Using Lotteries to Encourage Saving: Experimental Evidence from Kenya*

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Abstract

We conduct a field experiment to study the provision of prize-linked savings accounts (PLS)—savings products that incorporate stochastic returns to deposits—against standard, interest-bearing accounts. We randomly provided a mobile savings account with a fixed incentive, a PLS account, and a PLS account with feedback about winnings to 311 informal residents of Nairobi, Kenya and observe account activity over 60 days. Individuals saving with PLS with feedback made 42% more deposits on average over the project period than participants who received a fixed incentive of equal expected value. We do not observe any effects of the lottery incentive on the amount deposited with PLS or on savings with other products. Lastly, we find that use of PLS results in a 15% increase in gambling activity. We argue that the effects on deposit frequency are due to regret aversion, a hypothesis which receives support from the presence of larger treatment effects conditional on winning the lottery but being unable to claim the prize.

JEL Classification: D14, E21, G11

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I. Introduction

Saving is one of the most important avenues toward economic development; it provides a means to smooth over 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). Policies that expand access to financial services have successfully increased account ownership but have been less effective at encouraging usage (Dupas and Robinson 2013; Karlan et al. 2016). In Kenya, 27.5% of low income households own an account yet 9.9% save with a financial institution (Demirgüç-Kunt et al. 2015).

On the demand side, knowledge gaps, mistrust of financial institutions, and behavioral biases prevent the poor from saving as much as they would like. Product designs that target behavioral barriers have been shown to be extremely cost-effective, especially compared to direct subsidies.[1] 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 the application of social pressure (Dupas and Robinson 2013), can help individuals with time-inconsistent behavior follow through on saving.

This article examines the potential of prize-linked savings (PLS) accounts, a product that incorporates lottery-like payoffs to traditional savings, in improving financial inclusion. The key feature of PLS accounts is that users receive a probabilistic payoff in addition to, or in lieu of, certain interest payments. Common among PLS products is that consumers face no risk of negative returns, guaranteeing the full principal amount. PLS have been in use since at least the 18$^{\text{th}}$ century and presently exist in various forms around the globe (Murphy 2005; Kearney et al. 2010). Any kind of savings product that allows consumers to enter raffles or earn lottery tickets may be considered PLS. NS&I Premium Bonds in the U.K., the "A-Million-A-Month" Account in South Africa and Individual Development Accounts in the United States are some prominent examples of this type of savings product.

Our study analyzes how savings products might leverage preferences

[1] Karlan and Zinman (2018) estimate very low interest rate elasticities and limited impacts of easing account ownership requirements. Schaner (ming) boost short-run individual savings by USD 1.38 with rates of up to 20%.

for lotteries to influence savings behavior. Lottery expenditures as a proprtion of income is higher among poor households, which suggests that they may be especially responsive to lottery-like incentives (Brown, Kaldenberg, and Browne 1992; Kearney et al. 2010; Barnes et al. 2011). In the United States, demand for PLS is greatest among those who do not save regularly (Tufano, De Neve, and Maynard 2011). Shawn Allen Cole (2014) documents a broad demand for PLS across a demographics groups in South Africa. Absent effective savings technologies gambling may even be used to raise money for large, indivisible expenditures (Herskowitz 2016). Finally, there is evidence that usage of PLS displaces potentially costly gambling behavior (Atalay et al. 2014; Shawn Allen Cole 2014; Cookson 2016). Such findings make the product an attractive candidate for promoting financial inclusion among the poor.

We conducted a laboratory and field experiment to analyze the effects of PLS on savings behavior over time. We provided a mobile savings product to 311 informal residents in Nairobi, Kenya and observed account activity over a 60-day period. Using mobile savings allowed us to minimize transactions costs and to collect detailed data on individual transactions. Roughly one-third of our sample was randomly assigned a savings account which matched contributions at 5%. A second group was assigned an account that yielded stochastic returns equal in expectation to the 5% match through a daily lottery. For each day a participant made a deposit, they received a lottery ticket and an opportunity to win a prize instead of the certain match. We compared the match and lottery groups to determine how PLS impact savings behavior. A third group received the same lottery-linked account with the additional feature that participants received a lottery ticket and observed the lottery results every day regardless of saving. We tested this treatment aginst the basic PLS to determine whether feedback about hypothetical lottery results affected decisions to save.

We find that participants using PLS with feedback made 42% more deposits on average over the project period than participants receiving the matching incentive. This amounts to 5-6 additional days that treated participants made deposits to their savings account. There are no statistically significant differences in effects on saving between the basic PLS and PLS with feedback or between the control group and the basic PLS. We find no effect of PLS on total amount saved, a finding largely consistent with earlier experimental results. Consequently, we find no evidence of PLS displacing savings from other sources. We leverage exogenous variation in lottery results and identify regret aversion as a mechanism driving the increase in deposit frequency. In fact, we observe a 14 percentage point increase in the probability of using informal savings after having saved with PLS with feedback, especially among the unemployed. On gambling behavior, we find that participants who used PLS with feedback reported higher gambling activity 15 percentage points more than the control group.

This study contributes to the literature as one of the first randomized evaluations of PLS on saving. Moreover, the study's particular experimental design allows us to identify dynamic effects: participants make more frequent deposits to their accounts with lottery-like incentives. This result suggests that a non-pecuniary appeal of gambling, unrelated to prize amounts, may be enough to induce a change in savings behavior. PLS 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, PLS 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 PLS with feedback increased self-reported gambling activity relative to the control group. If PLS 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 PLS 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 PLS 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 presents a brief review of related literature, Section III describes our experimental design, Section IV outlines our estimation strategy, Section V discusses our main results, and Section VI concludes.

II. Related Literature

Literature on the demand for PLS is extensive, but evidence of a causal 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 PLS 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. Lottery-based incentives applied in 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.

III. Experimental Design

III.A. Context and Sample Frame

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. We drew a random sample of participants using SMS and phone calls from the Busara Center's active pool of over 11,000 Nairobi residents. Nearly 60% of our sample is female with a median 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.[2] Approximately 55% of our sample saves regularly with a majority of savers utilizing rotating savings and credit associations (ROSCA, a type of informal group savings. Average monthly savings among these individuals amount to USD 23. A small fraction of the sample save with M-Shwari, a mobile banking service offering a basic paperless account and access to credit. Transactions are made with M-Pesa, an SMS-based money system made accessible by the ubiquity of mobile phones in Kenya.[3]

The surge of mobile phone usage in Kenya has allowed the recent popularity of mobile sports betting. SportPesa, 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.

[2]This study was conducted with Kenyan shillings (KES). We report USD values calculated at purchasing power parity using a conversion factor for private consumption of 38.15 in 2013. The price level ratio of PPP conversion factor (GDP) to KES market exchange rate for 2011 was 0.444.

[3]Jack and Suri (2011) discuss details of the technology and its economic impact.

III.B. Data Collection

Participants were first invited to the lab at the Busara Center where they completed a computerized questionnaire and behavioral tasks. Experimental sessions included up to 25 participants at a time and were administered in English by research assistants. The following outlines the schedule of tasks during the lab portion of the study:

1. Coin toss task (Eckel and Grossman 2002)[4]
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

At the conclusion of the demographics questionnaire, participants received KES 200 for completing the session and an additional KES 50 for arriving on time. 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 enrolled in the 60-day savings program and randomly assigned to one of three incentive schemes: one fixed match and two lottery-based matches. Savings incentives are detailed in Section III.D. Each participant received KES 20 airtime credit and asked to practice saving using Sambaza. Participants then received business-card sized handouts which described their savings program and bonuses. 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.

All participants completed the savings 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. We find no evidence that completion of the endline survey correlates with treatment assignment.

[4]This elicitation method produces interval estimates of the coefficient of relative risk aversion, ρ, under the assumption of constant relative risk aversion. 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 boundary values as point estimates.

III.C. Mobile Savings Product

We implemented our 60-day mobile-phone 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 arrived at 8:00 daily summarizing 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 first message with the lottery results would be 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 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 tranfers equal to their plus a withdrawal fee compensation. The product we provided was a "lockbox" account where regular withdrawals outside of this opportunity were restricted. Transfers were made using the mobile money system M-Pesa to minimize transaction costs. M-Pesa accounts are associated with a SIM card and transactions are made via SMS. Participants could deposit and withdraw money from the account at any of more than 10,000 agents throughout Kenya, including agents in the informal settlements where our participants reside.

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, participant 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 balances plus withdrawal fees shortly after. Participants paid no explicit fees to participate in our program.

III.D. Treatment

Participants enrolled in the savings program were randomized into one of three different incentive schemes. Tables 1 reports summary statistics and tests for balance across treatment groups of several pre-treatment characteristics. We find no overall correlation between treatment assignment and these observable characteristics.[5]

1. *Matching contributions:* Participants in the matching group earned a 5% matching contribution on any amount that they saved on a particular day. This amount of the incentive and the participants' current balance were reported every morning via SMS. We take this group as our control group.
2. *Prize-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 morning, 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 expected earnings on this lottery ticket were equal to the 5% match earned in the control group—*i.e.* the payoffs were equivalent but by a mean-preserving increase in risk. Participants could only earn one lottery ticket per day. Our system entered winnings into the internal ledger and reported lottery results via SMS if participants in this group made a deposit. We henceforth refer to this group as the LOTTERY group.
3. *Prize-linked savings with feedback:* This scheme is identical to the lottery treatment except participants in this third group received lottery tickets with the first SMS message of the day regardless of deposits made. These tickets only became redeemable after participants had made a deposit before that day's lottery results were announced. Participants with winning lottery tickets who did not save could not claim the prize but received feedback on the lottery results daily. We henceforth refer to this group as the REGRET group.

[5] We account for the correlation of treatment to usage of a savings account in Section IV.

Table 1: Baseline balance by treatment group

	(1) Lottery - Control	(2) Regret - Control	(3) Lottery - Regret	(4) Control mean (SD)	(5) Obs.
Female	0.07	0.10		0.52	311
	(0.07)	(0.07)		(0.50)	
Age	0.78	0.72		30.75	303
	(1.39)	(1.34)		(9.83)	
Completed std. 8	-0.02	-0.02		0.99	311
	(0.02)	(0.02)		(0.10)	
Married/co-habitating	0.10	0.09		0.42	307
	(0.07)	(0.07)		(0.50)	
No. of children	0.23	0.24		1.75	311
	(0.24)	(0.25)		(1.70)	
Currently saves	0.05	-0.10		0.56	311
	(0.07)	(0.07)		(0.50)	
Total savings last month	-17.81	-7.04		58.82	311
	(11.92)	(12.60)		(106.26)	
Monthly income	-3.68	-0.59		112.05	311
	(17.69)	(16.91)		(137.13)	
Employed	0.05	-0.03		0.50	311
	(0.07)	(0.07)		(0.50)	
Coefficient of relative risk aversion	0.08	-0.03		1.16	311
	(0.18)	(0.17)		(1.27)	
Locus of control	0.48	-0.83		69.81	311
	(1.40)	(1.46)		(10.78)	
Standardized CPGI	-0.11	-0.22*		-0.00	311
	(0.13)	(0.12)		(1.00)	
Exp. discount factor	-0.05*	-0.01		0.33	311
	(0.03)	(0.03)		(0.20)	

Notes: The first three columns report the difference of means across treatment groups with SEs in parentheses. Column 4 reports the mean of the control group with SD in parentheses. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

IV. Empirical Strategy

IV.A. Average Treatment Effect

We use the following reduced-form specification to estimate the treatment effect of lottery incentives on participant outcomes.

$$Y_i = \beta_0 + \beta_1 \text{LOTTERY}_i + \beta_2 \text{REGRET}_i + \varepsilon_i \tag{1}$$

Y_i refers to the outcome variables for individual i measured after the end of the savings program. LOTTERY_i indicates assignment to the LOTTERY group and REGRET_i indicates assignment to the lottery with regret framing group. The omitted group is the control group. We test $\beta_1 = 0$ and $\beta_2 = 0$ to identify the effects of the lottery and lottery with feedback relative to the matching group. We additionally test $\beta_2 - \beta_1 = 0$ for differential effects between the two lottery treatments. Standard errors are clustered at the individual level.

To improve precision and control for potential selection bias, we apply covariate adjustment with a vector of baseline indicators.[6] We obtain the covariate-adjusted treatment effect estimate by estimating Equation 1 including the demeaned covariate vector $\mathbf{X}_i$ as an additive term and as an interaction with the treatment indicator.

$$\begin{aligned} Y_i = &\beta_0 + \beta_1 \text{LOTTERY}_i + \beta_2 \text{REGRET}_i + \mathbf{X}_i' \gamma_0 \\ &+ \text{LOTTERY}_i \mathbf{X}_i' \gamma_1 + \text{REGRET}_i \mathbf{X}_i' \gamma_2 + \varepsilon_i \end{aligned} \tag{2}$$

The set of indicators partitions our sample so that our estimate remains unbiased for the average treatment effect (Lin 2013). As in Equation 1, we test $\beta_1 = 0$ and $\beta_2 = 0$ to identify the effects of the lottery and lottery with feedback relative to the matching group and test $\beta_2 - \beta_1 = 0$ for differential effects between the two lottery treatments. Standard errors are clustered at the individual level. Equation 1 is our preferred specification and report results with covariate adjustment for robustness.

We control for the family-wise error rate (FWER) to correct for multiple inference. We compute adjusted p-values within categories of outcome variables using the free step-down resampling method (Westfall and Young 1993; Anderson 2008). This approach sets the size of the test to exactly the desired crticial value. We apply this correction over outcome variables in each family and separately for each hypothesis test. For each variable, we apply the procedure with 10,000 iterations and report both unadjusted and adjusted p-values.

[6]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.

IV.B. Minimum Detectable Effect Sizes

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.

$$\text{MDE}_{\hat{\beta}} = (t_{1-\kappa} + t_{\alpha/2}) \times \text{SE}(\hat{\beta}) \tag{3}$$

This metric is the smallest effect that would have been detectable given our current sample size. Thus, a MDE lower than our estimated treatment effect suggests that null results are due to a lack of statistical power. We calculate MDEs *ex post* with $\alpha = 0.05$ and 0.80 power for both treatment effects.

IV.C. Heterogeneous Treatment Effects

We analyze the extent to which the savings program produced heterogeneous treatment effects with the following specification.[7]

$$\begin{aligned} Y_i = &\beta_0 + \beta_1 \textsc{Lottery}_i + \beta_2 \textsc{Regret}_i + \delta_0 x_i \\ &+ \delta_1(\textsc{Lottery}_i \times x_i) + \delta_2(\textsc{Regret}_i \times x_i) + \varepsilon_i \end{aligned} \tag{4}$$

x_i is the binary dimension of heterogeneity measured before treatment assignment. δ_1 and δ_2 respectively identify the heterogeneous treatment effects of the lottery and lottery with feedback relative to $x_i = 0$. Standard errors are clustered at the individual level. We estimate this model with the following baseline variables as x_i: gender, marriage status, below age 30, completed std. 8, uses a savings account, above median monthly income, employment status, above median CPGI score, coefficient of relative risk aversion, above median indifference point.

IV.D. Testing for Regret Aversion

We can leverage the panel structure of our data and the exogeneity of the lottery results to formulate a test of regret aversion. If participants are saving in response to experienced regret of foregoing the prize, we expect to see an effect of winning the lottery above and beyond the group effect. Let $Y_{i,t}$ denote having made a deposit or the amount deposited by participant i in period t. $\textsc{Regret}_i$ is the treatment indicator and $\text{Win}_{i,t}$ is an indicator for having a winning lottery ticket announced in period t and earned in period $t-1$. We estimate the following equation conditional on assignment

[7] This is a slight abuse of notation as β denotes a different parameter than those in the previous regressions.

to the control group, assignment to the REGRET group, and not having saved one period prior.

$$Y_{i,t} = \pi_1 \text{REGRET}_i + \pi_2(\text{REGRET}_i \times \text{Win}_{i,t}) + \omega_t + u_{i,t} \tag{5}$$

π_1 is the marginal effect of REGRET for participants who did not win the lottery in period t while π_2 is the additional effect from winning. ω_t is a period-specific fixed effect. The control group is the comparison group. We omit the indicator for having won the lottery in the control group since those participants did not enter into the drawing. We test the null hypothesis of no regret aversion, $\pi_2 = 0$. Standard errors are clustered at the individual level.

IV.E. Time-Varying Treatment Effects

Using detailed daily transaction data, we can estimate treatment effects of PLS conditional on the time elapsed since the start of the savings program. We estimate day-specific effects with the following specification.

$$\begin{aligned} Y_{i,t} =& \beta_0 + \beta_1 \text{LOTTERY}_i + \beta_2 \text{REGRET}_i \\ &+ \sum_{t=2}^{60} \Big[\zeta_t \tau_t + \eta_t(\text{LOTTERY}_i \times \tau_t) + \theta_t(\text{REGRET}_i \times \tau_t) \Big] + u_{i,t} \end{aligned} \tag{6}$$

$Y_{i,t}$ is the outcome for individual i at period t, LOTTERY_i indicates assignment to the LOTTERY group, and REGRET_i indicates assignment to the REGRET group. τ_t is an indicator taking the value 1 in period t. The omitted group is the control group at $t = 1$. We can interpret the value of $\beta_1 + \eta_t$ and $\beta_2 + \theta_t$ as the effects of LOTTERY and REGRET, respectively, at period t. We test the null hypothesis of no time-varying treatment effects with a joint tests of $\beta_1 + \eta_t = 0$ and $\beta_2 + \theta_t = 0$, respectively, for $t = 2, \ldots, 60$. Standard errors are clustered at the individual level.

V. Results

V.A. PLS with Feedback Increases Deposit Frequency but Not Savings

By the end of the project, the median participant in the control group contributed USD 3.86 to the mobile savings account over 8 deposits. The total saved by the control group amounts to less than 5% of the median monthly income (USD 77.24) and 17% of the median monthly savings (USD 22.91). 13% of the control group did not use their accounts at all. Despite the relatively high 5% rate of return on deposits, minimal saving is consistent with estimates of low interest rate elasticities (Karlan and Zinman 2018).

Table 2 compares the lottery results with the expected probabilities of each type of lottery match. The mean prize award is 0.05 of deposits ($p > 0.10$).

Table 2: Observed and expected lottery results

	Freq.	Pct. observed	Pct. expected
No match	7065	81.49	62.43
One match	1518	17.51	22.22
Two matches	86	0.99	1.23
Complete match	1	0.01	0.00

Notes: The first column tabulates the frequency of observed lottery ticket matches. The second and third columns report the observed and expected probabilities, respectively, of each type of lottery match. A lottery ticket was a random sequence of four numbers between 1 and 9, inclusive. 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. If all numbers matched, a prize of 200 times the daily savings was awarded.

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 match incentive. Table 3 reports a moderately sized effect of 0.38 SD over the average frequency of deposits in the control group. This result is further robust to the inclusion of control variables and significant at the 10% level with FWER adjusted p-values. These effects exhibit no heterogeneity across demographic characteristics, risk attitudes, or temporal discounting.[8] We do not find strong evidence of an effect of the lottery incentive alone against either REGRET or the control group ($p < 0.10$), but estimates suggest that the magnitude of this treatment effect is comparable to that of REGRET. Panel A of Figure 1 traces the cumulative path of deposits made over the duration of the project. Average deposits for the LOTTERY and REGRET groups are greater than for the control group over all periods. We are able to statistically distinguish total values at the end of the 60-day period for REGRET and not for LOTTERY. Table 4 displays the minimum detectable effect sizes for each outcome and shows that the present experimental design is powered to detect effects only larger than what we estimate for the LOTTERY group. A larger sample size may thus be required to reject null effects of LOTTERY at the 5% level.

Participants in REGRET had an additional 5 days on average that they chose to make at least one deposit relative to the control ($\hat{\beta} = 4.94, p <$

[8]We report the full set of heterogeneity results in an online appendix.

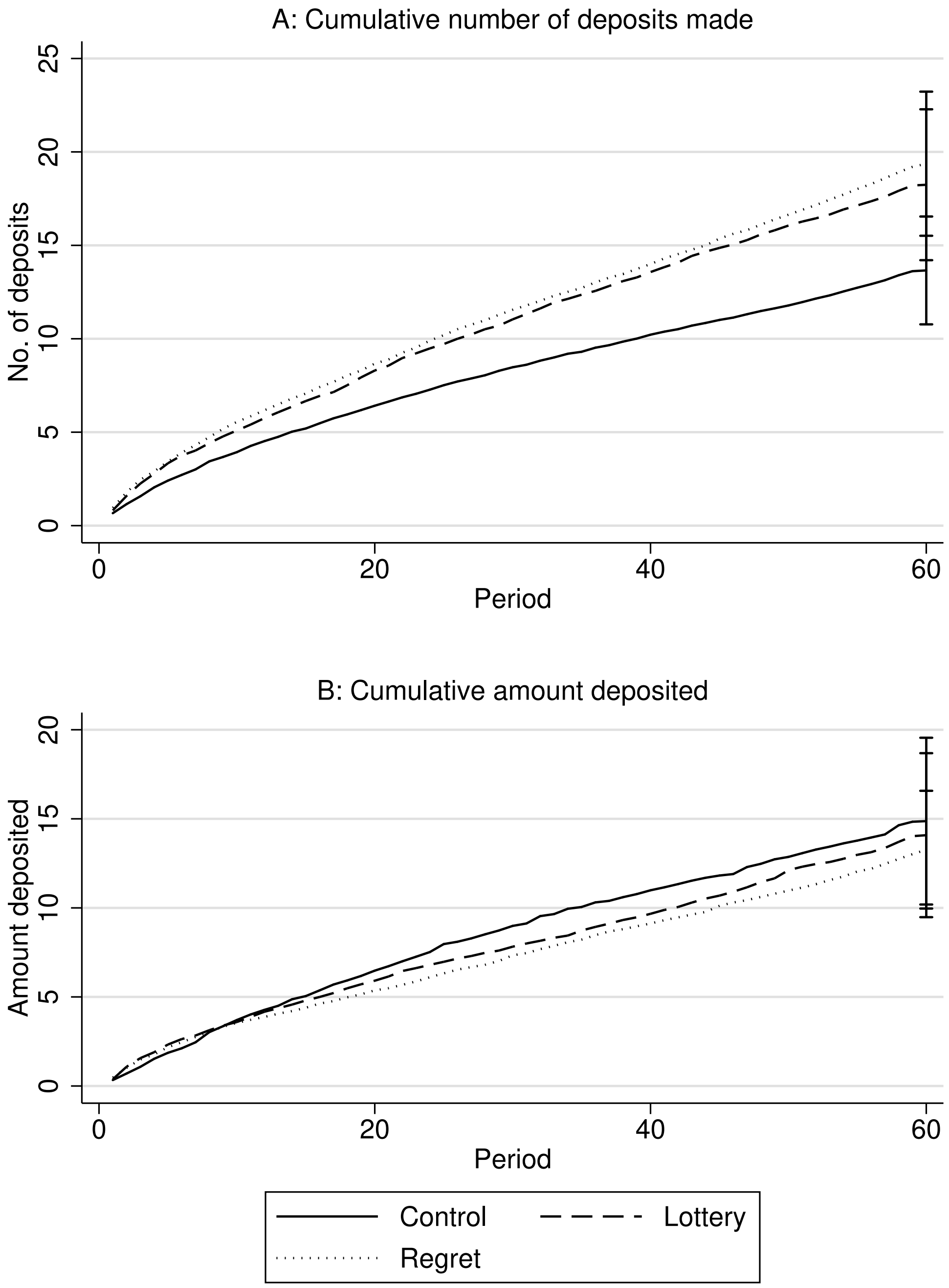
Table 3: Treatment effects – Mobile savings

	Effect estimates			Sample	
	(1)	(2)	(3)	(4)	(5)
	Lottery	Regret	Regret-Lottery	Control Mean (SD)	Obs.
Total no. of deposits	4.59*	5.71**	1.13	13.66	311
	(2.52)	(2.45)	(2.84)	(15.08)	
	[0.19]	[0.06*]	[0.90]		
No. of days saved	3.93*	4.94**	1.01	11.78	311
	(2.05)	(2.08)	(2.32)	(12.93)	
	[0.17]	[0.06*]	[0.90]		
Total deposit amount	-0.79	-1.60	-0.81	14.87	311
	(3.34)	(2.91)	(2.88)	(24.48)	
	[0.83]	[0.59]	[0.90]		
Total withdrawal amount	0.53	1.63**	1.10	1.07	311
	(0.94)	(0.74)	(1.02)	(4.53)	
	[0.83]	[0.07*]	[0.60]		

Notes: Columns 1–3 report OLS estimates of the treatment effect. Standard errors are in parentheses and FWER adjusted p-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.

0.05). This suggests that the effect on the frequency of deposits occurs on the daily margin; it is not driven by participants saving more within periods. This result is robust to covariate adjustment and is significant at the 10% level after FWER adjustment. The effect of PLS alone is smaller in magnitude to REGRET ($\hat{\beta} = 3.93$) and is significant at the 10% level.

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 totals by the end of the project are 95% confidence intervals for the group means.

While effects on number of deposits are considerable, we find no effect of either treatment on total amount deposited over the project period. Panel B of Figure 1 plots 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. Table 4 reports MDEs larger than what we estimate, suggesting a lack of statistical power and not the absence of an effect. Participants with PLS and feedback withdrew a larger amount relative to the control on average ($\hat{\beta} = 1.63, p < 0.05$) but we are unable to detect statistically significant differences in the final balance across treatment groups.

Table 4: Minimum detectable effect sizes

	(1) Lottery	(2) Regret-Lottery	(3) Control Mean (SD)	(4) Obs.
Total no. of deposits	7.09	7.98	13.66 (15.08)	311
No. of days saved	5.77	6.52	11.78 (12.93)	311
Total deposit amount	9.38	8.10	14.87 (24.48)	311
Total withdrawal amount	2.65	2.87	1.07 (4.53)	311
Total savings last month	70.69	67.60	80.31 (112.74)	284
M-Pesa savings last month	17.80	14.89	20.42 (44.67)	284
ROSCA savings last month	18.98	20.61	22.24 (42.18)	283
Saves with a ROSCA	0.20	0.20	0.54 (0.50)	284
Gamble more	0.14	0.17	0.12 (0.32)	284

Notes: Columns 1–2 report the minimum detectable effect sizes of the lottery treatment compared to control and the regret treatment against the lottery, respectively, with $\alpha = 0.05$ and 0.8 power. Columns 3–4 report the control group means and SDs and size of the analytic sample.

Our results are consistent with a general finding among earlier experiments on lottery-based incentives that behavioral responses occur on the extensive, but not intensive, margin. Brune (2015) test a stochastic incentive in Malawi wherein tea harvesters entered a lottery conditional on attendance and could increase their probability of a prize according to their output. That study found significant improvements in workplace attendance persisting over 13 weeks without changes in output. Loibl et al. (2016), examining features of the Individual Development Account program in the U.S., find no effect of PLS over certain returns of equal expected value. A recent experiment by (Gertler et al. 2017) randomized the provision of PLS across banks in Mexico and observed a 43% increase in the number of account openings in the month the lotteries were in effect but without affecting balances. Tests of PLS in the lab arrive at results slightly different from our own. 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 prize-linked and regular savings than with regular savings alone. Dizon and Lybbert (2016) replicate this design in an experiment conducted in Haiti and indentify a 22% increase in savings.[9] 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. Their designs differed from the present study in one important respect: subjects were provided budgets in order to make portfolio allocations. With a median monthly income of USD 77, households in our study may be too liquidity constrained to sustain a larger balance with PLS. Baseline correlations suggest that monthly income is predictive of savings in the mobile product *ceteris paribus* though we do not observe heterogeneity of the treatment effect conditional on income.

That potential savers respond to PLS by making more frequent deposits can be rationalized as the subdivision of lotteries to reduce risk Samuelson (1963). Under this hypothesis, risk averse individuals will subdivide bets over a greater number of gambles so that the risk of a low return is minimized. Since PLS offers returns equivalent in expectation to the matching incentive, we would expect risk averse individuals to save no more with PLS than with the standard account even without liquidity constraints.[10] Yet another explanation for our findings is the "entertainment" utility hypothesis of gambling (Conlisk 1993). That is, consumers will behave in ways that enable them to make gambles because they derive some fixed benefit from simply playing and irrespective of potential earnings. Under this hypothesis, 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. Recall that participants indeed had almost five more active days—and thus play the lottery five more times—than the control group. Unsurprisingly, participants are

[9] Dizon and Lybbert (2016) emphasize that savings responded to the presence of the stochastic component rather than expected returns.

[10] Samuelson (1963) proves that expected utility maximizers would accept a sequence of identical lotteries if and only if they accept a single instance of the lottery.

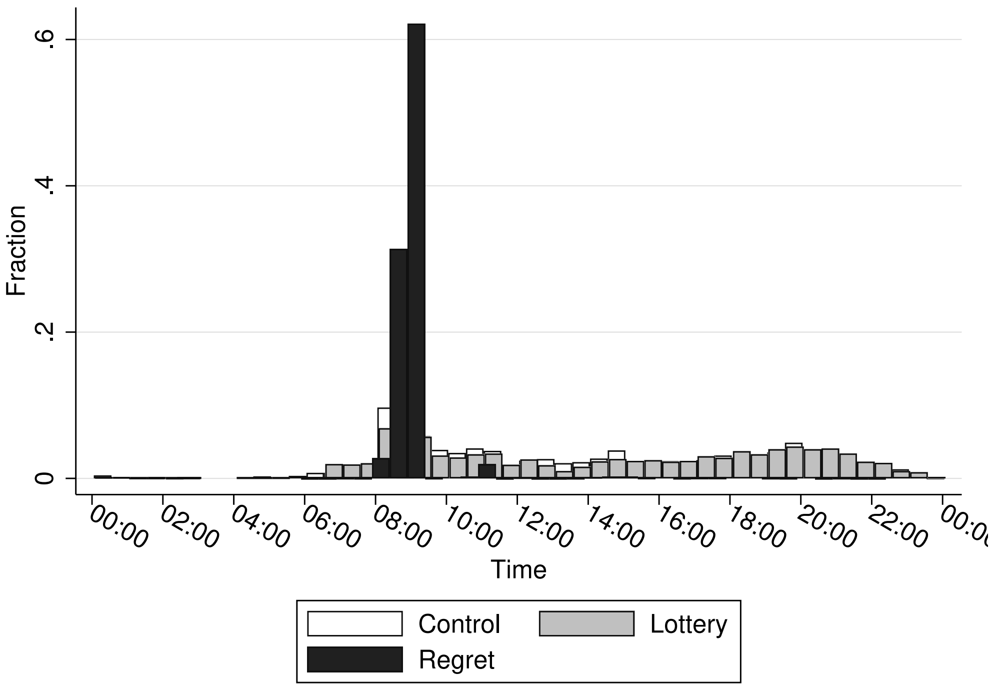
not making more deposits *within* days since this does not affect lottery eligibility. Thus, the overall effect of PLS is to encourage savers to make more deposits without a corresponding increase in amount saved.

V.B. Regret Aversion is a Potential Mechanism

Our estimates on the effect of LOTTERY are smaller in magnitude to the effect of REGRET and are not significant at the 5% level. Why might PLS with feedback elicit a larger behavioral response than PLS alone? Two alternative, but not mutually exclusive mechanisms could be driving these results. First, recall that participants in REGRET automatically received lottery tickets at the beginning of each day while LOTTERY participants had to make deposits before obtaining a ticket. Participants who exhibit loss aversion will thus be more willing to save to prevent losing a ticket already in hand (Kahneman and Tversky 1992). Second, participants save to minimize anticipated regret from winning the lottery but being unable to claim the prize. This mechanism is inactive in PLS treatment alone because that group only receives lottery results after saving.

We estimate Equation 5 to test for the presence of regret aversion distinct from other potential channels. Under the regret aversion hypothesis, we expect to observe an additional effect on saving after participants receive feedback on a winning ticket rather than a losing ticket. Table 5 shows that the treatment effect is 2.2 percentage points ($p < 0.05$) higher after learning about a winning ticket than learning about a losing one. This effect is small but is statistically distinguishable from the portion of the treatment effect arising from other mechanisms ($\hat{\beta} = 0.02, p > 0.1$). Thus participant behavior is consistent with the minimization of anticipated regret. Figure 2 plots the distribution of deposits over time and shows timing suggestive of regret aversion. Nearly all deposits made in the REGRET group ocurred within an hour of announcing the lottery results from the previous day. Deposits similarly peaked between the results and the disbursement of that day's lottery ticket. We observe no similar pattern in the LOTTERY and control groups.

Figure 2: Timing of deposits



Notes: This figure plots the empirical distribution of timing of all deposits over the project period. Each bin spans 30 minutes with a height equal to the fraction of all deposits within each treatment group. Participants received the first SMS at 8:00 that summarized 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 second SMS encouraging them to save that day. Participants in REGRET received a new lottery ticket with the second message.

Table 5: Regression of depositing on treatment and lottery results

	Made a deposit
Regret	0.02
	(0.02)
Regret × Won lottery	0.02^{**}
	(0.01)
Adjusted R^2	0.068
Control mean	0.20
Fixed effects	Period
Observations	9539

Notes: This table reports estimates of a regression of having saved at period t on the regret treatment and lottery results conditional on not having saved the previous period. We further restrict the analytic sample to those in Regret and control. The unit of observation is individual-by-period. Standard errors are in parentheses and clustered at the individual level. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

V.C. Effect on Deposits Persists Over Time

The preceding results provide evidence that PLS with feedback compels individuals to make more frequent deposits.

We additionally find no statistically significant heterogeneity by interacting the treatment effect with period indicators.

V.D. PLS with Feedback Encourages Particpation in Informal Saving

A related objective of this study is to examine whether PLS act as complements or substitutes to existing savings products. Unsurprisingly, we do not find evidence that PLS crowds out saving by other means since there was no treatment effect on amount saved with PLS. Table 6 reports no statistically significant effect on total saving through M-Pesa or ROSCAs. We do find, however, that respondents in REGRET are 14 percentage points ($p < 0.05$) more likely to save with a ROSCA compared to the control group and 16 percentage points ($p < 0.05$) more likely relative to LOTTERY. This result is robust to the inclusion of covariates but is not significant when correcting for multiple inference ($p < 0.1$). We find no effect of LOTTERY on informal saving. Table 7 reports heterogeneous treatment effects on our primary outcomes by employment status. Column 3 shows that the segment of our sample who are unemployed drive the effect on ROSCA participation. Overall, we do not find that PLS cannibalizes savings from other sources in line with earlier experimental results (Atalay et al. 2014; Filiz-Ozbay et al. 2015; Dizon and Lybbert 2016).

V.E. PLS with Feedback Increases Gambling

At endline, we ask participants whether participants gambled more than they usually do apart from participating in the savings program. As reported in Table 8, we find that participants in the REGRET group self-report higher gambling behavior the savings program. On average, treated participants are 15 percentage points ($p < 0.05$) more likely to report gambling than the control group. This finding is robust to covariate ajdustment and is significant at the 10% level after FWER adjustment. We find no average effects for participants in the LOTTERY group but Table 7 shows that LOTTERY induces increased gambling among the unemployed ($\hat{\beta} = 0.17, p < 0.05$). Our measure for gambling activity is susceptible to experimenter demand though it is unclear in what direction this might bias our estimate. With this caveat in mind, the effect provides some evidence of a complementary relationship between PLS and broader gambling behavior.

Atalay et al. (2014) and Dizon and Lybbert (2016) both observe large reductions in gambing expenditure in order to finance savings with PLS. It

Table 6: Treatment effects – Savings outside the project

	Effect estimates			Sample	
	(1)	(2)	(3)	(4)	(5)
	Lottery	Regret	Regret-Lottery	Control Mean (SD)	Obs.
Total savings last month	18.45	-17.87	-36.32	80.31	284
	(25.16)	(14.64)	(24.06)	(112.74)	
	[0.87]	[0.53]	[0.36]		
M-Pesa savings last month	-5.42	-6.71	-1.29	20.42	284
	(6.34)	(5.49)	(5.30)	(44.67)	
	[0.87]	[0.53]	[0.82]		
ROSCA savings last month	1.48	7.37	5.89	22.24	283
	(6.76)	(6.79)	(7.33)	(42.18)	
	[0.97]	[0.53]	[0.69]		
Saves with a ROSCA	-0.02	0.14**	0.16**	0.54	284
	(0.07)	(0.07)	(0.07)	(0.50)	
	[0.97]	[0.17]	[0.10]		

Notes: Columns 1–3 report OLS estimates of the treatment effect. Standard errors are in parentheses and FWER adjusted p-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.

is possible that while PLS is a substitute for gambling with a cash windfall (as in their study), it interacts differently when indviduals already have a history of gambling. Cookson (2016) offered individuals in Nebraska access to an PLS 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 substition of casino gambling with PLS. 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 PLS and could explain the difference in our findings.

VI. Conclusion

By taking advantage of savers' preference for gambling, stochastic incentive schemes like PLS represent a promising policy tool to overcome behavioral barriers to saving. We conducted a randomized experiment testing a PLS product with informal residents in Nairobi, Kenya. Utilizing a mobile savings platform, we randomly assign respondents to a savings account with a certain, matching incentive, 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 compar-

Table 7: Heterogeneous effects – Primary outcomes by employed

	(1) Total no. of deposits	(2) Total deposit amount	(3) Saves with a ROSCA	(4) Gamble more
Lottery	4.67	-1.66	-0.03	0.17**
	(3.69)	(3.74)	(0.10)	(0.07)
Lottery × Employed	-0.56	1.04	0.01	-0.21**
	(5.11)	(6.51)	(0.14)	(0.10)
Regret	9.02***	1.18	0.31***	0.17***
	(3.28)	(3.69)	(0.10)	(0.07)
Regret × Employed	-6.82	-5.57	-0.34**	-0.04
	(4.91)	(5.89)	(0.14)	(0.11)
Employed	4.53	6.42	0.30***	0.14**
	(2.93)	(4.77)	(0.10)	(0.06)
Constant	11.42***	11.69***	0.39***	0.04
	(1.76)	(3.10)	(0.07)	(0.03)
Adjusted R^2	0.011	0.002	0.066	0.026
Control mean	13.66	14.87	0.54	0.12
Lottery p-value	0.25	0.91	0.77	0.63
Regret p-value	0.55	0.34	0.77	0.15
Observations	311	311	284	284

Notes: This table reports OLS estimates of the treatment effect and its interaction with baseline. Standard errors are in parentheses. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. We also report the p-values for joint tests on the direct treatment effect conditional on the baseline covariate = 1.

ing he 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 matched payments 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 the end of the savings program.

If PLS 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, PLS may be able to attract new savers to open accounts. PLS 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 PLS 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 PLS and fixed-incentive schemes

Table 8: Treatment effects – Gambling

	Effect estimates			Sample	
	(1)	(2)	(3)	(4)	(5)
	Lottery	Regret	Regret-Lottery	Control Mean (SD)	Obs.
Gamble more	0.06	0.15^{***}	0.08	0.12	284
	(0.05)	(0.06)	(0.06)	(0.32)	
	[0.61]	[0.05*]	[0.48]		
Gamble less	-0.02	0.04	0.06	0.16	284
	(0.05)	(0.06)	(0.05)	(0.37)	
	[0.88]	[0.79]	[0.55]		
More tempted to gamble	0.09	0.05	-0.04	0.47	284
	(0.07)	(0.07)	(0.07)	(0.50)	
	[0.61]	[0.79]	[0.56]		
Less tempted to gamble	-0.01	0.03	0.04	0.06	284
	(0.03)	(0.04)	(0.04)	(0.25)	
	[0.88]	[0.79]	[0.55]		

Notes: Columns 1–3 report OLS estimates of the treatment effect. Standard errors are in parentheses and FWER adjusted p-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.

when it comes to encouraging savings and show that product design is crucial in determining welfare implications.

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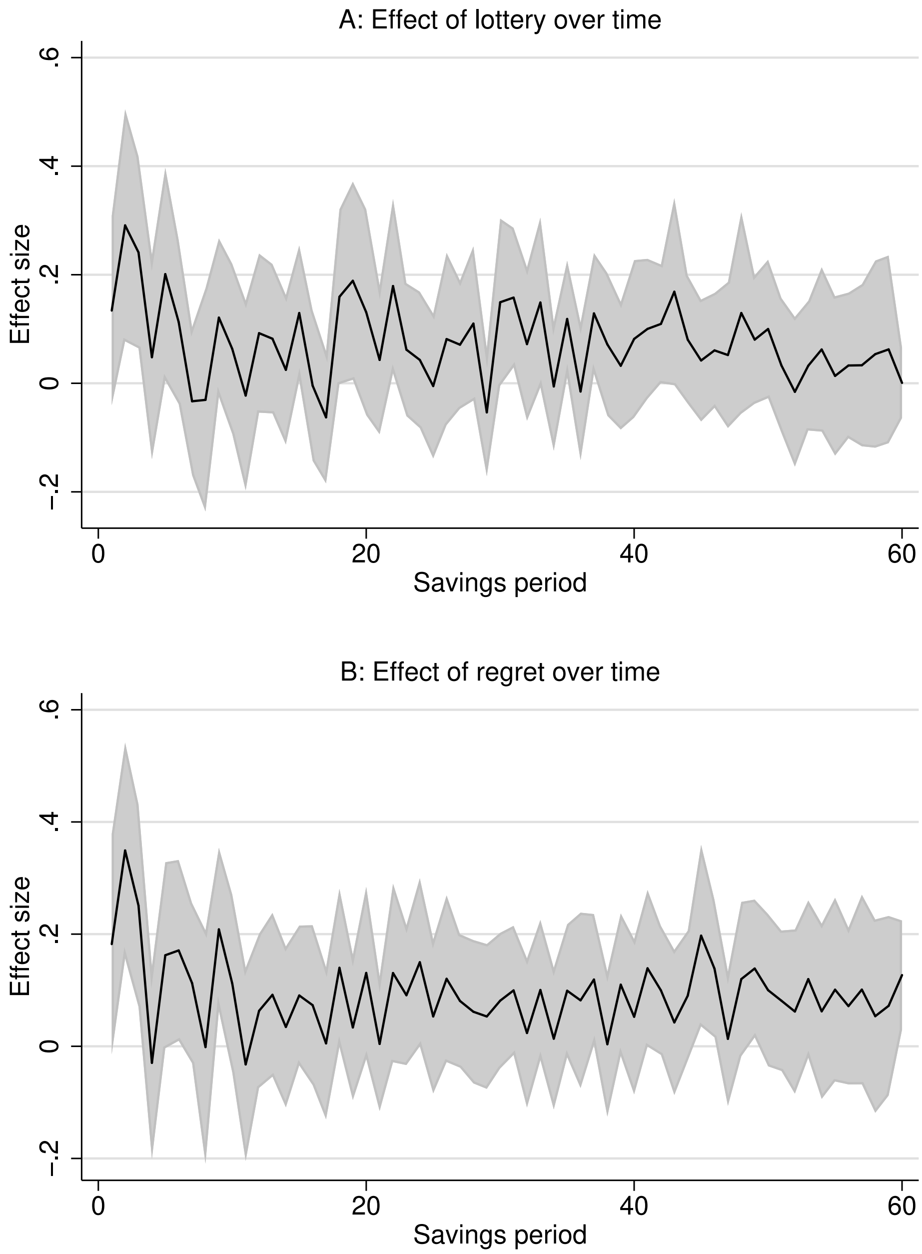
Figure 3: Effects over time – Number of deposits



Notes: Panel A plots the treatment effect of LOTTERY on number of deposits as a function of savings period. Panel B plots the treatment effect of REGRET on number of deposits as a function of savings period. Shaded areas represent period-specific 95% confidence regions.