Leveraging the Lottery for Financial Inclusion: Lotto-Linked Savings Accounts in Haiti

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I. Introduction

About 80% of the Haitian population lacks access to formal financial services (Demirgüç-Kunt et al. 2014). But where banks are scarce, lotto stalls are abundant. Wagers on lotto numbers are the most familiar financial transaction for the working poor. We conducted a framed lab-in-field experiment among the working poor in Port-au-Prince to test whether lotto-linked savings (LLS) can leverage this familiarity with the lotto to divert wagers to higher-returning savings accounts. In contrast to a traditional savings account with a fixed and certain interest rate, an LLS account offers a lottery credit in lieu of interest payments. This experiment explores whether LLS may catalyze greater financial inclusion by providing the working poor who regularly play the lottery a compelling and familiar gateway to formal savings and ultimately to broader financial services and products.

Savings products that offer a chance of winning a prize in lieu of interest, generally referred to as prize-linked savings (PLS), have existed in different forms since the eighteenth and nineteenth centuries in Europe (Guillen and Tschoegl 2002; Kearney et al. 2010) and have been offered and piloted more

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widely in recent decades.[1] In Sweden, for example, an estimated 8% of the total government debt is in the form of bonds with lottery coupons (Green and Rydqvist 1997). In Latin America, it is often raffles for in-kind prizes (e.g., gold, cars) rather than cash lotteries that provide the random incentive to save. Until 2014, legal and regulatory barriers in the United States made it difficult to offer PLS products, but recent pilots launched by credit unions in various states show promise.[2] In a pilot in Nebraska, 56% of individuals who saved in response to a "Save to Win" pilot had no formal savings at baseline, suggesting extensive margin expansion in access to and usage of savings in response to a PLS product (Cookson 2018).[3] However, a large experiment in California with individual development accounts that included a lottery feature finds no effect on total savings on either intensive or extensive margins, ostensibly because of other binding constraints (Loibl, Jones, and Haisley 2018).

Outside of North America and Europe, there is some evidence of PLS-induced savings effects among the emerging middle class. Commercial banks in upper-middle-income countries, such as Mexico, Argentina, and South Africa, have launched PLS pilots and programs. First National Bank in South Africa, for example, launched its "Million-a-Month" PLS product in 2005. On the basis of this 18-month pilot, an evaluation found that the PLS product increased savings by 38% and that this savings increase was financed primarily by a reduction in traditional lottery ticket purchases (Cole, Iverson, and Tufano 2014).[4] Banco del Ahorro Nacional y Servicios Financieros (Bansefi) of Mexico conducted a randomized controlled trial to test the impact of a PLS promotion on savings behavior. This 2-month promotion attracted new bank accounts from new customers, who continued to save long after the promotion ended (Gertler et al. 2019).

While this is encouraging for PLS products in general, the relevance of this evidence for our study is limited by the fact that the underlying products are

[1] For example, in the seventeenth and eighteenth centuries, England and France issued various securities that paid a premium to a subset of holders instead of paying a set interest rate. At the close of the nineteenth century, versions of PLS existed across much of Europe (Levy-Ullman 1896). The United Kingdom Premium Savings Bonds still exist today. These bonds pay a certain interest and automatically enroll the holder in a monthly lottery with prizes ranging from £50 to £1 million. Guillen and Tschoegl (2002) detail the variants of PLS products that have been offered in Mexico, Colombia, Venezuela, Argentina, Sweden, Japan, Germany, Turkey, Kenya, Indonesia, Spain, and Pakistan.

[2] The 2014 America Savings Promotion Act (HR 3374) specifically permits the use of "savings promotion raffles."

[3] For details about this program, see https://www.savetowin.org. The program is currently available for credit unions in 10 US states.

[4] Although it was successful as a pilot, it ultimately failed to translate into a commercial product due in part to pressure from the official South African lottery system.

designed as savings promotions to appeal to primarily middle-class clients with preexisting access to financial services. The South African study targeted bank employees with average individual incomes more than double the average national household income. In sharp contrast, the target population in our study is poor, food insecure, and almost completely unbanked, and they frequently wager a large share of their daily income on the lotto (33% according to Bernstein 2015). As a result, the LLS product we test in Haiti differs considerably—in both design and intended effect—from seemingly similar PLS products in middle- and high-income contexts.

Instead of simply testing the efficacy of PLS as a savings promotion among the poor in a low-income context, we aim to evaluate a lottery-linked version that is specifically designed to leverage Haitians' familiarity with and passion for their lottery in order to divert wagers to savings as a first step toward financial inclusion. Haitians spend as much as US$1.5 billion per year at over 35,000 independently owned lottery stalls (Bhatia 2010). This is equivalent to US$220 per year for each individual over age 15, a staggering figure in a country where the per capita gross domestic income is US$810. With well-known and consistent rules, the Haitian lottery, which uses numbers drawn in the New York lottery, is transparent and offers known odds in a country where instability and corruption characterize many institutions and uncertainty defines daily life (Wilson and Levin 2010). Our LLS product takes this pervasive Haitian lottery as its point of departure and offers lotto credit rather than prize raffles in lieu of interest. This prototype LLS product, which intentionally builds on the basic Haitian lottery game and is designed to be scaled conceivably as a commercial product, aims to reveal the viability of savings as a pathway to wealth accumulation to those currently staking their future on lotto wagers.

Whereas lottery play and gambling more generally were rarely considered serious topics for empirical economic research, the popularity of behavioral economics and finance in the past decade seems to have legitimized this vein of work, including research on LLS-type products (Kearney et al. 2010; Tufano, De Neve, and Maynard 2011). Among poor populations more comparable to our research setting in Haiti, two such studies are worth highlighting. First, Brune (2015) conducts a randomized controlled trial (RCT) with 1,600 piece-rate workers at a large tea-producing firm in Malawi to test the effect of bonuses on worker performance. He finds some (weak) evidence that lottery bonuses increased worker attendance more than fixed bonuses. Second, Herskowitz (2021) explores the motives behind gambling on professional soccer matches in Kampala, Uganda. This study of 1,700 men who regularly bet on soccer matches finds that such betting may be a rational response for people with binding liquidity constraints who are striving to purchase a lumpy good. Through a mix of RCT and lab-in-field evidence, the role of both a lack of savings options and a target lumpy expenditure in observed gambling behavior is clear. Our analysis aims to contribute to this emerging area of research, which considers lottery play and gambling impulses among the poor to be legitimate research topics with important welfare and, potentially, policy implications.

We conduct an experiment in which participants make a series of portfolio allocation decisions that include lottery and LLS options. Specifically, participants in the experiment allocated 300 Haitian gourdes (HTG; US$4.8) across (i) consumption paid directly to participants in 2 weeks, (ii) a real-world lotto credit available for participants to use in 2 weeks, (iii) a traditional savings product that pays the principal with interest in 8 weeks, and (iv) the LLS product, which paid the principal—either partially or entirely—along with lotto credit in 8 weeks.[5] To leverage familiarity with the lotto, the lottery options in the experiment consisted of the most basic and most familiar lotto product in Haiti. The LLS product allows participants to "invest" their lotto credit on the basis of numbers of their choosing, since number selection is a critical aspect of the Haitian lotto culture (Bhatia 2010). We find that the introduction of LLS increased total savings by 22%, an increase in savings that is financed by reductions in the amount allocated to lotto and to traditional savings along with smaller reductions in consumption. This LLS-induced increase in savings is nearly double the savings response we observe in the experiment with an increase in the (conventional) interest rate from 5% to 20%.[6]

In addition, our experiment allows us to test for the effects of different LLS features on total savings. First, we compare two LLS products that differed in the extent of the lotto component. One LLS product had a higher savings-to-lotto ratio wherein more of the principal was secured. We find that such an LLS product with a higher savings-to-lotto ratio resulted in a larger increase in total savings, suggesting that a reduction in the intensity of the lotto component did not reduce the amount allocated to LLS. In other words, the presence of a small lottery component appears to be sufficient to trigger a sizable increase in savings. Second, we compare two LLS products that differed in their expected return relative to the traditional savings product. One LLS product had an expected return equal to the traditional savings product, while another offered a lower expected return. We find no difference in total savings between these

[5] Our experiment is largely inspired by the design in Atalay et al. (2014). In contrast to this online experiment in the United States, our experiment involved a real-world lotto product with a version of LLS offered to the working poor in Haiti.

[6] The LLS-induced increase in savings of 22% is the estimated difference in savings between decision rounds with LLS (rounds 3–6) and rounds without LLS (rounds 1 and 2). The estimated effect of increasing the interest rate from 5% to 20% is 12%, which is estimated as the difference between rounds with a 5% interest (rounds 2, 4, and 6) and rounds with a 20% interest (rounds 1, 3, and 5).

two LLS products, again suggesting that individuals respond to the presence of a lotto component rather than to the extent of this component.

Using a risk experiment adapted from Tanaka, Camerer, and Nguyen (2010), we explore which mechanisms might be driving the effect of LLS on total savings. On average, individuals in our sample were, by these experimental measures, risk loving and tended to overweight small probabilities. We do not find any evidence that the effects of LLS on total savings differ by risk aversion. However, we find that the effect of LLS on total savings was higher for those who more heavily weighted small probabilities. This implies that such a behavioral bias may be driving the effectiveness of LLS we observe. This result is in line with a growing literature on the design of interventions that prove to be effective at increasing savings by acknowledging and leveraging behavioral biases, such as self-control and inattention (Ashraf, Karlan, and Yin 2006; Dupas and Robinson 2013; Karlan et al. 2016; Dizon, Gong, and Jones 2020). Those who allocated more to lottery than to savings before LLS also increased savings more aggressively when exposed to LLS.[7]

II. Experiment and Data

Our paper is one of three studies that experimentally evaluate the impact of a savings product with a lottery component. All three studies find that a lottery component increased total savings. Filiz-Ozbay et al. (2015) conducted lab experiments with students at a US university. They find that a .01% probability of receiving a large prize causes subjects to increase their savings by about 4% relative to a traditional savings device with the same expected return. Moreover, they—like we—find that nonlinear probability weighting is likely driving the result. Atalay et al. (2014) conducted online experiments with a sample representative of the US population and with another sample that had lower income and less savings. They find that introducing an LLS product increased total savings by 25%, quite similar in magnitude to our own finding. Our portfolio allocation game closely followed the experimental design of Atalay et al. (2014), but we used fewer decision rounds and additionally elicited risk preferences to further understand the underlying mechanism for the result.

Our research setting—one of the poorest and most food-insecure countries in the world—stands in stark contrast to these other studies. Unlike Atalay et al. (2014) and Filiz-Ozbay et al. (2015), we combine a real-world lotto product,

[7] The correlation between overweighting small probabilities and initial allocation to lottery before LLS is low, indicating that these two measures are not one and the same. Both the overweighting of small probabilities and the initial allocation to the lottery may be operating in different ways to increase the effectiveness of the LLS product in increasing savings.

instead of experimentally designed lottery payoffs, with existing mobile money accounts as the basis for a prototype LLS product designed to appeal to the Haitian poor who stake their financial future on lotto wagers instead of savings. In July 2016, we conducted lab-in-field experiments with this LLS product with 306 participants in four different locations in Port-au-Prince. In each location, we conducted four sessions, one in the morning and another in the afternoon across 2 days. Each of the 16 sessions had 16–20 participants, with an average of 19 participants per session. Local mobilizers recruited participants before each session.[8] The mobilizers were instructed to recruit lottery players who had a mobile phone and a Digicel SIM card. Digicel is the largest mobile service provider in Haiti, with a 72% market share.[9]

A. Registration and Survey

Upon arriving in the session, the participant went through the registration process, where an enumerator took down the participant's details. Our experiment leveraged Mon Cash, a mobile money platform operated by Digicel in partnership with Scotiabank. The Mon Cash platform allows for deposits, withdrawals, transfers, airtime purchase, payment of goods and services, and receipt of international transfers via mobile phone transactions. The enumerator first transferred a show-up fee of 158 HTG to the participant's Mon Cash account.[10] This was done to build trust in the payment system that we would use in the experiments and to verify that the participant's Mon Cash account was functional.

The enumerator also activated the participant's short message service (SMS)-lotto account, if it was not already activated. This account is used to wager on a lotto conducted via SMS that is operated by a licensed Haitian company. Using a mobile phone and a Digicel SIM card, individuals can play various lottery games on the SMS-lotto platform. The SMS-lotto platform offers five different lottery games, three of which are well-known *borlette* games traditionally played in lottery stalls across Haiti, namely, *bolet* (and *maryaj*), three *chiffres*, and five *chiffres*.

[8] The sessions took place in four different areas in Port-au-Prince, and each area had a different assigned mobilizer. We would first secure a venue in an area (say, a school), and then the mobilizer would recruit individuals close to the venue. The mobilizers were less technically skilled than the enumerators, and they were never in the enumerator training sessions. As such, they were unaware of the purpose and content of the study. Mobilizers were further instructed with the details provided in the "Recruitment" section of the "Experimental Protocol and Script," provided in the appendix.

[9] Digicel has operations in 31 markets in Central America and the Caribbean and in the Asia-Pacific region.

[10] Throughout this paper, we use Haitian gourdes as the currency. At the time of the study, the exchange rate was US$1 = 63 HTG. The show-up fee was 150 HTG, and an additional 8 HTG was paid to the participant to cover the cost of withdrawing the funds from a Mon Cash agent.

We explain the bolet game in more detail below.[11] After the registration process, which verified that the participant had active Mon Cash and SMS-lotto accounts, the participant was assigned to another enumerator, who conducted a brief survey. On average, this survey took about 10 minutes to complete and included questions on demographic characteristics, income, food security, assets, lottery play, savings, credit, and time preferences.[12]

B. Portfolio Allocation Decisions

After all of the participants in a session had completed the survey, a group session was conducted where the facilitator explained the portfolio allocation game. Each participant would be asked to individually make a series of six decisions. In each decision, the participant would allocate 300 HTG in denominations of 10 HTG across three or four of the following financial products, which varied in the rate of return, risk, and timing of payments:

Consumption. Participant receives the full principal in 2 weeks via Mon Cash.

Lotto. Participant receives the full principal as SMS-lotto credit in 2 weeks.

Savings. Participant receives the full principal plus interest in 8 weeks via Mon Cash.

LLS. Participant receives the principal—either partially or entirely—in 8 weeks via Mon Cash plus SMS-lotto credit cum interest in 8 weeks.

The amount allocated to consumption was paid back in 2 weeks, instead of immediately, to ensure that individuals did not allocate to consumption simply because they did not trust that any delayed payments would be made. Our experiment thereby largely reduces the role that trust in future payments being made has on explaining the differences in allocations between consumption and savings. This delay in consumption payout also reduces the role that present bias plays in the allocations.

Since the lotto product used in the experiment was the familiar bolet game, it needed little explanation to participants. In the bolet game, an individual chooses a two-digit number from 00 to 99 and an amount to bet on that chosen number. The winning numbers are based on the New York lottery, which draws

[11] See Bernstein (2015) for a more extensive discussion on the Haitian lottery and these different game types.

[12] As a strategy used to ensure that a session was completed in a timely manner, the survey was conducted for some participants after instead of before the portfolio allocation game. The survey was conducted for 45% of all participants after the portfolio allocation game. This should not matter in the estimations of effects below because we use a fixed effects specification. Robustness checks, available upon request, show that results are similar to a random effects specification, which includes a dummy variable for whether the survey was conducted after the game.

three winning numbers, where the first is a three-digit number and the second and third are two-digit numbers. If the chosen number matches the last two digits of the winning number in the first position, then you win 50 times your bet; if it matches the winning two-digit number in the second position, then you win 20 times your bet; and if it matches the winning two-digit number in the third position, then you win 10 times your bet. There are two drawings every day, one at 12:30 p.m. and another at 7:00 p.m. The net expected return on this simple bolet game is −20%.[13] With the aid of SMS-lotto promotional flyers, the facilitator explained to each group how to play this familiar bolet game on a mobile phone. In practice, individuals could later play any game in the SMS-lotto platform with the SMS-lotto credit they receive. Using administrative data generated from each participant's SMS-lotto account, we find that 91% of those who used the SMS-lotto credit they received used it to play the bolet game, validating the use of the bolet game as the basis for our calibration of the return of the prototype LLS product. This provides support for our benchmarking of the LLS product to traditional savings.

In the group session, the facilitator explained only the basic form of each financial product. After which, individuals were assigned to an enumerator who asked the participant to make three practice decisions to familiarize participants with the game and then six actual decisions with real payouts. All decisions were made in private and recorded by enumerators on tablets. Across each decision round, we varied which products were offered and the features of the savings and LLS products. The features of the financial products, including gross expected returns, across each decision round are summarized in table 1.

The LLS product offered a combination of some security of the principal and receiving some percentage of the principal as lottery coupons. For example, for "same-return" LLS in decision round 3, the amount allocated to LLS would pay back 65% of the principal in 8 weeks and provide the participant with SMS-lotto credit worth 50% of the amount allocated to LLS. This particular LLS configuration yields a net expected return of 5%. In each decision round where the LLS product was offered, the enumerator explained to the participant the proportion of the principal that would be secured and the proportion of the principal that would be paid out as lotto credit, but the enumerator did not mention the expected return for LLS.

To test for the effects of LLS, we compare rounds with and without LLS. Mechanically, this entails comparing rounds with four options versus rounds with only three options. While a simple allocation rule (i.e., an $n + 1$ heuristic) might

[13] For every x allocated to bolet, it pays $-x$ with 0.97 probability, $49x$ with 0.01 probability, $19x$ with 0.01 probability, and $9x$ with 0.01 probability. Thus, the bolet pays $-0.2x$ in expectation.

TABLE 1
FINANCIAL PRODUCTS ACROSS DECISION ROUNDS

Round	Consumption, 2 Weeks (% Return)	Lotto, 2 Weeks (% Expected Return)	Savings, 8 Weeks (% Return)	LLS Same Return as Savings, 8 Weeks (% Expected Return/% from Principal, % from Lotto)[a]	LLS Lower Return than Savings, 8 Weeks (% Expected Return/% from Principal, % from Lotto)[a]
Practice:					
A	100	80			
B	100		105		
C	100		120		
Pre-LLS:					
1	100	80	105		
2	100	80	120		
With high-risk LLS:					
3	100	80	105	105/65, 50	100/60, 50
4	100	80	120	120/80, 50	115/75, 50
With low-risk LLS:					
5	100	80	105	105/85, 25	100/80, 25
6	100	80	120	120/100, 25	115/95, 25

Note. LLS = lotto-linked savings.
[a] Between-session variation.

influence the results, we take as given the design and results of Atalay et al. (2014), which show that the $n + 1$ heuristic does not drive the results.[14] Also note that unlike most PLS products, the LLS product we test deliberately focuses on configurations that return only part of the principal. This is part of envisioning how this work can inform the design of a real-world LLS product, which considers that existing lotto products in Haiti do pay negative expected returns.

We rely on variation in product design to test the effect of specific features of the traditional savings and LLS products on total savings. First, the LLS product in decision rounds 5 and 6 had less of a lottery component and more of a savings component than the LLS product in decision rounds 3 and 4. That is, the LLS product in decision rounds 5 and 6 secured more of the principal and provided less lotto credit. This allows us to test for the effectiveness of an LLS product by the degree of its lotto component. Additionally, the order of decision rounds was randomized within session. Some individuals made decisions following order A (series 1, 2, 3, 4, 5, and 6), while others made decisions following order B (series 1, 2, 5, 6, 3, and 4). This allows us to control for ordering effects when testing for the effectiveness of an LLS product by the degree of its lotto component. Second, for morning sessions, the same-return LLS was used, so that in each round, the expected return of the LLS product was equal to the

[14] Additionally, analysis of heterogeneous effects helps uncover potential mechanisms that drive allocations to LLS that are beyond the $n + 1$ heuristic. See sec. V.

return on the savings product. For afternoon sessions, the "lower-return" LLS was used, so that in each round, the expected return of the LLS product was less than the return on the savings product. This allows us to test for the effectiveness of an LLS product that would cost a financial service provider less than a traditional savings account.[15] Last, we varied the interest rate of the savings product across rounds. This allows us to test the effectiveness of an LLS product depending on the return on traditional savings.

During the one-on-one exercise to elicit allocation decisions, the enumerator briefly highlighted what was changing across each decision round. Before each allocation decision, the enumerator showed the participant a token with a token number indicating what number the decision was. The participant was reminded each time that one of the six allocation decisions would randomly be selected to be paid out for real. To improve understanding and increase salience, individuals were given 300 HTG in real 10 HTG notes to allocate to cups that represented a financial product.[16] Moreover, placed in front of each cup were small cards that illustrated the features of the particular financial product. After each decision was made, the enumerator verbalized the payoffs depending on the participant's allocation, and the participant was allowed to revise the allocation if desired.[17] This ensured that the participant comprehended the payoffs correctly.

After all of the participants completed their individual decisions, they convened as a group, and one of six tokens was drawn from a bag to decide which decision round was to be paid. Then each participant met with the assigned enumerator, who gave the participant a card that summarized the amounts and dates of the payouts.

III. Descriptive Statistics

The participants in this study consisted of poor and vulnerable individuals who were frequent lotto players and had little savings. Table 2 presents sample descriptive statistics. The sample was 74% male and 93% single. The average age

[15] While attendance in a morning or afternoon session was not random, we show in table A1 that key baseline variables are balanced between those who attended a morning session and those who attended an afternoon session: their risk aversion, probability weighting, savings, lotto bet amount on a usual day, income, and employment are not statistically different. However, those who attended a morning session were slightly more likely to be food insecure.

[16] There is, of course, a possible external validity concern that making decisions with an endowment (i.e., a windfall) is not the same as making decisions with own income. Thus, individuals might be more likely to play the lotto with an endowment. However, an alternative argument would be that individuals in this experimental context might be less likely to play the lotto because the product was delivered via a novel mobile money platform that very few have experience interfacing with.

[17] To operationalize this, enumerators were given payout matrixes that allowed them to easily mention the payoffs depending on the participant's allocation.

TABLE 2
SAMPLE DESCRIPTIVE STATISTICS

	Mean	Standard Deviation	Observations
	A. Survey Measures		
Age	26.7	15.4	287
Male	.74	.44	288
Household size	5.39	2.30	288
Single	.93	.25	288
Student	.30	.46	287
Unemployed	.21	.41	287
Income, 1 week	1,669	3,251	288
Food expenses, 1 month	8,260	12,352	288
Food security (1): anxiety about household food	.79	.41	287
Food security (2): no food in household	.54	.50	283
Food security (3): slept at night hungry	.49	.50	287
Food security (4): whole day without eating	.39	.49	285
Has refrigerator	.32	.47	288
Has flush toilet	.35	.48	288
Days per week play lotto	4.68	2.13	271
Bet amount on a usual day	173	268	274
Ever played lotto on mobile phone	.20	.40	288
Plays lotto to achieve a goal	.81	.39	274
Plays lotto for an investment	.33	.47	274
Save in sol	.12	.33	288
Save in bank	.090	.29	288
Save: balance in bank	52,714	194,464	26
Save informally (at home or with neighbor or friend)	.29	.45	288
Save: balance in informal savings	50,94.1	10,171.2	83
Borrowed money from family	.31	.46	285
Outstanding credit balance from family	2,329	3,894	89
	B. Risk Elicitation		
Risk aversion parameter	1.20	.44	301
Prelec probability-weighting parameter	.63	.20	301
	C. Portfolio Allocation, Round 1		
Consumption	.40	.18	306
Lotto	.22	.13	306
Savings	.38	.17	306

Note. All currency amounts are in Haitian gourdes. The food security measures are yes and no responses to the following questions: (1) In the past 4 weeks, did you worry that your household would not have enough food? (2) In the past 4 weeks, was there ever no food at all in your household because there were no resources to get more? (3) In the past 4 weeks, did you or any household member go to sleep at night hungry because there was not enough food? (4) In the past 4 weeks, did you or any household member go a whole day without eating anything because there was not enough food?

in the sample was 26.7 years, and the average household size was 5.4. Monthly household food expenses were roughly 8,259 HTG (or US$131). The sample consisted of food-insecure individuals. Over a 4-week period, 79% of households worried that their household would not have enough food, and 39% of households experienced at least one whole day where at least one household member went without eating because there was not enough food.

Lotto play was high, and savings was low in our sample. On average, participants played the lotto 4.7 days a week and spent 173 HTG on a usual day on the lotto. However, only 20% of the individuals in the sample had played the lotto on their mobile phone. Preempting this unfamiliarity with mobile phone lottery, we explained the SMS-lotto platform more extensively in the lab games. About 81% mentioned that they play the lotto to achieve a goal, and about 31% mentioned that they play the lotto for an investment purpose. This is consistent with the observation that lotto stalls serve as the prime financial service provider in Haiti. Only 9% of participants saved in a bank or formal financial institution, but among those who saved in a bank, the average balance was 52,713 HTG. About 29% of participants saved at home or with their neighbor or friend, and among those who reported informal savings, the average balance was 5,094 HTG.

The mean risk-aversion parameter σ was 1.2, and the mean probability-weighting parameter α was 0.63, suggesting that, on average, the participants were risk loving and tended to overweight small probabilities.[18] The elicitation of preference parameters is discussed in detail in the appendix (available online). Recall that our mobilizers were asked to recruit lottery players into the study, which may explain the risk-loving character of our sample. And although lottery play is rampant among the working poor in Haiti, our analysis may not necessarily extrapolate to those, say, in rural areas who may be less likely to play the lotto.

In the panel C of table 2, we show that in the first round of decisions, where the return on savings was 5%, participants on average allocated 40% of the 300 HTG to consumption, 22% to lotto, and 38% to savings. Allocation to savings was high, which likely stemmed from the high in-game savings return of 5% over a 6-week period and the fact that allocation decisions were being made on endowments provided in the experiment.

IV. Effect of LLS on Total Savings

Our main goal is to estimate the effect of introducing LLS on total savings. As a preliminary yet fundamental descriptive exercise, table 3 presents the mean allocation to each financial product across subjects for each round. We find that relative to rounds 1 and 2, where LLS was not offered, the mean allocation to the savings instrument decreased in rounds 3–6, whereas the mean allocation to LLS (mechanically) increased. However, the sum of allocation to savings and

[18] In Tanaka, Camerer, and Nguyen (2010), the mean σ in their Vietnamese sample was 0.59 and 0.63, and the mean α was 0.74. Thus, our sample was uniquely risk loving but comparable in its probability weighting to a sample of individuals from Vietnamese villages.

TABLE 3

MEAN ALLOCATION TO EACH FINANCIAL PRODUCT BY ROUND

	Round 1: Interest 5%, No LLS	Round 2: Interest 20%, No LLS	Round 3: Interest 5% with LLS, Principal 65% + Lotto 50%	Round 4: Interest 20% with LLS, Principal 80% + Lotto 50%	Round 5: Interest 5% with LLS, Principal 85% + Lotto 25%	Round 6: Interest 20% with LLS, Principal 100% + Lotto 25%
	A. Mean Share of Endowment Allocated to Each Financial Product					
Consumption	.40	.38	.31	.31	.31	.30
	(.18)	(.19)	(.18)	(.18)	(.18)	(.18)
Lotto	.22	.19	.14	.14	.15	.14
	(.13)	(.11)	(.09)	(.09)	(.09)	(.09)
Savings	.38	.43	.26	.28	.26	.29
	(.17)	(.19)	(.13)	(.14)	(.13)	(.14)
LLS			.29	.28	.29	.27
			(.16)	(.15)	(.17)	(.15)
	B. Mean Amount Allocated to Each Financial Product					
Consumption	119.6	113.4	92.61	91.70	93.69	90.26
	(53.49)	(56.19)	(53.11)	(53.83)	(54.97)	(54.55)
Lotto	65.10	57.73	42.87	41.65	43.61	41.34
	(38.58)	(33.06)	(27.19)	(26.24)	(27.83)	(26.82)
Savings	115.0	129.5	78.91	83.27	77.42	87.06
	(52.05)	(58.11)	(38.01)	(43.30)	(38.81)	(42.69)
LLS			85.46	82.75	86.63	80.84
			(47.95)	(44.44)	(50.89)	(44.55)
	C. Breakdown of Sample by Saving Allocation					
Saved any	.98	.99	1.00	.99	.99	1.00
	(.13)	(.10)	(.06)	(.08)	(.10)	(.06)
Saved max	.00	.01	.00	.00	.00	.00
	(.00)	(.10)	(.00)	(.06)	(.00)	(.00)

Note. Standard deviations are reported in parentheses. Rounds 1 and 2 are games where lotto-linked savings (LLS) was not offered, while rounds 3–6 include LLS in various forms. "Saved any" refers to the proportion of the sample that allocated any amount to savings or the LLS financial product, and "Saved max" refers to the proportion of the sample that allocated the entire amount to the savings product. There were 306 observations for each round.

LLS is a poor description of total savings, since the LLS product combines both a savings and a lotto component. As such, our primary and default definition of total savings is the total secured principal that will be paid in 8 weeks (either from the savings or the LLS product). For comparison, we additionally present regression results for alternative definitions of savings that also include delayed payments in the form of interest and expected lottery winnings. Note that initially, almost all subjects allocated to savings (99% of the sample in round 2); however, virtually none of the individuals allocated the entire endowment to the savings instrument (1% of the sample in round 2). This suggests that changes in savings will come from the intensive not the extensive margin within the context of this experiment.

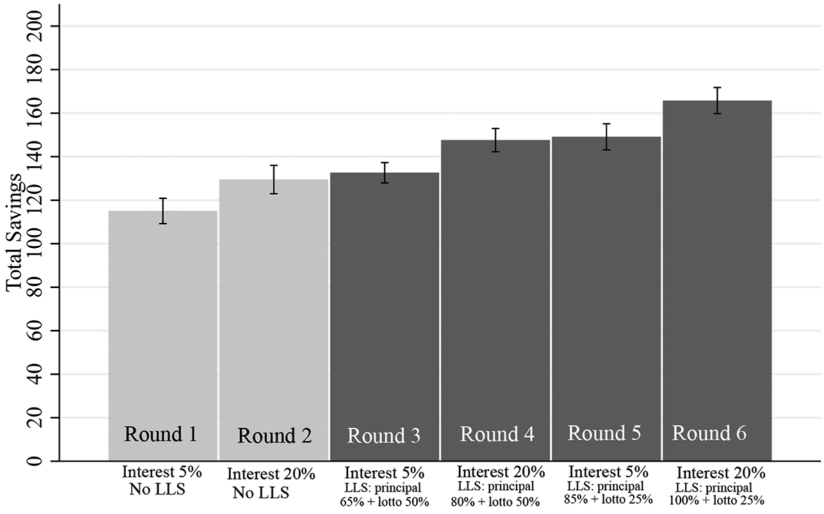

Figure 1. Mean of total savings in each decision round. Each bar represents the mean of total savings in each decision round. Total savings is defined as the total secured principal received in 8 weeks, which equals the sum of the amount allocated to savings (excluding interest) and the amount allocated to lotto-linked savings (LLS) that will be paid back with certainty (excluding lotto winnings). The darker bars represent rounds where LLS was offered (rounds 3–6). The bands indicate a 95% confidence interval for the calculated mean.

In figure 1, we present the mean across participants of total savings for each decision round. The darker bars represent decision rounds where LLS was offered (see table 1 for a summary of financial product features).[19] Comparing round 1 with round 3 (5% return, with and without LLS) and round 2 with round 4 (20% return, with and without LLS), we find a statistically significant increase in mean total savings when LLS was offered. Furthermore, we find that reducing the lotto component in an LLS product increases mean total savings even more (comparing round 3 with round 5, and round 4 with round 6). This suggests that even higher increases in total savings can be achieved with an LLS product that has less of a lotto component and instead secures more of the principal.

To more accurately quantify the effect of LLS on total savings, we estimate the following equation using panel fixed effects:

$$\mathrm{TS}_{ir} = \alpha_i + \beta_1 \mathrm{LLS}_{ir} + \beta_2(\boldsymbol{F}_{ir}) + \epsilon_{ir}, \tag{1}$$

where TS_{ir} is total savings of individual i in decision round r, α_i is an individual fixed effect that controls for any round-invariant individual characteristics,

[19] In fig. A1 (figs. A1–A3 are available online), we present the mean across participants of allocation to the lotto for each decision round. This shows a reduction in lotto spending in rounds where LLS is introduced. A more formal analysis of the effect of LLS on lotto spending is presented in sec. V.

LLS_{ir} is a dummy variable equal to 1 in a round where LLS was offered, and $\boldsymbol{F}_{ir}$ is a vector of dummy variables equal to 1 in a round where a given feature of the LLS or savings product was present. Specifically, we test for the effects of two features of the LLS product: the degree of its lotto component (which was lower in rounds 5 and 6 than in rounds 3 and 4) and the return of LLS relative to the savings product (which was lower in afternoon sessions than in morning sessions). Additionally, we test for the effect of the interest rate on total savings and heterogenous impacts of LLS by the savings interest rate (which was lower in rounds 1, 3, and 4). We use robust standard errors clustered at the individual level in all specifications. We do not cluster at the session level, as we have only 18 sessions, which would result in too few clusters for adjusting standard errors (Cameron and Miller 2015). However, clustering at the session level does not affect the statistical significance of the main results, which use within-individual variation.[20]

Estimation results are presented in table 4. We find that the introduction of LLS increases total savings by 26.5 HTG (col. 1), equal to a 22% increase relative to the mean when LLS was not offered. An alternative measure of total savings might include the certain interest that would be received in the traditional savings product. Using this measure, we show that the introduction of LLS increases total savings by 21.1 HTG (col. 2), equal to a 15% increase relative to the mean when LLS was not offered. And yet another measure of total savings might include both the certain interest and the expected winnings on the delayed lotto embedded in LLS. Using this measure, we find that the introduction of LLS increases total savings by 46.3 HTG (col. 3), equal to a 34% increase relative to the mean when LLS was not offered.[21]

[20] Alternatively, we had also conducted wild-*t* bootstrapped standard errors clustered at the session level in a pooled ordinary least squares regression (see Cameron, Gelbach, and Miller 2008). The standard errors do not vary much.

[21] The estimated effects of LLS for the three definitions of total savings (table 4, cols. 1–3) are similar to simply taking the difference in average of savings in the four rounds where LLS was offered and the average of savings in the two rounds where LLS was not offered for each individual and then averaging this difference in means across all individuals. Making this individual-level calculation is possible given the within-individual variation in the offer of LLS. However, we note that there are differences between the estimated relative effect size calculated from the sample presented in table 4 and the average individual-level effect sizes. For each individual, we take the effect (the average savings difference between the LLS rounds and non-LLS rounds) and divide this by the average savings in non-LLS rounds, and then we average this individual-level effect size across all individuals. This yields an average effect size of 40% for total savings, 33% for total savings (with interest), and 54% for total savings (with interest and expected winnings). These are all larger than the relative effect sizes presented in table 4.

TABLE 4
EFFECT OF LOTTO-LINKED SAVINGS (LLS) ON TOTAL SAVINGS

	Total Savings (1)	Total Savings + Interest (2)	Total Savings + Interest + Expected Winnings (3)	Total Savings (4)	Total Savings + Interest (5)	Total Savings + Interest + Expected Winnings (6)
Effect of LLS on total savings:						
(a) LLS offered	26.5***	21.1***	46.3***	19.3***	16.9***	52.7***
	(2.2)	(2.1)	(2.2)	(3.3)	(3.6)	(3.9)
(b) Low-risk LLS				17.4***	17.7***	.8
				(1.9)	(2.0)	(2.2)
(c) Low-return LLS				−4.5	−3.6	−6.9
				(4.5)	(5.0)	(5.3)
(d) High-interest savings				14.4***	34.6***	34.6***
				(2.9)	(3.3)	(3.3)
(e) LLS × high interest Savings				1.4	−5.6	−6.8*
				(3.1)	(3.5)	(3.7)
Mean, LLS not offered	122.2***	138.1***	138.1***	115.0***	120.8***	120.8***
	(1.5)	(1.7)	(1.8)	(2.0)	(2.1)	(2.2)
Effect size in percent relative to the mean when no LLS offered:						
(a) LLS	21.7	15.3	33.6	15.8	12.2	38.2
	[1.9]	[1.9]	[2.2]	[2.9]	[2.8]	[3.2]
(a + b) LLS, low risk				30.0	25.0	38.7
				[3.3]	[3.0]	[3.4]
(a + c) LLS, low return				12.1	9.6	33.2
				[2.6]	[2.5]	[2.7]
(a + e) LLS, high interest				17.0	8.2	33.3
				[3.2]	[3.2]	[3.7]
Observations	1,835	1,835	1,835	1,835	1,835	1,835
Participants	306	306	306	306	306	306

Note. Total savings is defined as the total secured principal received in 8 weeks, which equals the sum of the amount allocated to savings (excluding interest) and the amount allocated to LLS that will be paid back with certainty (excluding lotto winnings). Individual fixed effects are included in the regressions. Robust standard errors clustered by individuals are shown in parentheses below the point estimates. Bootstrapped standard errors, using 500 replications, are shown in brackets below the effect sizes in percent.
* $p < .10$.
*** $p < .01$.

Testing for the effects of LLS features (col. 4), we first find that a lower-risk LLS product with a smaller lotto-to-principal ratio resulted in a larger increase in total savings. An LLS product that had a higher lotto component increased savings by 16%, whereas one that had a lower lotto component increased savings by 30%, relative to when LLS was not offered. Mechanically, a product with a lower lotto component would have a higher savings component in the sense that a larger percentage of the principal would be secured. As such, the lesson we learn here is that contributions to an LLS product are unaffected by the degree of the lotto component. So, similarly, when including both secured interest and expected delayed lottery winnings in the savings measure (col. 6), we find that a lower-risk LLS product has no differential effect on the total delayed amount to be received.[22]

Second, we find that the introduction of an LLS product that had a lower expected return than the traditional savings product had a similar impact on total savings as one that had an expected return equal to the traditional savings product (col. 4). We find no evidence of differential LLS impacts by its return relative to the traditional savings product. The point estimate of moving from high- to low-return LLS is a decrease in total savings of 4.5 HTG, which is not statistically significantly different from zero. However, within a confidence interval of 95%, this estimate ranges from −13.3 to 4.5. Alternatively, we can gauge the overlap in the estimated effect of high- versus low-return LLS. Within a confidence interval of 95%, the effect of high-return LLS ranges from 10%−21%, whereas that of low-return LLS ranges from 7%−17%, relative to when LLS was not offered.

Last, we show that a higher interest on the traditional savings product increased total savings by 14.4 HTG, equal to a 13% increase (col. 4). Mechanically, including interest payments in the savings definition increases the effect of the interest (cols. 5, 6).[23] Note that the estimated effect of LLS (22%) is almost double the increase in the effect of increasing the interest rate from 5% to 20%. Furthermore, the effectiveness of an LLS product in increasing total savings is unaffected by the interest rate on the traditional savings product. Note that, by experimental design, a decision round with a higher interest on the savings product also meant a higher return on the LLS product. In table A2 (tables A1–A6 are available online), we show that the same results hold when we remove individual fixed effects and run a random effects model while controlling for the randomly assigned order, specifically game order A versus game order B. Furthermore, in table A3, we show that the estimates are similar when using either only the sample of individuals with game order A or only the sample with order B.

Given that our motivation for testing an LLS product is to understand whether it might offer a promising gateway to financial inclusion, it is important

[22] In contrast to our experimental measures of risk aversion shown in fig. A3, which suggest that our subjects are largely risk loving, this result suggests that they are risk averse in some dimension. We suspect that some combination of higher-order risk preferences (especially skewness preferences) and responses to the experimental risk elicitation task explains this contradiction, but we are unable to directly test this explanation.

[23] An alternative estimate of the effect of increasing the interest rate from 5% to 20% is to estimate the effect on savings in moving from rounds 2, 4, and 6 to rounds 1, 3, and 5. This estimation suggests that the interest rate effect is 11.6%.

to analyze how subjects in the experiment financed the estimated LLS-induced increases in total savings. To this end, we assess how the availability of LLS changed allocations to the other financial products. As discussed earlier, in table 3, we see that the mean allocation to the savings product decreased in rounds 3–6 relative to rounds 1 and 2. Note that the mean allocation to the consumption and lotto products also decreased. To better estimate the magnitude of these reductions, we estimate equation (1), but we instead use the amounts allocated to consumption, lotto, and traditional savings as dependent variables.

Estimation results are presented in table 5. The introduction of LLS decreased the amount allocated to all of the other financial products. Particularly, dividing $\hat{\beta}_1$ by the mean contribution when LLS was not offered, we find that LLS decreased the amount allocated to consumption by 22%, to lotto by 39%, and to traditional savings by 33% (and by as much as 40% for an LLS product

TABLE 5
EFFECT OF LOTTO-LINKED SAVINGS (LLS) ON PORTFOLIO ALLOCATION

	Consumption (1)	Lotto (2)	Traditional Savings (3)
Effect of LLS on portfolio allocation:			
(a) LLS offered	−25.8***	−23.6***	−40.9***
	(3.0)	(2.3)	(3.7)
(b) Low-risk LLS	−.2	.3	1.2
	(1.8)	(.8)	(1.9)
(c) Low-return LLS	−1.1	3.1	7.1
	(4.0)	(3.0)	(4.9)
(d) High-interest savings	−6.2**	−7.4***	14.4***
	(2.5)	(1.7)	(2.9)
(e) LLS × high-interest savings	4.0	5.7***	−7.4**
	(2.9)	(1.9)	(3.1)
Mean, LLS not offered	119.6***	65.1***	115.0***
	(1.8)	(1.4)	(2.0)
Effect size in percent relative to no LLS offered:			
(a) LLS	−22.1	−38.5	−33.4
	[2.5]	[3.2]	[2.6]
(a + b) LLS, low risk	−22.3	−38.0	−32.5
	[2.6]	[3.2]	[2.6]
(a + c) LLS, low return	−23.1	−33.4	−27.7
	[2.5]	[3.5]	[2.7]
(a + e) LLS, high interest	−18.7	−29.2	−39.5
	[2.6]	[3.2]	[2.8]
Observations	1,836	1,835	1,836
Participants	306	306	306
Mean, LLS not offered	116.5	61.4	122.2

Note. Dependent variables are amounts allocated to consumption, lotto, and traditional savings. Robust standard errors clustered by individuals are shown in parentheses below the point estimates. Bootstrapped standard errors, using 500 replications, are shown in brackets below the effect sizes in percent.
** $p < .05$.
*** $p < .01$.

offered alongside traditional savings with high interest). By bootstrap, we estimate standard errors on the percentage reductions of these other financial products. A simple comparison suggests that the percentage reductions in lotto and traditional savings would have overlapping confidence intervals. Thus, it would seem that LLS is seen as an alternative to lotto and to traditional savings equally and least so to consumption.

Our key result is that LLS increases savings—the amount of money that one chooses to hold for future use. This is likely welfare enhancing, as it may, for example, enable individuals to build emergency funds that can be used to address shocks.

In addition, we explore other issues that are related to whether LLS improves welfare. First, in table A4, we show the effect of the introduction of LLS on the expected portfolio return, which, in each game round, is the sum of consumption, expected lottery winnings in 2 weeks and 8 weeks, all savings, and all interest payments. This rather basic measure of portfolio return ignores risk and time delays to returns. Because LLS diverts funds away from lottery and the LLS products are designed to have the same expected return as traditional savings, the introduction of LLS increases the expected portfolio return by 2%–3%. Taken together, these results suggest that the lottery component of our prototype LLS product prompts individuals to sufficiently redirect funds from negative-return lottery to positive-return savings products that their overall returns improve. We take this as encouraging, albeit experimental, evidence of the potential of LLS to enhance financial inclusion, particularly as mobile-money-based financial services expand.

Second, as a small step beyond this lab-in-field setting, we can use the adminstrative SMS-lotto data to assess how individuals used their lotto credit after participating in the experiment. Several individuals chose to add their own money to their SMS-lotto account after using their lotto credit. This raises the troubling possibility that LLS may draw people to lottery play rather than nudge them in the other direction. We find that individuals who respond most aggressively to the LLS product in the experiment are (marginally) less likely to contribute their own funds to their SMS-lotto balance (with a correlation of -0.09). Thus, at least in the context of our experiment and using data we can observe, the LLS product did not lead to more lottery play. This is but a first step, and any larger LLS pilot will require more careful evaluation in this regard. Such evaluation and product design should consider and test different motives to ensure that gambling is not encouraged. For example, using administrative data from Digicel and the SMS-lotto firm, the market can be segmented into gamblers and nongamblers and the product can be targeted only to gamblers.

V. Heterogeneous Effects of LLS

Using data from the survey and the risk preference elicitation, we investigate heterogeneous effects of the LLS product by wealth and expenses, gender, savings, lottery play, and time and risk preferences. We estimate the following equation using panel fixed effects:

$$\text{TS}_{ir} = \alpha_i + \gamma_1 \text{LLS}_{ir} + \boldsymbol{\gamma_2}(\text{LLS}_{ir} \times \boldsymbol{X_i}) + \epsilon_{ir}, \tag{2}$$

where we interact the LLS_{ir} indicator variable with $\boldsymbol{X_i}$, a vector of round-invariant individual characteristics and preferences. For wealth, we use principal component analysis to construct a wealth index from variables that indicate asset ownership.[24] For expenses, we use the household monthly food expense measure. For food insecurity, we use the sum of the food insecurity questions.[25] For savings, we use the sum of balances in various savings vehicles, namely, savings in a *sol*, in a bank or other formal financial institution, at home or with friends and neighbors, or in a mobile money account. For lottery play, we construct a rough measure of weekly lotto expenses by multiplying the usual number of days in a week that one plays the lotto by the amount one bets on the lotto in a usual day. For all the above measures, we standardize the variables to have a mean of 0 and standard deviation of 1.[26]

Estimation results are presented in column 1 of table 6. Albeit somewhat noisy, we find evidence that participants from poorer households were less likely to increase savings when offered LLS. The effect of LLS on the participant's total savings increases with household wealth and food expenses and decreases with food insecurity. But only the food expense interaction term is statistically significant at the 5% level. Whereas for their sample of students, Filiz-Ozbay et al. (2015) find that the effects of a PLS option encourages savings particularly among males, we find no similar differential effects of the LLS product by gender in our sample.

In column 2 of table 6, estimation results are presented for the survey measures of savings and lottery play. We find no statistically significant evidence of differential effects of the LLS product by savings and lottery play, although the direction of the effects indicate that LLS might be more effective for individuals who have higher savings and lower lotto expenses. It is also important to note

[24] We ask about ownership of a motorcycle, refrigerator, television, sofa, gas stove, generator, flush toilet, solar panel, and *tap tap* (a small truck converted to a shared taxi).

[25] This is a set of six questions regarding food anxiety and the lack of food in the household.

[26] As a robustness check, we use an alternative normalization of the variables, where we divide the difference between the subjects' variable and the minimum of the variable by the difference between the maximum and minimum of the variable. The estimates, which use this alternative normalization, are presented in table A5 and are similar in sign, significance, and relative magnitude.

TABLE 6
HETEROGENEOUS EFFECTS OF LOTTO-LINKED SAVINGS

	Total Savings (1)	Total Savings (2)	Total Savings (3)
LLS offered	29.21***	26.66***	26.34***
	(4.33)	(2.31)	(1.80)
LLS offered × wealth index	.63		
	(2.40)		
LLS offered × food expenses	2.31**		
	(.97)		
LLS offered × food insecurity	−1.14		
	(2.30)		
LLS offered × male	−2.51		
	(5.09)		
LLS offered × savings		1.04	
		(.68)	
LLS offered × lotto spending		−2.05	
		(2.00)	
LLS offered × in-game savings			−20.04***
			(2.57)
LLS offered × in-game lotto			8.94***
			(2.31)
Mean, LLS not offered	120.59***	120.63***	122.22***
	(1.51)	(1.54)	(1.20)
Observations	1,721	1,709	1,835
Individuals	287	285	306

Note. Total savings is defined as the total secured principal received in 8 weeks. The following variables are transformed into their standard normal version: wealth, food expenses, food insecurity, savings, and lotto spending. In-game savings is the average savings in rounds 1 and 2, and in-game lotto is the average lotto spending in rounds 1 and 2. In-game savings and in-game lotto are also transformed into their standard normal version. Robust standard errors clustered by individuals are shown in parentheses.
** $p < .05$.
*** $p < .01$.

that our entire sample already has very little savings and plays the lotto frequently. This may explain why we are unable to precisely estimate any differential effects across these dimensions and may suggest that these results may be quite different with a different population.

Alternatively, we estimate heterogeneous effects of LLS by initial in-game savings defined as the average of savings in rounds 1 and 2 and initial in-game lotto spending defined as the average of lotto spending in rounds 1 and 2. These in-game measures of savings and lotto spending are also standardized to have a mean of 0 and standard deviation of 1. Estimation results are presented in column 3 of table 6. A 1 standard deviation increase in savings in the initial rounds decreases the effect of LLS on total savings, whereas a 1 standard deviation increase in lotto spending in the initial rounds increases the effect of LLS on total savings. These estimated heterogeneous effects using in-game variables are much larger than the heterogeneous effects estimated using the survey measures of baseline savings and lotto play. These results suggest that the LLS product works for those with initially low savings and high lotto spending.[27] Atalay et al. (2014) similarly find that a PLS account was more effective for those with initially lower levels of savings.

We further estimate heterogeneous effects of the LLS product by time and risk preferences. In the survey, we asked two simple questions to elicit time preferences.[28] Although not incentive compatible, this allows us to elicit crude measures of the discount rate and present bias. In column 1 of table 7, we present differential effects of LLS by the discount rate and present bias and find no statistically significant differential effects. This is similar to our finding that the effect of LLS on total savings was unaffected by the interest rate on the traditional savings option.

Using the risk preference elicitation exercise (discussed in detail in the appendix), we test for differential effects of LLS by whether a person is more risk loving (higher values of σ) and whether a person is more likely to overweight probabilities (higher values of $-\alpha$). The LLS product is expected to appeal to individuals who are more likely to overweight the likelihood of a gain, so we expect the LLS product to be more effective for those who overweight small probabilities of large gains. Filiz-Ozbay et al. (2015) provide a richer discussion on the role of some of these behavioral biases on lottery play. We standardized the constructed risk and probability-weighting preference parameters. Results are presented in column 2 of table 7. We find that each 1 standard deviation increase in $-\alpha$ increases the effect of LLS on total savings by 4.85 HTG. Thus, an increase in the overweighting of small probabilities increases the effect of LLS on total savings.[29] On the other hand, we do not find statistically significant

[27] Using nonstandardized measures, we find that each increase of 1 HTG in initial in-game savings decreases the effect of LLS on total savings by 0.40 HTG and that each increase of 1 HTG in initial in-game lotto increases the effect of LLS on total savings by 0.27 HTG. We have also tested for heterogeneous effects of LLS by savings and lotto spending in the initial practice rounds. The direction of the heterogeneous effects are similar, but the estimates are smaller and slightly more imprecise. We have also tested whether individuals whose savings allocations are most sensitive to the interest rate hike between rounds 1 and 2 respond differently when offered the LLS product. We find some weak evidence that the savings response to the LLS product weakens as an individual's sensitivity to the interest rate before LLS is offered increases.

[28] First, we ask "if you could choose 1,000 HTG in 1 week or 1,200 HTG in 1 month after that, what would you choose?" If the individual chose 1,000 HTG, then we ask how much they would need to receive in a month to be willing to wait. Second, we ask "if you could choose 1,000 HTG in 6 months or 1,500 HTG in 7 months, what would you choose?" If the individual chose 1,000 HTG, then we ask how much they would need to receive in 7 months to be willing to wait.

[29] The correlation between the probability-weighting parameter (α) and initial in-game lotto spending (the average of lotto spending in rounds 1 and 2) is 0.001, suggesting that these two measures are not one and the same. Initial spending in the lotto is thus not driven by the overweighting of small probabilities. Moreover, the overweighting of small probabilities and the initial allocation to the

TABLE 7

HETEROGENEOUS EFFECTS OF LOTTO-LINKED SAVINGS (LLS): TIME AND RISK PREFERENCES

	Total Savings (1)	Total Savings (2)
LLS offered	28.96***	26.57***
	(3.13)	(2.23)
LLS offered × discount rate	.03	
	(.17)	
LLS offered × present bias	−6.59	
	(4.78)	
LLS offered × more risk loving		−3.00
		(2.56)
LLS offered × probability overweighting		4.85**
		(2.43)
Mean, LLS not offered	120.47***	122.21***
	(1.53)	(1.48)
Observations	1,721	1,805
Individuals	287	301

Note. Total savings is defined as the total secured principal received in 8 weeks. The following variables are transformed into their standard normal version: risk loving and probability overweighting. Robust standard errors clustered by individuals are shown in parentheses.
** $p < .05$.
*** $p < .01$.

differential effects by whether a person is more risk loving, σ. It is of course entirely possible—even likely—that a preference for skewness would show measurable differential effects of LLS on savings, but our risk elicitation experiment does not allow us to compute such higher-order risk preferences.

That we find differential effects of LLS by initial lotto spending and overweighting of small probabilities suggests that a simple allocation rule (i.e., $n + 1$ heuristic) is not a key mechanism. Unless the heuristic applies only to the exact same individuals who, for example, overweight small probabilities, it is likely we are capturing something beyond this simple heuristic.

VI. Conclusions

We conducted experimental games in Port-au-Prince in which participants were asked to make a series of portfolio allocation decisions across three or four options: consumption, lotto, traditional savings, and LLS, which combined features of the lotto and traditional savings options. We find that the introduction of LLS increased total savings by 22%, nearly double the savings response due to increasing the interest rate from 5% to 20%. This increase in total savings came largely from reductions in the amount allocated to both lotto and traditional savings.

lottery are likely operating in different ways to increase the effectiveness of the LLS product in increasing savings.

Furthermore, we test various features of the LLS product. First, we find that an LLS product with a higher savings-to-lotto ratio resulted in a higher increase in total savings, suggesting that a reduction in the intensity of the lotto component did not reduce investment in the LLS product. Second, we find no difference on the effect on total savings between an LLS product that had an expected return equal to traditional savings and another that had a lower expected return. Finally, we use a risk preference elicitation exercise to explore which mechanisms might be driving the effect of LLS on total savings. On average, individuals in our sample were risk loving and had a greater tendency to overweight small probabilities. We also find that the effect of LLS on total savings was higher for those who more heavily weighted small probabilities, implying that such a behavioral bias may be driving the effectiveness of LLS.

Overall, our research adds to the emerging literature that demonstrates the effectiveness of savings products with a lottery component (Atalay et al. 2014; Filiz-Ozbay et al. 2015). The LLS product we explore leverages a unique Haitian lotto culture to improve savings among the poor. Such increases in savings and access to savings accounts may further serve as a gateway to broader financial inclusion and access to other financial services, such as credit and insurance. The viability of an LLS product like the one we test hinges on the potential profitability for the private firms offering the lotto and savings services. Our results suggest that a lower-expected-return LLS product would be the most cost effective means of increasing savings among the products we tested in our experiment. While this is encouraging from a market viability perspective, there are several additional factors that deserve careful consideration.

First, it is unclear what interest rate should be the benchmark for evaluating an LLS product because interest-bearing mobile money savings accounts do not yet exist in Haiti. It is, however, clear that the effective interest rates offered in our experimental products are likely higher than sustainable rates. At least part of the effect we find is due to relative comparisons between the allocations; how much is attributable to the absolute (effective) interest rate remains an open question that requires additional testing. Second, to the extent that an LLS product provides an effective gateway to a broader range of financial services, private firms may find it profitable to formulate loss-leader or other cross-subsidization strategies that leverage dynamic interdependencies with other products to improve market viability of an LLS product. Third, if continued piloting corroborates the finding that an LLS product may promote financial inclusion among the working poor, there may also be interest in the donor or nongovernmental organization (NGO) community to help offset some of the costs associated with offering the product. For example, an NGO may be willing to offer modest matched savings support whereby a portion of the interest incentive is initially covered by donors interested in financial inclusion.

Finally, while oveweighting small probabilities is one aspect we are able to test, a more in-depth exploration as to what motivates individuals to choose lotto products may contribute to further refinements in the design of LLS products. For example, the recent model in Laajaj (2017) on endogenous time horizons provides another potential motivation for the effects of our LLS product.[30] In this model, those with poor future prospects suffer a cognitive dissonance penalty when thinking about this gloomy future and, as a result, adopt truncated time horizons to avoid this penalty. This can unleash a behavioral poverty trap in which "poverty and shortsightedness reinforce each other" (Laajaj 2017, 194). Such endogenously truncated time horizons may partly explain the intensity of lotto play among Haiti's working poor, whose short planning windows favor lotto "investments" over savings. In such a context, anything that reduces the degree of truncation and thereby extends time horizons can trigger more promising, longer-term investments. From this perspective, the prototype LLS product we test may enable those with truncated time horizons to discover the value and viability of saving over a longer period. A more complete understanding of the motivations behind lotto play may provide better grounding for designing LLS products that effectively redirect lotto wagers into more productive savings. While these and other considerations loom large, we consider the findings of this experiment to be an intriguing first step toward channeling Haitians' passion for lottery play to more productive financial strategies.

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[30] Herskowitz (2021) proposes a different model to explain gambling behavior among the poor that involves indivisible expenditures in the presence of liquidity and credit constraints. This is similar to the notion of a transformational sum that has been invoked as an explanation for lottery behavior among the poor in developed countries.

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