Using Lotteries to Attract Deposits

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Abstract

Despite the importance of deposit financing for lending, banks in developing countries struggle to attract deposits. In a randomized experiment across 110 bank branches throughout Mexico, a lottery incentive based on net monthly deposits caused a 36% increase in the number of accounts opened and a 21% increase in the number of deposits during the lottery months. Nearly all new accounts (96%) were opened by households previously unbanked at any bank. The temporary two-month incentive had a persistent 2–3 year impact on the flow of deposits and stock of savings, and increased the present value of branch profits by 6%.

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1 Introduction

Banks finance three-quarters of their assets with deposits (Hanson, Shleifer, Stein and Vishny, 2015), which are a more stable source of funding than short-term debt (Ivashina and Scharfstein, 2010). Not only do banks compete for deposits, but variation in banks' productivity of attracting deposits explains the majority of the variation in bank value (Egan, Lewellen and Sunderam, 2022). In developing countries, however, banks struggle to attract deposits: the fraction of households that are unbanked remains high, and conditional on having a bank account most households do not deposit significant amounts (Demirgüç-Kunt, Klapper, Singer and Ansar, 2022; Dupas, Karlan, Robinson and Ubfal, 2018). This is true even though access to formal bank accounts leads low-income households to accumulate more wealth, to better cope with income shocks, and—since additional earnings can be safely saved—to increase labor supply and income (Bruhn and Love, 2014; Callen, De Mel, McIntosh and Woodruff, 2019; Célérier and Matray, 2019; Stein and Yannelis, 2020).

Prize-linked savings (PLS) accounts are a potential solution (Cole, Tufano, Schneider and Collins, 2007; Kearney, Tufano, Guryan and Hurst, 2011). These accounts offer lottery tickets for cash prizes as an incentive to save, often in lieu of paying a fixed interest rate. The number of lottery tickets received is typically a function of the amount of new savings accumulated. Like a traditional lottery, PLS offers a small chance at winning a large prize. Unlike a traditional lottery, PLS customers keep the principal that they deposit into their account. Households seeking skewness might therefore open a PLS account if offered, and might substitute from gambling to saving in the PLS account (Cole, Iverson and Tufano, 2021; Cookson, 2018). Once opened, PLS accounts may lead to more persistent savings since lotteries tend to be habit-forming (Guryan and Kearney, 2010).

There are several reasons that people might seek skewness in returns and thus be drawn to gambling or, in our case, PLS. First, they may overweight small probabilities (Kahneman and Tversky, 1979), perhaps due to a tendency to have optimistic beliefs (Brunnermeier, Gollier and Parker, 2007). Evidence from lab experiments shows that PLS accounts are indeed appealing to those who overweight the small probability of receiving a large return (Filiz-Ozbay et al., 2015; Dizon and Lybbert, 2021). Alternatively, people may have nonconcavities in their utility function (Friedman and Savage, 1948), which can arise from demand for indivisible assets in the presence of financial constraints (Kwang, 1965; Hartley and Farrell, 2002). In the context of sports betting, Moskowitz and Vasudevan (2023) find evidence consistent with the latter mechanism rather than the former.

People might also experience utility from dreaming about winning the lottery prize even when they do not win (Bhatia, 2010).

PLS products are prolific. They were introduced in the 1690s in Great Britain (Cohen, 1953) and remain a popular product in the UK today, with about one-fifth to one-fourth of households participating in a PLS product (Tufano, 2008). Since then, PLS products have been introduced by both public and private financial institutions in more than 40 countries across six continents (Figure A1a and Table AI). PLS products were illegal in the US until 2014, when Congress passed the American Savings and Promotion Act, which permitted states to legalize PLS. Many states did: today, 32 states allow financial institutions to offer PLS products (Figure A1b). In nearly all states where PLS is legal, there are examples of PLS products offered by credit unions and/or banks (Table AII).[1] Although PLS products are common around the world, there is limited evidence on whether they are effective at inducing people to open accounts and save.

We worked with a bank in Mexico to conduct a randomized controlled trial (RCT) of prize-linked savings. We randomly assigned 40 out of 110 bank branches across 19 states to offer PLS over a two-month period, and measured the effects on account openings, deposits, and savings over the subsequent five years.

To participate in the lottery for the cash prizes, people had to open or already have an account at one of the treatment branches and had to save in the account over the two-month period. Each 50 pesos (US$4) of new savings earned one lottery ticket; our partner bank awarded one thousand small prizes of 400 pesos (US$31) and two large prizes of 10,000 pesos (US$777) at the end of each of the two lottery months. While the expected return was unknown ex ante to potential savers since it depended on other account holders' savings responses to the incentive, ex post it was equivalent to a 1.4% annual interest rate.

The branch-level randomization makes our estimates relevant for a bank considering offering PLS to attract deposits, as we measure the effect of PLS on the branch-level flow of deposits, stock of savings, and present value of profits. We also decompose these branch-level effects into the extensive margin effect on new account openings, the stock

[1] In numerous countries, regulators have stifled PLS, often because state-run lotteries prefer to maintain rents from having a monopoly over state-sanctioned gambling. For example, in South Africa, First National Bank was sued by the National Lotteries Board and the Supreme Court deemed their PLS product to be a violation of the country's Lottery Act (Cole, Iverson and Tufano, 2021). Now that PLS is legal in the US, while national banks typically do not offer PLS products—possibly because they do not view it to be worthwhile to offer a product that they can only offer in certain states—Walmart and GreenDot offer a PLS product, as do a number of FinTech apps.

of savings in these additional new accounts, and the intensive margin effect on the stock of savings of those who already had an account. After two months, the lotteries ceased, so that the benefits of saving in treatment and control branches no longer differed, allowing us to study the persistent effects of this temporary incentive to save.

Our main results are the following. First, offering PLS caused a 36% increase in the number of accounts opened and a 21% increase in the number of deposits during the lottery months in treatment branches relative to control branches. After the final lottery-eligible day, the daily treatment effect on account openings abruptly fell to zero, which suggests that the possibility of winning a large prize was indeed driving the effect. Second, using data from Mexico's Central Bank to link individuals' accounts across different banks, 96% of account openers at treatment branches during the lottery months were previously unbanked. Third, there was a persistent effect on the *flow* of deposits at treatment branches for about three years after the lotteries ended. As a result, the stock of savings increased steadily over time in treatment branches relative to control branches, and the branch-level difference in the stock of savings is statistically significant from about eight months to 2.5 years after the lotteries ended. Fourth, accounting for the bank's return on deposits and its operational, marketing, and incentive costs, we estimate that offering the lottery incentive for two months led to a 6% increase in the present value of branch profits.

We then decompose these branch-level results by separating accounts opened prior to the lottery months from those opened during the lottery months. Because both types of accounts were eligible to receive the lottery prizes, deposits in both types of accounts could increase due to the PLS offer. We find, however, that the effects are driven by accounts opened during the lottery months: while there is no statistically significant effect on the stock of savings over the five years after the lotteries for accounts opened prior to the lottery months, there is a large effect for accounts opened during the lottery months. The difference between treated and control branches in the stock of savings in accounts opened during the lottery months increased gradually after the lotteries and is statistically significant from about four months to five years after the lotteries.

These results are still from regressions where accounts are aggregated to the branch level, and could be driven by a combination of the extensive and intensive margins at the account level. On the extensive margin, more accounts were opened at treatment branches during lottery months, which would lead to higher branch-level deposits even if deposits per account were equal across treatment and control branches. On the intensive margin, new account openers at treatment branches could deposit more than new account openers at control branches, both during and after the lotteries. We find that the branch-level results on the stock of savings are driven by the extensive margin: treatment branches have more accounts than control branches due to the lottery incentive, and new account holders at treatment and control branches save at similar rates.

Our results show that a temporary PLS incentive was highly effective at providing an incentive to open bank accounts and increasing branch-level deposits and savings, particularly when compared to interest rate interventions in other studies. Karlan and Zinman (2018) and Kast, Meier and Pomeranz (2018) find that increasing deposit interest rates from 1.5% to 3.0% and from 0.3% to 5.0%, respectively, did not have a statistically significant effect on account openings or the flow of deposits. Furthermore, the (statistically non-significant) point estimates they find for the effect on account openings—3.5% and 2.0%, respectively—are an order of magnitude smaller than our intervention's 36% effect on account openings. The magnitude of the interest rate increases in these studies are even larger than the increase in the *expected* interest rate in our study from about 0.2% in the control group to 1.4% in the treatment group.[2]

We make three main contributions. First, we provide evidence from an at-scale RCT on the effectiveness of a popular savings product at increasing account opening and deposits. Other studies we are aware of on the effects of PLS on savings are observational studies or lab experiments. Cole, Iverson and Tufano (2021) compare those who open PLS accounts to others in a difference-in-differences event study framework and show that PLS account openers increased savings substantially. As Burke (2021) points out, these results could be driven in part by selection, which we circumvent in our RCT. Bharadwaj and Suri (2020) exploit the start and end of a PLS promotion to mobile money users in Kenya using a before/after comparison, and also find evidence of a substantial intensive margin effect on savings; they do not measure effects on account openings, and cannot study long-term effects. Other papers focus not on savings but on the crowd-out of gambling. Cookson (2018) exploits the rollout of a PLS product across credit unions in Nebraska, finding that the availability of PLS led to a reduction in gambling expenditures at casinos, consistent with evidence from Cole, Iverson and Tufano (2021) and Dizon and Lybbert (2021) that saving in a PLS account acts as a substitute for gambling.[3]

[2]For PLS products, the expected interest rate is not known ex ante because it depends on how much people save. This was the primary reason that we could not conduct the "ideal" experiment that would include another arm in which we offered the equivalent expected interest rate (1.4%) with certainty.

[3]Consistent with PLS crowding out gambling expenditures, Herskowitz (2021) shows that gambling is used by the poor to save for indivisible assets. Another financial product that may be a substitute for gambling is stocks with lottery-like payoffs (Barberis and Huang, 2008; Kumar, 2009).

Second, by randomizing the product offering at the bank branch level, we are able to measure the overall effect of offering PLS on branch-level deposits and the present value of bank profits. We are also able to decompose the branch-level effects into the effect on new accounts opened, on deposits into those new accounts, and on deposits into accounts opened prior to the PLS offer (which were also eligible to receive the lottery prizes). In contrast, other studies have typically randomized at the individual level and *either* restricted the experimental sample to non-account holders and incentivized opening a new account (e.g., Cole, Sampson and Zia, 2011; Dupas and Robinson, 2013; Dupas, Karlan, Robinson and Ubfal, 2018) *or* restricted the experimental sample to account holders and randomized an incentive or nudge to save more (e.g., Beshears et al., 2015; Blumenstock, Callen and Ghani, 2018; Karlan, McConnell, Mullainathan and Zinman, 2016).

Third, we measure impacts for nearly five years after the *temporary* incentive ended, and provide evidence of a *persistent* impact on the flow of deposits at treatment branches that gradually declines over time but persists for about three years. Most RCTs testing various savings interventions measure impacts over substantially shorter time horizons: the median of the number of months over which savings is measured in RCTs is 12 months, and the 90th percentile is 36 months (Table AIII). Tracking savings for five years after a savings intervention is rare; two exceptions are Horn, Jamison, Karlan and Zinman (forthcoming) who measure effects of financial education and account access over five years, and Bruhn, Garber, Koyama and Zia (2022) who measure effects of financial education over nine years.

2 Institutional Context

2.1 Financial Inclusion and Prize-Linked Savings in Mexico

Mexico's financial market is dominated by five large banks with a combined 90% market share (Ponce, Seira and Zamarripa, 2017), and these banks struggle to serve the poor (Castellanos, Jiménez-Hernández, Mahajan and Seira, 2020). Overall, financial inclusion in Mexico is low: about 37% of the adult population has a bank or mobile money account. It is even lower for low-income Mexicans: only 26% of those with incomes in the bottom 40% of the income distribution have an account (Demirgüç-Kunt et al., 2018). While microfinance institutions have rapidly expanded access to credit (Angelucci, Karlan and Zinman, 2015), they have not aggressively pursued savings products. On the other hand, several commercial banks in Mexico have offered PLS products, including BBVA, HSBC, and Santander (Table AI).

2.2 Partner Bank

To promote financial inclusion, the Mexican government founded the National Savings and Financial Services Bank (Bansefi) in 2001. Its mission is "to contribute to the economic development of the country through financial inclusion…to strengthen savings and loans mainly for low-income segments." Bansefi focused on fostering savings for the poor through low-cost savings accounts with no minimum balance. At the time of our experiment in 2010, Bansefi had 494 branches and about 5 million accounts, many of them opened directly by the government to pay conditional cash transfers. Bansefi has tried to locate its branches in relatively low-income areas. It concentrates on offering savings accounts with no minimum balance and no fees, but generally pays low interest rates (0.2% per year at the time of our intervention). Mobile and internet banking were extremely rare in Mexico at the time of our experiment, and Bansefi did not offer these services.

Bansefi has tried to be innovative in how to attract low-income savers. One of their strategies, beginning in 2005, was to offer PLS accounts. They offered these accounts through several campaigns from 2005–2009 but did not rigorously measure the effectiveness of this strategy; after a change of management, Bansefi discontinued PLS campaigns in 2009. In 2010, we partnered with Bansefi and the Inter-American Development Bank (IADB) to test if PLS accounts attract new clients and generate more bank deposits.

3 Experimental Design

3.1 Branch Sample

To economize on the cost of the experiment and because other savings incentives were operating at certain Bansefi branches, we first selected a subset of branches that would participate in our experiment. Bansefi proposed excluding branches that offered a matched savings program with commitment device features, called *Premiahorro*. Excluding the branches that offered this product left us with 214 out of the initial 494 branches for our sampling frame. To reduce variance and have more power, we removed approximately the smallest 25% and largest 25% of branches from the sampling frame. Finally, to reduce implementation costs, we focused on states that had at least two branches meeting these selection criteria. After applying these conditions, our sampling frame consisted of 110 Bansefi branches spanning 19 of Mexico's 32 states throughout the entire country from Baja California to the Yucatan Peninsula. Experiments on savings rarely have this extent of geographical breadth.[4]

3.2 Randomization

Within the 110 Bansefi branches in our sampling frame, we conducted a non-stratified randomization to assign 40 branches to treatment. Table 1 shows that treatment and control branches have balanced covariates: there are no statistically significant differences between treatment and control branches across seventeen locality- and branch-level variables, nor do the variables jointly predict treatment in an omnibus F-test ($p = 0.62$). Figure 1 shows the locations of treatment and control branches. The average road distance between a treatment branch and the closest control branch is 73.8 kilometers (km).

3.3 PLS Incentive

The experiment was carried out in 2010. The PLS incentive and dates of the two lotteries were advertised starting September 12, 2010, in treatment branches only. The dates of the two lotteries were October 12 and November 12, 2010. The PLS accounts were advertised through posters inside the branch and loud-speaker cars on nearby streets. Due to budget restrictions, the loud-speaker car advertising happened only in September 2010.[5] Figure A2 shows the timeline of the experiment and an example of Bansefi's advertisements of the savings lotteries, which reads "save in a *debicuenta* account and multiply your money" and illustrates how a small amount of savings could turn into 10,000 pesos (the large prize). Bank tellers were trained to answer questions regarding the rules of the lotteries.

Two types of accounts were eligible to win the lottery prizes: accounts that had already been opened any time prior to the lottery months and accounts newly opened during the lottery months. To participate in the lottery, a client had to increase her stock of savings by at least 50 pesos over the month preceding the lottery. Specifically, the bank compared an account's ending balance at the close of business the day before the lottery to the ending balance one month prior (or to zero if the account had not yet been opened one month prior) to determine how many electronic lottery tickets to award to the account holder. Every 50 pesos increase in the stock of savings entitled the client to one electronic ticket. The incentives were only active from September 12 to November 11, 2010 (the day before

[4]Two notable exceptions are the multi-country savings experiments in Dupas, Karlan, Robinson and Ubfal (2018) and Karlan, Savonitto, Thuysbaert and Udry (2017).

[5]This turns out to be useful, as it enables us to rule out that the treatment effects in the second lottery month from mid-October to mid-November could be driven by this type of advertising continuing afterward (Section 6.3).

the last lottery was conducted), and this was known ex ante as it was stated by the bank tellers and specified in the advertisements.

Other than the lottery prizes, the other aspects of the account were identical to those of accounts in control branches. Accounts in both treatment and control branches continued to earn a low 0.2% annual fixed interest rate on deposits (in addition to the potential lottery winnings in treatment branches). The prizes included one thousand small prizes of 400 pesos (US$31) and two large prizes of 10,000 pesos (US$777) at the end of each of the two lottery months. The probability of winning was endogenous to total participation and was therefore not known ex ante; for each prize, the probability of winning for a client would be equal to her number of tickets divided by the total number of tickets earned across all 40 treatment branches.

We were not able to conduct the "ideal" experiment that would include another arm in which we offered the equivalent expected interest rate (1.4%) with certainty because the expected return depends on the amount of savings across all accounts, which was not known by us ex ante. One alternative would be to fix the expected return by conducting an independent and identically distributed (i.i.d.) draw for each individual with a fixed probability of winning, but this implies risk for the bank in both directions. The bank could calculate its cost in expectation, but it would be possible that fewer people win than expected and people conclude that the lotteries were a scam and lose trust in the bank, or that more people win than expected and the bank incurs a higher cost than expected. In the United States, there is a market for sweepstakes insurance to insure companies conducting raffles from this risk, but this market is not well-developed in Mexico.

A second alternative would be to have several comparison arms at different fixed interest rates given that the expected return of the PLS product is unkown. For example, an experiment could have four comparison arms offering 1%, 2%, 3%, and 4% fixed interest rates and one arm offering PLS. The idea of this design would be that the ex post return of the PLS product (which is not known ex ante) would hopefully be in this range and close to one of the exact values of these comparison arms. We did not implement this design as it has obvious power limitations given how many comparison arms would be required to approximate the ex-ante-unknown return of PLS, and thus it was not feasible for our branch-level randomization with a limited number of bank branches.

4 Data

4.1 Administrative Data from Bansefi

We use three types of administrative data from Bansefi. First, we use data on every account opened by clients at treatment and control branches from January 2007 through May 2011, which we use to construct a data set of the number of new accounts opened at each branch each day. Second, we use transactions data over an 8.5-year period from over 3.5 years prior to our experiment to nearly five years after. Specifically, for each account we observe data on all transactions from the later of January 2007 or the date the account was opened through July 2015.[6] Third, we use data on the addresses and geocoordinates of all Bansefi branches.

Table 1, panel A, presents means for these data across treatment and control branches. There are a few notable summary statistics. First, these are small bank branches: excluding the bank accounts that Bansefi administers for recipients of government social programs, there are only around 127 total accounts at each branch. Each month, 3.7 accounts are opened per branch. This reflects that Bansefi positions its branches in relatively low-income areas, but also underscores the difficulty of attracting the unbanked. However, this low number of new accounts opened per branch per month is not uncommon in Mexico: 3.7 accounts per branch per month is at the 32nd percentile of the distribution of account openings across all commercial bank branches in Mexico. On average, there are 35 deposits and 62 withdrawals made at each branch per month.

The data used throughout the paper are restricted to accounts opened voluntarily by clients, and not those opened by the government to deposit cash transfers to cash transfer recipients (which are studied in Bachas, Gertler, Higgins and Seira, 2021; Higgins, 2022). The reason is that in this experiment we study the choice to open accounts, but cash transfer recipients do not choose when to open their accounts; instead, they are opened for them automatically by the government.

[6] Prior to March 2008, these data were missing transaction codes which enable us to separate different types of deposits (e.g., a deposit made by the client vs. an interest payment). Thus, for variables where we restrict to deposits and withdrawals made by clients rather than the bank, we restrict to data from 2009 onward, defining 2009 as the baseline period when we control for the baseline value of the outcome variable, and estimating regressions from January 2010 onward. We use the data from 2007–2008 for the calculation of the present value of bank profits because the amount of lending the bank can do is a function of the total stock of savings across all accounts, regardless of whether deposits into the account were made by the client or the bank. More detail is provided in Appendix B.

4.2 Confidential Auxiliary Data

Mexico's Central Bank allowed us to query a confidential database that links accounts within individuals across banks in Mexico, which thus allowed us to observe whether new account openers during the lottery months already had bank accounts at other banks. Specifically, we did not access this data set directly; Bansefi shared a list of account numbers (for accounts opened in treatment branches during lottery months) with Mexico's Central Bank, and they returned an anonymized data set indicating which of these account holders had accounts at any other bank prior to opening their Bansefi account.

4.3 Publicly-Available Auxiliary Data

We use several publicly-available auxiliary data sets. First, we use locality-level data based on the 2005 Census to test for balance of sociodemographic characteristics that are not present in our administrative banking data. Table 1, Panel B, presents means for these data across localities where treatment and control branches are located. Second, we use data from Mexico's National Institute of Statistics and Geography (INEGI) on the geocoordinates of all commercial bank branches in Mexico to measure the distance between the branches in our experiment and other bank branches. These data are from the 2017 National Statistical Directory of Economic Units (DENUE), and we restrict to bank branches added to the data set by 2011 (the earliest complete wave of the data). Third, we use road data from OpenStreetMap (i) to measure the road distance from each Bansefi branch in our experiment to the nearest control group branch, and to the nearest branch of any bank, and (ii) to determine whether Bansefi branches are located on a "large road" for a heterogeneity test of potential advertising effects. We use historical OpenStreetMap roads data from 2014, which is the earliest date that these data are available and thus is the closest date to 2010, the year of our experiment. Fourth, we use quarterly data from Mexico's National Banking and Securities Commission (CNBV) from 2010. We use the CNBV data to calculate the number of accounts and branches by bank by municipality to estimate where in the size distribution of monthly account openings Bansefi's branches fall. Fifth, we use state shapefiles from INEGI to produce the map in Figure 1.

5 Results

5.1 Branch-Level Results

To estimate branch-level results, which are the most relevant results for a bank considering offering a PLS product, we estimate the following specification separately for each month (or two-month period) t:

$$y_{jt} = \alpha_t + \gamma_t T_j + \theta y_{j0} + \varepsilon_{jt}, \tag{1}$$

where y_{jt} is an outcome at branch j in period t, T_j is a dummy variable indicating that branch j was randomly assigned to treatment, and y_{j0} is the baseline value of the outcome variable. The baseline outcome is included to increase power by absorbing pre-existing variation across branches (McKenzie, 2012).[7]

Account openings. Figure 2 shows the results of estimating equation (1), where the outcome is the number of accounts opened in branch j during month t. We plot the estimated γ_t for each month along with their 95 percent confidence intervals. The first thing to note is that in the eight pre-lottery months, there is no difference between treatment and control branches in the number of accounts opened (as should be expected by virtue of randomization). Then, in the first lottery month, the point estimate is positive but not particularly large or statistically significant. But in the second lottery month, an additional 2.1 accounts per branch are opened in treatment branches ($p < 0.01$), which represents a 67% increase compared to control branches. Finally, we plot γ_t for seven post-lottery months, to look for any persistence in account opening when lotteries were no longer being implemented: we find no lasting effect on account openings after the lottery incentive was removed.

An immediate concern is that there could be substitution across branches in account openings: for example, individuals who would have opened an account that month in a control branch may substitute to opening that account in a treatment branch instead. There are three reasons that this is highly unlikely. First, the average distance from a treatment branch to the closest treatment branch is 73.8 km. Second, the control mean of 3.6 accounts opened per month during the two-month lottery period is very close to

[7]We define baseline as being from January 12, 2009 to January 11, 2010, since we use the 8 months beginning January 12, 2010 for pre-treatment placebo tests using the same specification (1). To calculate y_{j0} we construct y_{jt} at the branch $\times$ month (or two-month) level for each of the baseline months (or two-month periods), then average within branch over the months (or two-month periods) in the baseline period.

the average number of accounts opened per month in control branches during the two-month period before the lotteries (3.7). We fail to reject the null hypothesis that the control mean during the lottery months is different than the control mean during the pre-lottery months ($p = 0.81$). Third, we conduct additional tests that find no evidence of substitution across branches in Section 6.1.[8]

Next, we explore how the effect on the number of accounts opened evolved over time during the two-month lottery period. We plot the treatment effect *by day* in Figure 2b. To more clearly visualize the effect of the lotteries, we also plot a local linear regression, estimated separately for the pre-lottery period, the lottery period, and the post-lottery period. Prior to the introduction of lotteries, there was no difference between treatment and control branches in the number of accounts opened per day. When the lotteries were introduced in mid-September, the treatment effect steadily increased over time, reaching about 0.1 new accounts per branch per day by the end of the lottery period.

There are various potential reasons that the treatment effect increased over time during the lottery months. More individuals might have learned about the lotteries over time through word of mouth, or the first announcement of lottery prize winners on October 12 might have led to "local buzz" about the product that further increased lottery openings in the second month (Guryan and Kearney, 2008; Cole, Iverson and Tufano, 2021). We don't study the local buzz effect directly since, contrary to Guryan and Kearney (2008) and Cole, Iverson and Tufano (2021), most treatment branches had prize-winners due to the large number of prizes.

Immediately after the final lottery on November 12, the treatment effect abruptly falls to zero. More formally, we estimate the daily treatment effect to the left and right of the "discontinuity" (final day of the lottery incentive) using a local linear regression with a triangular kernel and mean-squared error optimal bandwidth (Imbens and Kalyanaraman, 2012), separately on each side of the discontinuity. The estimate to the left of the discontinuity is 0.10 accounts per branch per day ($p < 0.01$), and to the right of the discontinuity is 0.02 accounts per branch per day (not statistically significant, $p = 0.27$). The difference between the estimates to the left and right of the discontinuity is significant at the 5% level using both conventional confidence intervals and the robust bias-corrected confidence intervals recommended by Calonico, Cattaneo and Titiunik (2014).

[8] In Section 6.2 we test for a second type of substitution, not from control Bansefi branches but from branches of other banks. We find no evidence of this type of substitution.

Branch-level deposits and withdrawals. We next estimate specification (1) with the branch-level number of deposits or withdrawals, as well as the volume of pesos deposited or withdrawn, as the outcome. When constructing these variables, we only include deposits and withdrawals made by the client, and exclude deposits made by the bank (e.g., interest payments or lottery prize winnings).[9] We aggregate the data to two-month periods to reduce noise and because the lotteries lasted for two months.

Figure 3 plots the regression coefficients for each two-month period, with the lottery months highlighted in purple. Panel a shows that the number of deposits was similar across treatment and control branches before the lotteries and that they increase to 15.8 more deposits per branch in treatment branches during the two lottery months, which corresponds to 21% of the control branch mean number of deposits. The flow of the number of new deposits at treatment branches continues to be higher after the lottery months for about three years. In sum, the lottery incentive was effective at attracting deposits, and the effect of the temporary incentive on the flow of new deposits persisted even after the incentive was removed. Panel b shows that the number of withdrawals may have increased as well, but in contrast to the results for deposits, the coefficients for withdrawals are imprecisely estimated; we cannot reject the null hypothesis of no effect on the number of withdrawals.

Panels c and d of Figure 3 focus on the peso amounts of deposits and withdrawals. We use the inverse hyperbolic sine (IHS) transformation which allows us to interpret results as percentage changes while allowing for zeros (Bellemare and Wichman, 2020). Variables measured in currency are often notoriously noisy, and the estimates have large confidence intervals. The point estimates from panel c show an increase in the amount of deposits in pesos of 26% in the lottery months, but the point estimate is not statistically significant ($p = 0.124$). There is a statistically significant increase in the amount of deposits at the 10% significance level in 6 periods over the two years after the intervention, and a statistically significant increase at the 5% significance level in 2 periods. In panel d, there is a statistically significant effect on the amount withdrawn in just 1 period after the intervention.

The statistically significant and persistent results on the number of deposits and the nonsignificant but imprecise estimates on the number of withdrawals, combined with the sometimes significant but imprecise estimates on the volume of pesos deposited, suggest that a larger stock of savings might have accumulated at treatment branches due to the PLS product being offered. We turn to this next.

[9] More detail is provided in Appendix B.1.

Branch-level stock of savings. Figure 4a displays the effect of the lottery incentive on the stock of savings at the branch level. The construction of the branch-level stock of savings variable is described in detail in Appendix B.2; importantly, we exclude the lottery prize winnings when constructing the stock of savings to avoid a treatment effect being mechanically driven by the lottery prize payments only happening in treatment branches. From the bank's perspective, the stock of savings (rather than the flow) is the relevant measure as the stock of savings in the bank at a given time determines the amount they can lend using deposit financing. We find a steady increase in the stock of savings in treatment branches relative to control branches that appears to start right after the lotteries were implemented. The difference in cumulative savings between treatment and control branches reach the highest point in Apr–May 2012, a year and a half after the lotteries ended. At that time, the stock of savings in treatment branches was more than double the stock of savings in control branches.

We next decompose the branch-level stock of savings results into effects on accounts opened prior to the lottery months and those opened during the lottery months (still aggregating up to the branch level, within each type of account). Because both types of accounts were eligible to receive the lottery prizes, deposits in both types of accounts could increase due to the PLS offer. For accounts opened prior to the lottery months, we do not see any statistically significant effect on the stock of savings (Figure 4b). For accounts opened during the lottery months, we see a gap in the stock of savings that grows over time starting in the lottery months, and that reaches a peak in Feb–Mar 2013, over two years after the lotteries (Figure 4c). The difference between treated and control branches in the stock of savings in accounts opened during the lottery months is statistically significant from about four months to five years after the lotteries.

5.2 Account-Level Results

In the previous subsection, we showed that the branch-level results on the stock of savings are driven by accounts opened during the lottery months. This could be due to two factors among newly opened accounts. On the extensive margin, more accounts were opened at treatment branches during the lottery months, which would lead to higher branch-level deposits even if deposits per account were equal across treatment and control branches. On the intensive margin, among accounts opened during the lottery months, the PLS incentive may have led new account openers to deposit *more* than new account openers at control branches. We find some evidence that both of these factors contributed to the branch-level results on deposits and withdrawals in Figure 3; however, the intensive margin effect on deposits and withdrawals contributed little to the effect on branch-level stock of savings in Figure 4. Instead, the effect of PLS on the branch-level stock of savings was mostly driven by the extensive margin of more accounts being opened in treatment branches.

We estimate the following specification for accounts opened during the lottery months (September 12 to November 11, 2010) in treatment and control branches, separately for each two-month period t:

$$y_{it} = \alpha_t + \gamma_t T_{j(i)} + \varepsilon_{it}, \tag{2}$$

where i indexes accounts, and there is no control for baseline levels of the outcome variable since these accounts were not opened yet during the baseline months. We use the same outcome variables as before: the number of deposits or withdrawals, IHS of pesos deposited or withdrawn, and IHS of the stock of savings in pesos. $T_{j(i)}$ is equal to one for accounts opened in treatment branches. We use data for the same time period as before, through July 2015. Standard errors are clustered at the branch level since this is the level of treatment.

Because the PLS incentive caused an increase in account openings, new account openers in treatment and control branches during the lottery months are not necessary balanced on characteristics (and we cannot test this as we do not observe account holders' characteristics or pre-treatment account activity since they did not have accounts prior to the lottery incentive). Nevertheless, it is useful to estimate (2) at the account level to see whether deposits per account by new account openers were equal in treatment and control branches (which would imply that the effects in Figure 3a were driven entirely by the extensive margin, i.e., by more accounts being opened in treatment branches).

Figure A3 shows that in the lottery months, the effect on the number of deposits and amount deposited is not statistically significant, whereas account openers in treatment branches do make fewer withdrawals and withdraw less money than those in treatment branches. In the months shortly after the lottery incentive ended, there are some statistically significant and positive effects on the number of deposits and amount deposited. When we look at the stock of savings for accounts opened during the lottery months (Figure A4), we do not find evidence of a difference in the stock of savings between accounts opened during the lottery months in treatment vs. control branches.

Importantly, the above results suggest that those *induced* to open accounts by the lottery persistently continued making *at least as many* new deposits in their accounts as other account openers, and kept a similar stock of savings in their accounts for at least five years. If they were instead "gaming" the incentives by depositing money to take advantage of the chance at winning the lottery prize only to withdraw it shortly after, we would instead observe negative coefficients for deposits and positive coefficients for withdrawals shortly after the lottery incentive ended; furthermore, we would expect the stock of savings in treatment accounts to be *lower* than that of control accounts after the incentive ended.

We next test whether accounts opened during the lottery months were more likely to be closed after the lottery incentive ended, which would also suggest that people were "gaming" the incentives, or that the people induced to open accounts by the lottery incentive decided that having an account was not worth it in the absence of the incentive. It was nevertheless uncommon for accounts to be closed in the years following the lotteries. Three years after the lotteries, 92% of accounts remained open, and nearly five years after the lotteries, 71% remained open. We test whether there was *differential* closing of accounts opened during the lottery months across treatment and control branches by estimating specification (2) with a dummy variable equal to 1 if the account had been closed by month t as the outcome. We do not find any differential probability of closing the account for accounts opened during the lottery months in treatment vs. control branches (Figure A5a). We repeat this test with a more stringent outcome variable equal to 1 if the account had been closed by month t or had a less than 50 pesos stock of savings if open, and again find no differential probability of no longer saving in the account using this measure (Figure A5b).

5.3 Present Value of Bank Profits

An advantage of having a branch-level experiment is that it naturally allows us to calculate branch profits while taking into account any within-branch crowding in or crowding out of accounts and clients. While this is obvious, there are very few savings experiments randomized at the branch level (Table AIII), and we could not find others that measured profits.

A simple way to calculate the profitability of the lottery incentive is the following. First, we calculate the present value of interest revenue from deposits in treatment branches and control branches. We define revenue for branch j on day d as the stock of savings in that branch-day $\textit{Savings Stock}_{jd}$ multiplied by the average daily interest rate the bank earns on assets r_{daily}, which we impute from the Mexican banking sector. Specifically, we calculate the annual interest rate the bank earns on assets using a measure analogous to the ones used in Drechsler, Savov and Schnabl (2021) and Wang, Whited, Wu and Xiao

(2022). We take interest earnings on all loans in 2011 for all commercial banks in Mexico net of estimated losses from loans, and divide this by total loans outstanding in 2011. The resulting annual interest rate that banks earn on assets is $r_{annual} = 8.4\%$.[10]

We take into account the time profile of revenue from deposits by using a discounted sum across days for the period from September 12, 2010 (the first day of the lottery incentive period, which we denote day 0) through July 11, 2015 (which we denote day D). This long period is consistent with the long-run effects of the lottery we document. For the discount factor, we use the rate paid on one-month Mexican Federal Treasury Certificates (CETES) as of September 15, 2010, which was 4.5% annualized; we convert this to a daily rate i_{daily}.[11] The present value of revenues (PVR) for all treatment branches $j \in \mathscr{T}$ summed over all days d is then:

$$PVR^T = \sum_{j \in \mathscr{T}} \sum_{d=0}^{D} \frac{Savings\ Stock_{jd} \cdot r_{daily}}{\left(1 + i_{daily}\right)^d}. \tag{3}$$

We calculate the analogous quantity for control branches, PVR^C, by instead summing over control branches $j \in \mathscr{C}$.

Next, we calculate the present value of costs (PVC) of treatment and control branches. The costs include the cost of managing all accounts in each branch, the fixed interest rate paid on the stock of savings in the accounts, and—in treatment branches—the cost associated with the experiment. To calculate the account management cost, for each month and branch we multiply the number of accounts in branch j and month m, A_{jm}, by the monthly average cost of managing each account, c. The average cost of managing an account was reported to us by Bansefi, and is 14 pesos (1.1 USD) per account per month. We sum across branches and months m using the same sample period as we did for revenues (which we denote month 0 to month M), discounting by the monthly CETES rate $i_{monthly}$ to obtain the present value of management costs.

The interest costs (excluding the lottery prizes, which enter the calculation of PVC separately) are the stock of savings in branch j on day d multiplied by the daily fixed interest rate. The annual interest rate that Bansefi paid on both treatment and control branch accounts was only 0.2%, which we convert to a daily rate f_{daily}. We sum across branches and days, discounting by the daily discount rate i_{daily} to obtain the present value

[10] This calculation is meant to be simple and does not factor in other benefits for banks of being financed with deposits, e.g. better insulation from shocks (Ivashina and Scharfstein, 2010). Additional details on the construction of $Savings\ Stock_{jd}$ for the profits calculation are provided in Appendix B.7.

[11] The source of the CETES rate is `https://tiie.com.mx/cetes-2010/`.

of interest payments on deposits.

The cost of offering PLS, incurred only for treatment branches $j \in \mathscr{T}$, comprised the lottery prizes and the marketing costs. The lottery prizes at treatment branches P_m^T equal 420,000 pesos (32,627 USD) per month aggregated over all treatment branches, for each of the two months in which the lottery was offered. The marketing cost at treatment branches Q_m^T was limited: aggregated across all treatment branches, Bansefi spent 100,000 pesos (7,768 USD) on two posters per branch and loud-speaker cars that operated in late September 2010 and drove around streets close to the branch. The marketing cost was incurred entirely in the first month $m = 0$ as this is when the posters were printed and when the loud-speaker cars operated.[12] Thus, the PVC at treatment branches is:

$$PVC^T = \sum_{j \in \mathscr{T}} \left[\sum_{m=0}^{M} \frac{c \cdot A_{jm}}{(1+i_{monthly})^m} + \sum_{d=0}^{D} \frac{Savings\ Stock_{jd} \cdot f_{daily}}{(1+i_{daily})^d} \right] + \sum_{m=0}^{1} \frac{P_m^T + Q_m^T}{(1+i_{monthly})^m}. \tag{4}$$

The PVC for control branches PVC^C is analogous, replacing $j \in \mathscr{T}$ with $j \in \mathscr{C}$, and $P_m^T + Q_m^T$ with $P_m^C + Q_m^C = 0$.

Using the present value of revenues and costs, we obtain the present value of profits. To account for differences in the number of branches between the treatment and control groups, we scale aggregate profits by dividing them by the number of branches in the treatment and control group, respectively B^T and B^C. We calculate additional profits caused by the lottery incentive as $(PVR^T - PVC^T)/B^T - (PVR^C - PVC^C)/B^C$. Because of randomization, this difference between treatment and control branches estimates the causal effect of the lottery incentive on the present value of profits. Using this calculation, we find that treatment branches earn 6.2% higher profits than control branches due to offering the lottery incentive.

6 Alternative Explanations

In this section, we test for alternative explanations of the treatment effects of PLS on the number of account openings.

6.1 Substitution from Control Branches

Substitution across branches could occur if a person that would have opened an account in a control branch opens it instead at a treated branch due to the lotteries. If this were

[12] In other words, $Q_0^T = 100{,}000$ pesos and $Q_1^T = 0$; we include Q_1^T in (4) for ease of notation.

the case, some of the effect would be due not to an aggregate increase in the number of new accounts being opened at Bansefi, but to a reallocation of newly-opened accounts across branches. In addition to the evidence presented earlier that the average number of accounts opened in control branches does not decrease during lottery months, we can test whether the treatment effect is stronger in treatment branches located closer to control branches, as would be predicted if substitution were driving the results (under the assumption that substitution would occur more among closer branches).

We thus estimate the following specification:

$$y_j = \alpha + \theta y_{j0} + \gamma T_j + \xi \textit{Nearby branches}_j + \phi T_j \times \textit{Nearby branches}_j + \varepsilon_j, \tag{5}$$

where $\textit{Nearby branches}_j$ is one of two measures of how far each branch is from a control Bansefi branch, and the outcome variable y_j is the number of accounts opened at branch j over the two lottery months. The first measure of $\textit{Nearby branches}_j$ is a dummy variable equal to 1 if branch j is below the median road distance to the closest control branch.[13] Table 2, column 2, shows the results (while column 1 shows the results from the same regression (5) without the $\xi \textit{Nearby branches}_j + \phi T_j \times \textit{Nearby branches}_j$ terms for comparison). The coefficient on the interaction term is not statistically significant; furthermore, the sign of the point estimate is negative, which is the opposite of the expected sign if substitution were occurring (as treatment branches closer to control branches would then have a larger treatment effect). The second measure is a continuous measure of log distance to the closest control branch. The coefficient on the interaction term is again not statistically significant, and although the point estimate has the sign that would be expected if substitution were occuring, the magnitude of the coefficient is very small.[14]

6.2 Substitution from Other Banks

Would the additional accounts opened at treatment branches during lottery months have instead been opened at other banks in the absence of the PLS incentive? While this would not affect the validity of our estimates measuring the effect of a bank offering PLS on the number of accounts opened at that bank, it affects whether PLS products increase finan-

[13] Appendix B.8 describes how road distances to the closest control branch and to the closest branch of any commercial bank are computed.

[14] The coefficient of –0.07 multiplied by the interquartile range of log distance to the closest control branch of 3.25 is only –0.23 accounts, compared to a control mean of 6.8 accounts opened over the lottery period. Thus, using the point estimate even though it is not statistically significant, going from the 25th percentile to the 75th percentile of distance to the closest branch only leads to 0.23 fewer accounts opened, compared to a control mean of 6.8 accounts opened over the lottery period.

cial inclusion. To test this, we estimate specification (5) with three additional measures of $Nearby\ branches_j$ that capture distance to branches of any commercial bank rather than to control Bansefi branches.

The first measure is a continuous measure of the number of commercial bank branches within a 1 km road distance, where the median Bansefi branch has 5 commercial bank branches located within 1 km. The coefficient on the interaction term is again not statistically significant and very close to zero (Table 2, column 4).[15] The second measure is a dummy for branch j being below the median distance to the closest commercial bank branch, and the third measure is a continuous measure of log distance to the closest commercial bank branch. We again do not find evidence of a heterogeneous treatment effect for these two measures, as the coefficients on the interaction terms are not statistically significant, with one having the opposite sign of what would be expected if substitution were occurring (column 5) and the other having the expected sign (column 6). Taken together, these results suggest that the new account openers incentivized by PLS would not have instead opened accounts in other banks in the absence of Bansefi offering PLS.

An additional piece of evidence against substitution across banks driving the effect we document is that the new account openers are nearly all previously unbanked individuals. Using a database accessed at Mexico's Central Bank that links bank accounts within individuals across banks, we find that 96% of new account openers in treatment branches in lottery months were previously unbanked at any bank. Thus, PLS attracted previously unbanked individuals, who are unlikely to be people who would have opened accounts anyway in other bank branches given the very slow change to the proportion of people in Mexico with a bank account over time (Demirgüç-Kunt et al., 2018).

6.3 Marketing Effect

One worry is that advertising the PLS accounts could increase account openings through a pure advertising effect. In other words, it could be the marketing of bank accounts in general, rather than the appeal of the lottery incentive, that led to increased account openings.

Of course for the lottery to have an effect people need to know about it. The question here is not if the marketing is necessary (it is), but whether it is sufficient to increase

[15] The coefficient of 0.06 multiplied by the interquartile range of the number of branches within 1 km of 6.0 is only 0.36 accounts, compared to a control mean of 6.8 accounts opened over the lottery period. We do not include an analogous heterogeneity test by number of branches within 1 km in the tests for substitution from control Bansefi branches, as no branches in our sample are within a 1 km road distance of a control branch.

account openings in the absence of the PLS incentive. To separate the effect of marketing from the effect of the lottery, the ideal setting would be to have had a third group of branches with marketing but no lotteries. Sample size limits restricted us from running this ideal experiment. Nevertheless, we have two pieces of evidence that suggest that marketing alone is not driving the effect.

Two types of marketing took place: loud-speaker cars on nearby streets and signs in the branches. Due to budget constraints, the advertising through loud-speaker cars only occurred in late September 2010. Thus, a pure marketing effect driven by the loud-speaker car marketing would be expected to be more concentrated at the beginning of the two-month lottery period, which is not what we find in Figure 2b. Furthermore, even if this advertising took time to take effect, if loud-speaker car advertising were indeed the cause of the treatment effect (independent from the lottery incentive), we would not expect to see a sudden drop in account openings immediately after the last lottery on November 12. Because the loud-speaker car advertising only occured in September 2010, the combination of the gradual increase in the daily treatment effect over the two-month lottery period and its sharp discontinuity after November 12 in Figure 2b provides strong evidence against a marketing effect driven by the loud-speaker cars.

For the sign advertising in branches, it is more likely that people would see these signs if the branch is located on a larger street with more people passing by. Thus, we would expect a larger effect in branches located on larger streets if the in-branch marketing is driving the effect. We test this hypothesis by estimating the following specification:

$$y_j = \alpha + \theta y_{j0} + \gamma T_j + \xi Large\ road_j + \phi T_j \times Large\ road_j + \varepsilon_j, \qquad (6)$$

where $Large\ road_j$ is a dummy indicating that Bansefi branch j is located on a large road and the outcome variable y_j is the number of accounts opened at branch j over the two lottery months. Using the OpenStreetMap street classification, we follow Talamas (2022) and classify primary, secondary, or tertiary roads as large roads, and classify residential roads as not large roads.[16] Approximately half (48%) of Bansefi branches are located on large roads using this definition. Table 2, column 7, presents the results of this estimation. We do not find evidence that being located on a large road increases the treatment effect, as ϕ is not statistically significant.

[16]There are two road classifications larger than primary roads: motorways and trunk roads. None of our Bansefi branches are located on motorways or trunk roads. Among the branches located on non-large roads in our sample, 49 are located on residential roads and 8 are located on four additional categories of non-large roads: service roads, living streets (a type of residential road with additional rules), pedestrian roads, and footways.

7 Conclusion

Lotteries have been popular for centuries, as have prize-linked savings products where lottery incentives are used to induce people to save. However, we still know relatively little about their effectiveness. Although a literature exists around PLS products, most papers are observational or lab-in-the-field experiments. We measure the effect of PLS using an RCT across 110 bank branches covering 19 states in Mexico. Additionally, we have data to measure deposits and savings for five years after the intervention, which is rare in RCTs of savings interventions.

We find that offering prize-linked savings accounts increases deposits by inducing new savers to open accounts. The effects of PLS on account openings that we find are an order of magnitude larger than the effects of increases to fixed interest rates in other studies (which are larger than the increase in the *expected* interest rate in our study). Moreover, we find that although the lottery prizes were only offered over a two-month period, these temporary incentives created a persistent increase in the branch-level *flow* of deposits and stock of savings, driven by the additional accounts that were opened in treatment branches during the lottery months. Finally, taking into account revenues from additional deposits attracted by PLS and the costs of maintaining the additional accounts and providing and marketing the PLS incentive, the product was profitable for the bank, increasing branch-level profits by 6.2%.

We are not able to answer *why* a temporary lottery incentive had a persistent effect on the flow of deposits. First, why did a lottery incentive have such a large effect on account openings compared to changes to guaranteed interest rates in other studies? Second, why did those who opened an account due to the lottery incentive continue depositing into the account after the incentive was removed?

On the *large* effect of PLS in our study compared to the null effects of changes to guaranteed interest rates in other studies (Karlan and Zinman, 2018; Kast, Meier and Pomeranz, 2018), there is evidence that people tend to overweight small probabilities (Tversky and Kahneman, 1992; Wu and Gonzalez, 1996) and that there are high rates of return to the indivisible assets they could purchase if they win the lottery prize (Balboni et al., 2022; Kaboski, Lipscomb, Midrigan and Pelnik, 2022). Furthermore, we speculate that the reason other studies have found that low-income households' savings are inelastic to changes in a guaranteed interest rate is that these small (in dollar terms) interest rate payments get swamped by other costs the poor face to save, such as the cost of traveling to the bank (Bachas, Gertler, Higgins and Seira, 2018).

On the *persistent* effect of PLS on the flow of deposits after the lottery incentive was removed, several mechanisms could be at play. One possibility is that there are fixed costs of opening an account, and that the lottery incentive tilted the cost-benefit calculation towards opening an account for those at the margin; then, once the fixed cost had already been incurred, the marginal benefit of using the account after the incentive was removed exceeded the marginal cost. The fixed cost explanation, however, is inconsistent with studies that subsidize the fixed cost of opening an account but do not find persistent effects on savings (e.g., Cole, Sampson and Zia, 2011; Dupas, Karlan, Robinson and Ubfal, 2018). A second possibility is learning-by-using (Breza, Kanz and Klapper, 2022; Agarwal, Cho, Choi and Klapper, 2023). A bank account may be an experience good (Giné and Goldberg, 2023), where new account holders learn that the benefits of saving outweigh the costs only by saving in the account for a couple of months, which the lottery prize incentivizes them to do. Future research could focus on disentangling the mechanisms underlying the large and persistent treatment effects of PLS.

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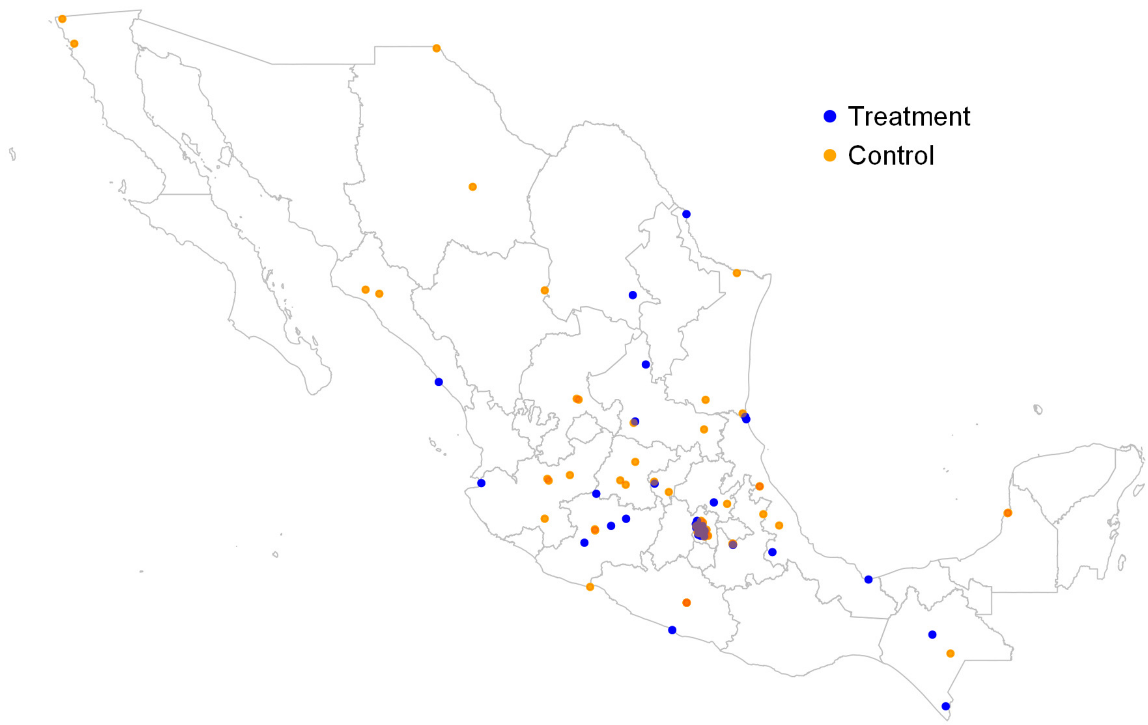
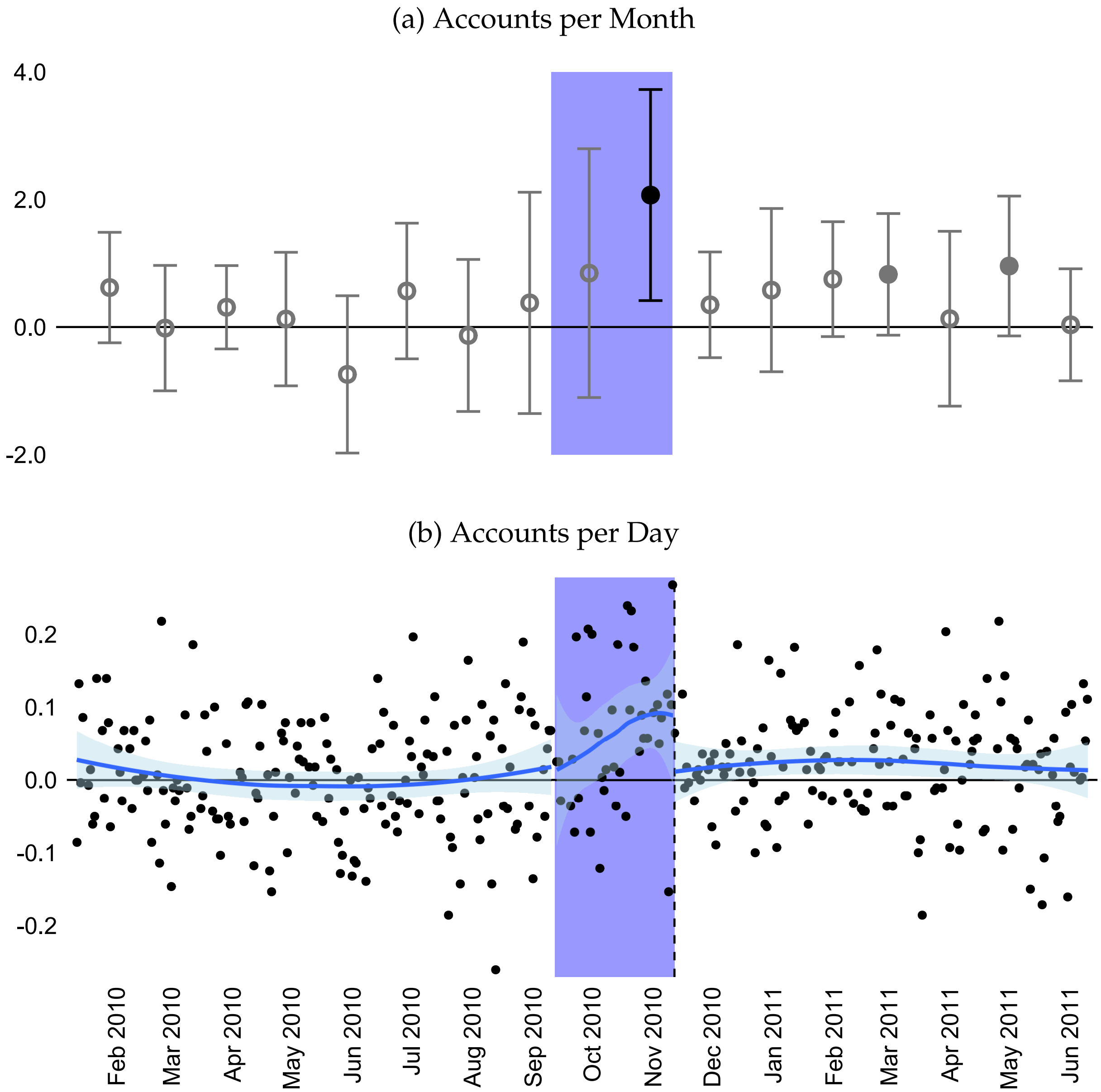
Figure 1: Treatment and Control Branches



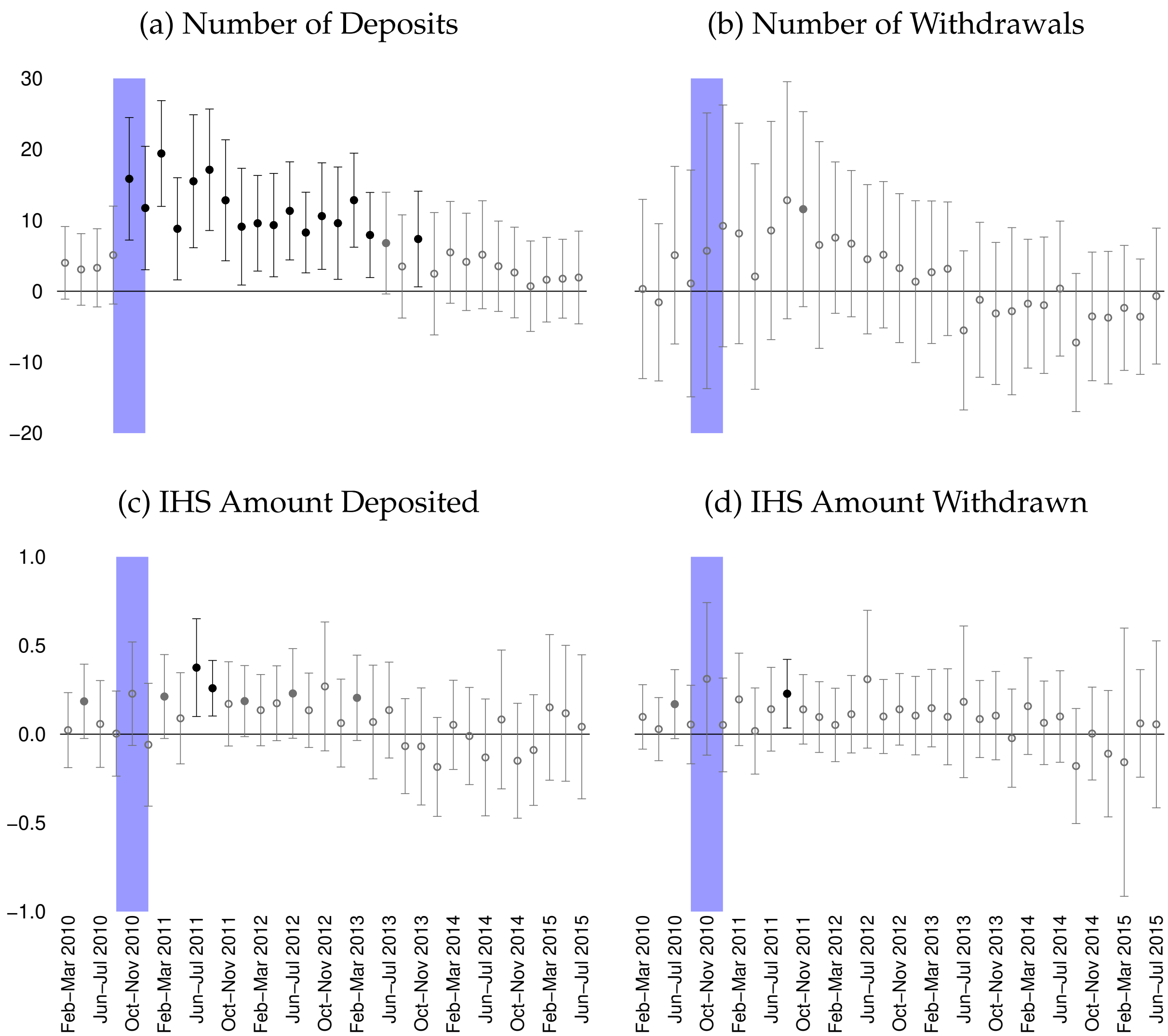
This figure shows the geocoordinates of the 40 treatment and 70 control Bansefi branches in our experiment.

Figure 2: Effect of Lotteries on Number of Accounts Opened per Branch



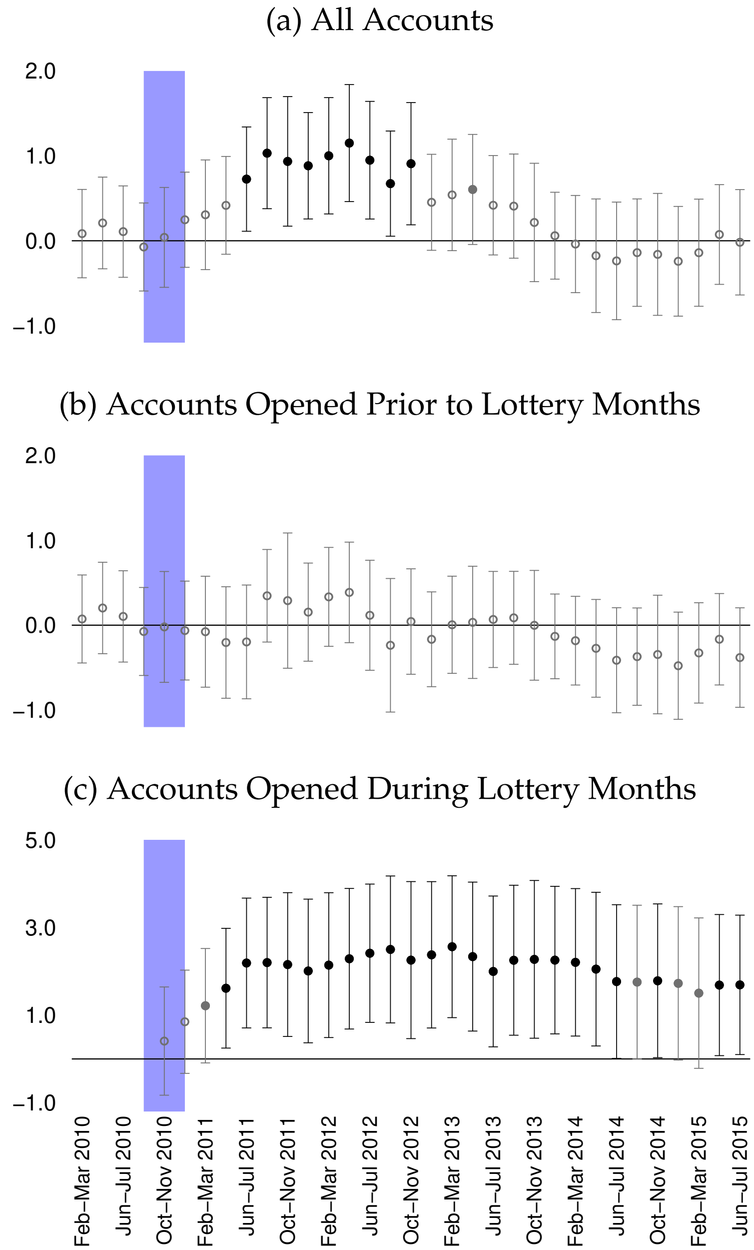
This figure shows the treatment effect of the lottery incentive on the number of accounts opened at the branch level. Panel a plots the γ_t coefficients from specification (1), where the dependent variable is the number of accounts opened in branch j in month t, using administrative data from Bansefi. Each one-month period refers to the period from the 12th of the prior month to the 11th of the month listed: for example, Oct 2010 refers to the period from September 12, 2010 to October 11, 2010. Each coefficient is from a separate regression for that month, and $N = 110$ branches in each regression. Whiskers denote 95% confidence intervals. Standard errors are clustered at the branch level. Filled black circles indicate results that are statistically significant at the 5% level, filled gray circles indicate statistically significant at the 10% level, and hollow gray circles indicate not statistically significant. Panel b plots the treatment effect on number of accounts opened by day. The blue line is a local linear regression, estimated separately for days before, during, and after the lottery incentive. The light blue shaded area around the local linear fit shows its 95% confidence interval. The final lottery on November 12, 2010 is represented by a dashed vertical line. In both panels, the purple highlight indicates the timing of the lottery (Oct–Nov 2010).

Figure 3: Effect of Lotteries on Branch-Level Flow of Deposits and Withdrawals



This figure shows the treatment effect of the lottery incentive on the branch-level number of deposits (panel a), number of withdrawals (panel b), inverse hyperbolic sine (IHS) of the amount of pesos deposited (panel c), and IHS of the amount of pesos withdrawn (panel d). The figure plots the γ_t coefficients from specification (1). Each two-month period refers to the period from the 12th of the month prior to the first month listed to the 11th of the second month listed: for example, Oct–Nov 2010 refers to the period from September 12, 2010 to November 11, 2010. The purple highlight indicates the timing of the lottery (Oct–Nov 2010). The dependent variables in all panels are winsorized at the 95th percentile within each treatment group by day prior to aggregating to the two-month period level. Each coefficient is from a separate regression for that two-month period, and $N = 110$ branches in each regression. Standard errors are clustered at the branch level. Whiskers denote 95% confidence intervals. Filled black circles indicate results that are statistically significant at the 5% level, filled gray circles indicate statistically significant at the 10% level, and hollow gray circles indicate not statistically significant.

Figure 4: Effect of Lotteries on Branch-Level Stock of Savings



This figure shows the treatment effect of the lottery incentive on the branch-level inverse hyperbolic sine (IHS) of the stock of savings in pesos. Panel a uses transactions from all accounts, panel b restricts to accounts opened prior to the lottery months, and panel c restricts to accounts opened during the lottery months. The figure plots the γ_t coefficients from specification (1) without controlling for the baseline mean, since the stock calculation only begins from January 12, 2010 (for more details, refer to Appendix B.2). Each two-month period refers to the period from the 12th of the month prior to the first month listed to the 11th of the second month listed: for example, Oct–Nov 2010 refers to the period from September 12, 2010 to November 11, 2010. The purple highlight indicates the timing of the lottery (Oct–Nov 2010). The net amount deposited is winsorized at the 95th and 5th percentiles (since it can take negative values) within each treatment group by day prior to aggregating to the stock of savings at the two-month period level. Each coefficient is from a separate regression for that two-month period, and $N = 110$ branches in each regression. Standard errors are clustered at the branch level. Whiskers denote 95% confidence intervals. Filled black circles indicate results that are statistically significant at the 5% level, filled gray circles indicate statistically significant at the 10% level, and hollow gray circles indicate not statistically significant.

Table 1: Summary Statistics and Balance

Variable	Control (1)	Treatment (2)	Difference (3)
Panel A: Branch-Level Data			
Accounts opened per month	3.86	3.51	–0.35
	(0.48)	(0.36)	(0.60)
Number of deposits	35.18	34.54	–0.64
	(2.29)	(2.51)	(3.39)
Number of withdrawals	62.49	62.06	–0.43
	(4.18)	(5.33)	(6.76)
Amount withdrawn (IHS of pesos)	12.11	12.14	0.03
	(0.10)	(0.12)	(0.16)
Amount deposited (IHS of pesos)	12.11	12.12	0.00
	(0.10)	(0.13)	(0.16)
Stock of savings (IHS of pesos)	11.86	11.73	–0.13
	(0.11)	(0.14)	(0.18)
Total number of accounts per branch	132.67	116.87	–15.80
	(11.36)	(10.91)	(15.72)
Road distance to closest control branch (km)	62.73	73.83	11.09
	(15.40)	(17.59)	(23.33)
Panel B: Locality-Level Data (2005)			
Log population	12.73	12.88	0.15
	(0.13)	(0.14)	(0.19)
Bansefi branches per 100,000	0.93	0.81	–0.12
	(0.08)	(0.09)	(0.12)
% illiterate	3.86	3.78	–0.08
	(0.23)	(0.42)	(0.48)
% not attending school	3.40	3.43	0.03
	(0.14)	(0.24)	(0.28)
% with dirt floors	3.14	2.89	–0.25
	(0.40)	(0.62)	(0.74)
% without piped water	3.10	3.05	–0.05
	(0.56)	(0.66)	(0.86)
% without electricity	4.83	5.26	0.43
	(0.23)	(0.30)	(0.38)
Average occupants per room	1.00	0.97	–0.03
	(0.02)	(0.03)	(0.03)
F-statistic			0.85
p-value			[0.62]
Number of observations	70	40	110

This table shows average baseline characteristics in the control group (column 1) and treatment group (column 2), and tests for balance by regressing each variable separately on a treatment dummy (column 3). Panel A uses baseline administrative banking data, where baseline is defined as the monthly average from January 12, 2009 to January 11, 2010 since we use the 8 months beginning January 12, 2010 as placebo tests in some specifications. The number of deposits, number of withdrawals, and the inverse hyperbolic sine (IHS) of peso amounts are all winsorized at the 95th percentile within treatment group by day prior to aggregating to the branch by month level (see Appendix B.1); the stock of savings is constructed from the net amount deposited winsorized at the 95th and 5th percentiles (since it can take negative values) within treatment group by day prior to aggregating to the branch by month level (see Appendix B.2). Panel B uses locality-level data from the 2005 Census. The F-statistic and p-value are from an omnibus F-test where the binary treatment indicator is regressed on all the branch-level and locality-level variables reported in panels A and B in a single regression. Standard errors are clustered at the branch level. In column 3, * indicates $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table 2: Heterogeneous Effects of Lotteries on Number of Accounts Opened per Branch

	Number of Accounts opened between lotteries						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment branch	2.45**	2.81*	2.70	2.07	2.85*	1.82	2.12
	(1.12)	(1.65)	(1.99)	(1.85)	(1.60)	(1.52)	(1.53)
Below–median road distance to closest control branch		0.04					
		(1.14)					
Treatment × below–median distance to closest control branch		–0.83					
		(2.26)					
Log road distance to closest control branch (km)			–0.09				
			(0.27)				
Treatment × log distance to closest control branch (km)			–0.07				
			(0.63)				
Commercial branches within 1 km road distance				–0.15			
				(0.16)			
Treatment × commercial branches within 1 km				0.06			
				(0.26)			
Below–median distance to closest commercial branch					–0.49		
					(1.12)		
Treatment × below–median distance to closest commercial branch					–0.97		
					(2.18)		
Log distance to closest commercial branch (km)						0.15	
						(0.53)	
Treatment × log distance to closest commercial branch (km)						–0.59	
						(1.20)	
Branch located on large road							–0.18
							(1.12)
Treatment × branch located on large road							0.71
							(2.25)
Accounts opened, control mean	6.80	6.80	6.80	6.80	6.80	6.80	6.80
Average heterogeneity variable (levels)		0.50	61.8	5.25	0.50	0.53	0.48
Number of observations	110	110	110	110	110	110	110

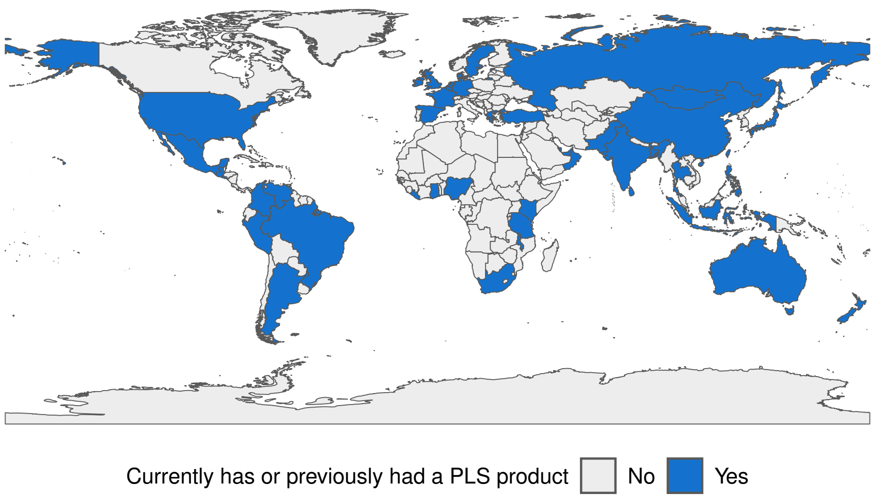
This table shows heterogeneous treatment effects of the lottery incentive on the number of accounts opened per branch over both lottery months (between September 12 and November 11, 2010). It shows the treatment effect of the lottery incentive in column 1, and includes interactions with (a) a dummy indicating below-median road distance to the closest control branch in column 2, (b) log road distance to the closest control branch in kilometers (km) in column 3, (c) the number of commercial branches within a 1 km road distance in column 4, (d) a dummy indicating below-median road distance to the closest commercial branch in column 5, (e) log road distance to the closest commercial bank branch in column 6, and (f) a dummy indicating that the branch is located on a large road in column 7. Estimations control for the average baseline number of account openings, where we define baseline as being from January 12, 2009 to January 11, 2010 and average over two-month periods within the baseline period. All continuous variables are winsorized at the 95th percentile within treatment group. Standard errors are clustered at the branch level. * indicates $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Internet Appendix

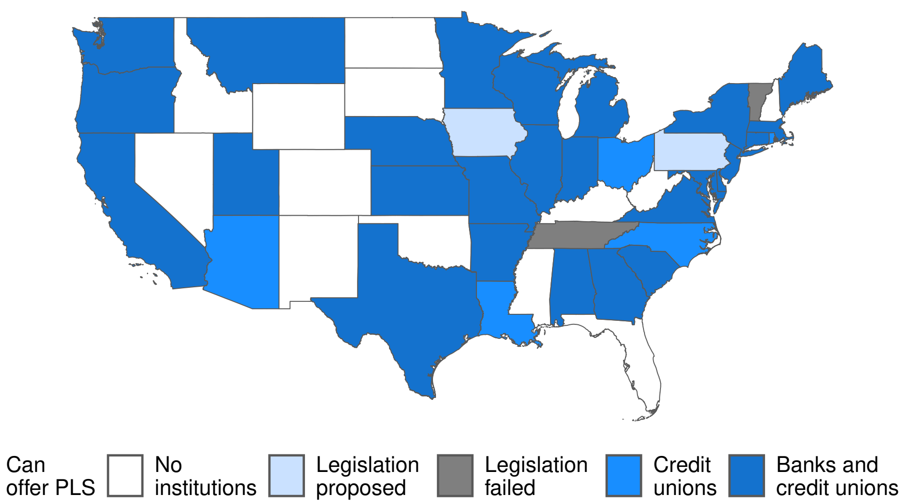
A Appendix Figures and Tables

Figure A1: PLS Products

(a) Around the World



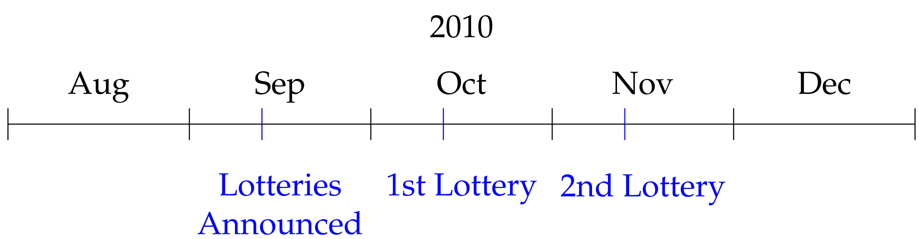
(b) In the US



This figure shows which countries currently have or previously had a PLS product (panel a), and which states in the United States legally allow credit unions and banks to offer PLS products (panel b). Panel a uses the data from Table AI; to produce the data, we started with data in Cole, Tufano, Schneider and Collins (2007) and Kearney, Tufano, Guryan and Hurst (2011) and updated it by searching the internet for additional, more recent PLS products. Panel b shows which states legally allow credit unions and banks to offer PLS products; in all states in which credit unions are legally allowed to offer PLS products, we found examples of credit unions offering PLS products.

Figure A2: Experiment description

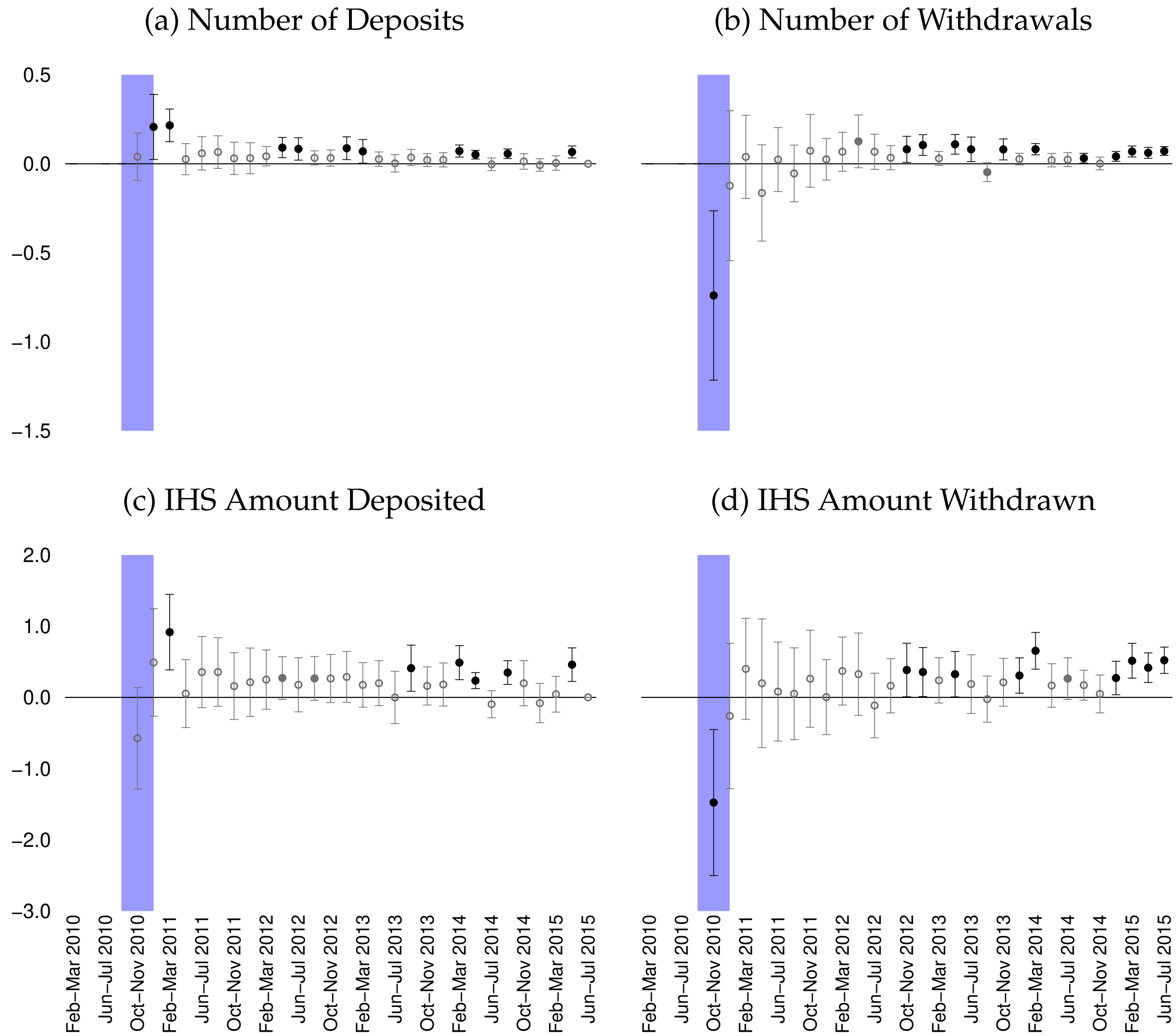
(a) Timeline of the experiment



(b) Poster

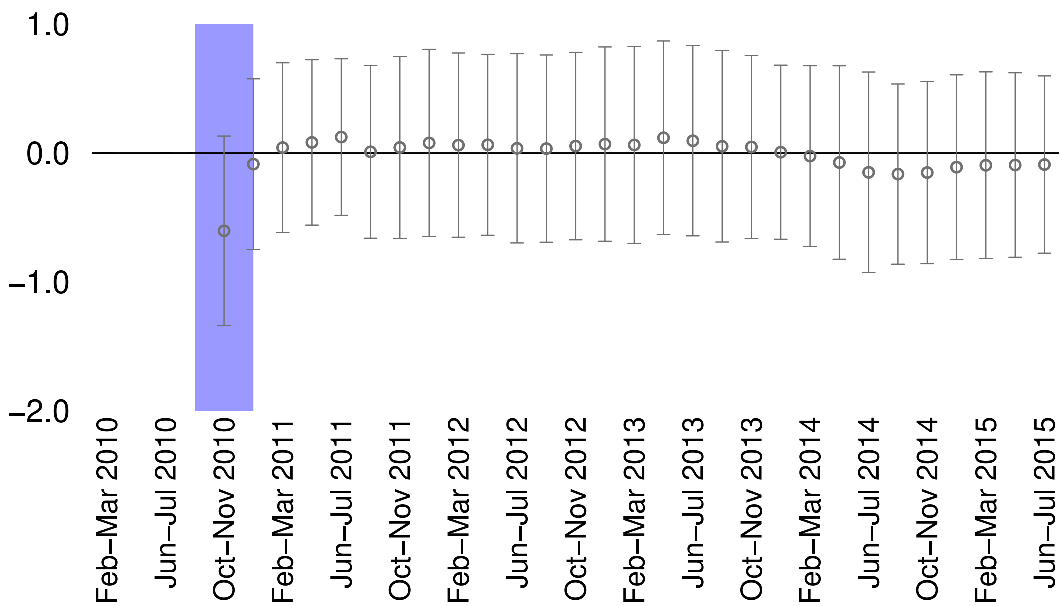
This figure shows the timeline of our RCT (panel a), and the poster that was hung in treatment branches describing the PLS product (panel b).

Figure A3: Account-Level Results for Accounts Opened During Lottery Months



This figure shows the effect of the lottery incentive on the account-level number of deposits (panel a), number of withdrawals (panel b), inverse hyperbolic sine (IHS) of the amount of pesos deposited (panel c), and IHS of the amount of pesos withdrawn (panel d) for accounts opened during the lottery months. The figure plots the γ_t coefficients from specification (2). Each two-month period refers to the period from the 12th of the month prior to the first month listed to the 11th of the second month listed: for example, Oct–Nov 2010 refers to the period from September 12, 2010 to November 11, 2010. The purple highlight indicates the timing of the lottery (Oct–Nov 2010). The dependent variable is winsorized at the 95th percentile within each treatment group by two-month period. Each coefficient is from a separate regression for that two-month period, and $N = 891$ accounts in each regression. Standard errors are clustered at the branch level. Whiskers denote 95% confidence intervals. Filled black circles indicate results that are statistically significant at the 5% level, filled gray circles indicate statistically significant at the 10% level, and hollow gray circles indicate not statistically significant.

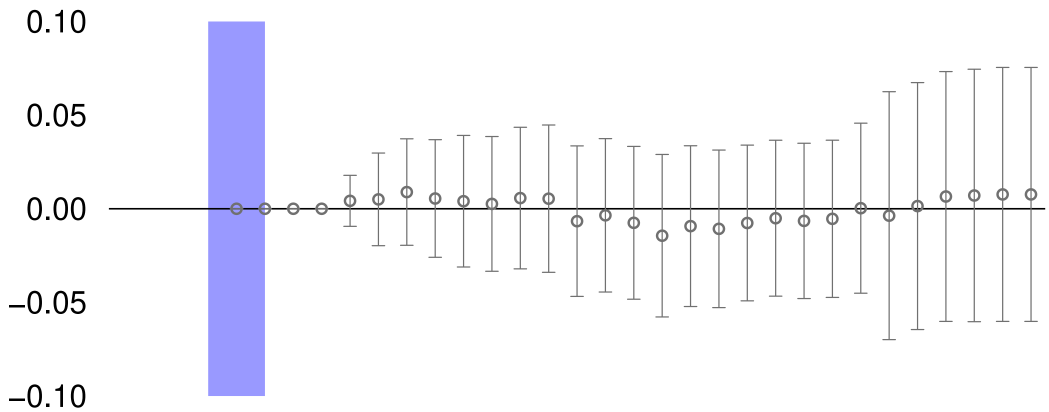
Figure A4: Account-Level Stock of Savings for Accounts Opened During Lottery Months



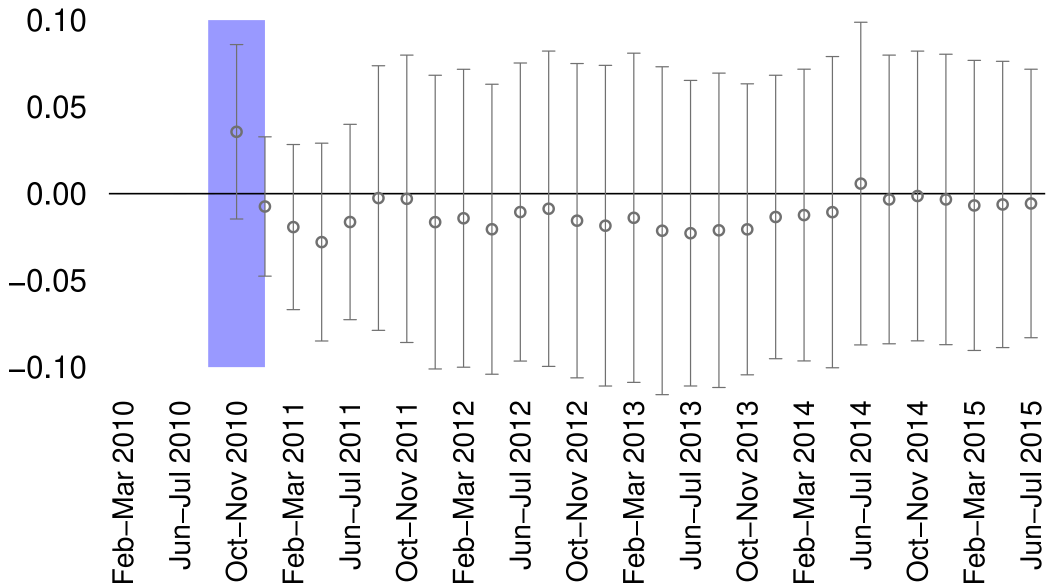
This figure shows the effect of the lottery incentive on the account-level inverse hyperbolic sine (IHS) of the stock of savings in pesos for accounts opened during the lottery months. The figure plots the γ_t coefficients from specification (2). Each two-month period refers to the period from the 12th of the month prior to the first month listed to the 11th of the second month listed: for example, Oct–Nov 2010 refers to the period from September 12, 2010 to November 11, 2010. The purple highlight indicates the timing of the lottery (Oct–Nov 2010). The dependent variable is winsorized at the 95th percentile within each treatment group by two-month period. Each coefficient is from a separate regression for that two-month period, and $N =$ 891 accounts in each regression. Standard errors are clustered at the branch level. Whiskers denote 95% confidence intervals. Filled black circles indicate results that are statistically significant at the 5% level, filled gray circles indicate statistically significant at the 10% level, and hollow gray circles indicate not statistically significant.

Figure A5: Account Inactivity of Accounts Opened During Lottery Months

(a) Account Closed



(b) Account Closed or Stock of Savings < 50 pesos



This figure shows the effect of the lottery incentive on account closings (panel a), and account closings or the stock of savings being less than 50 pesos (panel b) for accounts opened during the lottery months. The figure plots the γ_t coefficients from specification (2). Each two-month period refers to the period from the 12th of the month prior to the first month listed to the 11th of the second month listed: for example, Oct–Nov 2010 refers to the period from September 12, 2010 to November 11, 2010. The purple highlight indicates the timing of the lottery (Oct–Nov 2010). Each coefficient is from a separate regression for that two-month period, and $N = 891$ accounts in each regression. Standard errors are clustered at the branch level. Whiskers denote 95% confidence intervals. Filled black circles indicate results that are statistically significant at the 5% level, filled gray circles indicate statistically significant at the 10% level, and hollow gray circles indicate not statistically significant.

Table AI: PLS Products around the World

Country (1)	Program (2)	Institution (3)	Year Start (4)	Year End (5)	Web Source (6)
Argentina	”El libretón” account	BBVA Banco Frances del Rio de la Plata	1997	2018	Yumpu (bbva);Rankia Website (2018)
Argentina	Prize-Linked Savings	Santander Banco Rio de la Plata	1997	Unknown	NA
Australia	Save to Win Account	Bank of Queensland	2011	2012	BOQ Website (2010)
Bahrain	MyHassad	Ahli United Bank	2020	2022	Ahli United (Savings Account)
Bangladesh	Prize Bonds	Bangladesh Bank	1972	To date	All Result BD Website (2021)
Belgium	Government Lottery Bonds	Belgian government	1921	Unknown	Gilson et al. (2014)
Brazil	HiperFundo account	Banco Bradesco	2003	To date	Bradesco Website;Bradesco pdf (2020)
Brunei/ Borneo	BIBD Aspirasi 3	Bank Islam Brunei Darussalam (BIBD)	2019	To date	Borneo Bulletin Website (2021);BIBD Website
China	Second Nationalist Government Lottery Loan bond	Republic of China	1926	1927	Joelscoins Website
Colombia	”El libretón” account	Banco Bilboa Vizcaya	1997	Unknown	NA
Colombia	”El libretón” account in 2006, any account in 2019	BBVA Colombia	2006	2020	Semana Website (2006);BBVA Website;BBVA pdf
Colombia	Ahorrar sÍ Paga	Banco Agrario de Colombia	2014	2014	Bancoagrario Website (2014)
Denmark	Premieobligationer	Danmarks Nationalbank	1972	Unknown	NA
El Salvador	Experiencia Promerica	ClubPromerica	2021	2021	ClubPromerica (El Salvador)
France	Government Lottery Bonds	Credit Foncier	Late 1870s	1970s	Yates (2012)

Gambia	GTBank Save and Win	GTBank	2016	2016	The Point Website (2016)
Gambia	Sakhanal Savings Account	Vista Bank	At least 2013	To date	Vista Bank Website;FIBANK Website (2013)
Germany	Gewinnsparen account (PS Sparen und Gewinnen)	Gewinnsparverein e.V. (and local branches)	1952	Unknown	NA
Ghana	FBNBank Save and Win	FBNBank Ghana Limited	2017	2019	allAfrica Website (2019);GhanaWeb Website (2018)
Ghana	WinBig	Ecobank Ghana	2012	2013	NewsGhana Website (2012)
Greece	Ethniko Lahiotoro Danio	Bank of Greece	Unknown	Unknown	ISDA (1997)
Guatemala	Ahorro con Sorteo	Banco Promerica	2019	To date	Banco Promerica Website
Guatemala	Contiefectivita (cuenta de anselmo)	Banco G&T Continental	1999	To date	GTC Bank (Contiefectivita)
Guatemala	Cuenta de Ahorros Serie A	Banco G&T Continental	Unknown	To date	GTC Website
Holland	NA	Estates General	1709	1713	The Free Library Website (2014)
India	Premium Prize Bonds	Reserve Bank of India	1963	Unknown	Internet Archive (Gazette of India)
Indonesia	SIMPEDES	Bank Rakyat Indonesia	1986	To date	HNB Website;BRI (Simpedes);OED Precis pdf (1996);USAID pdf;Robinson (2002)
Ireland	Prize Bonds	The Prize Pond Company on behalf of the National Treasury Management Agency (NTMA)	1956	To date	Ireland State Savings Website;Ireland State Savings pdf;RTE Website (2018)
Japan	Lottery linked 1-year time deposits	Jonan Shinkin Bank	1994	Unknown	LA Times Website (1995);Trevor (2001)

Japan	Postal Savings System	Japanese Government	1940s	1975	Guillen and Tschoegl 2002 citing Hulme 1995
Kenya	Premium Bond	Kenya Post Office Savings Bank	1978	At least 1999	NA
Kenya	Stawisha na M-Shwari	CBA	2016	2016	NA
Liberia	Save and Win	United Bank for Africa (UBA)	2019	2020	allAfrica (Liberia)
Malawi	Dabo Dabo Deposits promotion	FINCA Malawi	2021	2021	Finca (Dabo Dabo);Malawi24 Website (2021)
Malawi	Nkhawa Njee	FINCA Malawi	2017	2017	Finca (2017)
Malawi	Pa Mawa	Malawi Standard Bank	2018	2018	The Maravi Post Website (2018)
Mexico	"El libretón" account	BBV probursa	1996	2000	Schulz (2004);WritePass Website (2016)
Mexico	"Superlibreta" account	Santander Mexico	1997	Unknown	Beatriz et al. (2000)
Mexico	Ahorro Flexible HSBC "Monederos"	HSBC	2021	2021	HSBC (Flexible Savings)
Mexico	Libretón	BBVA Bancomer	2000	2017	BBVA Bancomer (2012 Annual Report);Kearney et al (2010)
Mexico	Promoción Gana un Mini	HSBC	2020	2021	HSBC (mini cooper)
Mongolia	XacHuu Birthday Promotional Campaign	XacBank	2014	2014	XacBank (Disneyland)
Mongolia	XacLottery	XacBank	2011	2013	XacBank (Lottery winners)
New Zealand	Bonus Bonds	ANZ Banking Group (Ministry of Finance and Post Office) Savings Bank until 1990)	1970	2020	Bonus Bonds Website;MSN news website (2020)
Nigeria	DiamondXtra	Access Bank	2008	To date	Access (DiamondXtra account);Access (DiamondXtra rewards)

Nigeria	Double Your Target Savings Promo	Ecobank Nigeria	2017	2018	Invest Advocate Website (2017)
Nigeria	Ecobank Super Rewards	Ecobank Nigeria	2021	To date	EcoBank Nigeria Facebook post
Nigeria	Fund & Win	Access Bank	2021	2021	Access Facebook Post
Nigeria	Save and Excel (I-Save-I-Win)	First Bank of Nigeria	2011	2011	Kanz, World Bank (PowerPoint)
Nigeria	UBA Savings Promo	United Bank for Africa (UBA)	2021	To date	UBA (Savings account);UBA (Bumper account);FinancialEDGE Website (2021)
Nigeria	Win Big with Ecobank	Ecobank Nigeria	2012	2012	PFCAfrica website
Nigeria	XploreFirst Account	First Bank of Nigeria	2018	2019	FirstBank (XploreFirst)
Oman	al Mazyona	Bank Muscat	2013	To date	Bank Muscat (Current Account);Times of Oman Website (2018);Bank Muscat (Winners)
Oman	Bushra Prize Savings Account	Alizz Islamic Bank	Unknown	To date	Alizz Islamic Website;Alizz Islamic Youtube Video
Oman	Maisarah Prize Account	Maisarah Islamic Banking Services	2016	To date	Maisarah-Oman Website
Oman	Mandoos Savings Account	HSBC	2013	To date	HSBC (Mandoos Savings Account);HSBC pdf (Prize Draws);HSBC pdf (Terms and Conditions);Albawaba Website (2014);Oman Observer (2017);Zawya (2015);Albawaba Website (2013)

Oman	Mandoos Savings Account	Oman International Bank	1992	2012	Arabian Business Website (2012)
Oman	Meethaq Hibati	Meethaq	2018	To date	Meethaq Website (2018);Meethaq (Savings account)
Oman	Savings Scheme	BankDhofar	2015	To date	Zawya Website (2021)
Pakistan	CarAmad account	Bankers Equity	1999	2001	NA
Pakistan	Crorepati	Habib Bank	1999	2001	NA
Pakistan	maala-maal account	Muslim Commercial Bank	1999	2001	NA
Pakistan	Prize Bonds	State Bank of Pakistan, National Savings Organization	1972	To date	Saving Website
Pakistan	ZarAmaad account	United Bank	1998	2001	NA
Peru	Cuenta Ganadora	BBVA Peru	2019	2019	BBVA Website (Soles and Benefits)
Peru	Cuenta Premio BCP	Banco de Credito BCP	2011	To date	BCP (Prize Account);BCP (Award Account);Noticias Website (2011)
Philippines	Premyo Bonds	Bureau of the Treasury	2019	To date	Treasury Website (Premyo Bonds Calculator);Treasury Website (Draw Guidelines)
Qatar	Misk Account	Qatar Islamic Bank (QIB)	2020	To date	QIB Website;Gulf Times (2020)
Qatar	Thara'a	Dukhan Bank	2015	To date	Dukhan Bank Website
South Africa	A-Million-a-Month Account	First National Bank	2005	2008	NA
South Africa	Win 10x Your Deposit Competition	First National Bank	2017	To date	FNB Website;FNB (Competition rules)
Spain	El libretón	Banco Bilboa Vizcaya	1990	2004	Cincodias Website (2003);El Mundo (2004)

Sri Lanka	Pathum Vimana	Hatton National Bank (HNB)	1993	2013	Daily Mirror (2012);HNB Website (Vimana 2014);HNB Website (21st year)
Sri Lanka	Ridee Rekha certificates	National Savings Bank	1997	To date	GIC Website
Sri Lanka	SMILE Savings (Postal Savings)	National Savings Bank	2010	At least 2018	GIC Website (Savings Accounts);NSB Website
Sweden	Swedish Lottery Bonds (Premieobligationer)	Swedish National Debt Office (Riksgalden)	1918	2016	Riksgalden Website (2000);Riksgalden Website (Q and A);Riksgalden Website (2018);Rydqvist (2014)
Tanzania	Save and Win (Save + Win)	First National Bank	2015	2016	FNB Tanzania Website;IPP Media Website (2016)
Tanzania	WinBig	EcoBank Tanzania	2012	2012	Issamichuzi Blog (2012);EcoBank Tanzania LinkedIn
Thailand	Kaset-Mung-Kung 4 savings lottery	Bank for Agriculture and Agricultural Cooperatives	2019	2022	Bangkok Post Website (2019);BAAC (Savings Certificate)
Thailand	Om Sap Thaweesin Savings Card Deposits	Bank for Agriculture and Agricultural Cooperatives	2017	To date	BAAC (Savings Certificate)
Thailand	Special Premium savings certificate and Digital Salak on MyMo	Government Savings Bank (GSB)	1943	To date	Bangkok Post Website (2019);MyMo by GSB Website (Digital 1 year);MyMo by GSB Website (Digital 2 year)

Thailand	Wimarnmek Savings Certificate	Government Housing Bank (GHB)	2019	To date	Bangkok Post Website (2019)
Turkey	NA	Demirbank (now HSBC)	1950	1958	Cosar (1999);Cosar (2013)
Turkey	NA	İş Bank	1930	1976	Cosar (2013)
Turkey	NA	Yapi Kredi Bank	1944	1976	Cosar (2013)
Turkey	NA	Ziraat Bank	1936	1976	Cosar (2013)
UAE	ADCB Millionaire Destiny Savings Account/Emirati Millionaire Savings Account	Abu Dhabi Commercial Bank	2009	To date	MyMoneySouq Website;ADCB Website
UAE	ADIB Ghina Savings	Abu Dhabi Islamic Bank	At least 2012	To date	ADIB Website
UAE	Al Awwal Savings Certificate	Union National Bank	2013	To date	The National News Website (2021)
UAE	ChildFirst Savings Account	First Gulf Bank	2012	At least 2015	UAE Raffles Facebook Post;Albawaba (2013)
UAE	Emirati Al Awwal Islamic certificate	First Abu Dhabi Bank (FAB)	2020	To date	Bank FAB Website
UAE	Emirati Al Awwal Savings Certificate	First Abu Dhabi Bank PJSC	Unknown	To date	Bank FAB Website
UAE	FirstSavings	First Gulf Bank	2012	2015	MarketScreener (2013)
UAE	Kunooz Savings Account	Emirates Islamic Bank	At least 2013	2022	Emirates Islamic Website;The National News Website (2021)
UAE	Mabrook Savings Account	Commercial Bank International	2013	To date	CBI UAE Website
UAE	Mashreq Millionaire certificates	Mashreq Bank	1995	To date	Mashreq Bank Website;Mashreq (Terms and Conditions)
UAE	National Bonds Rewards Programme	Government of Dubai, ICD (Investment Corporation of Dubai)	2013	To date	National Bonds Website

UK	Million Adventure	Bank of England	1694	1710	Wikipedia (Million Lottery);The National Archives Website;Murphy (2005)
UK	Nationwide Building Society Mutual Reward Bond	Nationwide	2020	2021	Nationwide Website (Mutual Reward Bond)
UK	NS&I Premium Bonds	National Savings and Investments (NS&I)	1956	To date	NS&I (Premium bonds);NS&I (About premium bonds);Online-Casinos (2019)
UK	PrizeSaver	HM Treasury with 14 credit unions	2018	2021	PrizeSaver Website
UK	Start to Save	Nationwide	2020	2022	Nationwide Website (Start to Save)
USSR	State Domestic Lottery Bonds	NA	1982	Unknown	Toronto Star Website (2012)
Venezuela	"El libretón" account	Banco Bilboa Vizcaya	1997	Unknown	NA

This table describes current or past PLS products offered around the world. It includes details on the country, product name, bank or financial institution, years over which the product was offered, and a link to more details about the product (if available). To produce the data, we started with data in Cole, Tufano, Schneider and Collins (2007) and Kearney, Tufano, Guryan and Hurst (2011) and updated it by searching the internet for additional, more recent PLS products.

Table AII: State-Level PLS Regulations in the US

State	Banks Legal	Credit Unions Legal	Regulation	Year	Examples	Links
(1)	(2)	(3)	(4)	(5)	(6)	(7)
AK	No	No	NA	NA	NA	NA
AL	Yes	Yes	HB 355	2017	SaveNow WinLater (Impact Alabama); Save To Win	al.com
AR	Yes	Yes	HB 1642	2015	Save to Win; Cents to Win Prize Savings Program (Centennial Bank)	Centennial Bank Twitter
AZ	No	Yes	HB 2471	2016	Save to Win	First Credit Union
CA	Yes	Yes	SB 1055	2018	Big Prize Savings Account (America First Credit Union); PLAY Program (Mission SF Federal Credit Union)	Commonwealth - PLS Bill California;American First Credit Union;Federal Reserve Bank of San Francisco
CO	No	No	NA	NA	NA	NA
CT	Yes	Yes	HB	2013	Save to Win; FirstPrize $avings Account (First County Bank); Great Prize Savings (Newtown Savings Bank)	First County Bank;Save to Win Website;Newton Savings Bank
DE	Yes	Yes	HB 31	2017	WINcentive Savings (Del-One FCU)	Del-One Federal Credit Union
FL	No	No	NA	NA	NA	NA
GA	Yes	Yes	HB 193	2019	Save to Win	Save to Win Website

HI	Yes	Yes	HB 1163	2019	Save to Win; WINcentive Savings	Save to Win Participation Agreement
IA	In Progress	In Progress	HSB 62	Unsigned	NA	The Iowa Legislature
ID	No	No	NA	NA	NA	NA
IL	Yes	Yes	HB 2477	2015	Save to Win, Saver's Sweepstakes	Altra Federal Credit Union
IN	Yes	Yes	HB 1235	2014	Super Savings (Centra Credit Union); Save to Win	Save to Win Website
KS	Yes	Yes	SB 390	2016	Save to Win	Save to Win Website
KY	No	No	NA	NA	NA	Kentucky Law Journal
LA	No	Yes	HB 681	2016	NA	Lucky Lagniappe Savings
MA	Yes	Yes	SB 2374	2016	Lucky Piggy Savings Account (BankFive); WINcentive Savings (City of Boston Credit Union)	BankFive
MD	Yes	Yes	HB 990; HB 786	2010; 2012	None Identified	The Baltimore Sun;Connecticut General Assembly
ME	Yes	Yes	LD 1673	2010	None Identified	Maine Legislature Bill Tracking
MI	Yes	Yes	HB 5022	2016	Save to Win (Michigan Credit Union League)	Commonwealth - Save-to-win
MN	Yes	Yes	HF1127	2015	WINcentive Savings (Minnesota Credit Union Network)	Commonwealth - WIN-centive

MO	Yes	Yes	HB 2125	2016	Save to Win; Save to Prosper (St Louis Community Credit Union)	Save to Win Website;St.Louis Community Credit Union
MS	No	No	NA	NA	NA	NA
MT	Yes	Yes	SB 25	2017	WINcentive Savings	CUtoday
NC	No	Yes	HB H628	2019	Save to Win	Save to Win Website
ND	No	No	NA	NA	NA	NA
NE	Yes	Yes	LB 524; LB 160	2011; 2015	NA	The Lincoln Journal Star;Save to Win Website
NH	No	No	NA	NA	NA	NA
NJ	Yes	Yes	S 2495	2014	U Win Savings Program (United Teletech Financial Credit Union)	United Teletech Financial;Federal Credit Union
NM	No	No	NA	NA	NA	NA
NV	No	No	NA	NA	NA	SaverLife Nevada PDF
NY	Yes	Yes	A09037A	2014	Lucky Savers	New York Credit Union Association - Lucky Savers;New York Credit Union Association - Participating Credit Unions
OH	No	Yes	HB 489	2019	Save to Win; Bucks for Buckeyes (Ohio Credit Union League)	Bucks for Buckeyes
OK	No	No	NA	NA	NA	NA
OR	Yes	Yes	HB 2893	2014	Save to Win; Win-Win CD Account	Community Bank

PA	In Progress	In Progress	HB 331	In Committee	Fund My Future (State Treasurer)	Commonwealth - FUND My Future;Pennsylvania General Assembly
RI	No	Yes	SB 2399	2010	None Identified	Justia US Law
SC	Yes	Yes	SB 652	2016	Save to Win	Save to Win Website
SD	No	No	NA	NA	NA	NA
TN	No	No	SB 1052	Failed in 2020	NA	Bill Track *50* (TN)
TX	Yes	Yes	HB 471	2017	Save to Win, Prize Savings Account (Neighborhood Credit Union)	Neighborhood Credit Union
UT	Yes	Yes	SB 86	2019	Save to Win	Freedom Credit Union
VA	Yes	Yes	HB 1487	2014	Save to Win (People's Advantage FCU); Jackpot Savings Program (Blue Ridge Bank)	Blue Ridge Bank
VT	No	No	HB 148	Failed in 2020	NA	Bill Track *50* (VT)
WA	Yes	Yes	SB 5232	2011	Save to Win; Win-Win CD Account	Community Bank
WI	Yes	Yes	AB 283	2017	Save to Win; Saver's Sweepstakes	The Wisconsin Credit Union League
WV	No	No	NA	NA	NA	NA
WY	No	No	NA	NA	NA	NA

This table describes the regulatory framework around PLS products across states in the US. The "Banks Legal" column indicates whether it is legal for banks to offer PLS products, and the "Credit Unions Legal" column indicates whether it is legal for credit unions to offer PLS products. The "Regulation" and "Year" columns provide details about the relevant state-level regulation. The "Examples" and "Links" columns give details about PLS products offered in the states in which it is legal. NA indicates not applicable.

Table AIII: Comparison of Savings Studies

Study (1)	Intervention (2)	Type (3)	Variation (4)	Months (5)
Abarcar, Barua and Yang (2020)	Access, Education	RCT	Individual	12
Abebe, Tekle and Mano (2018)	Education, Reminder	RCT	Individual	7
Abraham, Akbas, Ariely and Jang (2020)	Access, Lottery	RCT	Individual	3
Aggarwal, Brailovskaya and Robinson (2023)	Access	RCT	Individual	18
Aggarwal, Brailovskaya and Robinson (2020)	Access	RCT	Individual	15
Ahmad, Lensink and Mueller (2023)	Access	RCT	Individual, Branch	2
Akbaş, Ariely, Robalino and Weber (2016)	Access, Reminder	RCT	Individual	6
Aker et al. (2020)	Access	RCT	Individual	12
Ashraf, Karlan and Yin (2006*b*)	Commitment account	RCT	Individual	12
Ashraf, Karlan and Yin (2006*a*)	Assistance	RCT	Local Area	15
Ashraf (2009)	Financial decision	Lab	Individual	0
Ashraf, Karlan and Yin (2010)	Access	RCT	Individual	30
Ashraf, Aycinena, Martínez A and Yang (2015)	Access, Assistance	RCT	Individual	48
Atkinson, De Janvry, McIntosh and Sadoulet (2013)	Reminder, Commitment	RCT	Branch	36
Attanasio, Bird, Cardona-Sosa and Lavado (2019)	Education, Tech, Reminder	RCT	Local Area	25
Avdeenko, Bohne and Frölich (2019)	Access	RCT	Individual	6
Azevedo, Lafortune, Olarte and Tessada (2019)	Reminder	RCT	Individual	7
Bachas, Gertler, Higgins and Seira (2021)	Access	Quasi	Local Area	20
Baker, Benmelech, Yang and Zhang (2023)	Other	Quasi	Individual	36
Banerjee et al. (2022)	Access, Technology	RCT	Individual, Local Area	36
Bastian, Bianchi, Goldstein and Montalvao (2018)	Access, Education	RCT	Individual	12
Batista and Vicente (2020*b*)	Access, Education	RCT	Individual	24
Batista and Vicente (2020*a*)	Access, Tech	RCT	Individual	36
Beaman, Karlan and Thuysbaert (2014)	Saving groups	RCT	Local Area	36
Berry, Karlan and Pradhan (2017)	Education	RCT	School	9

Beshears et al. (2015)	Other	RCT	Individual	1
Beshears, Choi, Laibson and Madrian (2017)	Other	Quasi	Firm	11
Beshears et al. (2022)	Access	Quasi	Individual	53
Beshears et al. (2020)	Other	RCT	Individual	12
Beshears, Dai, Milkman and Benartzi (2021)	Other	RCT	Individual	10
Bharadwaj and Suri (2020)	Access	Quasi	Individual	8
Blumenstock, Callen and Ghani (2018)	Direct transfers	RCT	Individual	24
Breza and Chandrasekhar (2019)	Access, Assistance	RCT	Individual	21
Breza, Kanz and Klapper (2022)	Access, Direct transfers	RCT	Individual	24
Bruhn and Love (2009)	Access	Quasi	Local Area	24
Bruhn et al. (2016)	Education	RCT	Individual	16
Bruhn, Garber, Koyama and Zia (2022)	Education	RCT	School	113
Brune, Giné, Goldberg and Yang (2016)	Direct transfers	RCT	Individual	24
Brune, Giné, Goldberg and Yang (2017)	Direct transfers	RCT	Individual	1
Brune, Chyn and Kerwin (2021)	Other	RCT	Individual	22
Buehren (2011)	Education	RCT	Individual	6
Buehren et al. (2005)	Commitment account	RCT	Individual	14
Burke, Luoto and Perez-Arce (2018)	Access	RCT	Individual	6
Bussiere, Kalantzis, Lafarguette and Sicular (2013)	Other	Quasi	Individual	12
Calderone et al. (2018)	Education	RCT	Individual	11
Callen, De Mel, McIntosh and Woodruff (2019)	Deposit Collection	RCT	Individual	24
Carter, Laajaj and Yang (2016)	Tech	RCT	Individual	28
Cassidy and Fafchamps (2020)	Savings Groups	Quasi	VSLA group	48
Chamon, Liu and Prasad (2013)	Other	Quasi	Individual	84
Soman and Cheema (2011)	Other	RCT	Individual	3
Chetty et al. (2014)	Other	Quasi	Individual	48
Chin, Karkoviata and Wilcox (2015)	Access	RCT	Individual	5
Choi, Laibson, Madrian and Metrick (2002)	Other	Quasi	Individual	36
Choi, Haisley, Kurkoski and Massey (2017)	Other	RCT	Individual	12
Choukhmane, Coeurdacier and Jin (2023)	Other	Quasi	Household	NA
Choukhmane (2021)	Other	Quasi	Individual	36
Cole, Sampson and Zia (2011)	Education	Quasi	Individual	6

Cole, Iverson and Tufano (2021)	Lottery	RCT	Individual	24
De Mel, McIntosh and Woodruff (2013)	Tech	RCT	Individual	13
De Mel, McIntosh, Sheth and Woodruff (2022)	Assistance, Tech	RCT	Individual	23
Dizon, Gong and Jones (2020)	Reminder, Tech	RCT	Individual	8
Dizon and Lybbert (2021)	Lottery	Lab	Individual	0
Doi, McKenzie and Zia (2014)	Education	RCT	Individual	23
Drexler, Fischer and Schoar (2014)	Education	RCT	Individual	12
Duflo et al. (2006)	Other	RCT	Individual	1
Dupas and Robinson (2013)	Access	RCT	Individual	6
Dupas and Robinson (2013*b*)	Education, Access	RCT	Savings group	33
Dupas, Green, Keats and Robinson (2014)	Access, Assistance	RCT	Individual	12
Dupas, Keats and Robinson (2019)	Access, Assistance	RCT	Individual	24
Dupas, Karlan, Robinson and Ubfal (2018)	Access, Assistance	RCT	Individual	18
Field et al. (2021)	Account, Education	RCT	Local Area	36
Flory (2018)	Education	RCT	Local Area	24
Frisancho (2023)	Education	RCT	School	33
Fuentes et al. (2017)	Education	RCT	Individual	12
Gargano and Rossi (forthcoming)	Other	Quasi	Individual	18
Gertler, Higgins, Scott and Seira (2023)	Lottery	RCT	Branch	58
Giné and Goldberg (2023)	Access	RCT	Individual	12
Habyarimana and Jack (2018)	Access, Technology	RCT	Individual, School	6
Horn, Jamison, Karlan and Zinman (forthcoming)	Education	RCT	Young-adult club	60
Imbens, Rubin and Sacerdote (2001)	Other	Quasi	Individual	72
John (2017)	Commitment account	Quasi	Individual	6
John (2020)	Commitment account	RCT	Individual	3-6
Kaboski, Lipscomb, Midrigan and Pelnik (2022)	Lottery	Lab	Individual	18
Kaiser and Menkhoff (2022)	Education	RCT	Market	45
Karimli, Ssewamala and Neilands (2014)	Education, Access	RCT	School	24
Karlan and Valdivia (2011)	Education	RCT	Savings group	24
Karlan and Linden (2016)	Commitment account	RCT	School	24
Karlan, McConnell, Mullainathan and Zinman (2016)	Saving groups	RCT	Local Area	30
Karlan et al. (2016)	Reminder	RCT	Individual	3-24

Karlan and Zinman (2018)	Commitment account	RCT	Individual	12
Kast and Pomeranz (2022)	Access	RCT	Savings group	12
Kast, Meier and Pomeranz (2018)	Savings groups, Interest	RCT	Individual, Savings group	12
Ksoll, Lilleør, Lønborg and Rasmussen (2016)	Access	RCT	Local Area	24
Laajaj (2017)	Access	RCT	Individual, Local Area	24
Lipscomb and Schechter (2018)	Access, Reminder	RCT	Individual	12
Madrian and Shea (2001)	Other	Quasi	Individual	14
Medina and Pagel (2023)	Other	RCT	Individual	1
Mehrotra, Somville and Vandewalle (forthcoming)	Access, Assistance	RCT	Individual	6
Moscoe, Agot and Thirumurthy (2019)	Access	RCT	Individual	2
Nam et al. (2013)	Access	RCT	Individual	18
Olafsson and Pagel (2022*b*)	Other	Quasi	Individual	72
Olafsson and Pagel (2022*a*)	Other	Quasi	Individual	5
Prina (2015)	Access, Assitance	RCT	Individual	12
Rodríguez and Saavedra (2019)	Reminder, Education	RCT	Individual	12
Salas (2014)	Education, Reminder	RCT	Savings group	9
Schaner (2015)	Interest rate	RCT	Individual	6
Schaner (2017)	Access	RCT	Individual	36
Schaner (2018)	Interest rate	RCT	Individual	36
Seshan and Yang (2014)	Education	RCT	Individual	13
Somville and Vandewalle (2018)	Direct transfers	RCT	Individual	6
Somville and Vandewalle (2023)	Access, Assitance	RCT	Individual	9
Stein and Yannelis (2020)	Access	Quasi	Individual	NA
Supanantaroek, Lensink and Hansen (2017)	Education	RCT	School	3
Suri and Jack (2016)	Access	Quasi	Individual	72
Zinman (2009)	Other	RCT	Individual	12

This table describes the intervention type, type of study (RCT, quasi-experimental, or lab/lab-in-the-field experiment), level of variation, and number of months after the intervention over which outcomes are measured for papers on the effects of various savings interventions. For the level of variation, studies at the individual level and household level are both listed as "Individual." NA indicates not applicable. The duration for Choukhmane, Coeurdacier and Jin (2023) and Stein and Yannelis (2020) are specified as NA since they present the average effect across multiple post-treatment periods rather than estimating effects for a specific amount of time post-treatment.

B Construction of Variables

B.1 Branch-Level Deposits and Withdrawals

To construct the branch-level number of deposits, number of withdrawals, and amounts deposited and withdrawn in pesos, we begin with *transactions-level data for all accounts*. Since we want to focus on the transactions made by the clients (to measure the effect of the lottery incentive on their activity), we restrict to deposits and withdrawals made by the client, and exclude transactions made by the bank (e.g., interest payments or lottery prize winnings).

Once we have the transactions made by the account holders, we:

1. Aggregate transactions-level data to four measures at the account $\times$ day level: number of deposits, number of withdrawals, amount deposited in pesos, and amount withdrawn in pesos. Note that this data set is an unbalanced panel, as some account-days have no transactions.

2. Aggregate these measures from the account $\times$ day level to the branch $\times$ day level, by summing over all the transactions made by accounts belonging to branch j: $y_{jd} = \sum_{i \in j} y_{id}$. Note that this data set is also an unbalanced panel as even some branch-days have no transactions.

3. Create a balanced panel imputing zeros for missing branch-day observations in the balanced panel, since these missing observations represent branch-days with no transactions.

4. Winsorize the four measures (number of deposits, number of withdrawals, amount deposited, and amount withdrawn) at the 95th percentile at the branch-day level, separately for the treatment and control groups. We do the winsorization separately by treatment arm to avoid truncating a true treatment effect.

5. Aggregate these measures to the branch $\times$ two-month level by summing over all the days in the two-month period within each branch. For each two-month period t, each measure at the branch $\times$ two-month level is $y_{jt} = \sum_{d \in t} y_{jd}$. These are the variables that we use in Figure 3.

B.2 Branch-Level Stock of Savings

Because we do not observe the starting balance of each branch as of the start date of our data, we do not know the exact savings stock at each branch over time. Instead, we construct a measure of the branch-level stock of savings relative to the beginning of our pre-treatment period (January 12, 2010) by setting the stock of savings at each branch to zero as of January 11, 2010.

To construct the branch-level stock of savings, we begin with the balanced panel of amount deposited and amount withdrawn at the *branch level* created in steps 1–3 of Appendix B.1. We then:

1. Construct a measure of net amount deposited at the branch $\times$ day level:

$$Net\ Amount\ Deposited_{jd} = Amount\ Deposited_{jd} - Amount\ Withdrawan_{jd}. \tag{7}$$

2. To reduce noise in the stock measure, winsorize *net amount deposited* at both the 95th and 5th percentiles (since this variable also takes negative values and thus has outliers in both tails), at the branch $\times$ day level, separately for the treatment and control groups. We do the winsorization separately by treatment group to avoid truncating a true treatment effect. This helps deal with outliers on days where a high amount is either deposited or withdrawn at a branch.

3. Compute the stock of savings at branch j on day d as the stock of savings on day $d-1$ plus the net amount deposited on day d from steps 1–2, bottom-coded at zero. The bottom coding is done because we are not accounting for all the deposits and withdrawals made to a branch since its inception (since we do not observe the branch's starting stock of savings), so the stock could fall below zero. Bottom coding ensures the measure remains non-negative. That is:

$$Savings\ Stock_{jd} = \max(Savings\ Stock_{j,d-1} + Net\ Amount\ Deposited_{jd}, 0). \tag{8}$$

4. To compute the average stock of savings at branch j in two-month period t, we take the mean of the stock of savings across all days in the two-month period. This is the variable that we use in Figure 4a.

Table BI shows an example of a branch's activity and the corresponding measure of the stock of savings, where day 0 is January 12, 2010.

Table BI: Example Branch Activity

Day	Amount Deposited	Amount Withdrawn	Net Amount Deposited	Stock of Savings
0	11,400	10,000	1,400	1,400
1	1,620	11,600	–9,980	0
2	20,780	8,352	12,428	12,428
3	14,660	3,350	11,310	23,738
4	6,650	4,422	2,228	25,966
5	3,450	5,235	–1,785	24,181
6	2,300	16,774	–14,474	9,707
⋮	⋮	⋮	⋮	⋮

This table shows a stylized example of the construction of stock of savings.

For the branch-level stock of savings *by account-type* (accounts opened prior to lottery months in Figure 4b and accounts opened during lottery months in Figure 4c), the only difference compared to the description above is that instead of using all account transactions on amounts deposited and withdrawn to aggregate from the account × day level to the branch × day level, we restrict data to either accounts opened prior to the lottery months or accounts opened during the lottery months.

B.3 Account-Level Deposits and Withdrawals

For the measures used in Figure A3, we restrict to accounts opened during the lottery months. As in Appendices B.1 and B.2, we restrict to transactions made by clients and exclude those made by the bank (e.g. interest payments and lottery prize winnings). Using these data, we create a balanced panel at the account × day level (imputing zeros for days when a particular account made no deposits or withdrawals). Unlike for the branch-level outcomes, we cannot winsorize at the day level before aggregating because at the *account* × day level, a large part of the distribution consists of zeros (and on some days, even the 95th percentile of deposits or withdrawals at the account × day level is zero). Thus, winsorizing would truncate the majority of the non-zero data at the account × day level.

Next, we aggregate the data to the account × two-month level by summing over the number of deposits, number of withdrawals, amount deposited, and amount withdrawn over all days in the two-month period. Then, because we did not winsorize at the day level before aggregating, we winsorize these measures at the account × two-month level at the 95th percentile, separately for the treatment and control groups.

B.4 Account-Level Stock of Savings

For the measure used in Figure A4, we construct the stock of savings at the account-level for accounts opened during the lottery months by setting the stock to zero before the period of interest (i.e., the lottery period, since these accounts were not opened prior to the lottery period) and then constructing stock of savings in a similar manner as for the branch-level measure. Specifically, we:

1. Begin with a balanced panel at the account × day level for accounts opened during the lottery months, constructed in Appendix B.3, and construct a measure of net amount deposited at the account × day level:

$$Net\ Amount\ Deposited_{id} = Amount\ Deposited_{id} - Amount\ Withdrawn_{id}. \tag{9}$$

2. Compute the stock of savings in account i on day d as the stock of savings on day $d-1$ plus the net amount deposited on day d from step 1, bottom-coded at zero. The bottom coding is done for the same reasons as described in Appendix B.2. (Note that we do not winsorize net amount deposited at the account × day level prior to this step for the reason described in Appendix B.3.) That is:

$$Savings\ Stock_{id} = \max(Savings\ Stock_{i,d-1} + Net\ Amount\ Deposited_{id}, 0). \tag{10}$$

3. To compute the average stock of savings in account i in two-month period t, we take the mean of the stock of savings across all days in the two-month period.

4. Because we did not winsorize at the day level before aggregating, we winsorize the stock of savings at the account × two-month level at the 95th percentile, separately for the treatment and control groups. This is the variable that we use in Figure A4.

B.5 Inactive Account

We construct two measures of account inactivity at the account × two-month level that we use in Figure A5:

1. Account closed. We use an indicator variable equal to one if the account was closed during two-month period t or in any period before that.

2. Account closed or stock of savings less than 50 pesos. Using the stock of savings measure constructed at the account level from Appendix B.4, this indicator variable equals 1 if the average stock of savings in the account across all days in the two-month period is less than 50 pesos, or if the account was closed during two-month period t or in any period before that.

B.6 Expected Interest Rate

The expected interest rate is the ex post average rate of return depositors earned from the additional deposits made due to the PLS incentive. We include all accounts (both those opened prior to the lottery and those opened during the lottery) as both types of account were eligible to receive the lottery prizes. Since the lottery tickets are only awarded for additional deposits that occur after the lottery incentive began, we use the same stock construction method as above but we only begin the calculation when the lottery period began (September 12, 2010) since what matters for the average rate of return is the incremental stock of saving deposited into the accounts once the lottery period began.

Because bank account holders can deposit and withdraw each day and earn interest on their deposits based on how long the deposits were in their account, we calculate for each account by day the daily stock of savings accumulated since the lotteries began, denoted $Savings\ Stock_{id}$ for account i on day d. The construction of $Savings\ Stock_{id}$ is identical to that in Appendix B.4 steps 1–3; we do not winsorize (step 4) because the expected interest rate is a function of the change in the stock of savings across all accounts including outliers.

We write the total amount of lottery winnings won by account i over the lottery months as $Winnings_i$. Denoting the annual interest rate that we are calculating as r_{annual}, the daily interest rate that is paid on a particular day's stock of savings is then $r_{annual}/365$. The annual rate of return on daily deposits is then defined implicitly by the following identity:

$$\sum_i Winnings_i = \sum_i \sum_d \frac{r_{annual}}{365} Savings\ Stock_{id}. \tag{11}$$

Rearranging equation (11), the annualized average rate of return, or annualized expected interest rate on deposits, is defined as

$$r_{annual} = 365 \cdot \left(\frac{\sum_i Winnings_i}{\sum_i \sum_d Savings\ Stock_{id}} \right). \tag{12}$$

Because a large fraction of the deposits in treatment branches would have occurred even in the absence of the PLS product being offered, to calculate the expected interest rate on *additional* savings caused by offering PLS, we replace $\sum_i \sum_d \textit{Savings Stock}_{id}$ in (12) with the total incremental stock generated by PLS. The incremental stock generated *per branch* is calculated by comparing the daily stocks at treatment and control branches:

$$\textit{Incremental Stock per Branch} = \frac{1}{B^T} \sum_{i \in \mathscr{T}} \sum_d \textit{Savings Stock}_{id} - \frac{1}{B^C} \sum_{i \in \mathscr{C}} \sum_d \textit{Savings Stock}_{id}, \quad (13)$$

where $i \in \mathscr{T}$ denotes accounts in treatment branches, $i \in \mathscr{C}$ denotes accounts in control branches, and B^T and B^C denote the number of treatment and control branches, respectively. We then calculate the aggregate incremental stock caused by PLS across all treatment branches by multiplying *Incremental Stock per Branch* by the number of treatment branches B^T. We then replace $\sum_i \sum_d \textit{Savings Stock}_{id}$ in (12) with this measure of total incremental stock to calculate the annualized expected interest rate on the incremental deposits caused by PLS, which is the measure we report in the paper.

B.7 Profits

The construction of the profits measure is described in Section 5.3. This section details the construction of the measure of stock of savings at the branch × day level, i.e. $\textit{Savings Stock}_{jd}$ used in equation (3).

For this calculation we *do* want to include deposits that preceded our study period and we also want to include deposits made by the bank (e.g., interest payments and lottery prize payments) in the measure of the stock of savings, as the bank earns returns on all of these deposits until they are withdrawn. Thus, unlike in Appendix B.2, to calculate $\textit{Savings Stock}_{jd}$ in equation (3) we use all transactions since January 2007 (the first month from which we have data available) and use all deposits and withdrawals since 2007. We then calculate the branch × day level net amount deposited (amount deposited minus amount withdrawn). As a result of using all transactions since 2007, there are no branch × day level observations with a net amount deposited less than zero in the relevant period (starting September 12, 2010). Furthermore, we do not winsorize the measure of net amount deposited since the bank earns returns on all deposits including outliers. We then sum the branch × day level net amount deposited from day 0 (day of the first transaction in our data) to day d:

$$\text{Savings Stock}_{jd} = \sum_{\tau=0}^{d} \text{Net Amount Deposited}_{j\tau}. \tag{14}$$

B.8 Nearby Branches

We construct five measures of *Nearby branches*$_j$ in specification (5) to test whether the treatment effect of PLS is coming from substitution away from opening accounts in (i) control branches and (ii) commercial bank branches. These measures are:

1. Below-median road distance to closest control branch
2. Log road distance to closest control branch in km
3. Commercial branches within 1 km road distance[17]
4. Below-median road distance to closest commercial bank branch
5. Log distance to closest commercial bank branch in km

As a first step for calculating these five variables and the measure of whether a branch is located on a large road in Appendix B.9, we manually compare the geocoordinates of the 110 Bansefi branches in our experiment according to the shapefiles provided by Bansefi to the geocoordinates given for the same branches using the addresses provided by Bansefi and the Google Places API. The process of obtaining geocoordinates for each branch from Google Places involves the following steps:

1. Obtain a Google Places API key by following the Google Maps Platform instructions (`https://developers.google.com/maps/documentation/places/web-service/get-api-key`). Paste the key in the script (see the replication package for more details).
2. Import addresses of each branch from the administrative data provided by Bansefi, and drop addresses with a street name but without a number. The precise location for the branches without a number cannot be obtained with a Google Places search, so these branches are not included in the comparison between the geocoordinates provided by Bansefi and the geocoordinates from Google Places; for these branches, we retain the geocoordinates provided by Bansefi in our analysis.

[17] We do not include a similar measure of "control branches within 1 km road distance" because there are no Bansefi branches in our experiment that are within 1 km of a control branch.

3. Look up the remaining branches on Google Places, extracting the decimal longitude and latitude. This step is not perfectly replicable as identical searches can produce different results over time.

4. For the five branches with more than one result from Google Places API, we determined that the first result was correct by manually looking up the coordinates on Google Maps Street View and confirming that there is either still a Bansefi or Banco del Bienestar branch at that location, or that the buildings at that location appeared to be commercial rather than residential, indicating that a Bansefi branch could have existed there in 2010.[18]

After getting coordinates that correspond to the addresses of each Bansefi branch, we process the shapefiles provided by Bansefi and compare the two sets of coordinates. We followed the following steps:

1. Manually define the location of one Bansefi control branch (branch #842). This branch was the only branch in the experimental sample of 110 branches that did not include geocoordinates in the administrative shapefiles. We looked up the branch address on Google Maps and recorded the coordinates.

2. Obtain the distance between the Bansefi shapefiles branch locations and the coordinates obtained from Google Places. The median distance is 178 meters between each location, indicating that geocoordinates from the two data sets are similar.

3. Compare the Street View resulting from the two different geographic coordinate sources (Google Places query based on addresses and geocoordinates from Bansefi shapefiles). After comparing manually, we find that looking up addresses on Google Places results in a more precise bank branch location than using coordinates provided by Bansefi: Street View searches for Google Places locations showed a Bansefi or Banco del Bienestar sign, or showed a commercial location where a bank branch could have been located. Figure B1 provides an example of a comparison where we observe a Bansefi branch on the Google Place location Street View.

4. Define the final coordinates of a bank branch as the geocoordinates from Google Places based on the branch address, except for branches where the location could not be determined from the address. Specifically, of the 110 branches in the experiment:

[18] Bansefi was rebranded as Banco del Bienestar in 2019.

(a) 77 branch addresses were matched with a location using the branch address and the Google Places API.

(b) 12 branches did not have a street number in the address. We used the Bansefi shapefile geocoordinates for these branches.

(c) 21 branch addresses were not found in the Google Place search. We used the Bansefi shapefile geocoordinates for these branches.

Figure B1: Comparison Between Bansefi Shapefile and Address Locations

(a) Bansefi Geocoordinates Street View (b) Bansefi Address Street View

This figure shows a comparison of the Google Street View of the geocoordinates provided by Bansefi (panel a) and using the addresses provided by Bansefi with Google Places (panel b). The result using the addresses from Bansefi are more accurate, as there is a Bansefi branch on Google Street View in panel b.

Once we have defined precise branch locations, we obtain travel distances to the nearest control Bansefi branch and the nearest commercial bank branch. We follow the following steps:

1. Set up Open Source Routing Machine (OSRM) using the 2014 Mexico OpenStreetMap dataset, which is the earliest OpenStreetMap data available for Mexico and hence the closest date to the experiment. OSRM is a routing engine that uses street shapefiles to calculate road distances. As specified in the replication package, set up of OSRM can take up to 12 hours to complete.

2. For each set of distance calculations, obtain a matrix of travel distances containing the distance in meters from each element of the origin set (the 110 Bansefi branches in our experiment) to each element in the destination set (either control Bansefi branches or all commercial bank branches).

3. Obtain the minimum distance for each branch and convert distances from meters to km. This results in measures of the minimum road distance to the nearest control Bansefi branch and nearest commercial bank branch.

4. Generate dummies indicating below-median distances for each distance variable. Each median is obtained over the full sample of 110 branches (i.e. not grouped by treatment status). This results in measures of below-median distance to the closest control branch and below-median distance to the closest commercial bank branch.

5. Take logs of the minimum road distance variable for the measures of log road distance to the nearest control or commercial branch.

6. Use road distances to the nearest commercial branch to calculate the number of commercial branches within a 1 km road distance. (We do not include a similar measure for control branches because there are no Bansefi branches in our experiment that are within 1 km of a control branch.)

B.9 Branch Located on Large Road

We use OpenStreetMap's classification of roads to determine which of the branches in our experiment are located on large roads. We use the dummy variable indicating that a branch is located on a large road, $\textit{Large road}_j$, in specification (6) to test whether treatment effects are larger for branches located on larger roads with more people passing by.

As a first step, we take the branch geocoordinates from Appendix B.8 and match them to roads using OSRM. Specifically, we follow the following steps:

1. Load OSRM. (The setup of OSRM is described in Appendix B.8, with more detail in the replication package.)

2. Search for the road where the branches are located using the Nearest Road service. The queries take the geocoordinates of each branch as an input and return coordinates for the location of the five closest roads to each branch. We keep the road closest to each branch.

3. Manually look up road locations. This exercise showed that eight matches were wrong. Alternative road coordinates were manually defined and replaced with the matches obtained by the server. Most of these cases were branches in strip malls on large avenues, with queries returning side streets that were physically closer to the branches but not where the branches were actually located.

4. The result is a dataset with coordinates of the closest point located on the closest road to each branch. Figure B2 contains an example of branch coordinates along with the coordinates obtained for the closest road.

Figure B2: Bansefi Branch Location and Closest Road Location

This figure shows the geocoordinates of an example Bansefi branch and the geocoordinates of the closest point located on a road, which we use to determine whether the Bansefi branch is located on a large road.

Now that we have geocoordinates of the closest point on the closest road to each branch, we extract OpenStreetMap metadata about these roads, using the following steps:

1. Define a bounding box as a box adding and subtracting 0.0001 decimal degrees around the road location, the equivalent of a 22 square meter box bounding the road coordinates.

2. Query the server for all roads that pass through this bounding box. Only two roads were not found, and these were manually updated.

3. Most roads do not have information on number of lanes nor official width, but all roads are tagged with a road type. We extract the metadata on road type.

We then followed Talamas (2022) in defining large roads as primary, secondary and tertiary roads; no branches in our sample are located on the two categories of road that are larger than primary roads, namely motorways and trunk roads. The remaining road categories on which our branches are located—residential roads, service roads, living streets (a type of residential road with additional rules), pedestrian roads, and footways—are classified as not large. Approximately half (48%) of the Bansefi branches are located on large roads using this definition.

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