The Role of Regret in Prize-Linked Savings: Experimental Evidence from Kenya*

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We conducted a field experiment that provided 311 low-income individuals a prize-linked savings account (PLS)—a product that incorporate stochastic returns to deposits. Individuals saving with PLS made 42% more deposits on average compared to those with standard savings accounts. By varying feedback on lottery results, we find some evidence that the treatment effect is consistent with regret aversion and that recent experiences of regret induce more subsequent deposits. We do not observe any effects of PLS on the amount deposited or on savings with other products. Lastly, we document evidence that use of PLS results in a 15% increase in gambling activity.

JEL Classifications: D14, D91, O16

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1. Introduction

The ability to save is one of the most important avenues toward economic development; it provides a means to smooth consumption under incomplete insurance and it makes possible productive investments in the presence of credit constraints. There exists, however, a host of obstacles that prevent poor households from accruing savings to their advantage. There are supply-side limitations to formal finance, such as high initial and transaction costs, that may be prohibitive for the poor. As a result, they often have to resort to methods of saving that can be costly and have limited functionality (Banerjee and Duflo 2007; Collins et al. 2009; Karlan et al. 2014; Schaner 2011).

On the demand side, knowledge gaps, mistrust of financial institutions, and behavioral biases prevent the poor from saving as much as they would like. In Sub-Saharan Africa, less than 20% of banked adults make more than 2 deposits in a month (Demirgüç-Kunt et al. 2015). Policies that target costs on the supply side are known to increase account ownership but have been less effective at encouraging usage (Dupas and Robinson 2013; Karlan et al. 2016). Meanwhile, savings products designed to address 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 (Chetty and Friedman 2014; Somville and Vandewalle 2018; Thaler and Benartzi 2004) address undersaving due to limited attention. Binding commitment devices, in the form of account restrictions (Ashraf et al. 2006) or the application of social pressure (Dupas and Robinson 2013), can help individuals with time-inconsistent behavior follow through on saving. This literature has demonstrated the importance of product design or "choice architecture" in financial decisionmaking.

This article examines the potential of prize-linked savings (PLS)— a savings product that incorporate lottery-like payoffs—in encouraging saving. The key novelty of PLS accounts is that users receive a stochastic payoff in addition to, or in lieu of, interest payments. Common among PLS products is that consumers face no risk of negative returns, at least guaranteeing the full principal amount. PLS have been in use since at least the 17$^{\text{th}}$ century and presently exist in various forms around the globe (Kearney et al. 2010; Murphy 2005). In the United States, PLS accounts are primarily offered at the state level by local credit unions. The American Savings Promotion Act, signed into law in 2014, authorized financial institutions to provide PLS nationwide. Any kind of savings product that allows consumers to enter raffles or earn lottery tickets conditional on deposits may be considered PLS. Products available in Latin America, for instance, offer in-kind prizes. Lottery bonds (like the NS&I Premium Bonds in the U.K.) are closely

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

related products that provide consumers with the opportunity to redeem bonds for an amount greater than their face value. These products all offer the stochastic component with an eye towards incentivizing further saving.[2]

We conducted a 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. 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. Each day, participants received a lottery ticket whose earnings were proportional to amount saved that day instead of a certain match. We compared the match and lottery groups to determine how PLS impacts savings behavior. We find that participants using PLS made 42% more deposits on average over the project period than participants receiving the matching incentive. This effect occurs on the daily margin: there are 5 additional days when treated participants made deposits to their savings account. We find no effect of PLS on total amount saved, a finding largely consistent with earlier experimental results. Consequently, we find no evidence that PLS displaced savings from other sources.

We also examined the effect of PLS on gambling consumption. While the lottery component could act as a substitute to other forms of gambling, addiction theories of gambling (Becker and Murphy 1988) suggest complementarities between past and current consumption. Increased access to savings in general could decrease demand for gambling if it is used a means to finance lumpy expenditures in the presence of financial constraints (Herskowitz 2016). To understand the causal relationship between PLS and gambling, we estimate effects of PLS on overall gambling by our participants and find that the risk of gambling more than at baseline is increased by a factor of three compared to those in the control group. We do not observe heterogeneous effects on any outcomes by preferences for risk or by an index of problem gambling.

There are a host of theoretical explanations behind the apparent demand for PLS as opposed to standard savings. Our second objective is to quantify the role of regret aversion (Bell 1983; Loomes and Sugden 1982) as a specific mechanism driving our treatment effects. Regret theory refers to a class of behavioral models that posit that individuals minimize anticipated regret arising from the comparison of prospects against foregone outcomes. In the context of uncertainty, regret aversion has been shown to promote apparently risk averse and risk-seeking behavior (Zeelenberg et al. 1996).

To test for regret aversion, we implemented a third treatment wherein participants received the same PLS account with the additional feature that they received a lottery

[2] It has also been argued that PLS might be advantageous in the Islamic banking sector since lottery winnings can be understood to be in compliance with Sharia law which forbids earning guaranteed interest on assets (Ruth 2018).

ticket and observed the lottery results only after they had made a deposit that day. That feedback about hypothetical lottery results affects decisions to save is a unique prediction of regret aversion. We find that when the resolution of lotteries depends on saving, the effect of PLS on deposits is 20% smaller than when feedback is always given. In other words, participants assigned to PLS with feedback are motivated to save (and enter the lottery) in anticipation of regret they may feel from having a winning ticket but not being able to claim the prize. Exploiting the longitudinal nature of our data, we also find that recent experiences of regret (winning the lottery without claiming the prize) increase the likelihood of making a deposit in subequent days by a magnitude equal to 25% of the daily PLS treatment effect. Together, our evidence points to the importance of regret aversion in demand for PLS.

This study is one of the first randomized evaluations of PLS in a low-income setting[3]. Much of what we know about the behavioral effects of PLS comes from laboratory studies which provide evidence of a positive effect of stochastic returns on saving. 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 endowments 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. An evaluation of PLS in the field is important since its behavioral effects can be moderated by economic conditions, such as liquidity constraints, that do not exist in lab settings.

Outside the laboratory, evidence surrounding PLS is limited[4]. Our study comes closest to Dizon and Lybbert (2016), which is a lab experiment conducted with low-income participants in Haiti. The experiment adapts the portfolio allocation task of Atalay et al. (2014) and found that the introduction of PLS increased gross savings by 22% financed by a reduction in lottery expenditure and conventional savings. They also find evidence that it is the presence of lottery incentives and not expected returns or the degree of risk that matters for savings. Gertler et al. (2017) is the first field experiment studying the long-term effects of PLS in Mexico. They found that bank branches offering PLS saw 41% more account openings and that these continued to be used five years after the

[3]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.

[4]Lottery-based incentives in other domains, including labor supply (Brune 2015) and health-related behaviors (Bjorkman Nyqvist et al. 2015; Kimmel et al. 2012), are found to have significant effects.

lottery incentives expire. We contribute to this line of research by studying a yet untested feature of PLS (feedback on lottery results) and in so doing we quantify the role of regret aversion as an underlying behavioral mechanism.

Our experiment also joins a strand of the behavioral literature studying the applicability of regret aversion. Zeelenberg and Pieters (2004) is a cross-sectional study of Dutch lotteries that showed that feedback about winnings in one lottery elicits feelings of regret and that it influences decisions to play. Barberis et al. (2006) argues that regret aversion is a possible explanation for the reluctance of households to participate in the stock market. Filiz-Ozbay and Ozbay (2007) finds evidence that regret felt by bidders who lose can explain overbidding in first price auctions. As in this study, they utilize the manipulation of feedback about the winning bid to test for regret aversion. Our study contributes to this literature in examining the implications of regret aversion for household finance and in repeated settings. Namely, we find evidence that regret aversion appears more intense among those who recently experienced regret. A recent paper by Imas et al. (2016) argues that regret aversion results in an *undervaluation* of lotteries in repeated settings because frequent experiences of regret may dissuade individuals from making risky choices and because feedback provides individuals the opportunity to learn about the incentives. Strack and Viefers (2019) study a stopping problem when agents are regret averse. They show that decisionmakers a reluctant to stop because they utility depends on the best foregone outcome from the entire history. Regret aversion in repeated settings remains a largely unexplored area of research.

The remainder of the paper is structured as follows. Section 2 describes our experimental design, Section 3 outlines our estimation strategy, Section 4 presents and discusses our main results, and Section 5 concludes with a discussion on extensions and welfare implications.

2. Experimental Design

2.1 Sampling Frame

We conducted our experiment in conjunction with the Busara Center for Behavioral Economics and with 311 participants recruited from Nairobi's low-income neighborhoods. Three quarters of our sample reside in Kibera, the city's largest informal settlement. We drew a random sample of participants over 18 years old 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 employed in our sample largely work in retail or are students. The median

PPP-adjusted monthly income among those employed is USD 77.[5] Compared to the wider Nairobi population, men, married individuals, and those with tertiary education are underrepresented in the Busara sample. Individuals in our sample are also more likely to identify as a member of the Luo tribe compared to the Kenyan population since a considerable fraction of informal residents in Nairobi are of Luo origin (Haushofer et al. 2014).

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. The average stock of savings among these individuals amount to USD 23. A significant fraction of savers also report using M-Shwari, a mobile service that provides a lockbox account and offers short-term credit.[6] Mobile transactions are made with M-Pesa, an SMS-based money system made accessible by the ubiquity of mobile phones in Kenya.

The surge of mobile phone usage in Kenya has driven 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.

2.2 Data Collection

Participants were first invited to the lab at the Busara Center where they completed a computerized questionnaire and several 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)[7]
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)

[5]Monetary payouts were in Kenyan shillings (KES). We report USD values calculated at purchasing power parity using a conversion factor for private consumption of 38.84 in 2013. The price level ratio of PPP conversion factor (GDP) to KES market exchange rate for 2011 was 0.444.

[6]The savings product we designed is comparable to M-Shwari in that it also offered commitment savings and a 5% return on deposits.

[7]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.

6. Demographics questionnaire

At the conclusion of the demographics questionnaire, participants received KES 200 (USD PPP 5.15) 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 data collected during this period before the beginning of 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 that provides a certain match on deposits and two lottery-based matches. Treatment groups are detailed in Section 2.4. Each participant received KES 20 airtime credit and asked to practice saving using the mobile platform. Participants then received business-card sized handouts that described their savings program and incentives. 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 and 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.

2.3 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 (in units of the local currency) 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 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 transfers plus a withdrawal fee compensation. The product we provided was a "lockbox" account where regular withdrawals outside of this opportunity were prohibited.[8] 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 those located 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 net of withdrawals shortly after. Participants paid no fees to participate in our program.

[8]This feature is commonplace among informal savings and is increasingly incorporated into savings interventions. Ashraf et al. (2006) provides evidence that commitment savings increases long-term savings among women who exhibit present-biased time preferences. Commitment savings may also be advantageous when intrahousehold bargaining hinders saving (Banerjee and Duflo 2007; Schaner 2011). Since the lockbox feature was present in all of the treatment groups it does not change our interpretation of the treatment effect.

2.4 Treatment

Participants were randomized into one of three different incentive schemes described below and summarized in Table 1.

1. *Matching contributions:* Participants in the matching group earned a 5% matching contribution on any amount that they saved on a particular day.[9] The match was automatically added to the mobile account the following morning. The amount of the incentive and the participants' daily balance were reported every morning via SMS. We take this group as our control group.

2. *Prize-linked savings with feedback:* Every day, all treated participants earned a lottery ticket transmitted via SMS that could win a cash prize in proportion to the amount they saved on the same day.

   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 expected payoffs were equivalent but by a mean-preserving increase in risk.[10]

   Our system entered winnings into the internal ledger and reported lottery results via SMS the following day. Participants with winning lottery tickets who did not save could not claim the prize but received feedback on their lottery results daily. We henceforth refer to this group as the PLS-F group.

3. *Prize-linked savings without feedback:* This scheme is identical to the PLS treatment except participants in this third group only received lottery tickets and were made aware of their potential winnings if they made a non-zero deposit the previous day. While participants in the PLS group obtained lottery results everyday, participants in this group only found out about lottery matches conditional on saving. We henceforth refer to this group as the PLS-N group. We leverage the variation of the information regime in this treatment group to test for regret aversion as detailed in Section 4.2.

[9] We chose such a high return rate to induce participation in the program in light of observed low interest rate elasticities among the poor. The rate is constant across treatment arms since we are interested in the effect of the lottery feature.

[10] Table 11 details the lottery results and the objective probabilities of each type of lottery match.

Table 1: Summary of treatment conditions

Treatment	Control Group	PLS-F (feedback)	PLS-N (no feedback)
Incentive type	Certain	Stochastic	Stochastic
Expected match rate	5%	5%	5%
View lottery results	-	Always	On deposit

Notes: 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.

3. Empirical Strategy

3.1 Average Treatment Effect

We use the following reduced-form specification to estimate the treatment effect of lottery incentives on aggregate participant outcomes over the entire savings period.

$$Y_i = \beta_0 + \beta_1 \mathrm{PLS}_i^N + \beta_2 \mathrm{PLS}_i^F + \varepsilon_i \tag{1}$$

Y_i refers to the outcome variables for individual i measured after the end of the savings program. PLS_i^N indicates assignment to the PLS-N group and PLS_i^F indicates assignment to the PLS-F group. The omitted group is the control group. We test $\beta_1 = 0$ and $\beta_2 = 0$ to identify the effects of PLS and PLS without feedback relative to the control group. Since all treatment groups are equivalent in expectation, any effects relative to the control group can be attributed to the stochastic nature of the incentive. We additionally test $\beta_2 - \beta_1 = 0$ for differential effects between the two PLS treatments. Effects between the two PLS treatments can be attributed to the differences in feedback, which we interpret as evidence for regret aversion in Section 4.2. Statistical inference is based on heteroskedastic-robust standard errors.

We might expect that the errors of each outcome variable are correlated. Instead of estimating these equations separately, we 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 groups[11] for Equation 1.

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 (Anderson 2008; Westfall and Young 1993). 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.

3.2 Heterogeneous Treatment Effects

We analyze the extent to which the savings program produced heterogeneous treatment effects with the following specification.[12]

[11] Grouping of dependent variables was specified in a pre-analysis plan. We document differences between the present analysis and the pre-analysis plan in Table 10.

[12] β here denotes a different parameter than those in the previous regressions.

$$
\begin{aligned}
Y_i =& \beta_0 + \beta_1 \text{PLS}_i^N + \beta_2 \text{PLS}_i^F + \delta_0 x_i \\
& + \delta_1 (\text{PLS}_i^N \times x_i) + \delta_2 (\text{PLS}_i^F \times x_i) + \varepsilon_i
\end{aligned} \tag{2}
$$

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 two PLS products relative to individuals whose $x_i = 0$. We conduct inference with heteroskedstic-robust standard errors. We report estimates with the following baseline variables as x_i: an indicator for prior usage of a savings account, an indicator for scoring above the median in the CPGI scale, an indicator for being classified as risk averse in the coin toss task, and an indicator for having an above median average indifference point as measured in the titration task.[13]

4. Results

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 among the poor (Karlan and Zinman 2018). Table 2 reports summary statistics and tests for balance across treatment groups of several pre-treatment characteristics. We find no overall imbalance based on a test of joint significance across all observables.[14]

4.1 PLS Increases Deposit Frequency

We find that participants in the PLS-F 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. Column 2 of Table 3 reports a moderately sized effect of 0.38 SD over the average frequency of deposits in the control group. This result is significant at the 10% level with FWER adjusted p-values. Participants in the PLS-F treatment had an additional 5 days on average that they chose to make at least one deposit relative to the control ($\hat{\beta} = 4.94, p < 0.05$). This suggests that the effect on the frequency of deposits occurs on the daily extensive margin; it is not driven by participants depositing more often within a single day. This result is also significant at the 10% level after FWER corrections.

[13] We report heterogeneous treatment effects for a wider set of baseline variables in an online appendix.

[14] Our results are robust to the inclusion of control variables. Those results are reported in the online appendix.

Effects on saving exhibit no heterogeneity across prior savings usage, problem gambling, risk attitudes, and temporal discounting (Tables 12-15).

Panel A of Figure 1 traces the cumulative path of deposits made over the duration of the project. The average number of deposits for the PLS-F group are greater than for the control group over all periods, though we are only able to statistically distinguish cumulative values at the end of the 60-day period.

While effects on number of deposits are considerable, we find no effect of either PLS 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. Participants in the PLS-F group 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. In fact, account balances at the end of the trial are highest among the control group (mean = 14.13, std. dev. = 23.11). More frequent deposits without a corresponding increase in savings and larger withdrawals by PLS-F users suggest that they may be using their deposits to finance lottery plays and not to build a stock of savings.

Our results are consistent with a general finding among earlier field 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, allowing workers to enter the lottery, but without changes in output. Loibl et al. (2016), examined features of the Individual Development Account program in the U.S. and found no effect on savings. 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.[15]

Other experimental results, which *do* find effects on amount saved, are to some degree at odds with 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 a field experiment conducted in Haiti and identify a 22% increase in savings.[16] 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

[15]We do not measure account openings since our study opened accounts for all participants in the sample.

[16]Dizon and Lybbert (2016) emphasize that savings responded to the presence of the stochastic incentive rather than its degree of risk or the expected returns.

the return is stochastic than when it is deterministic. The experimental design of these three studies differed from this one in two respects. First, subjects in those lab studies were supplied with an endowment with which to make portfolio allocations. With only 11% of our sample receiving a regular income and with a median monthly income of USD 77, households in our study may be too liquidity constrained to sustain a larger balance with PLS[17]. Baseline correlations suggest that monthly income is predictive of savings in the mobile product, though we do not observe heterogeneity of the treatment effect conditional on income level or regularity. Second, our sample already had access to a number of competing savings products, both formal and informal, which could have dampened demand for PLS savings. To be sure, this discussion does not preclude other factors which could affect the interpretation of the results presented here.

[17]The presence of liquidity constraints is also the explanation offered in Loibl et al. (2016) to rationalize a lack of an effect for their PLS product in the US.

Table 2: Baseline balance by treatment group

	(1) PLS-N – Control	(2) PLS-F – Control	(3) PLS-N – PLS-F	(4) Control mean (SD)	(5) Obs.
Female	0.07	0.10	0.03	0.52	311
	(0.07)	(0.07)	(0.07)	(0.50)	
Age	0.78	0.72	-0.05	30.75	311
	(1.39)	(1.34)	(1.35)	(9.83)	
Completed std. 8	-0.02	-0.02	0.00	0.99	311
	(0.02)	(0.02)	(0.02)	(0.10)	
Married/co-habitating	0.10	0.09	-0.01	0.42	311
	(0.07)	(0.07)	(0.07)	(0.50)	
No. of children	0.23	0.24	0.01	1.75	311
	(0.24)	(0.25)	(0.25)	(1.70)	
Currently saves	0.05	-0.10	-0.15**	0.56	311
	(0.07)	(0.07)	(0.07)	(0.50)	
Total savings last month	-17.81	-7.04	10.77	58.82	311
	(11.88)	(12.55)	(9.23)	(106.26)	
Monthly income	-3.68	-0.59	3.09	112.05	311
	(17.63)	(16.85)	(15.46)	(137.13)	
Employment status	0.05	-0.03	-0.08	0.50	311
	(0.07)	(0.07)	(0.07)	(0.50)	
Coefficient of relative risk aversion	0.08	-0.03	-0.12	1.16	311
	(0.18)	(0.17)	(0.18)	(1.27)	
Locus of control	0.48	-0.83	-1.31	69.81	311
	(1.40)	(1.46)	(1.37)	(10.78)	
Standardized CPGI	-0.11	-0.22*	-0.11	-0.00	311
	(0.13)	(0.12)	(0.12)	(1.00)	
Exp. discount factor	-0.05*	-0.01	0.04	0.33	311
	(0.03)	(0.03)	(0.03)	(0.20)	
Joint test p-value	0.44	0.72	0.42		

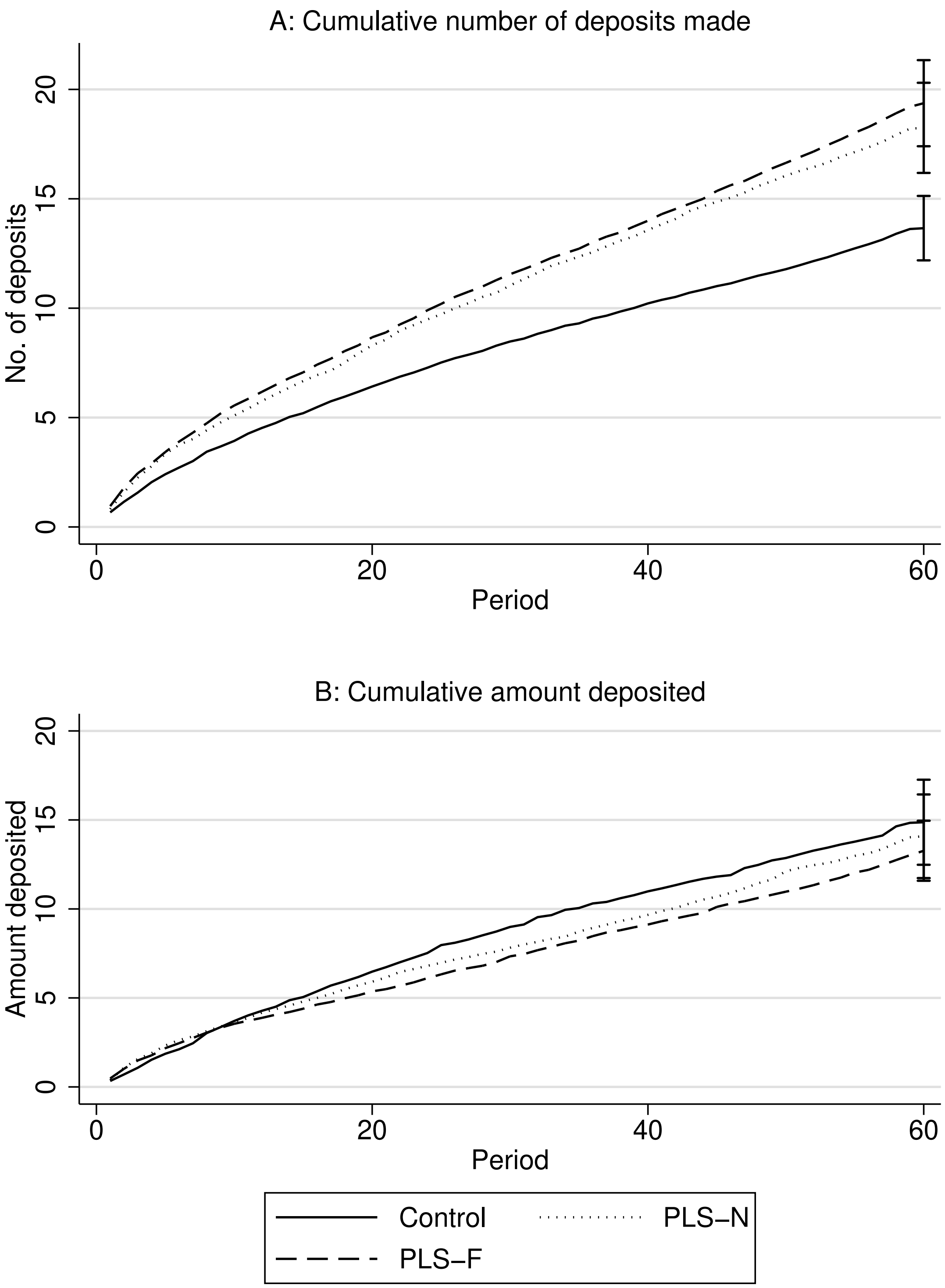
Notes: The first three columns report the difference of means across treatment groups with standard errors in parentheses. Column 4 reports the mean of the control group with SD in parentheses. The bottom row reports the p-value of a joint test of significance for each hypothesis. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table 3: Treatment effects – Mobile savings

	Effect estimates			Sample	
	(1)	(2)	(3)	(4)	(5)
	PLS-N	PLS-F	PLS-F – PLS-N	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*]	[0.89]		
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*]	[0.89]		
Total deposit amount	-0.79	-1.60	-0.81	14.87	311
	(3.34)	(2.91)	(2.88)	(24.48)	
	[0.83]	[0.59]	[0.89]		
Total withdrawal amount	0.53	1.63**	1.10	1.07	311
	(0.94)	(0.74)	(1.02)	(4.53)	
	[0.83]	[0.06*]	[0.61]		

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.

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 within one standard error of the group means.

4.2 The Role of Regret Aversion

That potential savers respond to PLS by making more frequent deposits without a corresponding increase in balance 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, but merely spread out deposits, than with the standard account. 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 consumption utility from simply playing, 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 had almost five more active days—and thus play the lottery five more times—than the control group. Unsurprisingly, participants are not making more deposits *within* days since this does not affect lottery eligibility. For these reasons we might expect the PLS to induce more deposits without a corresponding increase in amount saved.

Our study was designed to investigate the role of regret aversion as a specific mechanism explaining demand for PLS. Regret theory is a class of behavioral models that incorporate an individual's aversion to feelings of regret—an emotional response to foregone opportunities due to action or inaction. Bell (1983) and Loomes and Sugden (1982) were the first to formalize the psychological intuition as a theory of behavior that generalizes expected utility theory. Where expected utility depends only on final outcomes, (dis)utility from regret depends on the difference between the realized outcome and the best foregone outcome. Hence, theories of regret are action-based rather than purely outcome-based. Regret theory further posits that individuals can anticipate the psychological consequences of the decisions and therefore take them into account ex ante. These class of theories received attention due to their ability to rationalize behavioral deviations from expected utility theory (e.g. the common ratio and common consequences effects).

Zeelenberg et al. (1996) demonstrates how the minimization of anticipated regret can rationalize risk-seeking behavior among risk-averse individuals in situations of *asymmetric feedback*. This describes, for example, choices between a lottery and a certain outcome where choosing the lottery leads to its resolution and choosing certainty typically does not involve a resolution. Since regret derives from a comparison of foregone outcomes, choosing the lottery and receiving feedback involves the possibility of regret while choosing certainty does not. A regret-minimizing individual would thus find certainty favorable beyond that implied by her risk preferences. Conversely, providing feedback on the lottery regardless of choice (a situation of *symmetric feedback*), removes or diminishes the anticipation of regret associated with certainty. That feedback influences the anticipation of regret in this way has received empirical support in other applications (Filiz-Ozbay and Ozbay 2007; Somasundaram and Diecidue 2017; Zeelenberg and Pieters 2004).[18]

We test for the effect of lottery feedback in our own experiment as a way of quantifying the contribution of regret aversion in the demand for our PLS treatment. Recall that in the PLS-F group, the lottery involves no possibility of losses and that lotteries are resolved regardless of saving. Regret manifests when individuals choose not to save but learn about foregone winnings from the lottery. Regret-minimization predicts that individuals on the margin make deposits in avoidance of this anticipated regret. We compare saving behavior with an alternate PLS treatment (PLS-N) which provided a day's lottery results if and only if the individual made a deposit on that day. Participants in this condition anticipate that choosing not to save involves no regret since they can remain unaware of their foregone lottery earnings. As a result, regret theory predicts greater deposits with feedback than without.[19]

Column 1 of Table 3 shows that the effect of PLS-N on the number of deposits are positive but smaller in magnitude to the effect of PLS-F and are significant at the 10% level ($\hat{\beta} = 4.59, p < 0.10$). Column 3 reports on the difference between the PLS-F and PLS-N conditions, which is not statistically significant. The effect of PLS-N on number of days saved follows a similar pattern ($\hat{\beta} = 3.93, p < 0.10$) compared to PLS with feedback. Taking these point estimates at face value, approximately 20% of the PLS treatment effect can be attributed to regret aversion.

4.3 Regret Aversion with Repeated Lotteries

In the previous section we've argued that our results confirm predictions of regret theory in one-shot settings. But recent work by Imas et al. show that these predcitions become ambiguous when considering repeated settings.

Lay out their arguments and additional complications from learning and anticipation of learning.

Present effect of experienced regret. Argue that in our case it might amplify anticipated regret rather than dissuade people.

Present luck results.

Present results on first round. Anticipated regret even overcomes anticipated learning.

Discuss differences in design/population that could explain different results.

[18]Theories of regret are distinct from theories based on "disappointment aversion" (Gul 1991) in that non-separabilities arise in those models from comparisons of outcomes *within the same lottery* and not between counterfactual choices. As a result, that class of theories does not predict a behavioral response to feedback manipulation of foregone lotteries.

[19]See Section A for a formalization of this argument.

In the theory of regret outlined in the previous section, the anticipation of regret determines ex ante choice. That model makes no predictions regarding the role of recently experienced regret in individual choice. If participants feel the sting of foregone lottery winnings from yesterday, are they more likely to act on their regret aversion today?

We can leverage the panel structure of our data and the randomness of the lottery results to estimate the effect of recently experienced regret on deposits. If participants are saving in response to experienced regret of foregoing the prize, we would expect to see an independent effect of winning the lottery among non-savers on the decision to save in the PLS-F group. Let $Y_{i,t}$ denote having made a deposit by participant i in period t. $\text{Win}_{i,t-1}$ is an indicator for having a winning lottery ticket from the previous day announced in period t. We estimate the following equation conditional on assignment to the PLS group and not having saved one period prior.

$$Y_{i,t} = \pi \text{Win}_{i,t-1} + \omega_t + u_{i,t} \tag{3}$$

π is the additional effect from winning but not being able to claim the prize. ω_t is a period-specific fixed effect. We test the null hypothesis of $\pi = 0$.

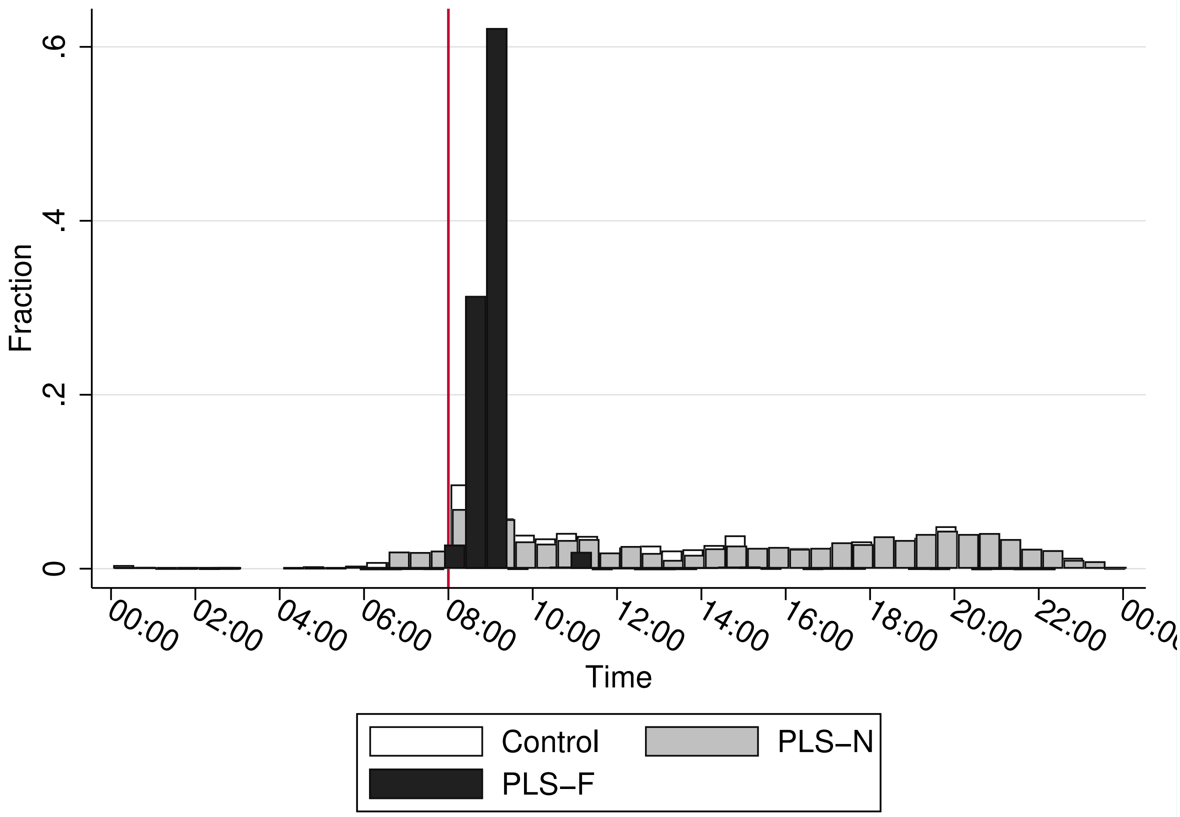
Table 4: Regression of deposits onlottery results

	Made a deposit
Winning ticket	0.02**
	(0.01)
Adjusted R^2	0.081
Control mean	0.20
PLS effect	0.08
Observations	4473

Notes: This table reports on a regression of having saved at period t on winning the lottery at t conditional on being in the PLS group and not having saved at $t-1$. The unit of observation is individual-by-period. The regression includes period fixed effects. Standard errors are in parentheses and clustered at the individual level. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table 4 shows that the treatment effect is 2 percentage points ($p < 0.05$) higher after learning about a winning ticket than learning about a losing one among non-savers. With an average daily effect of 0.08 percentage points of PLS-F, this accounts for a fourth of the group's treatment effect. Additional evidence comes from Figure 2, which plots the distribution of deposits over time and shows timing suggestive of regret aversion. Nearly all deposits made in the PLS-F group ocurred within an hour of announcing the lottery results from the previous day. We observe no similar pattern in the PLS-N and control groups. Together, the evidence suggests that recently experienced regret from foregone earnings improves the likelihood of entering the lottery in subsequent rounds.

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. A vertical line marks 8:00, when participants received the first SMS 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 PLS received a new lottery ticket with the second message.

Table 5: Regression of deposits on treatment in the first period

	No. of deposits made
PLS-N	0.15*
	(0.09)
PLS-F	0.28***
	(0.10)
Adjusted R^2	0.021
Control mean	0.67
Observations	311

Notes: This table reports on a regression of the number of deposits made in period 1 on each of the PLS treatments. The unit of observation is the individual. Standard errors are in parentheses and clustered at the individual level. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

4.4 The Effect of PLS on Consumption and Savings by Other Means

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 vis-à-vis the control group since there was no treatment effect on amount saved. This is in line with earlier experimental results (Atalay et al. 2014; Dizon and Lybbert 2016; Filiz-Ozbay et al. 2015). Table 6 reports no statistically significant effect on total saving through M-Pesa or ROSCAs. We do find, however, that respondents in PLS-F 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 the PLS-N condition. This result is robust to the inclusion of covariates but is not significant at the 10% level when correcting for multiple inference ($p < 0.10$). We find no effect of the PLS-N condition on informal saving. This finding is puzzling but one possible explanation is that participants may be borrowing from ROSCAs in order to fund their PLS deposits. It is important to note that while the lottery component per se does not displace other savings, we cannot reject the hypothesis that the introduction of mobile savings affects total saving since we do not observe a group of respondents who do not receive the product.

Table 7 reports effects on how savings from the program was used by participants after it ended. The dependent variables here are dummy variables that indicate whether savings were spent on any of these categories. While we detect some statistically significant effects on airtime and business-related expenditures, these are no longer significant once adjusted for multiple inference.

4.5 PLS Increases Self-Reported Gambling

At the end of the trial, we asked participants whether they gambled more, less, or the same as they usually do apart from participating in the savings program. We estimate a multinomial logit regression on this categorical outcome to examine how treatment affects responses. As reported in Table 8, we find that the risk of gambling more relative to no change is increased by a factor of 3.03 for participants in the PLS group compared to control ($p < 0.01$). The relative risk of gambling more also increases by a factor of 1.87 for PLS with feedback compared to without feedback. We find no average effects for participants using PLS without feedback. Examining participant input at the end of the experiment provides some evidence as to why PLS might encourage gambling. Table 9 reports that PLS users in either feedback condition feel they are more lucky compared to participants in the control group by almost 5 std. deviations[20] (PLS-F: $\hat{\beta} = 4.97, p < 0.01$; PLS-N: $\hat{\beta} = 4.77, p < 0.01$). It is plausible that winning money from

[20] This outcome was measured on a scale from 1-3 with 3 being the most lucky. Scales are standardized to mean zero and std. dev. 1 when used as outcomes in regressions.

Table 6: Treatment effects – Savings outside the project

	Effect estimates			Sample	
	(1)	(2)	(3)	(4)	(5)
	PLS-N	PLS-F	PLS-F – PLS-N	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)	
	[0.86]	[0.53]	[0.35]		
M-Pesa savings last month	-5.42	-6.71	-1.29	20.42	284
	(6.34)	(5.49)	(5.30)	(44.67)	
	[0.86]	[0.53]	[0.81]		
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.

the lottery may have altered individuals' perception of their chances of winning at large. Table 15 shows no heterogeneity of this effect by a baseline measure of problem gambling.

Atalay et al. (2014) and Dizon and Lybbert (2016) both observe large reductions in gambing expenditure in order to finance savings with PLS. 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% and credit the effect to substition of casino gambling with PLS. It is possible that while PLS is a substitute for gambling with a cash windfall (as in their study), it interacts differently when indviduals self-finance gambling. The PLS product we tested in our experiment may also be a poor substitute to other forms of gambling because the commitment feature prevents winners from immediately consuming their prizes. Our measure for gambling activity, as a self-report, is likely susceptible to experimenter demand and it is unclear in what direction this might bias our estimate. With this caveat in mind, the effect provides some experimental evidence of a complementary relationship between PLS and broader gambling behavior in our setting.

5. Conclusion

There is an abundance of evidence on the promise of product design in encouraging savings among the poor. We conducted a randomized experiment testing a prize-linked savings product with informal residents in Nairobi, Kenya. Utilizing a mobile savings platform, we randomly assigned 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 of 5% equivalent in expectation to the lottery prize so that comparing the two groups identifies the effect of stochastic incentives compared to deterministic incentives. After observing account transactions over a 60-day savings period, we find that participants in the PLS group made between 5-6 more deposit transactions than the matched payments group without a corresponding increase in amount saved. We further find no effects on amounts saved through other channels. Finally, treated participants increased their risk of gambling more than at baseline by a factor of three compared to the control group.

Comparing the two PLS treatments provides a test of regret aversion as an underlying mechanism behind the increased usage that we observe. Under this hypothesis, marginal savers will be induced to make deposits in order to minimize the anticipated regret from missing out on the prize. Our findings are consistent with regret minimization: individuals make 20% more deposits when they expect to be given feedback about lottery results regardless of their decision to save. Participants in the feedback condition are also 8% more likely to make a deposit as a result of having recently won a lottery without a prize. Our interpretation for this result is that recently experienced regret may intensify aversion to anticipated regret and further motivate deposits. Overall, we document important differences between PLS and fixed-incentive schemes when it comes to encouraging savings and show that theories of regret aversion hold sway in the design of savings products.

The welfare implications of our results are far from evident, especially since regret aversion, as a non-standard model of choice, complicates conventional normative approaches[21]. Nevertheless, our findings show that lottery-like incentives, particularly those that provide feedback on their outcomes, can be used by policymakers to influence financial decisions. Since PLS motivates deposits through the presence of a lottery, it could prove to be more cost-effective than a fixed subsidy of an equivalent, expected size. The caveat to this is that effectiveness of "regret lotteries" from an institutional perspective depends on the degree of regret aversion among their clientele. That statistic, to our knowledge, has yet to be measured.

[21] We direct readers to Bernheim (2009) for a discussion of recent behavioral approaches to welfare. Sarver (2008) in particular describes the tension between decisions over menus that preclude regret and decisions from a given menu that anticipate regret.

If PLS increases deposits but are ineffective at increasing the savings rate, can they still useful from a development perspective? Two recent field experiments suggest so. Schaner (2018) finds moderate short run effects of interest rate subsidies on savings but documents impressive effects on income and capital stock over two years after the incentive expired. They argue that the temporary incentives accomplished increased long run account usage through habit formation. Gertler et al. (2017) observes initially lower account balances among individuals who opened PLS accounts compared to control, but the 41% additional open accounts induced by PLS continued to be used in the long run and maintained balances comparable to the control group. If PLS opens the door for greater account usage in the short run (as we report in this study), then it could lead to persistent changes in financial decisionmaking over time.

The prevalance of gambling may also be a policy outcome of interest in its own right. PLS may be disadvantageous from this perspective if it merely represents another consumption opportunity for gambling and not as a vehicle to accumulate savings. While there is evidence from both the lab and the field that PLS displaces other forms of gambling as its substitute, we provide limited evidence that individuals with PLS accounts gamble relatively more than the control group.

Our study design is limited in that we could not observe participants' entire portfolio of financial decision over the trial period. That type of data could allow us to understand, for example, how lottery plays were being financed, how PLS affected the effective savings rate over time, and whether PLS changed participants' pattern of daily consumption. Moreover, the data could be used for structural estimation of behavioral parameters (as in Filiz-Ozbay et al. (2015)) in a mdoel of regret aversion. As discussed previously, a long-term examination of outcomes is promising and could have material consequences for welfare. Taking a longer perspective could also prove useful in the study of regret aversion over repeated settings, an area of research still at the frontier of the behavioral literature.

Table 7: Treatment effects – Expenditure

	Effect estimates			Sample	
	(1)	(2)	(3)	(4)	(5)
	PLS-N	PLS-F	PLS-F – PLS-N	Control Mean (SD)	Obs.
Airtime	-0.33**	-0.13	0.20*	0.35	284
	(0.15)	(0.19)	(0.12)	(1.47)	
	[0.36]	[0.99]	[0.74]		
Business-related	0.08*	0.10**	0.02	0.06	284
	(0.04)	(0.05)	(0.05)	(0.25)	
	[0.67]	[0.37]	[1.00]		
Durable goods	-0.06	-0.01	0.05	0.13	284
	(0.04)	(0.05)	(0.04)	(0.34)	
	[0.85]	[1.00]	[0.90]		
Loan repayment	-0.01	-0.02	-0.01	0.09	284
	(0.04)	(0.04)	(0.04)	(0.28)	
	[0.98]	[1.00]	[1.00]		
Food	0.04	-0.08	-0.12*	0.28	284
	(0.07)	(0.06)	(0.06)	(0.45)	
	[0.98]	[0.91]	[0.66]		
Rent and housing payments	-0.03	-0.00	0.03	0.11	284
	(0.04)	(0.04)	(0.04)	(0.31)	
	[0.98]	[1.00]	[0.99]		
Health-related	-0.02	-0.03*	-0.01	0.03	284
	(0.02)	(0.02)	(0.01)	(0.18)	
	[0.97]	[0.75]	[0.97]		
Other non-durables	0.01	0.03	0.02	0.01	284
	(0.02)	(0.02)	(0.03)	(0.10)	
	[0.98]	[0.89]	[0.99]		
Saved balance	0.04	0.06	0.02	0.07	284
	(0.04)	(0.04)	(0.05)	(0.26)	
	[0.97]	[0.89]	[1.00]		
School-related	0.08	0.02	-0.06	0.12	284
	(0.05)	(0.05)	(0.05)	(0.32)	
	[0.85]	[1.00]	[0.92]		
Transfers	0.02	-0.00	-0.02	0.02	284
	(0.03)	(0.02)	(0.03)	(0.15)	
	[0.98]	[1.00]	[0.99]		
Travel	-0.00	-0.00	0.00	0.02	284
	(0.02)	(0.02)	(0.02)	(0.15)	
	[1.00]	[1.00]	[1.00]		
Did not save	-0.02	-0.01	0.01	0.10	284
	(0.04)	(0.04)	(0.04)	(0.30)	
	[0.98]	[1.00]	[1.00]		

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.

Table 8: Multinomial treatment effects – Gambling behavior

	Relative risk ratio				Sample
	(1)	(2)	(3)	(4)	(5)
	Constant	PLS-N	PLS-F	PLS-F – PLS-N	Obs.
Gambled less	0.22***	0.91	1.69	1.86	284
	(0.06)	(0.38)	(0.66)	(0.76)	
Gambled more	0.16***	1.62	3.03***	1.87*	284
	(0.05)	(0.69)	(1.23)	(0.69)	

Notes: This table reports estimates from a multinomial logit regression of the categorial response on treatment assigment. Each row corresponds to a response category with the baseline value as . Column 1 reports the constant term corresponding to the mean of the control group. Columns 2–3 reports the treatment effect in relative risk ratios compared to the control group. Column 4 reports the difference between the two PLS treatments. Standard errors are in parentheses. Column 5 reports the number of observations in the analytic sample. Observations are at the individual level. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

Table 9: Treatment effects – Self-perceptions

	Effect estimates			Sample	
	(1)	(2)	(3)	(4)	(5)
	PLS-N	PLS-F	PLS-F – PLS-N	Control Mean (SD)	Obs.
Do you see yourself as a saver?	-0.20	-0.09	0.11	-0.00	284
	(0.15)	(0.14)	(0.15)	(1.00)	
	[0.48]	[0.90]	[0.77]		
Are you in general a lucky person?	4.77***	4.97***	0.20	-0.00	284
	(0.20)	(0.18)	(0.23)	(1.00)	
	[0.00***]	[0.00***]	[0.77]		
Do you feel you saved enough?	0.19	-0.09	-0.28*	0.00	284
	(0.15)	(0.15)	(0.15)	(1.00)	
	[0.48]	[0.90]	[0.23]		
How did you feel not saving?	-0.02	0.06	0.08	-0.00	284
	(0.16)	(0.15)	(0.16)	(1.00)	
	[0.88]	[0.90]	[0.77]		

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.

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A. A Simple Model of Saving with PLS

In this section, we formalize the intuition behind how feedback on lottery results works to increase the number of deposits to PLS accounts. We present a simple model of an individual facing a one-shot, binary decision to save or not save with PLS with and without feedback.

Regret-sensitive preferences are defined as follows. Let $f, g \in B$ be prospects in the individual's choice set. i indexes the events in a finite, discrete state space. An individual who chooses f when event i occurs evaluates outcomes according to a bivariate utility function.[22]

$$Q(f_i; g_i) \equiv u(f_i) + \gamma R(u(f_i) - u(g_i))$$

f_i is the return on the deposit when event i occurs, u is a von Neumann-Morgenstern utility function, and R is a regret function that evaluates utility differences between outcomes from the selected lottery to outcomes from the foregone lottery, state-by-state. $\gamma \geq 0$ is a parameter that governs the degree of regret/rejoicing.

Define $\Psi(f_i; g_i) \equiv Q(f_i; g_i) - Q(g_i; f_i)$ as the advantage, in utility terms, of obtaining f_i over g_i . Regret theory holds if there exists a continuous, strictly increasing function u and a continuous, strictly increasing, and skew-symmetric function Ψ such that

$$f \succsim g \leftrightarrow \sum_i p_i \cdot \Psi(f_i; g_i) \geq 0$$

where p_i is the probability of an event i. Skew-symmetry implies $R(0) = 0$ and means that no regret/rejoicing is experienced when outcomes are identical. The convexity of Ψ over gains corresponds to regret aversion. When Ψ is linear ($\gamma = 0$ in this parameterization), the individual behaves as if she were maximizing expected utility.

When an individual in the PLS treatment makes a deposit, her payoff y is proportional to a match rate applied to the deposit. The rate (and payoff) is determined by a lottery $\ell = (y_i, p_i)_{i=1}^4$ defined over the event of no matches, one match, two matches, and four matches of a sequence of numbers in a lottery ticket to a randomly generated sequence. When she chooses not to make a deposit, the individual obtains zero returns with certainty.

How does feedback regarding the resolution of the lottery affect anticipated regret? We postulate that when the decisionmaker makes a choice that obscures the resolution of the lottery that she is unable to make comparisons to foregone outcomes. In our model, a decision that results in no feedback is associated with an expected utility over final

[22]The theoretical literature does not typically make such a restrictive functional form assumption but we do so here for expositional purposes.

outcomes of $\sum_i p_i \cdot u(f_i)$.[23]

The expected utility associated with choosing to save with PLS in both the feedback and no feedback treatments is equivalent since saving will result in the resolution of the lottery in both conditions.

$$\sum_{i=1}^{4} p_i \cdot [u(y_i) + \gamma R(u(y_i) - u(0))]$$

The expected utility associated with not saving with feedback is

$$u(0) + \sum_{i=1}^{4} p_i \cdot \gamma R(u(0) - u(y_i))$$

but not saving without feedback attains the decisionmaker $u(0)$. Note that since there is no possibility of negative returns in the lottery, $R(u(0) - u(y_i)) \leq 0$ for all i. As a result, the expected utility from not saving without feedback is weakly less than the utility from not saving with feedback.

$$\sum_i p_i \cdot \Psi_{\text{PLS-F}}(y_i; 0) \geq \sum_i p_i \cdot \Psi_{\text{PLS-N}}(y_i; 0)$$

The relationship is a strict inequality if $\gamma > 0$: the individual exhibits some degree of regret aversion. This expression encapsulates our hypothesis that with regret aversion, marginal decisionmakers are more likely to make a deposit under PLS with feedback than PLS where feedback is conditional on saving.

[23]Original theories of regret (Bell 1983; Loomes and Sugden 1982) do not explicitly model the role of feedback on decisions. In light of empirical evidence, some applications disallow the calculation of the regret term when foregone lotteries remain unresolved as we do here (Filiz-Ozbay and Ozbay 2007; Strack and Viefers 2019) while others impose a less extreme assumption that regret/rejoicing is less intense without feedback (Humphrey 2004; Somasundaram and Diecidue 2017).

B. Additional Tables

Table 10: Deviations from pre-analysis plan

Pre-analysis plan	Modification	Reason for modification	Location(s)
Regression equations would include the baseline level of the individual outcome (ANCOVA) when available to increase statistical power.	The four outcome variables for which we do have baseline analogs (total saved in the previous month, amount saved with M-Pesa, amount saved in a ROSCA, use of ROSCA) were estimated without baseline levels of the dependent variables.	The regression equations were changed for these variables to allow the analysis to be consistent across all outcomes.	Table 6
We specified secondary analysis that involved estimating an autoregressive finite distributed lag model on savings outcomes at the daily level.	We estimate linear autoregressions without lagged treatment variables.	There is no treatment variation over time.	Supplementary Appendix Fig. 9-10
We included as an outcome variable the highest number of consecutive days in which an individual made a deposit.	We omitted this variable from the analysis.	There is little theoretical interest in the effect on this variable.	N/A

Multinomial
Randomization inference
cov adjustment
Regression on first day
Regression on winning lottery

Table 11: 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.

Table 12: Heterogeneous effects – Primary outcomes by currently saves

	(1) Total no. of deposits	(2) Total deposit amount	(3) Saves with a ROSCA	(4) Gamble more
PLS-N	8.07**	2.05	-0.00	0.06
	(4.07)	(4.49)	(0.11)	(0.07)
PLS-N ×				
Currently saves	-6.16	-5.25	-0.03	-0.00
	(5.23)	(6.57)	(0.15)	(0.10)
PLS-F	8.26**	3.42	0.24**	0.18**
	(3.23)	(3.77)	(0.10)	(0.07)
PLS-F ×				
Currently saves	-4.32	-9.25	-0.19	-0.06
	(4.87)	(5.64)	(0.14)	(0.11)
Currently saves	5.62**	7.44	0.14	0.09
	(2.82)	(4.56)	(0.10)	(0.06)
Constant	10.50***	10.69***	0.47***	0.07*
	(1.79)	(2.73)	(0.08)	(0.04)
Adjusted R^2	0.009	-0.004	0.015	0.015
Control mean	13.66	14.87	0.54	0.12
PLS-N p-value	0.56	0.51	0.71	0.45
PLS-F p-value	0.28	0.17	0.60	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.

Table 13: Heterogeneous effects – Primary outcomes by risk averse

	(1) Total no. of deposits	(2) Total deposit amount	(3) Saves with a ROSCA	(4) Gamble more
PLS-N	7.87**	-0.13	-0.10	0.08
	(3.63)	(4.91)	(0.10)	(0.08)
PLS-N ×				
Risk averse	-7.63	-1.99	0.18	-0.05
	(4.92)	(6.58)	(0.15)	(0.10)
PLS-F	7.83**	-3.09	0.13	0.15*
	(3.50)	(4.14)	(0.10)	(0.08)
PLS-F ×				
Risk averse	-4.62	3.09	0.02	-0.01
	(4.89)	(5.87)	(0.14)	(0.11)
Risk averse	0.50	-3.87	0.01	-0.05
	(2.97)	(4.76)	(0.10)	(0.07)
Constant	13.42***	16.71***	0.54***	0.14***
	(1.99)	(3.50)	(0.07)	(0.05)
Adjusted R^2	0.017	-0.007	0.016	0.015
Control mean	13.66	14.87	0.54	0.12
PLS-N p-value	0.94	0.63	0.41	0.65
PLS-F p-value	0.35	1.00	0.14	0.07
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.

Table 14: Heterogeneous effects – Primary outcomes by above median indiff. point

	(1) Total no. of deposits	(2) Total deposit amount	(3) Saves with a ROSCA	(4) Gamble more
PLS-N	3.06	-8.41	0.02	0.08
	(3.10)	(5.14)	(0.10)	(0.07)
PLS-N ×				
Above median indiff. point	3.71	16.03**	-0.07	-0.03
	(5.23)	(6.95)	(0.15)	(0.10)
PLS-F	9.75***	-4.02	0.24**	0.19**
	(3.47)	(5.07)	(0.10)	(0.08)
PLS-F ×				
Above median indiff. point	-7.98	4.62	-0.20	-0.09
	(4.88)	(5.88)	(0.14)	(0.11)
Above median indiff. point	0.63	-8.85*	0.08	0.02
	(2.95)	(4.82)	(0.10)	(0.07)
Constant	13.33***	19.42***	0.50***	0.11**
	(1.97)	(4.51)	(0.07)	(0.05)
Adjusted R^2	0.018	0.010	0.011	0.010
Control mean	13.66	14.87	0.54	0.12
PLS-N p-value	0.11	0.10	0.66	0.55
PLS-F p-value	0.61	0.84	0.70	0.22
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.

Table 15: Heterogeneous effects – Primary outcomes by above median cpgi

	(1) Total no. of deposits	(2) Total deposit amount	(3) Saves with a ROSCA	(4) Gamble more
PLS-N	2.53	-1.40	-0.09	-0.01
	(3.29)	(3.45)	(0.10)	(0.07)
PLS-N ×				
Above median CPGI	4.38	1.69	0.16	0.16
	(5.22)	(7.00)	(0.15)	(0.11)
PLS-F	6.17*	0.79	0.13	0.11
	(3.59)	(3.44)	(0.10)	(0.08)
PLS-F ×				
Above median CPGI	-1.79	-5.46	0.03	0.07
	(4.79)	(5.89)	(0.14)	(0.12)
Above median CPGI	-2.88	2.49	0.00	-0.06
	(2.93)	(4.86)	(0.10)	(0.07)
Constant	15.06***	13.66***	0.54***	0.15***
	(2.27)	(2.55)	(0.07)	(0.05)
Adjusted R^2	0.009	-0.010	0.013	0.014
Control mean	13.66	14.87	0.54	0.12
PLS-N p-value	0.09	0.96	0.48	0.06
PLS-F p-value	0.17	0.33	0.13	0.03
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.