

# Using Lotteries to Encourage Saving: Experimental Evidence from Kenya

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## Abstract

In this study, we evaluate the provision of lottery-linked deposit accounts (LLDA) – savings products that incorporate stochastic returns to deposits – against a standard, interest-bearing deposit account. We provided a mobile savings product to 311 informal residents in Nairobi, Kenya and observe account activity over a 60-day period. We found that participants with LLDA made 42% more deposits on average over the project period than participants receiving a matched incentive. This increase in account activity is due to participants making more deposits per day in order to enter the lottery. We do not observe any effects due to the lottery incentive on amount deposited over the project period. We show that when presented with potential winnings from previous days, participants with LLDA increased self-reported gambling activity by 15%. Our results suggest that the LLDA is a promising tool to improve savings among the poor and that product design has considerable implications for gambling behavior.

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## I. Introduction

Savings is one of the most important avenues toward economic development; it provides a means to smooth disastrous shocks and the ability to make profitable investments. There exists, however, a host of obstacles that prevent poor households from accruing savings to their advantage. In the absence of effective and affordable savings technologies, savings are susceptible to extraction by theft or by social claimants (Banerjee and Duflo 2007; Schaner 2011). Poor households often resort to methods of saving that can be costly and have limited functionality (Collins et al. 2009; Karlan, Ratan, and Zinman 2014). On the demand side, knowledge gaps, mistrust of financial institutions, and behavioral biases remain barriers to optimal saving. Product designs specifically targeting behavioral “frictions” have been shown to be extremely cost-effective compared to even direct savings subsidies. Track-keeping objects (Akbas et al. 2016), SMS reminders (Karlan et al. 2010), and default contributions (Thaler and Benartzi 2004; Chetty and Friedman 2014) address undersaving due to limited attention. Binding commitment devices, in the form of account restrictions (Ashraf, Karlan, and Yin 2006) or using social pressure (Dupas and Robinson 2013), can induce savings among individuals lacking self-control.

Our study focuses on a product that incorporates lottery-like payoffs to traditional savings accounts. Unlike standard interest-bearing savings accounts, lottery-linked deposit accounts (LLDAs) or prize-linked savings (PLS) provide stochastic returns as a function of deposits. Savers in this type of account forego interest for a probabilistic payoff and, common among most LLDAs, face no risk of losing their principal. This unique feature makes the product an attractive tool for promoting financial inclusion. Lottery expenditures demonstrate an inverse relationship with socioeconomic status, which suggests that poor households may be especially responsive to lottery-like incentive structures (Barnes et al. 2011). Furthermore, there is some evidence that usage of lottery-linked accounts displaces costly gambling behavior (Cookson 2016). LLDAs have been in use since at least the 17<sup>th</sup> century and presently exist in various forms around the globe (Murphy 2005; Kearney et al. 2010). NS&I Premium Bonds in the U.K., First National Bank’s “A-Million-A-Month” Account in South Africa and Individual Development Accounts (IDAs) in the United States are some prominent examples of this type of savings product.

Literature on the demand for LLDAs is extensive, but experimental evidence as to its effect on savings behavior is limited. Two recent experimental studies provide evidence of a positive effect of stochastic returns on saving for the future. Atalay et al. (2014) conducted an online portfolio-choice experiment that resulted in participants saving an additional 12 percentage points

more with lottery-linked and regular savings than with regular savings alone. Notably, participants who saw an increase in total savings shifted away from lottery expenditures and consumption rather than from regular savings. Filiz-Ozbay et al. (2015) found participants are more likely to delay payments with lottery-like returns compared to guaranteed interest of equivalent expected value. This finding suggests that lottery-linked schemes can be designed to be revenue neutral in expectation for account providers while still promoting savings. Outside the laboratory, evidence surrounding LLDAs is more limited and diverges somewhat from those findings. Loibl et al. (2016) conducted a randomized evaluation of IDAs in the U.S. that incorporated a lottery-based savings match. That study found no significant effect of the program relative to guaranteed matching, even when it was bundled with reminder calls and frequent deposit deadlines. They attribute the result to liquidity constraints among their sample, which potentially precluded the benefits of behavioral interventions.

The present study is a lab and field experiment testing the effects of lottery-linked savings on savings behavior. We provided a mobile savings product to 311 informal residents in Nairobi, Kenya and observed account activity over a 60-day period. We minimized barriers to saving by utilizing Safaricom’s *Sambaza* mobile savings technology. This platform allowed us to collect detailed data on participant transactions and to examine savings behavior over time. Roughly one-third of our sample was randomly assigned a savings account which provided a fixed 5% match daily to deposits made that day. A second group was assigned an account that yielded stochastic returns equal in expectation to the 5% match through a lottery conducted on a daily basis. For each day a participant makes a non-zero deposit, they received a lottery ticket and an opportunity to win a prize instead of the fixed match. We compared the match and lottery groups to determine how LLDAs impact savings behavior. A third group received the same lottery-linked account with the additional feature that participants receive a lottery ticket every day regardless of saving but could not claim the prize until after making a deposit. The key feature of this “regret” treatment is that participants observe the lottery results and potential prize at the end of each day. We tested this treatment against the lottery treatment to determine whether experienced regret from being unable to claim a prize affects decisions to save.

We found that participants using LLDAs with the regret framing made 42% more deposits on average over the project period than participants receiving the matched incentive. Moreover, this increase in account activity is due to participants making more deposits per day in order to enter the lottery. There were no significant differences in effects on saving between the regular LLDA

and the LLDA with regret framing. Interestingly, we find no effect of LLDAs on total amount saved or on the size of each deposit. Participants made smaller, more frequent deposits compared to the control group. We find no evidence of the LLDA displacing savings from other sources. On gambling behavior, we find that 27% participants in the regret framing self-report higher gambling activity compared to 12% in the control group.

This study contributes to the literature as one of the first randomized evaluations examining the impact of LLDAs on saving behavior. Moreover, the study’s unique experimental design allows us to identify dynamic effects – participants make more frequent deposits to their accounts when given lottery-based returns. This result suggests that a non-pecuniary appeal of gambling, unrelated to prize amounts, may be enough to induce a change in savings behavior. LLDAs may thus improve utilization among existing account holders and be able to attract new savers to open formal savings accounts. Frequent deposits may also have long-term benefits by encouraging the formation of a savings habit (Alessie and Teppa 2009). From a policy perspective, LLDAs may not be revenue neutral compared to matching if financial institutions incur greater transaction costs as a result of more frequent deposits.

Our study also shows that participants with LLDAs with regret framing increased self-reported gambling activity relative to the control group. If LLDAs contribute to problem gambling, the program is potentially welfare-decreasing for households susceptible to problem gambling. Cookson (2016) reports a 15% reduction of casino gambling in Nebraska as a result of enrollment in an LLDA bundled with an anti-gambling advertising campaign. The difference from our results suggests that additional program components could diminish effects on outside gambling. Overall, we document several advantages of LLDAs over fixed-incentive schemes when it comes to promoting financial inclusion and show that product design is crucial in moderating adverse effects on gambling behavior.

The remainder of the paper is structured as follows. Section II describes our experimental design, Section III outlines our estimation strategy, Section IV discusses our main results, and Section V concludes.

## II. Experimental Design

### A. Context

This study was conducted in conjunction with the Busara Center for Behavioral Economics in Nairobi with 311 participants residing in Kibera, one of Kenya’s largest urban slums. Nearly 60% of our sample is female with a me-

dian age of 28 years. Less than half of the participants in our sample reported that they are employed with only 5% reported receiving a regular income. The median PPP-adjusted monthly income among those employed is USD 77.<sup>1</sup> Approximately 55% of our sample saves regularly with a majority utilizing ROSCAs. Average monthly savings among these individuals amount to USD 23.

Gaming has been legal in Kenya since 1966, but the ubiquity of mobile phone usage has allowed the recent popularity of mobile sports betting. Sport-Pesa, one of the most popular mobile gambling services, reports over 800,000 registered users as of 2015 (Kemibaro 2015). In our sample, 24% of participants at baseline report that they have some problem with gambling. 11% of participants report that they gamble at a casino, bet money at racetracks or sporting events, played the sweepstakes, or played cards for money daily or more frequently in the last 12 months.

### *B. Data collection*

Participants were first invited to the lab at the Busara Center where they completed a computerized questionnaire and behavioral tasks. The following outlines the schedule of tasks during the lab portion of the study:

1. Coin toss task (Eckel and Grossman 2002)<sup>2</sup>
2. Titration task for temporal discounting (Cornsweet 1962)
3. Willingness-to-pay to play a lottery
4. Candian Problem Gambling Index (Ferris and Wynne 2001)
5. Internal locus of control (Rotter 1966)
6. Demographics questionnaire

Lab sessions took place over five weeks in May and June of 2014. We refer to this period before beginning the savings program as the baseline. Following the lab session, participants were randomly assigned to one of three incentive schemes – one fixed match and two lottery-based matches – to be implemented in the savings program. Each participant was given KSH 20 airtime credit and

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<sup>1</sup>We use a USD-KSH PPP conversion factor of 38.84 for 2013.

<sup>2</sup>This elicitation method produces interval estimates of the CRRA parameter,  $\rho$ . We take the midpoint of the upper and lower intervals as point estimates. For participants with  $\rho \geq 3.46$  and  $\rho \leq 0$ , we use these bounds as point estimates.

asked to practice saving using *Sambaza*. Participants were then sent home with business-card sized handouts which described their savings program. We provided participants simple instructions for saving and listed the number to our project phone. This was the number through which the savings program operated that also functioned as a help line for participants.

Participants enrolled in the savings program for two consecutive periods of 30 days starting from the day of a participant’s lab session. On a participant’s 30th day, a field officer called them and asked if they wished to withdraw any amount of their balance. Participants who requested withdrawals were sent M-Pesa transfers equal to their request plus the M-Pesa withdrawal fee. These withdrawals were recorded in our system’s ledger.

Following this, participants moved on to their second 30-day savings period. Participants were called and notified a few days before the end of their second 30-day period that the program would be ending soon. After receiving the end-of-day message on their 60th day, participants were unenrolled from the program and were no longer allowed to save. Field officers called participants to confirm final balances and sent M-Pesa transfers equal to total balance plus withdrawal fee shortly after. All participants had completed the program by August 2014. In September 2014, we called participants and conducted an endline survey that included questions on outside savings, gambling activity, and program feedback. We obtained endline surveys for all but 27 of the 311 participants.<sup>3</sup>

### C. *Mobile savings program*

We implemented our mobile-phone based savings program over Safaricom’s *Sambaza* airtime sharing service. Using *Sambaza*, Safaricom users can send airtime to each other free of charge. Participants saved into our program by sending airtime to a designated project phone that held the airtime in an account for each user.

Participants received two SMS messages every morning after the first morning of the project period. The first message was an end-of-day message that reported how much the participant saved the previous day, how much the participant earned through a matching contribution or winnings, and their total balance. An hour later, participants received a beginning-of-day message encouraging them to save that day. Participants were allowed to send in savings at any time but any savings sent in after the end-of-day message would be

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<sup>3</sup>Table 1 reports the final sample size and attrition between baseline and endline. Table 2 reports no differential attrition dependent of the treatment assignments.

counted towards the next day’s total. We used a custom-developed administrative system to manage the savings program. This system logged airtime sent to our project phone, maintained an internal ledger of balances, sent automated SMS confirmations after every transaction, and conducted the daily lottery game.

Participants were enrolled in the savings program for a total of 60 days, split into consecutive 30-day periods. After the first 30 days, participants were allowed to withdraw any amount of their savings up to the total balance. Outside of this opportunity, regular withdrawals were not allowed.

At the end of our experiment, we returned participants’ savings and accumulated incentives or winnings via an M-Pesa transfer. This M-Pesa transfer included the extra withdrawal fees needed to cash out an amount equal to the participant’s full account balance. Therefore, participants paid no explicit fees to participate in our program.

#### *D. Treatment*

Participants enrolled in the savings program were randomized into one of three different incentive schemes. Tables 3 report summary statistics and balance across treatment groups for several baseline characteristics. We do not find a correlation between treatment assignment and these observable characteristics.

1. **Matched incentive savings:** Participants in the matched group participated in a savings program where they earned a 5% matching contribution on any amount that they saved in a particular day. We take this group as our control group to be tested against the lottery incentives.
2. **Lottery-linked savings:** After saving a non-zero amount, participants earned a lottery ticket transmitted via SMS, which could win a cash prize in proportion to the amount they saved. A lottery ticket was a random sequence of four numbers between 1 and 9, inclusive. Each day, our administrative system randomly generated a winning sequence of four numbers. Prizes were awarded according to how well a participant’s lottery numbers matched the winning numbers. If the first or second numbers matched, a 10% match of savings was awarded. If *both* the first and second numbers matched, a 100% match of savings was awarded. Finally if all numbers matched, a prize of 200 times the daily savings was awarded. The earnings on this lottery ticket were equal in expectation to the 5% match earned in the control group. Our system processed the matching of lottery numbers and entered winnings into the internal

ledger. Participants could only earn one lottery ticket per day. We henceforth refer to this group as the LOTTERY group.

3. **Lottery-linked savings with regret:** This scheme is similar to the lottery treatment but participants in this third group were sent lottery tickets in their beginning-of-day text message. These tickets only became redeemable, however, after participants had saved a non-zero amount that day. Participants with winning lottery tickets who did not save that day did not win money from their lottery ticket. However, they were informed whether they would have won in their end-of-day message the next morning. We henceforth refer to this group as the REGRET group.

### III. Empirical Strategy<sup>4</sup>

#### A. Treatment effect

We use the following econometric specification for basic identification of the treatment effect.

$$Y_{i,t=1} = \beta_0 + \beta_1 \text{LOTTERY}_i + \beta_2 \text{REGRET}_i + \varepsilon_i \quad (1)$$

$Y_{i,t=1}$  refers to the outcome variables for individual  $i$  at endline,  $\text{LOTTERY}_i$  indicates assignment to the LOTTERY group, and  $\text{REGRET}_i$  indicates assignment to the lottery with regret framing group. The omitted group is the matched group.  $\beta_1$  and  $\beta_2$  respectively identify the treatment effects of the lottery and lottery with regret framing relative to the matched group. We will use an  $F$ -test to test the joint effect of both treatments to the comparison group and to compare the effects against one another.

To improve precision, we will also apply covariate adjustment with baseline indicators<sup>5</sup> in  $\mathbf{X}_i$ . We obtain the covariate-adjusted treatment effect estimate by estimating Equation 1 including the demeaned vector  $\dot{\mathbf{X}}_i = \mathbf{X}_i - \bar{\mathbf{X}}_i$  as an additive term and as an interaction with the treatment indicator.

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<sup>4</sup>We registered a pre-analysis plan documenting the experimental design, identification strategy, and outcomes of interest available at <https://www.socialsciscience.org/trials/893>. Project materials, including de-identified participant data, is available at <https://github.com/princetonbpl/akiba-lottery-pub>.

<sup>5</sup>We include as control variables 1. Participant is female, 2. Participant is younger than 30 years old, 3. Participant completed primary school, 4. Participant is married, 5. Participant has at least one child dependant, 6. Participant uses a savings account, and 7. Above median CPGI score



$$Y_{i,t=1} = \beta_0 + \beta_1 \text{LOTTERY}_i + \beta_2 \text{REGRET}_i + \dot{\mathbf{X}}_i' \gamma_0 + \text{LOTTERY}_i \dot{\mathbf{X}}_i' \gamma_1 + \text{REGRET}_i \dot{\mathbf{X}}_i' \gamma_2 + \varepsilon_i \quad (2)$$

The set of indicators partitions our sample so that our estimate remains unbiased for the average treatment effect (Lin 2013). We will report treatment effect estimates with and without covariate adjustment.

We might expect that the errors of each outcome variable are correlated. Instead of estimating these equations separately, we can estimate the system of seemingly unrelated regressions (SUR) to improve the precision of the coefficient estimates (Zellner 1962). SUR estimation is equivalent to OLS when the error terms are in fact uncorrelated between regressions or when each equation contains the same set of regressors. We perform joint estimation over outcome families for Equation 1 and Equation 2 separately.

Given that our survey instrument included several items related to a single behavior or dimension, we calculate sharpened  $q$ -values within each outcome family following (?) to control the false discovery rate (FDR). Rather than specifying a single  $q$ , we report the minimum  $q$ -value at which each hypothesis is rejected, following (Anderson 2008). We will report both standard  $p$ -values and minimum  $q$ -values in our analysis.

#### B. Minimum detectable effects

To determine whether our null findings identify the absence of a true effect or signify a lack of statistical power, we report the minimum detectable effect size (MDE) for each outcome.

$$MDE_{\hat{\beta}} = (t_{1-\kappa} + t_{\alpha/2}) \times SE(\hat{\beta}) \quad (3)$$

This metric is the smallest effect that would have been detectable given our current sample size. Commonly used in experimental design, we calculate MDEs *ex post facto* with  $\alpha = 0.05$  and 0.80 power for each pairwise comparison of our treatments and for comparisons of the treatment effect across dimensions of heterogeneity.

## IV. Results

#### A. LLDAs increase deposits made without increasing savings

This section presents our main results on savings behavior, documented in Table 4. We find that participants in the REGRET group made between 5-6 more

deposit transactions ( $\hat{\beta} = 5.71, p < 0.05$ ) over the entire project period compared to those receiving the fixed match. This effect is large, amounting to a 40% increase over the average number of deposits in the control group. These results are further robust to the inclusion of control variables and FWER adjustment. We do not find strong evidence of an effect of the lottery incentive *without* the regret component against either REGRET or control group. Nevertheless, point estimates suggest that the lottery alone could increase deposits by as much as 33% ( $\hat{\beta} = 4.59, p < 0.10$ ) compared to the control. Table 7 displays the MDE for each outcome and shows that the present experimental design is powered to detect effects on savings larger than what we estimate for the LOTTERY group. A higher powered study might be able to pick up more subtle effects of the lottery incentive. Panel A of Figure 1 traces the cumulative path of deposits made over the savings period. Average deposits for the LOTTERY and REGRET groups are greater than for the control group for all periods, and grows at a higher rate. We are able to statistically distinguish total values at the end of the 60-day period but the figure suggests the existence of a larger effect when examining longer time periods.

While effects on number of deposits are acute, we find no effect of either treatment on total amount deposited over the project period. Panel B of Figure 1 illustrates the cumulative deposit amounts, averaged by treatment group, over the 60-day period. We cannot distinguish total deposit amounts between any of the three incentive schemes. So while participants are making more deposits, the amount of each deposit is smaller on average than in the control group.

Our results are largely consistent with findings from previous randomized evaluations of lottery-based incentives on savings. Loibl et al. (2016), examining features of the Individual Development Account program in the U.S., find no effect of LLDAs over a fixed match of equal expected value. The study posits that severe liquidity constraints in the sample rendered behavioral interventions ineffective. With a median monthly income of USD 77, households in our study may be similarly cash-strapped and unable to allocate a greater portion of their budget to savings. Lottery-based incentives applied to other domains – including labor supply (Brune 2015) and health-related behaviors (Kimmel et al. 2012; Bjorkman Nyqvist et al. 2015) – are found to have significant effects.

The non-effect on savings we observe are at odds with the experimental literature. Atalay et al. (2014) conducted an online portfolio-choice experiment in the U.S. that resulted in subjects saving an additional 12 percentage points more with lottery-linked and regular savings than with regular savings alone. In an experiment with undergraduates, Filiz-Ozbay et al. (2015) found that

subjects are willing to accept a lower rate of return to delay a payment when the return is stochastic than when it is deterministic. A possible explanation is that effects depend on the rate of return offered by the deterministic match. In a companion experiment studying savings decision among 147 MBA students, we find that lottery-based returns increase savings for interest rates between 1-3%. These differences vanish when rates are increased to 5%, the rate offered in the present study. Instead of holding returns constant, Filiz-Ozbay et al. (2015) takes rates of return as the outcome with the subjects' choice set binary between consuming or saving the entire budget. Our null result on savings may be due to a ceiling effect not observed in previous experimental designs.

The pattern of our results suggest that our participants receive some benefit simply by playing the lottery. An increase in the number of deposits in the treatment group is expected if merely making a deposit on a certain day qualifies participants to play the lottery for that day. When we examine as an outcome the number of days saved, we find that participants indeed save almost 5 days more ( $\hat{\beta} = 4.94, p < 0.05$ ) – and thus play the lottery 5 more times – than the control group. Unsurprisingly, participants are not making more deposits *within* days since this does not affect lottery eligibility. Thus, the overall effect of the LLDA is to encourage savers to make more deposits in order to “play” without a corresponding increase in amount saved.

While we do not detect significant differences in deposits between the LOTTERY and REGRET groups, our estimates point to the importance of regret aversion in supplementing the choice to play by saving. Regret aversion will motivate making deposits if our participants anticipated feeling “loser regret” from information that they could have won had they played Filiz-Ozbay et al. (2015). This conforms to suggestive evidence from a cross-sectional study of Dutch lotteries that anticipated regret from winning but not playing relates to future decisions to enter the lottery (Zeelenberg and Pieters 2004).

#### *B. LLDAs encourage informal saving*

#### *C. LLDAs increase outside gambling behavior*

Our second research question asks whether LLDAAs act as complements or substitutes to existing gambling activity. At endline, we ask participants whether participants gamble more than they usually do after the savings program. As reported in Table 6, we find that participants in the REGRET group self-report higher gambling behavior after enrollment in the savings program. On average, treated participants are 15 percentage points ( $p < 0.05$ ) more likely to report gambling than the control group. We find no similar effects for partici-

pants in the simple LOTTERY group. While our measure for gambling activity is susceptible to experimenter demand, this finding provides some evidence of a complementary relationship between LLDAs and external gambling.

Cookson (2016) offered individuals in Nebraska access to an LLDA and observed cash withdrawals at casinos as a measure of gambling behavior. They find reductions in transactions between 7-15% accredit the effect to attribute-based substitution of casino gambling with the LLDA. One important difference in the savings program from the present study is the bundling of the account with an anti-gambling campaign. Such a feature may have counteracted external gambling associated with the LLDA and could explain the difference in our findings.

## V. Conclusion

By taking advantage of savers' preference for gambling, stochastic incentive schemes like LLDAs represent a promising policy tool to overcome behavioral barriers to saving. This study conducts a randomized evaluation of a LLDA with informal residents in Nairobi, Kenya. Utilizing a mobile savings platform, we randomly assign respondents to a savings account with a fixed match to deposits, a lottery incentive, and a lottery incentive with feedback on *ex post* potential lottery winnings. We set the fixed match equivalent in expectation to the lottery prize so that comparing the two groups identifies the effect of stochastic incentives compared to deterministic incentives holding amount constant. After observing account transactions over a 60-day savings period, we find that participants in the REGRET group made between 5-6 more deposit transactions than the fixed match group without a corresponding increase in amount saved. These results suggest that savers are making more deposits in order to "play" and experience a non-pecuniary benefit from the lottery. We further find that participants in the REGRET group are more likely to report increased gambling after the end of the savings program.

If LLDAs increase deposits but are ineffective at increasing a key outcome like savings, are they still useful from a policy perspective? If playing the lottery is appealing to potential savers, LLDAs may be able to attract new savers to open accounts. LLDAs can also improve utilization among existing account holders. Frequent deposits may have long-term benefits by encouraging the formation of a savings habit (Alessie and Teppa 2009). Compared to a fixed match, lottery incentives may not be revenue neutral if financial institutions incur greater transaction costs as a result of more frequent deposits. If LLDAs contribute to problem gambling, the program is potentially welfare-decreasing for poor households already susceptible to costly gambling

behavior. Additional program components, like an anti-gambling campaign, could diminish adverse effects on outside gambling. Overall, we document important differences between LLDAs and fixed-incentive schemes when it comes to encouraging savings and show that product design is crucial in determining welfare implications.

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## Appendix

Table 1: Treatment group by participation at endline

	Participation in endline		
	Attrited	Completed	Total
Interest	11	94	105
Lottery	8	95	103
Regret	8	95	103
Total	27	284	311

*Notes:* This table reports a cross-tabulation between treatment assignment and selection into the endline survey.



Table 2: Attrition by treatment group

	Unobserved at endline
Lottery	-0.03 (0.04)
Regret	-0.03 (0.04)
Constant	0.10*** (0.03)
Observations	311
Adjusted $R^2$	-0.004
Difference p-value	1.00
Joint p-value	0.75

*Notes:* This table reports a regression of selection on each of the treatment arms. Standard errors are in parentheses. \* denotes significance at 10 pct., \*\* at 5 pct., and \*\*\* at 1 pct. level.

Table 3: Summary statistics by treatment group

	Mean (SD, N)			Difference <i>p</i> -value		
	Control	Lottery	Regret	Lottery - Control	Regret - Control	Lottery - Regret
Female	0.52 (0.50) 105	0.59 (0.49) 103	0.62 (0.49) 103	0.32	0.16	0.67
Age	30.75 (9.83) 102	31.53 (9.98) 100	31.48 (9.27) 101	0.58	0.59	0.97
Completed std. 8	0.99 (0.10) 105	0.97 (0.17) 103	0.97 (0.17) 103	0.31	0.31	1.00
Married/co-habiting	0.42 (0.50) 104	0.52 (0.50) 101	0.51 (0.50) 102	0.15	0.21	0.83
No. of children	1.75 (1.70) 105	1.98 (1.71) 103	1.99 (1.84) 103	0.34	0.33	0.97
Constant relative risk aversion	1.16 (1.27) 105	1.25 (1.38) 103	1.13 (1.24) 103	0.64	0.85	0.52
Locus of control	69.81 (10.78) 105	70.29 (9.41) 103	68.98 (10.30) 103	0.73	0.57	0.34
Monthly income	112.05 (137.13) 105	108.37 (117.43) 103	111.46 (104.85) 103	0.84	0.97	0.84
Receives regular income	0.06 (0.24) 52	0.11 (0.31) 56	0.17 (0.38) 48	0.36	0.08*	0.38
Employed	0.50 (0.50) 105	0.54 (0.50) 103	0.47 (0.50) 103	0.49	0.68	0.27
Self-employed	0.24 (0.43) 78	0.21 (0.41) 72	0.20 (0.40) 81	0.61	0.49	0.87
No. of dependants	3.18 (2.58) 105	3.49 (2.60) 103	3.27 (2.32) 103	0.40	0.79	0.53
Subject is a dependant	0.23 (0.42) 105	0.28 (0.45) 103	0.25 (0.44) 103	0.38	0.69	0.64
Currently saves	0.56 (0.50) 105	0.61 (0.49) 103	0.47 (0.50) 103	0.47	0.17	0.04**
Total savings last mo.	58.82 (106.26) 105	41.01 (59.72) 103	51.79 (72.56) 103	0.14	0.58	0.25
Currently saves with ROSCA	0.58 (0.50) 105	0.57 (0.50) 103	0.66 (0.48) 103	0.91	0.24	0.20
ROSCA savings last mo.	13.83 (23.24) 105	15.46 (28.42) 103	15.92 (23.41) 103	0.65	0.52	0.90
M-Pesa savings last mo.	8.73 (30.53) 105	17.24 (87.04) 103	5.48 (20.51) 103	0.35	0.37	0.18

Notes: The first three columns report means of each row variable for each treatment group. SD are in parentheses with sample size. The last three columns report the *p*-value for a difference of means *t*-test between each group. \* denotes significance at 10 pct., \*\* at 5 pct., and \*\*\* at 1 pct. level.

Table 4: Treatment effects – Mobile savings by respondent

	No controls			With controls			Sample	
	(1) Lottery	(2) Regret	(3) Regret - Lottery	(4) Lottery	(5) Regret	(6) Regret - Lottery	(7) Control Mean (SD)	(8) Obs.
Total no. of deposits	4.59* (2.52) [0.13]	5.71** (2.45) [0.03]**	1.13 (2.84) [1.00]	4.53* (2.64) [0.16]	4.76** (2.42) [0.06]*	0.23 (2.86) [1.00]	13.66 (15.08)	311
No. of days saved	3.93* (2.05) [0.13]	4.94** (2.08) [0.03]**	1.01 (2.32) [1.00]	3.56* (2.06) [0.16]	4.19** (2.05) [0.06]*	0.63 (2.26) [1.00]	11.78 (12.93)	311
Daily avg. no. of deposits	0.08* (0.04) [0.13]	0.10** (0.04) [0.03]**	0.02 (0.05) [1.00]	0.08* (0.04) [0.16]	0.08** (0.04) [0.06]*	0.00 (0.05) [1.00]	0.23 (0.25)	311
Total deposit amt.	-0.79 (3.34) [0.48]	-1.60 (2.91) [0.13]	-0.81 (2.88) [1.00]	-0.32 (3.15) [0.58]	-1.46 (2.73) [0.13]	-1.14 (2.86) [1.00]	14.87 (24.48)	311
Total withdrawal amt.	0.53 (0.94) [0.40]	1.63** (0.74) [0.03]**	1.10 (1.02) [1.00]	0.31 (0.85) [0.55]	1.62** (0.77) [0.06]*	1.31 (0.94) [1.00]	1.07 (4.53)	311

Notes: Columns 1 - 2 report OLS estimates of the treatment effect. Columns 4 - 5 reports the estimates controlling for baseline covariates. Columns 3 and 6 report the  $p$ -values for tests of the equality of the two treatment effects. Standard errors are in parentheses and FDR-corrected minimum  $q$ -values are in brackets. Observations are at the individual level. \* denotes significance at 10 pct., \*\* at 5 pct., and \*\*\* at 1 pct. level. Stars on the coefficient estimates reflect unadjusted  $p$ -values.

Table 5: Treatment effects – Savings outside the study

	No controls			With controls			Sample	
	(1) Lottery	(2) Regret	(3) Regret - Lottery	(4) Lottery	(5) Regret	(6) Regret - Lottery	(7) Control Mean (SD)	(8) Obs.
Total savings last mo.	18.45 (25.16) [1.00]	-17.87 (14.64) [0.26]	-36.32 (24.06) [0.24]	16.75 (23.25) [1.00]	-12.44 (14.86) [0.43]	-29.19 (22.10) [0.38]	80.31 (112.74)	284
M-Pesa savings last mo.	-5.42 (6.34) [1.00]	-6.71 (5.49) [0.26]	-1.29 (5.30) [0.67]	-5.47 (6.06) [1.00]	-6.19 (5.38) [0.33]	-0.73 (5.27) [0.80]	20.42 (44.67)	284
ROSCA savings last mo.	1.48 (6.76) [1.00]	7.37 (6.79) [0.26]	5.89 (7.33) [0.39]	2.84 (6.26) [1.00]	7.85 (6.35) [0.33]	5.01 (6.85) [0.59]	22.24 (42.18)	283
Currently saves with ROSCA	-0.02 (0.07) [1.00]	0.14** (0.07) [0.21]	0.16** (0.07) [0.10]	-0.01 (0.07) [1.00]	0.14** (0.06) [0.13]	0.15** (0.07) [0.13]	0.54 (0.50)	284

Notes: Columns 1 - 2 report OLS estimates of the treatment effect. Columns 4 - 5 reports the estimates controlling for baseline covariates. Columns 3 and 6 report the  $p$ -values for tests of the equality of the two treatment effects. Standard errors are in parentheses and FDR-corrected minimum  $q$ -values are in brackets. Observations are at the individual level. \* denotes significance at 10 pct., \*\* at 5 pct., and \*\*\* at 1 pct. level. Stars on the coefficient estimates reflect unadjusted  $p$ -values.

Table 6: Treatment effects – Gambling behavior outside the study

	No controls			With controls			Sample	
	(1) Lottery	(2) Regret	(3) Regret - Lottery	(4) Lottery	(5) Regret	(6) Regret - Lottery	(7) Control Mean (SD)	(8) Obs.
Gamble more	0.06 (0.05) [0.84]	0.15*** (0.06) [0.03]**	0.08 (0.06) [0.54]	0.06 (0.05) [1.00]	0.16*** (0.05) [0.01]***	0.10* (0.06) [0.65]	0.12 (0.32)	284
Gamble less	-0.02 (0.05) [0.84]	0.04 (0.06) [0.62]	0.06 (0.05) [0.54]	-0.02 (0.05) [1.00]	0.03 (0.06) [1.00]	0.05 (0.06) [0.65]	0.16 (0.37)	284
More tempted to gamble	0.09 (0.07) [0.84]	0.05 (0.07) [0.62]	-0.04 (0.07) [0.54]	0.05 (0.07) [1.00]	0.03 (0.07) [1.00]	-0.02 (0.07) [0.76]	0.47 (0.50)	284
Less tempted to gamble	-0.01 (0.03) [0.84]	0.03 (0.04) [0.62]	0.04 (0.04) [0.54]	-0.00 (0.03) [1.00]	0.04 (0.04) [0.82]	0.04 (0.04) [0.65]	0.06 (0.25)	284

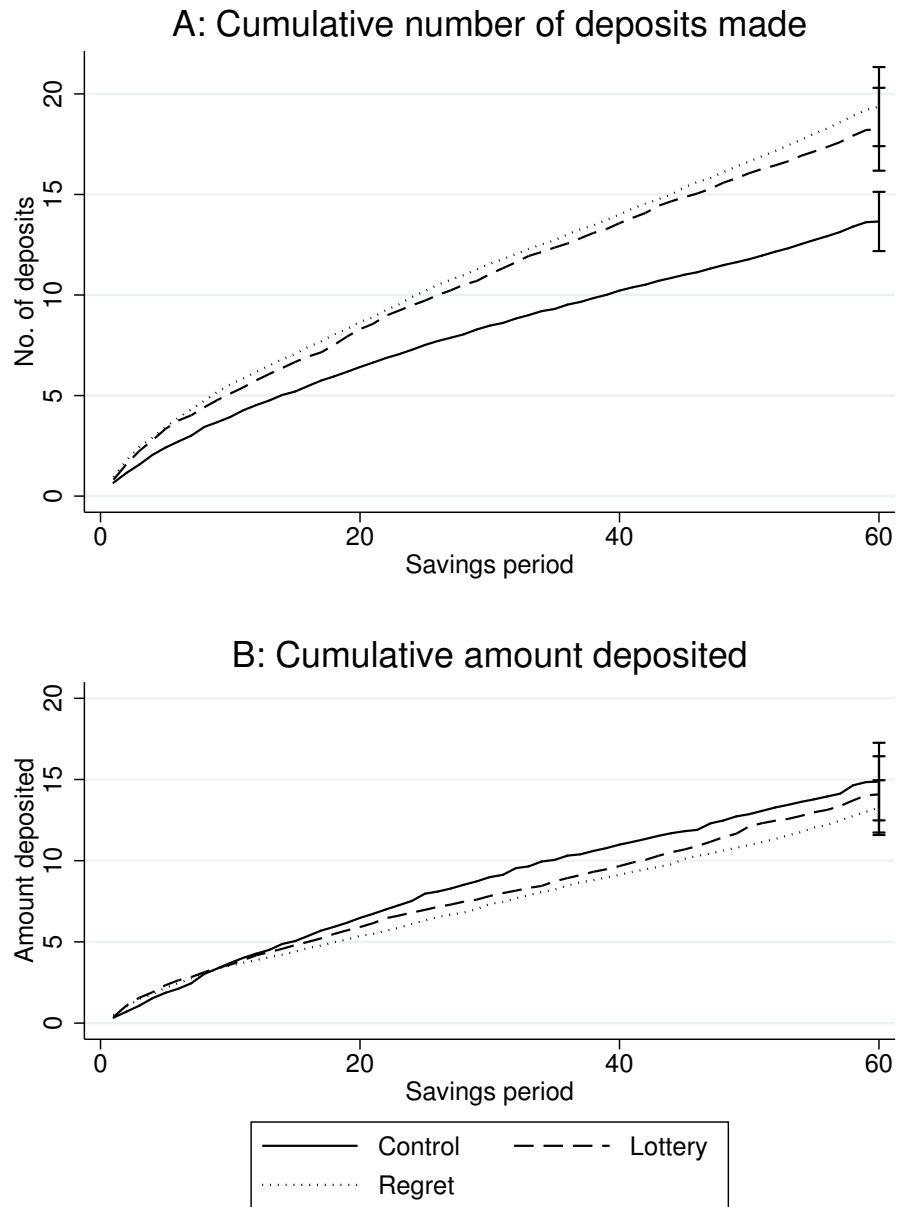
Notes: Columns 1 - 2 report OLS estimates of the treatment effect. Columns 4 - 5 reports the estimates controlling for baseline covariates. Columns 3 and 6 report the  $p$ -values for tests of the equality of the two treatment effects. Standard errors are in parentheses and FDR-corrected minimum  $q$ -values are in brackets. Observations are at the individual level. \* denotes significance at 10 pct., \*\* at 5 pct., and \*\*\* at 1 pct. level. Stars on the coefficient estimates reflect unadjusted  $p$ -values.

Table 7: Minimum detectable effect sizes

	(1) Lottery	(2) Control Mean (SD)	(3) N
Total no. of deposits	7.09	13.66 (15.08)	311
No. of days saved	5.77	11.78 (12.93)	311
Daily avg. no. of deposits	0.12	0.23 (0.25)	311
Total deposit amt.	9.38	14.87 (24.48)	311
Total withdrawal amt.	2.65	1.07 (4.53)	311
Total savings last mo.	70.69	80.31 (112.74)	284
M-Pesa savings last mo.	17.80	20.42 (44.67)	284
ROSCA savings last mo.	18.98	22.24 (42.18)	283
Currently saves with ROSCA	0.20	0.54 (0.50)	284
Gamble more	0.14	0.12 (0.32)	284

*Notes:* Column 1 reports the minimum detectable effect sizes of the lottery treatment compared to control on the row variables with  $\alpha = 0.05$  and 0.8 power. Columns 2 - 3 report the control group means and SDs and size of the analytic sample respectively.

Figure 1: Number of deposits and amount deposited over project period



*Notes:* Panel A plots the cumulative number of deposits made by the average participant over the 60-day savings period by treatment assignment. Panel B plots the cumulative amount deposited by the average participant. Error bars for the total number of deposits and total amount deposited by the end of the project are within one standard error of the mean.