is measured for $Time \geq 6$. Rather, we extend Figure 4. Data from subjects in the two controls are pooled. Figure 9 presents the average account balances for subjects in both phases.

Figure 9: Account Balances Centered on the Treatment



Data is centered on the beginning of the treatment. Open circles denote the average account balance of those within the *Control* group. Closed circles denote the average account balance of those within the *Treatment 4-1, No Guarantee* group. The squares denote the average account balance of those within the *Treatment 4-1, Guarantee* group. The triangles denote the average account balance of those within the *Treatment 3-2* group. The y-axis measures the average (per subject) account balance for those within each cohort each month.

Recall that since Phase 2 started at the conclusion of Phase 1, subjects in this phase had a twelve months to open savings accounts and save prior. Few in these cohorts did so. Therefore, it is fair to presume that Phase 2 considers individuals who would not be interacting in formal financial markets absent our intervention.

Once again, the treated subjects build up their savings both during and after the intervention (depicted by the "T4-1+G" line). Again, immediate liquidation after the conclusion of the mechanism does not occur. Balances continuously grow for those in Treatment 4-1 with the guarantee.

To confirm econometrically, we consider the pooled data set of account balances of the subjects from $Time = -12$ to $Time = 17$[17] and evaluate the time trend in wealth accumulation. We allow jumps in the time trend at the beginning and end of the intervention, and allow for the trend to adjust after each. Rather, we estimate

$$Balance_{it} = \alpha_0 + \alpha_1 Study_{it} + \alpha_2 Post_{it} + \alpha_3 Time_{it} + \alpha_4 Time_{it} \times Study_{it} + \alpha_5 Time_{it} \times Post_{it} + \epsilon_{it}. \quad (4)$$

The indicator *Study* equals one for the months where the Save to Win accounts are active and the indicator *Post* equals one for the periods after. That is, $Study = 1$ for $Time \in [0, 5]$ and $Post = 1$ for $Time \in [6, 17]$. This equation is estimated for each treatment cohort. Immediate liquidation would result in $\alpha_2 < 0$. Instead, if we promoted continued savings behavior after our intervention, then $\alpha_5 > 0$. Table 8 presents the results.

Table 8: Time Series

	Treatment 4-1, Guarantee	Treatment 4-1, No Guarantee	Treatment 3-2	Control
Time	447.74	756.15	928.57	102.94
	(416.42)	(3270.07)	(1264.27)	(446.03)
Time x Study	3409.75 ***	357.28	-662.23	1843.59 *
	(1260.43)	(4624.57)	(1787.95)	(1000.20)
Time x Post	2149.41 *	40.37	513.56	10142.36 **
	(1260.43)	(3464.39)	(1339.40)	(4687.39)
constant	5045.27	4925.58	5892.16	1041.98
	(3063.14)	(12735.08)	(4923.62)	(2542.77)
Study	12884.74 ***	10388.93	5134.49	8567.41 **
	(4728.42)	(16130.86)	(6236.49)	(3716.50)
Post	197.70	15034.60	-4298.32	10142.36 **
	(10760.05)	(3464.39)	(7241.67)	(4687.39)
R^2	0.179	0.016	0.054	0.089
N	792	1032	816	840

Dependent variable is the ending account balance for a subject for the month. Each column presents the results for the subsample defined. Standard errors presented in parentheses; *** 1%, ** 5%, * 10% level of significance.

Notice that the coefficient on $Time \times Post$, $\widehat{\alpha}_5$, is positive and statistically significant for Treatment 4-1 with the guaranteed prize. Thus, not only did this treatment produce the most savings during the intervention (as acknowledged by the positive and statistically significant coefficients on the intervention indicator and its interaction with time), but it improved the average savings rate over time. We interpret this as savings behavior being promoted.

The effect is small and statistically indistinguishable from zero for that treatment without the guarantee. Instead, those subjects experience a large (but statistically insignificant) level increase after the intervention's

[17] *Time* runs to a value of 17 since the study's intervention occurs for periods 0 through 5 of the re-centered data and the twelve months of post-intervention behavior occurs from period 6 to 17.

end. Again, the results for those in Treatment 3-2 point to its ineffectiveness. The high interest rate provided to those in the control promotes both savings during, and sustained savings after.

The final dimension to our post-intervention analysis is the question of whose savings behavior changed. Our presumption is that the inducement to save during the intervention encouraged a subset of the subjects to continue to save afterwards. An alternative is that the uncertainty discouraged potential savers. After our intervention ended, this group begins to save. To evaluate this, we differentiate our subjects by the number of months they participated by making a deposit. Some subjects were active only in the first month and did not return to the bank and make a deposit in any of the remaining five months. Moderate participants come back one more month. We classify the users of the program as those who make additional deposits in at least two more months, thus participating in contests at least one-half of the time. Table 9 compares their average account balances at the end of the intervention to their balances six months after.[18]

Table 9: Account Balance Growth

Number of months with a deposit after the initial month	Avg. balance at the end of the intervention	Avg. balance six months after the intervention	% change	N
0	7573.47	7231.63	-4.5%	98
1	21,609.09	31,836.36	+47.3%	22
2 or more	83,940.38	127,713.50	+52.1%	26

Subjects are classified by the number of months, after the first month of the intervention (when everyone must make a deposit), where at least one deposit is made.

First, as to be expected, those who made deposits in two or more months after the initial month have higher average balances.[19] There is a monotonic relationship between the number of months participating in the intervention and the growth rate of the accounts. In fact, those who did not participate see a reduction in their balance. These subjects presumably gained from the community meal and the opening account balance, but did not want to save. For those who frequently participated, they continued to grow and in fact escalated their average monthly deposits. This is especially noteworthy remembering that after the intervention, accounts did not pay any interest.

Result 5: *The Save to Win intervention has a longer-term improvement in wealth.*

Overall, our Save to Win intervention had a meaningful impact on savings in the community even after our intervention ended.

It is worth noting here the strong heterogeneous behaviors both during the experiment and after. Approximately two-thirds of the subjects did not participate in our mechanism after enrolling in the first month. Enrolling provided them with the 2500 UGX gift (and its compounded interest), but exposed them to the

[18] We choose six months because we have data on account balances for participants in both phases through six months.

[19] The account balance information provided does not include interest accumulation, but does include the prizes that were won in the contests.

losses from not winning the contest in that first month. After the intervention ended, they only withdrew a small proportion of their balance. The one-third who did participate built up substantial savings and continued to make deposits after the intervention.

7.2 Survey Responses

In December 2019 Embrace It Africa engaged in a survey of those who had participated in our Save to Win program. They asked questions about the growing use of mobile money accounts, participation in ROSCAs, and use of other SACCOs. We added a few questions to their short survey.[20] Specifically, we wanted to assess (i) what our intervention crowded out and (ii) what are their future plans with the account balances.

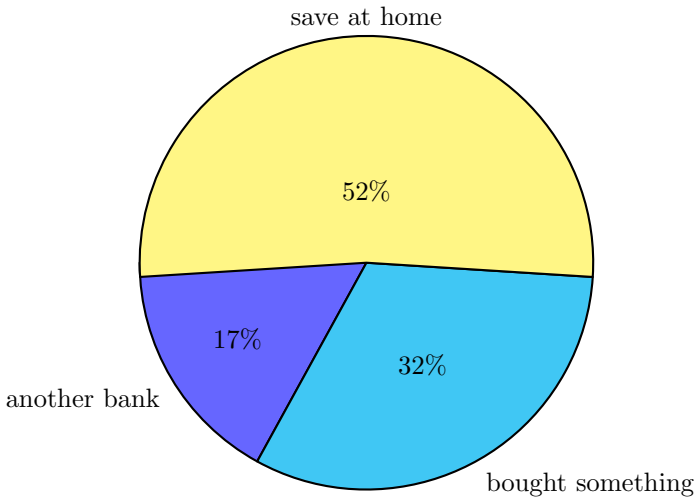
Survey data is notoriously unreliable. Subjects may not respond truthfully, instead providing answers that they feel the survey taker wants to hear. In addition, respondents may not be aware of their actual behavior at the margin. Nevertheless, we felt that the responses, while potentially imprecise, could be informative in providing a glimpse of our intervention's impact.

We feel that this is important in our context. We intervened in a poverty stricken community where income and wealth are insufficient. Educational attainment is low and health is poor. Rakai was the center of the HIV/AIDS pandemic in the 1980s and 1990s with high prevalence to this day. One concern is that saving at Mikwano requires that money to be taken away from another valued use. For another, we are introducing uncertainty into a world full of uncertainties with little formal or informal insurance.

The survey team was able to connect with 128 of our subjects (88%). The month of the survey corresponds to twelve months since the completion of Phase 1 and six months since the end of Phase 2. To address (i), we asked them "If you had not deposited money into the Save to Win account, which of the following would you have done with the money?" and asked them to select one of multiple choices. Increased savings at the bank must come from funds that would have been used for another purpose. Hence, it is important to understand the side effects (Medina, 2021). Figure 10 depicts the results.

[20] We also covered the labor costs of the survey.

Figure 10: What would you have done with the money you deposited?

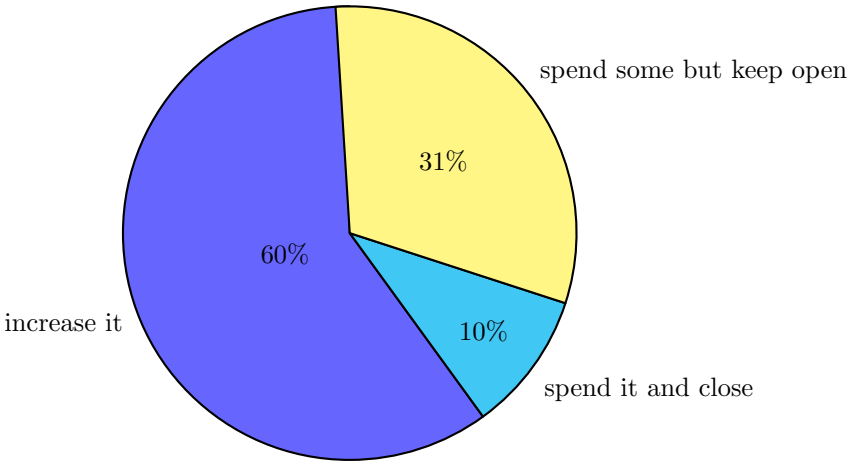


A slight majority would have saved the money at home. Thus, for these subjects, Mikwano was able to provide a safe place to save and our intervention encouraged it. Given that our subjects do not liquidate after the intervention, this risk mitigation seems to be a big part of our intervention's value. Also, about one-third of the subjects report forgone consumption.[21] Interestingly, for one-sixth of our subjects, we substituted away from using another bank. Since members of the Bethlehem community must travel a distance (without paved roads) to the nearest city to find another bank, our intervention presumably lowered the transaction cost to saving for these subjects.

We also asked our subjects in a multiple choice question what they plan to do with their balances in the future. Figure 11 depicts their responses.

[21] These responses were split between buying something for one's self and buying something for friends and family.

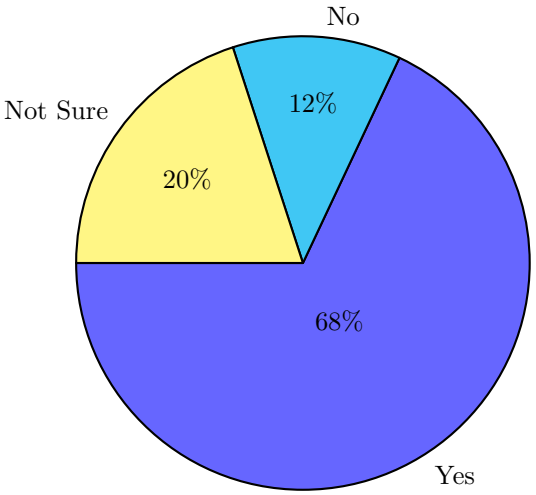
Figure 11: What will you do with your account balance?



Three-fifths of the respondents plan to further increase their balances, and only 10% plan to close their accounts. Given that approximately 88% did not have accounts prior to our intervention, we clearly brought financial inclusion to the community.

Finally, to gauge satisfaction with the Save to Win program, we asked simple evaluative questions. First, we asked subjects whether they would like to participate in the future if Mikwano was to offer the accounts. Figure 12 depicts their responses.

Figure 12: Would you like to participate in Save to Win in the Future?



Only 12% report that they would not. Therefore, we believe that for the vast majority of the community, our intervention was utility enhancing. Less encouraging, though, is the results from our follow up question asking which treatment they would have preferred to be in. Just over 70% of the subjects who said they would

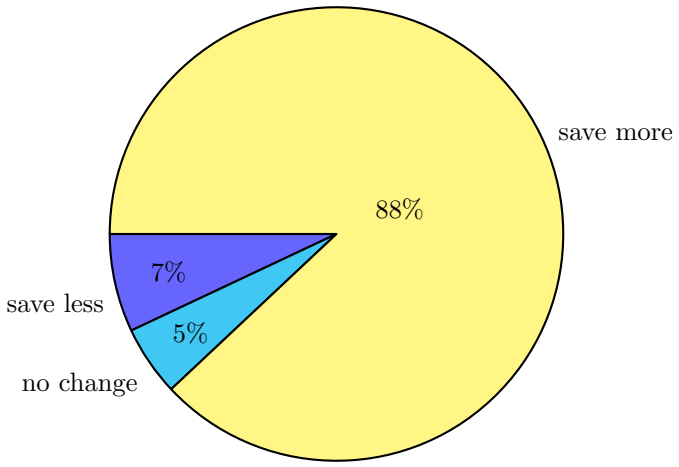
be willing to participate in the future report a preference for the control (accounts without the contest). No subject prefers the Treatment 3-2, which coincides with the lack of enthusiasm in our previously reported depositing behavior.

Digging further into these responses, interestingly, 75% of the subjects selected randomly to be in Treatment 3-2 report a preference for having been in the control. On the other hand, 70% of those chosen to be in the control would have preferred to have the contest included. The grass seems greener on the other side of the fence. Of those who were selected randomly to be in Treatment 4-1, a noticeable difference in responses arises between those who had the guaranteed minimum prize and those who did not. The proportion who prefer to continue with the contest is 75% higher for those who had the guarantee than those who did not.

Taken together, this suggests that it is important to design the Save to Win accounts properly. Treatment 3-2 did not inspire savings behavior and generated a dis-satisfaction with the contest linked savings. The guaranteed minimum prize promoted an interest in including the contest, while those who did not have the opportunity to participate wish they could have.

The divided opinions regarding the Save to Win program did not spill over and diminish participants' views on Mikwano. Figure 13 depicts their assessment of our partner organization.

Figure 13: Future savings at Mikwano?



The community was motivated by the savings product and respond eagerly to opportunities at the microfinance bank we partnered with.

8 Conclusion

We develop an innovative mechanism to promote saving in a poverty stricken area of the world. Our insight builds upon the findings in behavioral and experimental economics on contests, which consistently documents an excessive amount of competition in these environments. We link contests to savings in our Save to Win

accounts and randomize assignment to them varying the allocation of deposits and the determination of the prize's size. Our intuition is that the motivations that drive excessive competition in contests can be harnessed to promote savings.

Overall, we document an important increase in savings due to our intervention. In all cohorts, savings far exceeds that observed prior to the intervention and by those not currently in our treatments. Second, our control group benefits from the opening balance and high interest rate, but does not get to participate in the savings contest. Those in our treatments also have a portion of their deposits put into a contest. We show that our Save to Win account promotes saving, especially those treated with a guaranteed minimum prize. Third, we establish important and intuitive heterogeneous treatment effects. The introduction of contests promote savings of those who are not risk averse and who are patient. It is not used by those who have recently received microloans and are enjoyed by those who like to gamble. Fourth, we show that our intervention has long lasting effects on savings behavior as average balances actually grow over time.

Our objective was to implement a novel savings mechanism in the field, which was informed by research in behavioral and experimental economics, that can easily be implemented by existing microfinance institutions at a relatively low cost. We are constrained in both sample size and number of treatments by the population size of the community we worked with. Nevertheless, our positive results point the way to a potential valuable intervention for organizations striving to mitigate poverty.

Caution is prudent. Along with concerns about Save to Win's external validity in different communities with contrasting economic, social, legal, and cultural environments, the normative implications are unclear. Undeniably, our intervention built up savings account balances. What is uncertain is what was the opportunity cost of doing so. Given our savings results, we crowded out other forms of savings; especially informal, at-home saving. Thus, our benefit to most may have been mitigation of risk and lowered transaction cost to saving. Some report reduced consumption as well. It is unclear what disutilities were experienced so that subjects could engage in our intervention. Our sincere hope is that subjects expect more benefit from this savings than the costs incurred, and that negative spillovers were minimal. A second note of caution is that our treatment effect requires that not too much is put into the contest and that a minimum prize is ensured. Thus, the value of Save to Win accounts is sensitive to its design. Third, our results indicate that a zero-sum mechanism is relatively ineffective. While potentially desirable, as practitioners can implement it at little cost, to generate the competitiveness of contests an external subsidy is needed. Therefore, the savings contest may need to be coupled within an institution making microloans where the capital raised can generate the needed returns. If our mechanism was to be rolled out and scaled up, further evaluation of the best design parameters and a fuller understanding of the tradeoffs present is necessary and appropriate.

Much work still needs to be done. First, given that our most aggressive mechanism did not succeed, future work should search to identify the optimal balance between entries into the contest and proportion saved to find the optimal ratio. Second, like Ashraf, Karlan, and Yin's (2006) work on time preferences, it may be important to first identify those who are risk averse and offer them an alternative. It may be best,

for example, to let subjects choose their treatment as well. Finally, for the guaranteed minimum prize we picked one level. It was selected to be greater than what typically arose in the contests. We show that this guarantee was, in fact, crucial for the intervention's success. It requires external subsidization. How large of a prize to ensure is unexplored. What the treatment effect is for different size prizes would be useful knowledge for a nonprofit organization who must balance costs with social benefits.

9 References

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10 Appendix

10.1 Proofs

Proof of Hypothesis 1

For the interior solution (where $\widetilde{s} > 0$), complementary slackness requires that $\frac{\partial L}{\partial s} = 0$. Further, when $\Pi = \Pi^z$, it follows that $E(s;\tau) = (1-\tau)s$. Hence, from (3),

$$\frac{\partial J}{\partial s} - \frac{\partial u}{\partial c_1} + \delta \frac{\partial u}{\partial c_2}(1+i) = 0.$$

Applying the Implicit Function Theorem,

$$d\widetilde{s}\left[\frac{\partial^2 J}{\partial s^2}\right] + d\tau\left[\frac{\partial^2 J}{\partial s \partial \tau}\right] = 0.$$

Since it was assumed that $\frac{\partial^2 J}{\partial s^2} < 0$, it follows that if Assumption 1 holds, then

$$\frac{d\widetilde{s}}{d\tau} = -\frac{\frac{\partial^2 J}{\partial s \partial \tau}}{\frac{\partial^2 J}{\partial s^2}} < 0.$$

This verifies [2] and [3]. For the extensive margin, where $\widetilde{s} = 0$, complementary slackness requires that $\frac{\partial L}{\partial s} \leq 0$. When $\widetilde{s} = 0$, it follows that $E(s;\tau) = 0$. Since $u(c)$ is strictly increasing (so that the budget constraints bind), $\widetilde{s} = 0$ requires that

$$\frac{\partial J}{\partial s} - \frac{\partial u}{\partial c_1} + \delta \frac{\partial u}{\partial c_2}(1+i) \leq 0.$$

When this inequality holds, the individual chooses not to save. It follows immediately from Assumption 1 that an increase in τ expands the set of economic environments where savings is zero. This verifies [1].

QED

Proof of Hypothesis 2

Consider an interior solution where $\widetilde{s} > 0$. Complementary slackness requires that $\frac{\partial L}{\partial s} = 0$. When $\Pi = \Pi^g$, it follows that

$$E(s;\tau) = \left(\frac{(1-\tau)s}{(1-\tau)s + \sigma}\right)\Pi^g,$$

where σ is the amount put into the contest by the other contestants. As a consequence,

$$\frac{\partial E}{\partial s} = \left(\frac{(1-\tau)\sigma}{[(1-\tau)s + \sigma]^2}\right)\Pi^g.$$

Hence,

$$\frac{\partial^2 E}{\partial s^2} = \left(\frac{-2(1-\tau)^2\sigma}{[(1-\tau)s + \sigma]^3}\right)\Pi^g < 0,$$

and

$$\frac{\partial^2 E}{\partial s \partial \tau} = \left(\frac{(1-\tau)\sigma s - \sigma^2}{[(1-\tau)s + \sigma]^3}\right)\Pi^g.$$

Turning to the optimal savings decision by the individual, in an interior solution $\frac{\partial L}{\partial c_1} = \frac{\partial u}{\partial c_1} - \lambda_1 = 0$ and $\frac{\partial L}{\partial c_2} = \delta \frac{\partial u}{\partial c_2} - \lambda_2 = 0$. Hence, $\frac{\partial L}{\partial s} = 0$ is now

$$\frac{\partial J}{\partial s} - \frac{\partial u}{\partial c_1} + \delta \frac{\partial u}{\partial c_2}(1+i)\left[\tau + \frac{\partial E}{\partial s}\right] = 0.$$

Applying the Implicit Function Theorem,

$$d\widetilde{s}\left[\frac{\partial^2 J}{\partial s^2} + \delta \frac{\partial u}{\partial c_2}(1+i)\frac{\partial^2 E}{\partial s^2}\right] + d\tau\left[\frac{\partial^2 J}{\partial s \partial \tau} + \delta \frac{\partial u}{\partial c_2}(1+i)\left(1 + \frac{\partial^2 E}{\partial s \partial \tau}\right)\right] = 0$$

Comparing this to the comparative static prediction with the no-guarantee, zero-sum prize,

$$\frac{d\widetilde{s}}{d\tau} = -\frac{\frac{\partial^2 J}{\partial s \partial \tau} + N_\tau}{\frac{\partial^2 J}{\partial s^2} + N_s},$$

where N_s and N_τ are the new terms introduced in the two brackets. Since $\frac{\partial^2 E}{\partial s^2} < 0$ and $\frac{\partial u}{\partial c_2} > 0$, it follows that $N_s < 0$. As a consequence, the denominator continues to be negative. Further, since $1 + \frac{\partial^2 E}{\partial s \partial \tau} > 0$ and $\frac{\partial u}{\partial c_2} > 0$, it follows that $N_\tau > 0$. Consequently, if $\frac{\partial^2 J}{\partial s \partial \tau} < -N_\tau$, then the numerator is negative as well.

Consequently, under Assumption 2, $\frac{d\widetilde{s}}{d\tau} < 0$ for $\Pi = \Pi^g$. Further, since the denominator is negative, Assumption 2 implies that $\frac{d\widetilde{s}}{d\tau}$ is smaller for the contest with $\Pi = \Pi^z$ than under the contest with $\Pi = \Pi^g$.

Therefore, compared to the control, for each of our treatments (τ_H and τ_L) saving is greater in the contest $\Pi = \Pi^g$, which establishes [2].

For the extensive margin, where $\widetilde{s} = 0$, complementary slackness requires that $\frac{\partial L}{\partial s} \leq 0$. When $\widetilde{s} = 0$, it follows that $E(s;\tau) = 0$. Since $u(c)$ is strictly increasing (so that the budget constraint binds), $\widetilde{s} = 0$ requires that

$$\frac{\partial J}{\partial s} - \frac{\partial u}{\partial c_1} + \delta \frac{\partial u}{\partial c_2}(1+i) \leq 0.$$

When this inequality holds, the individual chooses not to save. It follows immediately from Assumption 2 that an increase in τ expands the set of economic environments where savings is zero more for contests with $\Pi = \Pi^z$ than $\Pi = \Pi^g$. This verifies [1].

QED

Proof of Hypothesis 3

Consider an interior solution, $\widetilde{s} > 0$. Applying the Implicit Function Theorem to (3),

$$d\widetilde{s}\left[\frac{\partial^2 J}{\partial s^2} + \delta \frac{\partial u}{\partial c_2}(1+i)\frac{\partial^2 E}{\partial s^2}\right] + d\delta\left[\frac{\partial u}{\partial c_2}(1+i)\left(\tau + \frac{\partial E}{\partial s}\right)\right] = 0.$$

It follows that

$$\frac{d\widetilde{s}}{d\delta} = -\frac{\frac{\partial u}{\partial c_2}(1+i)\left(\tau + \frac{\partial E}{\partial s}\right)}{\frac{\partial^2 J}{\partial s^2} + N_s}$$

As argued previously, the denominator is negative, regardless of whether the contest has a guaranteed minimum prize or a no-guaranteed prize. Further, since $\frac{\partial u}{\partial c_2}$ and $\frac{\partial E}{\partial s}$ are both positive, then the numerator is positive. Hence, $\frac{d\widetilde{s}}{d\delta} > 0$. This verifies [2].

For the extensive margin, where $\widetilde{s} = 0$, complementary slackness requires that $\frac{\partial L}{\partial s} \leq 0$. When $\widetilde{s} = 0$, it follows that $E(s;\tau) = 0$. Since $u(c)$ is strictly increasing (so that the budget constraint binds), $\widetilde{s} = 0$ requires that

$$\frac{\partial J}{\partial s} - \frac{\partial u}{\partial c_1} + \delta \frac{\partial u}{\partial c_2}(1+i) \leq 0.$$

When this inequality holds, the individual chooses not to save. It follows immediately that since $\frac{\partial u}{\partial c_2} > 0$ an increase in δ contracts the set of economic environments where savings is zero. This verifies [1].

QED

Proof of Hypothesis 4

Consider an interior solution, $\widetilde{s} > 0$. Applying the Implicit Function Theorem to (3),

$$d\widetilde{s}\left[\frac{\partial^2 J}{\partial s^2} + \delta \frac{\partial u}{\partial c_2}(1+i)\frac{\partial^2 E}{\partial s^2}\right] + d\theta\left[\frac{\partial^2 J}{\partial s \partial \theta} - \frac{\partial^2 u}{\partial s \partial \theta} + \delta \frac{\partial^2 u}{\partial c_2 \partial \theta}(1+i)\left(\tau + \frac{\partial E}{\partial s}\right)\right] = 0.$$

It follows that

$$\frac{d\widetilde{s}}{d\theta} = -\frac{\frac{\partial^2 J}{\partial s \partial \theta} + N_\theta}{\frac{\partial^2 J}{\partial s^2} + N_s}$$

where $N_\theta = -\frac{\partial^2 u}{\partial s \partial \theta} + \delta \frac{\partial^2 u}{\partial c_2 \partial \theta}(1+i)\left(\tau + \frac{\partial E}{\partial s}\right)$. As argued previously, the denominator is negative, regardless of whether the contest has a guaranteed minimum prize or a no-guaranteed prize. Further, so long as

$\frac{\partial^2 u}{\partial c_1 \partial \theta} \approx \delta \frac{\partial^2 u}{\partial c_2 \partial \theta}$ and $-1 + (1+i)\left(\tau + \frac{\partial E}{\partial s}\right) > 0$, then $N_\theta < 0$. Hence, from Assumption 3, the numerator is negative so that $\frac{d\widetilde{s}}{d\theta} < 0$. This verifies [2].

For the extensive margin, where $\widetilde{s} = 0$, complementary slackness requires that $\frac{\partial L}{\partial s} \leq 0$. When $\widetilde{s} = 0$, it follows that $E(s;\tau) = 0$. Since $u(c)$ is strictly increasing (so that the budget constraint binds), $\widetilde{s} = 0$ requires that

$$\frac{\partial J}{\partial s} - \frac{\partial u}{\partial c_1} + \delta \frac{\partial u}{\partial c_2}(1+i) \leq 0.$$

When this inequality holds, the individual chooses not to save. It follows immediately that so long as $\frac{\partial^2 u}{\partial c_1 \partial \theta} \approx \delta \frac{\partial^2 u}{\partial c_2 \partial \theta}$ an increase in θ expands the set of economic environments where savings is zero with Assumption 3 holding. This verifies [1].

QED

10.2 Risk Assessment Survey

Table 10 presents a reproduction of the risk assessment used.

Table 10: Risk Assessment

Decision number	Option A (certain amount)	Option B (50-50 lottery)
1	1000	(1000,0)
2	950	(1000,0)
3	900	(1000,0)
4	850	(1000,0)
5	800	(1000,0)
6	750	(1000,0)
7	700	(1000,0)
8	650	(1000,0)
9	600	(1000,0)
10	550	(1000,0)
11	500	(1000,0)
12	450	(1000,0)
13	400	(1000,0)
14	350	(1000,0)
15	300	(1000,0)
16	250	(1000,0)
17	200	(1000,0)
18	150	(1000,0)
19	100	(1000,0)
20	50	(1000,0)

10.3 Additional Outlier Test

The following figures provide the average monthly deposits (averaging over the six months of the intervention) for each subject in the Control and the two Treatment 4-1 cohorts. The subjects are ordered in increasing values. As one can see, subject 36 in the control has a behavior far different than any other subject.

Figure 14: Average Monthly Deposit for Control Subjects

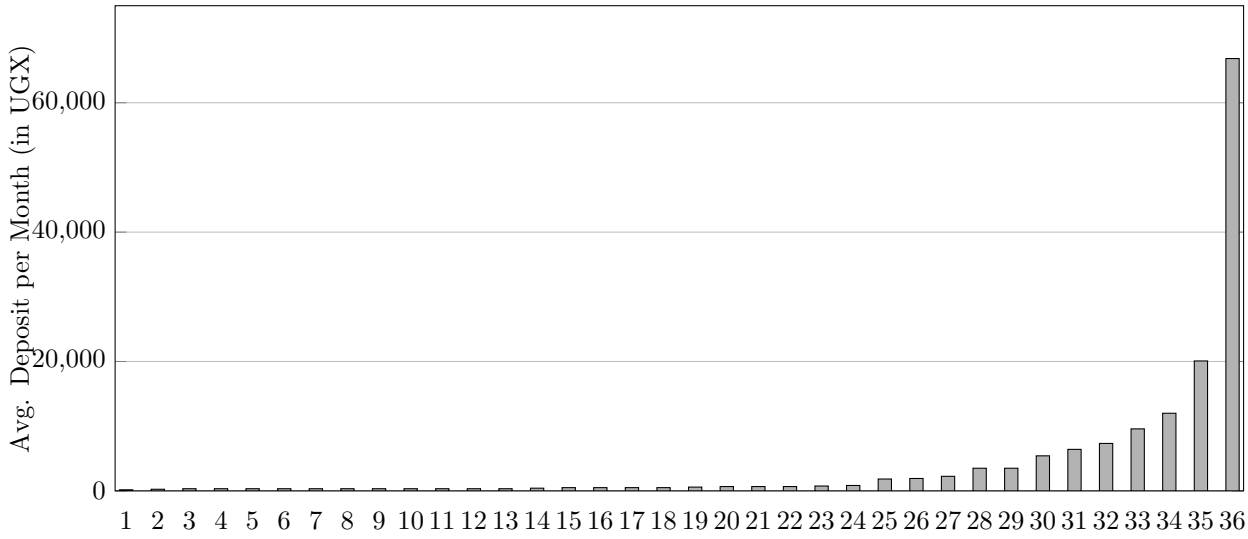


Figure 15: Average Monthly Deposit for Treatment 4-1, Guarantee Subjects

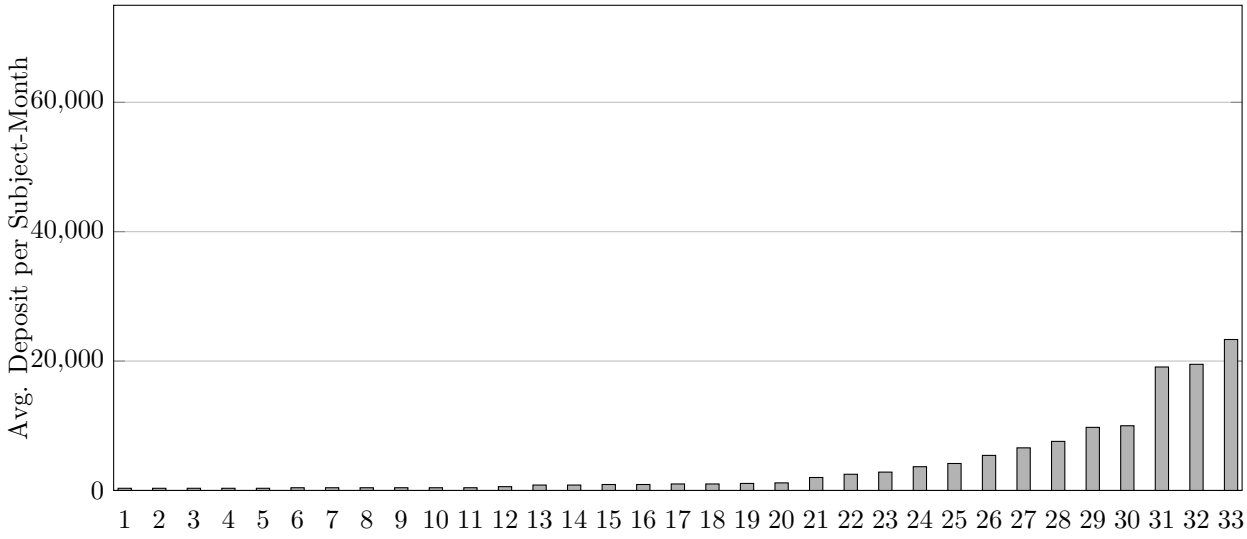


Figure 17 re-estimates the main specification (column 2 of Table 2) systematically dropping one subject from the control cohort. The coefficient on the treatment variable is depicted, and ordered in increasing value. The 95% confidence intervals are depicted as well. Every confidence interval does not include 0 and, hence, the main result is not sensitive to the exclusion of any other subject.

Figure 16: Average Monthly Deposit for Treatment 4-1, No Guarantee Subjects

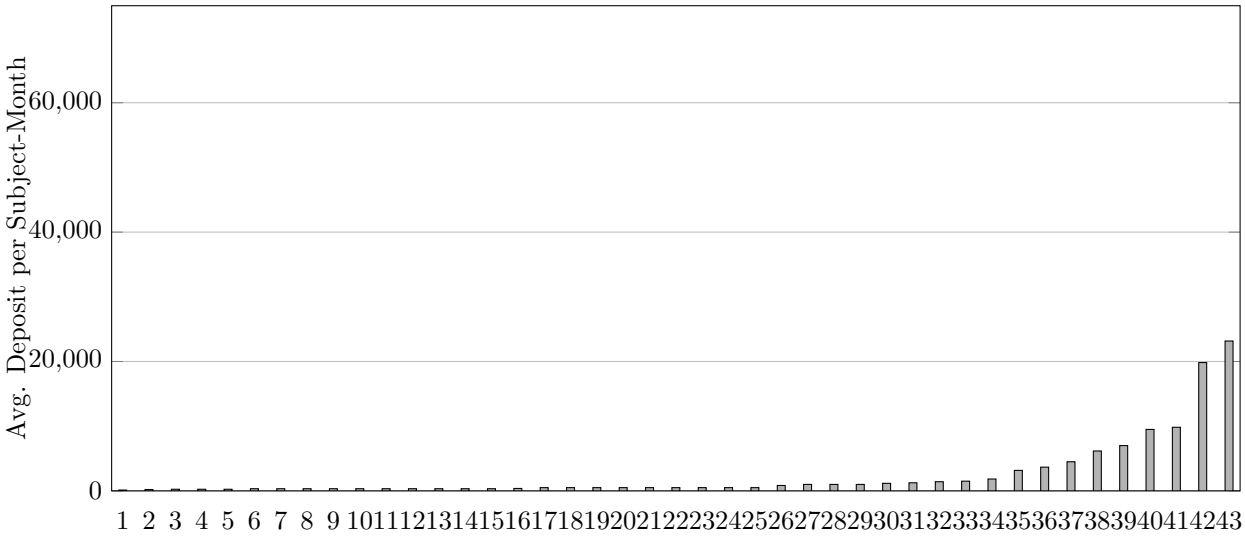
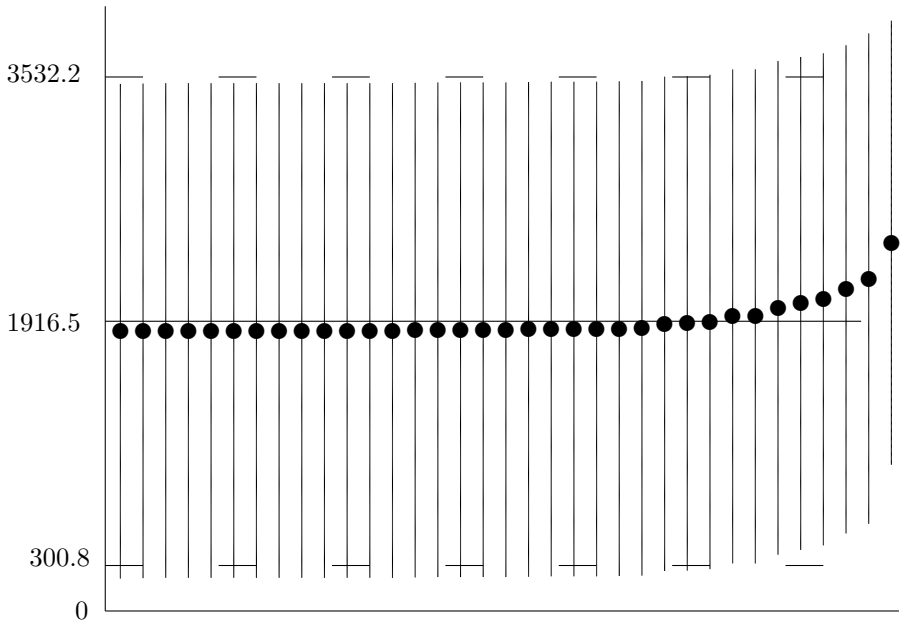


Figure 17: Treatment Effect Dropping One Subject



Each circle provides the estimated coefficient on *Treatment 4-1, Guarantee* from column 2 of Table 2, but dropping one subject from the Control. The estimates are ordered from the lowest estimated coefficient to the greatest. The vertical bars depict the 95% confidence intervals. The solid line is the estimated coefficient without dropping any other subjects (the one presented in column 2 of Table 2). The dashed lines are the 95% confidence interval.

10.4 Effect of Past Victories on Account Activity

The following table identifies how victories in previous months influence deposits.

Table 11: Effect of Past Victories

Dep. Var.= Deposit	(1)	(2)	(3)	(4)
Won Last Month	8795.51 **	8280.31 ***		
	(1710.56)	(1712.86)		
Won In the Past			3624.18 ***	2960.32 ***
			(993.71)	(1026.92)
Treatment Indicators Included?	No	Yes	No	Yes
R^2	0.030	0.047	0.015	0.030
AIC	18165	18156	18178	18171
Dep. Var.= Active	(1)	(2)	(3)	(4)
Won Last Month	0.3174 ***	0.2965 ***		
	(0.0919)	(0.0924)		
Won In the Past			0.1261 **	0.0990 *
			(0.0158)	(0.0552)
Treatment Indicators Included?	No	Yes	No	Yes
R^2	0.014	0.022	0.006	0.014
AIC	1058	1056	1064	1063

The variable *Won Last Month* equals one if the subject won a prize in the previous month. The variable *Won In the Past* is equal to one if the subject won a prize in any previous month. Columns (1) and (3) only include the indicator variable and a constant in a linear regression. Columns (2) and (4) also include the three treatment variables (with the control as the omitted, reference group).