

Increasing Financial Inclusion and Attracting Deposits through Prize-Linked Savings

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Abstract

Despite the benefits of saving in formal financial institutions, the take-up and use of savings accounts are low among the poor. In a randomized experiment across 110 bank branches throughout Mexico, an incentive to open and use a savings account—in which saving earned raffle tickets for cash prizes—caused a 41% increase in the number of accounts opened during the raffle months. Nearly all new accounts (96%) were opened by previously unbanked households. The temporary two-month incentive had a persistent three-year impact on the flow of deposits at treatment branches. Prize-linked savings can thus benefit both previously-unbanked households and banks.

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1 Introduction

Households and banks both benefit substantially when households save in formal financial institutions. Access to formal bank accounts enables 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). Banks, meanwhile, finance most of their assets with deposits, which are a more stable source of funding than short-term debt (Berlin and Mester, 1999; Hanson, Shleifer, Stein and Vishny, 2015). 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, Hortaçsu and Matvos, 2017; Egan, Lewellen and Sunderam, 2018). Despite these benefits for both households and banks, the take-up and active use of formal savings accounts by low-income households “remain puzzlingly low” (Karlan et al., 2016).

Prize-linked savings (PLS) accounts are a potential solution developed and implemented by both the public and private sectors around the world (Cole, Tufano, Schneider and Collins, 2007; Kearney, Tufano, Guryan and Hurst, 2011). These accounts offer raffle tickets as an incentive to save: often in lieu of paying a regular fixed interest rate, large cash prizes are awarded each month in a raffle. The number of raffle tickets a client receives is typically a function of the amount of new savings she accumulates. Like a traditional lottery, PLS offers a small chance at winning a large prize. PLS could thus be appealing to potential savers who overweight small probabilities (Kahneman and Tversky, 1979) or those with demand for uncertainty and skewness in income due to nonconcavities in their utility function (Friedman and Savage, 1948), which could arise from demand for indivisible assets in the presence of financial constraints (Kwang, 1965; Hartley and Farrell, 2002). Unlike a traditional lottery, PLS customers keep the principal that they deposited in the savings account; households seeking skewness might therefore substitute from gambling to saving in PLS accounts (Cole, Iverson and Tufano, forthcoming; Cookson, 2018).

We conduct a randomized controlled trial (RCT) of prize-linked savings. Among 110 branches of a bank in Mexico, we randomly assigned treatment branches to offer PLS over a two-month period, and we measure effects on account openings, deposits, and savings over the subsequent five years. To participate in the raffle for large 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 raffle ticket; our partner bank raffled one thousand small prizes of 400 pesos (US\$32) and two large prizes of 10,000 pesos (US\$809) at the end of each of the two months, paying the winners in their accounts. While the expected return was unknown to potential savers *ex ante* since it would depend on other account holders’ savings, *ex post* it was equivalent to a 2.4% annual interest rate. The branch-level randomization makes our estimates relevant for a bank considering offering PLS to attract deposits, and it allows

us to measure both the extensive margin effect on new account openings and the intensive margin effect on the savings of existing account holders. After two months, the raffles 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.

We find that offering PLS caused a 41% increase in the opening of savings accounts in treatment branches relative to control branches. Using a database from Mexico’s Central Bank that links bank accounts within individuals across banks, we find that 96% of account openers at treatment branches during the incentive months were previously unbanked (i.e., they had no prior bank account at any bank). The treatment effect on new account openings steadily increased over the two months that we offered the PLS incentive; in the second incentive month, treatment branches had 68% more accounts opened than control branches. After the final raffle, 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.

We also find a substantial and persistent increase in the flow of deposits at the branch level (both in the number of deposits and value deposited). On the other hand, we find no change in the flow of withdrawals, suggesting a substantial increase in savings.¹ We find a 22% increase in the number of deposits made at treatment branches during the incentive months, and we find that the impact on the number of deposits persists—gradually decreasing over time—for about three years after the PLS incentive was removed. Our results using the value of deposits are noisier and less conclusive, but qualitatively consistent with a persistent effect lasting at least two years after the PLS incentive was removed. While we do not have evidence on the precise mechanism that explains the temporary incentive’s lasting effect on the use of formal savings accounts, it is likely that by leading people to open an account and begin saving, the incentive led them to learn through experience about the benefits of saving—consistent with research that learning-by-doing is important for saving (Breza, Kanz and Klapper, 2020; Giné and Goldberg, 2020).

We then turn from branch-level to account-level results. Because the PLS incentive caused an increase in account openings at treatment branches, it also leads to selection; as a result, we would be unable to interpret an account-level regression including all accounts at treatment and control branches. Instead, we separately estimate results for two types of accounts. First, we analyze accounts that had already been opened at treatment and control branches prior to our experiment. New saving in these accounts was also eligible for the PLS incentive, and we exploit that the incentive was randomized across branches to measure its intensive margin impact on saving in these *previously opened accounts*. Our results show that the PLS incentive did have an intensive margin effect during the incentive months, but that this effect drops discontinuously after the incentive was

¹Results using a measure of savings at the branch level are noisy, with positive but statistically insignificant point estimates.

removed and is not persistent.

Second, we compare accounts opened during the incentive months in treatment and control branches. This comparison is subject to selection effects, but allows us to measure how new account openers who would not have opened accounts in the absence of the PLS incentive compare to those who opened accounts anyway during those months. We find that these new account openers deposited more than the control group in the second incentive month, but this effect also dissipated discontinuously and quickly after the incentive ended. Interestingly, however, the coefficients after the incentive was removed are zero or slightly positive rather than negative, indicating that these new account holders induced by PLS to open an account deposited *at least as much* into their account each month as regular account holders.

Taken together, these account-level results imply that the persistent branch-level results were driven largely by the PLS incentive leading previously unbanked households to open bank accounts and continue depositing in their accounts at similar rates as other account holders even after the incentive was removed.

We make three main contributions. First, we provide evidence from a branch-level RCT on the effectiveness of a popular savings product at increasing account opening and deposits. PLS products were introduced in the 1690s in Great Britain (Cohen, 1953) and between one-fifth and one-fourth of UK citizens participate in a PLS product today through Premium Bonds (Tufano, 2008). Prior to 2014, PLS accounts were illegal in the US, but survey evidence indicated high potential demand for the product, especially among unbanked households (Tufano, Neve and Maynard, 2011). After PLS products were made legal in the US by the 2014 American Savings Promotion Act, credit unions across the country began offering PLS accounts. Further evidence of the potentially high demand for PLS accounts among low-income households comes from lab experiments (Atalay, Bakhtiar, Cheung and Slonom, 2014; Filiz-Ozbay et al., 2015); this demand is particularly high among those who overweight the small probability of receiving a large return (Filiz-Ozbay et al., 2015; Dizon and Lybbert, forthcoming).

Additional evidence on the effect of PLS exploits the product's introduction in various countries. Cole, Iverson and Tufano (forthcoming) compare PLS account openers to existing account holders in a difference-in-differences event study framework and show that PLS account openers increased savings by substantially more than existing account holders and maintained a persistently higher savings stock than existing account holders, equivalent to about 1% of annual income. As Burke (2020) points out, these results could be driven in part by selection, which emphasizes the value of generating evidence on the persistent effect of PLS on savings through an RCT. Bharadwaj and Suri (2020) exploit the start and end of a PLS promotion to mobile money users in Kenya using a regression discontinuity in time, and also find evidence of a substantial intensive margin effect on savings in previously opened accounts; they do not measure the extensive margin on

account openings. 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 (forthcoming) and Dizon and Lybbert (forthcoming) that saving in a PLS account acts as a substitute for gambling.²

Our results suggest that a temporary PLS incentive is highly effective compared to alternatives at generating a persistent effect on saving. Unfortunately, our RCT does not allow us to directly compare the PLS incentive to a fixed interest rate with the equivalent guaranteed return (around 2.4% per year) because it was impossible to know the equivalent guaranteed return *ex ante*—and thus we could not include a treatment arm where we offered the same expected return. Nevertheless, other evidence suggests that for low-income households, changes in interest rates in the 0 to 5% range have neither an extensive margin effect on the opening of accounts nor an intensive margin effect on deposits (Karlan and Zinman, 2018; Kast, Meier and Pomeranz, 2018). Specifically, on the extensive margin, Karlan and Zinman (2018) find that increasing the interest rate from 1.5% to 3.0% led to a (statistically insignificant) 3.5% increase in account openings, while Kast, Meier and Pomeranz (2018) find that increasing the interest rate from 0.3% to 5.0% led to a (statistically insignificant) 2% increase in account openings—which can be compared to the 41% increase in account openings caused by offering PLS. On the intensive margin, Karlan and Zinman (2018) and Kast, Meier and Pomeranz (2018) again find statistically insignificant effects of offering these higher interest rates on both the number and value of deposits. Low-income households’ savings may be inelastic to changes in a small, fixed interest rate because 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, forthcoming).

Second, we measure impacts for nearly five years after the *temporary* incentive ended, and provide evidence of a *persistent* 2–3 year impact on the flow of deposits that gradually declines over time. 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 13 months, and the 90th percentile is 29 months (Horn, Jamison, Karlan and Zinman, 2020, Appendix Table 1). Tracking savings for five years after a savings intervention is rare, but a notable exception is Horn, Jamison, Karlan and Zinman (2020). As in this paper, Schaner (2018) looks at persistent impacts of a temporary incentive: she offered a “teaser” 20% annual interest rate valid for the first six months after an account is opened and observes savings after three years. While Schaner (2018) finds persistent effects on a number of outcomes including business profits, use of the accounts in her study dropped rapidly after the incentive was removed, with 5% making any transaction in the account one year after the incentive was offered, and 3% three years after. In contrast, in our study 30% of accounts opened at treatment branches during the incentive months

²Herskowitz (2021) shows that gambling is used by the poor to save for indivisible assets.

continued to be used one year after the incentive was offered and 14% continued to be used three years after.

Third, 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. We are thus able to estimate how profitable offering a PLS product was for our partner bank. This profitability depends on the effect of offering PLS on both the extensive margin (new accounts opened) and intensive margin (increased savings among existing account holders). Both of these margins are relevant for banks considering offering a PLS product, and our branch-level randomization allows us to measure both. In contrast, other papers have sought to carefully quantify behavioral responses to various savings interventions, and have thus focused on individuals. Due to this focus on individual behavior, these papers typically *either* restricted the experimental sample to non-account holders and offered an account with a new feature or incentive to open the account (e.g., Cole, Sampson and Zia, 2011; Dupas and Robinson, 2013; Dupas, Karlan, Robinson and Ubfal, 2018) *or* restricted the experimental sample to existing 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).³

2 Institutional Context

2.1 Prize-Linked Savings Around the World

Prize-linked savings were introduced in the 1690s in Great Britain (Cohen, 1953) and remain a popular product in the UK today, with between one-fifth and one-fourth of UK citizens participating in a PLS product through Premium Bonds (Tufano, 2008). Since then, PLS products have been introduced in at least 39 countries across six continents (Figure A2). 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, forthcoming).

In the US, PLS products were illegal nationally until 2014. The American Savings and Promotion Act then legalized PLS, but it still must be legalized by individual states. For example, Texas added a state constitutional amendment in 2017 to allow banks and credit unions to offer PLS. Figure A3 shows which US states have introduced legislation to allow credit unions and/or banks to offer PLS. In all states in which credit unions are allowed to offer PLS, we found examples of PLS products offered by credit unions. While national banks 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.

³Breza and Chandrasekhar (2019) and Schaner (2018) restricted the experimental sample to people who had expressed interest in opening an account, rather than people who already had an account.

2.2 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 is low: about 39% of the adult population has a bank or mobile money account. It is even lower for low-income Mexicans: only 29% of those with incomes in the bottom 40% of the income distribution have an account (Demirgürç-Kunt, Klapper, Singer and Oudheusden, 2015). While microfinance institutes like Compartamos Banco have rapidly expanded access to credit (Angelucci, Karlan and Zinman, 2015), they have not aggressively pursued savings products.

Several banks in Mexico have offered temporary PLS incentives in the past. In addition to the experiment we conducted with Bansefi, PLS products have been offered by Banamex, Banco Azteca, and HSBC.

2.3 A Simple Model of Prize-Linked Savings

Why would a temporary incentive to open formal savings accounts have a persistent effect on deposits at the branch level? In other words, why wouldn't marginal account openers who were incentivized to open an account by the lottery stop using the account after the incentive was removed? Two potential mechanisms are fixed costs of opening accounts and learning. To fix ideas of how these two mechanisms may generate both account openings as a result of the lottery and persistence in leaving those account active after the lotteries end, we lay out a very stylized model. In doing this our aim is only to organize ideas. We are not able to disentangle these mechanisms empirically.

In this simple model the borrower decides if she wants to open a bank account to save in it or not. There is a benefit $B(S_t)$ of saving S pesos in the bank account in period t , but there is also a per period cost of doing this. This cost depends for instance on the distance to the branch. But following the findings of Bachas, Gertler, Higgins and Seira (forthcoming) it seems that an important cost perceived by Bansefi's clients is the likelihood that their savings disappears. Obviating the cost of distance and other costs, we will simply write the cost as $C_t = (1 - \theta_t)S_t$, where θ_t is the subjective likelihood that the money is stolen. Borrowers start with a prior θ_0 of how likely is it that the bank will steal their money. Bachas, Gertler, Higgins and Seira (forthcoming) show that this prior tends to be high before experiencing the account, and that subsequent experience with the bank makes them learn that their account is safe.

Besides the per period cost, there is also a fixed cost of opening the the account γ . This is a once and for all cost and includes the trouble of going to the branch, bringing the required documents, among others. Some borrowers may live farther to the Bansefi branch than others —or just experience more discomfort dealing with account opening procedures— thus we let this cost

to be heterogeneous across borrowers i : $\gamma_i \sim F(\gamma)$.

Under these assumptions, and using a linear and separable utility function, borrower i opens the savings account if and only if $\sum_{t=0}^T \beta^t [B(S_t) - \mathbb{E}_\theta(C_t)] - \gamma_i \geq 0$. For simplicity we will also assume that S_t is exogenous. Simplifying the model even more by making it a 2-period model ($t = 1, 2$), and adding the value α of winning the lottery per every unit of saving S we have the following⁴:

$$\textbf{No lottery: } \text{borrower } i \text{ opens } \iff \gamma_i \leq [B - S\theta_1] + \beta[B - S\mathbb{E}(\theta_2)] \quad (1)$$

$$\textbf{With lottery: } \text{borrower } i \text{ opens } \iff \gamma_i - \alpha S \leq [B - S\theta_1] + \beta[B - S\mathbb{E}(\theta_2)] \quad (2)$$

Clearly, the existence of the lottery increases the value of opening the account. The fraction of borrowers opening accounts will be larger with lotteries. That is, there is an interval $[\underline{\gamma}, \bar{\gamma}]$ ⁵ of borrowers that would open with the lottery in $t = 1$ but not without the lottery, and the fraction of borrowers induced by the lottery to open the account in $t = 1$ is $[F(\bar{\gamma}) - F(\underline{\gamma})]$.

In period 2, those borrowers who opened accounts choose whether to close them or not, having learned about the cost of saving in the account and updated their expectation of the safety of their account to θ_2 .

$$\textbf{Closing: } \text{At } t = 2, \text{ borrowers will } \textit{not} \text{ close the account } \iff B - S\theta_2 \geq 0. \quad (3)$$

The set of borrowers with γ_i 's that belong to $[\underline{\gamma}, \bar{\gamma}]$ and that satisfy 3 are the borrowers that are induced to open the account as a result of the lottery, and that leave the account active even when there are no more lotteries. This set may be not empty even then there is no learning ($\theta_1 = \theta_2$), but is larger when borrowers learn to trust (i.e. when $\theta_2 < \theta_1$). Thus both learning and fixed costs could explain why our temporary incentive to save has lasting effects.

3 Experimental Design

3.1 Partner Bank

To promote financial inclusion, the Mexican government founded the National Savings and Financial Services Bank (Bansefi) in 2001. Its mission is “contributing 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, most of them opened directly by the government to pay conditional cash transfers. A minority of Bansefi accounts were instead opened voluntarily by the public—primarily

⁴Recall that in our experiment, the borrower got 1 electronic lottery ticket for each extra 50 pesos saved.

⁵ $\underline{\gamma} := [B - S\theta_1] + \beta[B - S\mathbb{E}(\theta_2)]$, and $\bar{\gamma} := \alpha S + \underline{\gamma}$.

by low-income households. Bansefi has tried to locate its branches in relatively underserved areas. It concentrates on offering savings accounts with no minimum balance, no fees, and low interest rates (about 0.09-0.16% per year). Bansefi has been innovative in how to attract low-income savers. One of their strategies, beginning in 2005, was to offer prize-linked savings accounts. They offered these accounts from 2005–2007 but did not rigorously measure the effectiveness of these accounts; after a change of management, Bansefi discontinued these accounts in 2008. 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.

3.2 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 branches that offered this product left us with 214 out of the initial 494 branches for our sampling frame. We further restricted the sample by excluding the largest and smallest branches from our sample, measured by the volume of new accounts opened in the first half of 2010. To reduce variance and have more power we removed approximately the smallest 25% and largest 25% of branches from the sampling frame. Finally, we focused on states that had at least two branches surviving our selection criteria. After applying these selection criteria, our sampling frame consisted of 110 Bansefi branches spanning 19 of Mexico’s 32 states throughout the entire country from Baja to the Yucatan Peninsula. One contribution of our paper is that experiments on savings rarely have this extent of geographical breadth.⁶

3.3 Randomization

Within the 110 Bansefi branches in our sampling frame, we conducted a simple randomization to assign 40 branches to treatment. Table 1 shows that treatment and control branches have balanced covariates. Figure 1 shows the locations of treatment and control branches.

3.4 PLS Incentive

The lotteries were announced in mid-September in treatment branches only, through posters inside the branch and loud-speaker cars on nearby streets. Due to budget restrictions, the loud-speaker advertising happened only in September 2010, which also enables us to rule out the treatment effect being due to this type of advertising. Two lotteries were held, the first on October 12, 2010 and the second on November 12, 2010. Figure A1 shows the timeline of the experiment and an example

⁶Two notable exceptions are the recent multi-country savings experiments in Dupas, Karlan, Robinson and Ubfal (2018) and Karlan, Savonitto, Thuybaert and Udry (2017).

of Bansefi’s advertisements of the savings lotteries, which reads “save in a *debi cuenta* account and multiply your money.” Bank tellers were prepared to answer questions regarding the rules of the lotteries.

Both existing account holders and anyone who opened a new account during the lottery months could participate. Furthermore, a client had to increase her savings in the month preceding the lottery by at least \$50 pesos to participate. Every \$50 pesos increase entitled the client to one electronic ticket. Note that the incentives were thus only active from mid-September to November 12, 2010 (the day of the last lottery). The number of tickets for a lottery were calculated as new savings accrued over the last month, divided by 50 and rounded down to an integer. Other than the lottery tickets, the other aspects of the account (including the interest rate) were identical to those of accounts in control branches. For a single prize, the likelihood of winning for a client would be equal to her number of tickets divided by the total number of tickets in all 40 treatment branches. The probability of winning is endogenous to total participation and was therefore not known *ex ante*. *Ex post*, the probability of the median saver winning a small prize was 1 in 26, while the probability of the median saver winning a large prize was 1 in 13,000.

3.5 Clients

A substantial proportion of Bansefi’s clients are beneficiaries of Mexico’s large cash transfer program Oportunidades, who receive their benefits directly in Bansefi *debi cuenta* accounts (see Bachas, Gertler, Higgins and Seira, forthcoming, for more detail). Oportunidades beneficiaries were also eligible for lottery prizes, but because their accounts are opened for them automatically by the government when they are enrolled in the program, we exclude Oportunidades accounts from the analysis.

4 Data

We use two types of administrative data from Bansefi for the accounts at the 110 branches included in our experiment. First, we have data on every account opened from 2008 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 have transactions data for pre-existing accounts and those opened during lottery months over a five year period (longer in the case of pre-existing accounts). Specifically, for accounts opened during the lottery months, we observe all transactions data from the date they were opened through July 2015. For accounts that existed before the lotteries in treatment and control branches we have transactions data from 2008–2015. Finally we use data from the 2005 Census to show balance across the localities in which our treatment and control branches are located for sociodemographic characteristics that are not present in our administrative bank data. Table 1 presents means for these data across treatment and control branches, and a t-test of equality of means. In all cases, we fail to reject the null hypothesis of equal means.

There are a few notable summary statistics from Table 1. First, these are small bank branches: excluding the bank accounts that they administer for recipients of government social programs, there are only 50–60 total accounts open at each branch. Each month, about 4 non-Oportunidades accounts are opened per branch. This reflects that Bansefi positions its branches in relatively underserved areas, but also underscores the difficulty of attracting the unbanked. On average, an account holder makes about 0.3 deposits and 1.1 withdrawals per month.

In addition, Mexico’s Central Bank allowed us to query a database that links accounts within individuals across banks in Mexico, which thus allowed us to observe whether new account openers already had bank accounts at other banks. We provided the Central Bank with the account numbers for the accounts opened at treatment branches during the incentive months, and they provided us with summary statistics on the fraction of these account openers that (i) previously had an account open at another bank prior to opening their Bansefi account or (ii) opened an account at another bank after opening their Bansefi account.

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 t :

$$y_{jt} = \alpha_t + \gamma T_j + \theta y_{j0} + \varepsilon_{jt}, \quad (4)$$

where y_j is an outcome at branch j , 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 (4) where the outcome is the number of accounts opened. In the eight pre-lottery months, there is no difference between treatment and control branches in the number of accounts opened (as expected by virtue of randomization). In the first lottery month, the point estimate is positive but not particularly large or statistically significant. In the second incentive month, an additional 2.13 accounts per branch per month are opened in treatment branches ($p < 0.01$), which represents a 68% increase compared to control branches. In the post-lottery months, during which there could be lasting effects from the lottery—e.g., through hype created during the lottery even after the incentive is removed—there is no statistically significant difference in account openings at treatment and control branches in six of the seven months

⁷We define baseline as being from January 12, 2008 to January 12, 2010, since we use the 8 months beginning January 13, 2010 as placebo tests using the same specification (4).

and a significant difference at the 10% level ($p = 0.08$) in one month. 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 the account in a treatment branch. However, the control mean of 7.39 account openings during the two-month lottery period is very close to the average number of accounts opened in control branches across all two-month periods before the lotteries (7.54 account openings per period), and we fail to reject that the control mean during the lottery months is different than the control mean during the pre-lottery months. We conduct additional tests to rule out the possibility of substitution across branches in Section 6.2.

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 3. 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. There is a clear trend: prior to the introduction of lotteries, there is no difference between treatment and control in the number of accounts opened per day. When the lotteries are introduced in mid-September, the treatment effect steadily increases over time, reaching about 0.1 new accounts per branch per day by the end of the lottery period. Then, immediately after the final lottery on November 12, the treatment effect abruptly falls to 0.⁸

There are various reasons that the treatment effect might increase over time during the lottery months. More individuals might be learning about the lotteries over time. In addition, the first announcement of lottery prize winners on October 12 might lead to “local buzz” about the product that further increases lottery openings in the second month (Guryan and Kearney, 2008; Cole, Iverson and Tufano, forthcoming).

Are new account openers 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 response to the PLS were previously unbanked.

We also directly test whether the account openings occurred in areas with a higher density of commercial bank branches of any bank, and do not find evidence of such a relationship.

Specifically, we estimate

$$y_{jt} = \alpha_t + \theta y_{j0} + \gamma T_j + \xi \text{Density of banks}_j + \phi T_j \times \text{Density of banks}_j + \varepsilon_{jt} \quad (5)$$

⁸More formally, we estimate the daily treatment effect to the left and right of the “discontinuity” (final day of lotteries) 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 0.03 accounts per branch per day (statistically insignificant from 0, $p = 0.18$). The difference between the estimates to the left and right of the discontinuity is significant at the 5% level using conventional confidence intervals, and significant at the 10% level when using the robust bias-corrected confidence intervals recommended by Calonico, Cattaneo and Titiunik (2014).

separately for each month t , where $Density\ of\ banks_j$ is either a continuous measure of how many commercial banks are within a 1-kilometer radius of Bansefi branch j or a dummy variable indicating that Bansefi branch j has greater than the median number of commercial bank branches within a 1-kilometer radius. The median number of bank branches within a 1-kilometer radius of the Bansefi branch is 11.5. Table 2 shows the results for each of the density measures described above, for each of the two lottery months ($t = \text{Oct. 2010}$ and $t = \text{Nov. 2010}$). For both measures of density and both months, we cannot reject that $\phi = 0$.⁹

Branch-level deposits and withdrawals We next estimate (4) with the branch-level number of deposits or withdrawals as the outcome. Figure 4 shows that there is an increase in the number of deposits of 6–10 deposits at treatment branches during the incentive months, which is a 22% increase on average. This effect is persistent: it declines gradually over the next three years with no immediate drop after the incentives ended. In contrast, there is no statistically significant effect on the number of withdrawals per branch during the incentive months, and no effect in most of the post-incentive months.¹⁰ Furthermore, the point estimates and confidence intervals of the effect on withdrawals are very small. Taken together, the results on deposits and withdrawals suggest that the PLS incentive likely led to an increase in the flow of savings at the branch level.

We next use the inverse hyperbolic sine of the flow of deposits in pesos; we use the inverse hyperbolic sine for a log-like transformation that accommodates zeros. These results are noisier but consistent with the findings for the number of deposits (Figure 5): we see suggestive evidence of a persistent effect (that is statistically significant in some months but not in others) for at least two years.

5.2 Account-level results

Because the PLS incentive caused an increase in account openings at treatment branches, it also leads to selection; as a result, we would be unable to interpret an account-level regression including all accounts at treatment and control branches. Instead, we separately estimate results for two types of accounts. First, we analyze accounts that had already been opened at treatment and control branches prior to our experiment. New saving in these accounts was also eligible for the PLS incentive, and we exploit that the incentive was randomized across branches to measure its intensive margin impact on saving in these *previously opened accounts*. Second, we compare accounts opened during the incentive months in treatment and control branches. This comparison is subject to selection effects, but allows us to measure how new account openers who would not

⁹In October 2010, for example, the point estimate using the dummy for higher density is an additional treatment effect of 0.17 ($p = 0.91$) and using the continuous measure of density the point estimate goes in the opposite direction: 0.05 less openings at treatment branches per additional commercial bank branch within 1 kilometer ($p = 0.44$). Using distance to the closest commercial bank branch (either a dummy for greater than the median distance or a continuous measure of distance), we also fail to reject $\phi = 0$ for either treatment month.

¹⁰In a small fraction of post-incentive months, there is a statistically significant but very small effect.

have opened accounts in the absence of the PLS incentive compare to those who opened accounts anyway during those months.

5.2.1 Previously opened accounts

We use previously opened accounts (i.e., opened before our RCT was conducted) at treatment and control branches to measure the effect of the lottery on the intensive margin. We estimate

$$Balance_{ijt} = \alpha_t + \gamma T_j + \beta Balance_{ij0} + \varepsilon_{ijt}, \quad (6)$$

separately for each month t , where $Balance_{ijt}$ is end-of-period balance of account i in branch j in month t , T_j is a dummy that equals 1 for treatment branches, we control for the amount won $Winnings_{ij}$ (summing over both lotteries), and $Balance_{ij0}$ is average baseline end-of-period balance. Standard errors are clustered at the branch level.

Figure 6 shows that the lotteries had an effect on deposits during the second lottery month, but the difference between the point estimates in the post-lottery months and the pre-lottery months are not statistically significant, suggesting that there was no persistent intensive margin effect on deposits.

5.2.2 New accounts

We next estimate (6) for accounts opened during the lottery months. When comparing accounts opened in treatment and control branches, there could of course be a selection effect: clients induced to open accounts by the lottery may not behave in the same way as clients who would have opened accounts anyway. This is precisely what we investigate. Since treatment caused a 68% increase in the number of account openings in the second month of the lotteries, about 40% ($= .68/1.68$) of those who opened accounts during this month in treatment branches were induced to do so by the lottery, while the other 60% would have opened accounts anyway. Hence, any differences we find between treatment and control can be multiplied by 2.5 to obtain an upper bound of the difference between those who were induced to open accounts by the lotteries and other account openers.

Figure 7 shows that during the second lottery month, new account openers induced to open accounts by the PLS incentive made about 0.2 more deposits per month on average than those who opened accounts in control months. After the lottery incentive ended, this effect drops, and is still statistically significant in two of the three months after the lottery ended, before becoming statistically insignificant in most post-lottery months. Importantly, this means 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. This rules out the concern that new account openers induced to open accounts by the lottery did so only for the purpose of participating in the lottery

and closed the account or left it unused after the lotteries end. If this were the case, we would observe negative point estimates in the post-lottery months and would conclude that temporary PLS incentives would not be a successful financial inclusion tool nor a way for banks to increase deposits.

We next show descriptive results on the use of accounts opened during the lottery months. It is uncommon for accounts to be outright closed in the years following the lotteries. Two years after the lotteries, 94% of accounts remain open; 3 years after, 91% remain open; and nearly 5 years after the lotteries, 71% of accounts remain open. Importantly, in no month can we reject that the proportion of accounts remaining open is equal across accounts opened during the second lottery month in treatment and control branches.

Accounts may remain open but be dormant with no savings. Figure 8 uses a stricter measure of survival and shows the proportion of accounts with at least a 50 peso end-of-period balance in treatment (blue squares) and control (orange circles). It graphs conditional means to account for the possibility of winning the lottery.¹¹ The proportion of accounts that have this amount of savings falls over time, but the levels and trend at which it falls are almost identical between accounts opened in the second lottery month in treatment and control accounts. One year after the lotteries, 74% of accounts remain in use by this measure; three years after, 47%; and nearly five years after, 35%. In all of these cases, we cannot reject the null hypothesis of no difference between lottery-month openers in treatment and control branches.

For accounts that remain in use, we examine active use of the account using three measures from the literature: at least one deposit in the last six months (Schaner, 2017), at least two deposits in the last six months (Dupas and Robinson, 2013), and the more long-term measure of at least five deposits in the last two years (?). Using the least restrictive measure of at least one deposit, about 57% of accounts are active users six months after opening the account in the month from April 13 to May 12, 2011.¹² In most periods, there is no statistically significant difference in this measure; in the periods where there is a statistically significant difference, the treatment accounts use the account *more*. Using the slightly more restrictive measure of at least two deposits, about 27% are active users initially, and 12% are active users after five years. Finally, using ?'s (?) longer-horizon measure, the proportion of active users falls from 26% two years after the lotteries to 18% nearly five years after. Again we either cannot reject equal activity between treatment and control accounts or find slightly *more* active use among treatment accounts.

Figure 9 shows the distribution of deposits and withdrawals in surviving accounts in accounts

¹¹Specifically, the conditional mean is the coefficient α from a regression of $\mathbb{I}(Balance_{ij} > 50) = \alpha + \psi Winnings_{ij} + \varepsilon_{ij}$ separately for each month t , where \mathbb{I} is the indicator function that equals 1 if its argument is true and 0 otherwise.

¹²This is six months after the final lottery, and therefore is not contaminated from deposits required to open the account in the first place.

opened during the lottery month in treatment branches (outlined in blue) and control branches (solid orange). While use falls over time, the distribution of use between the treatment and control group looks similar, and in both a group of very active users making 20 or more transactions per year (about 10% of clients) persists in the long term.

6 Alternative Explanations

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

6.1 Marketing Effect

One worry is that advertising that occurred outside of branches in the surrounding area (through loud-speaker cars) 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.

Due to budget constraints, the advertising through loud-speaker cars only occurred in September 2010. Thus, a marketing effect would be expected to be more concentrated at the beginning of the two-month lottery period, which is not what we find in Figure 3. Furthermore, even if this advertising took time to take effect, if 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. The sharp discontinuity in the daily treatment effect after November 12 (Figure 3) provides strong evidence against this alternative explanation.

6.2 Substitution across 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 the case, some of the effect would not be due to new accounts, but just an (inefficient) reallocation of accounts to other branches. In addition to the evidence presented earlier that the average number of account openings in control branches does not decrease during lottery months (which would be expected if the treatment effect were due to substitution across branches), we can test whether the treatment effect is stronger in treatment branches located closer to control branches. We thus estimate

$$y_j = \alpha + \theta y_{j0} + \gamma T_j + \xi Distance\ to\ control_j + \phi T_j \times Distance\ to\ control_j + \epsilon_j, \quad (7)$$

separately for each month t , where $Distance\ to\ control_j$ is either a dummy indicating whether the geodesic distance between Bansei branch j and its closest control branch is further than the median distance, or a continuous measure of geodesic distance between branch j and its closest

control branch in kilometers.¹³ If individuals opening accounts indeed substitute across branches due to the lotteries, we expect $\gamma < 0$. Table 3 shows the results for each of the distance measures described above, for each of the two lottery months ($t = \text{Oct. 2010}$ and $t = \text{Nov. 2010}$). We do not find evidence of substitution across branches.

7 Conclusion

In an experiment in 110 bank branches across Mexico, we find that prize-linked savings accounts can increase saving on the extensive margin by inducing new savers to open accounts. The lottery prizes were only offered over a two-month period, and we find that these temporary incentives created long-term changes in savings behavior for a substantial portion of those induced to open accounts by the lottery incentives. Taken together, our results suggest that prize-linked savings accounts can encourage the unbanked to open bank accounts and *persistently* increase the flow of new deposits for banks that offer PLS.

¹³We measure $Distance_{j,t} to control_j$ for both treatment and control branches so that ξ is identified. For control branches, it is the distance between that branch and the nearest other control branch.

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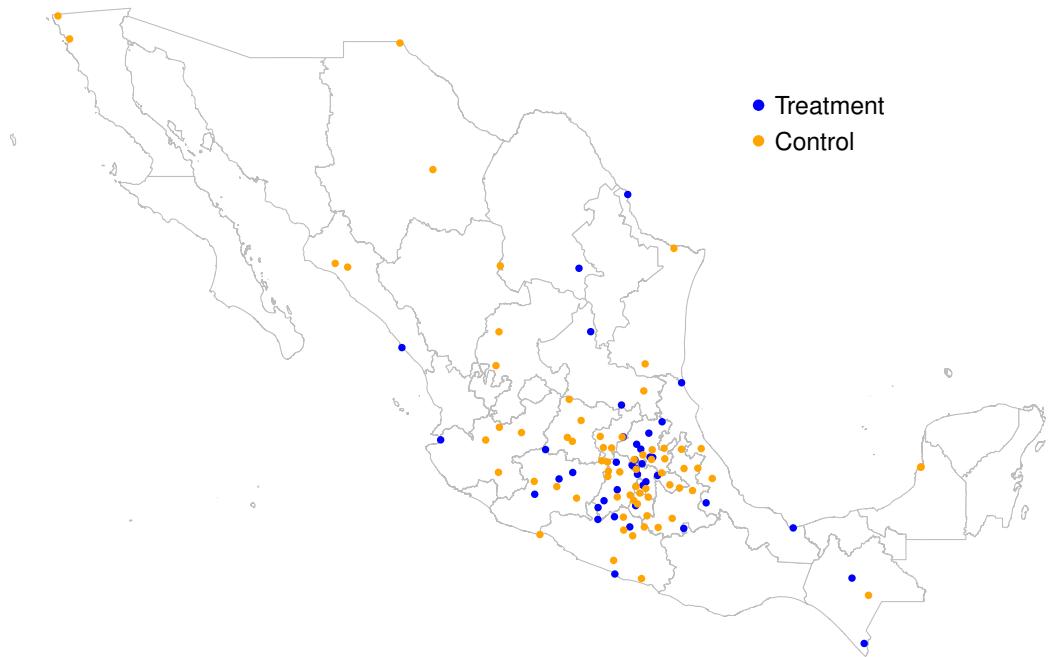
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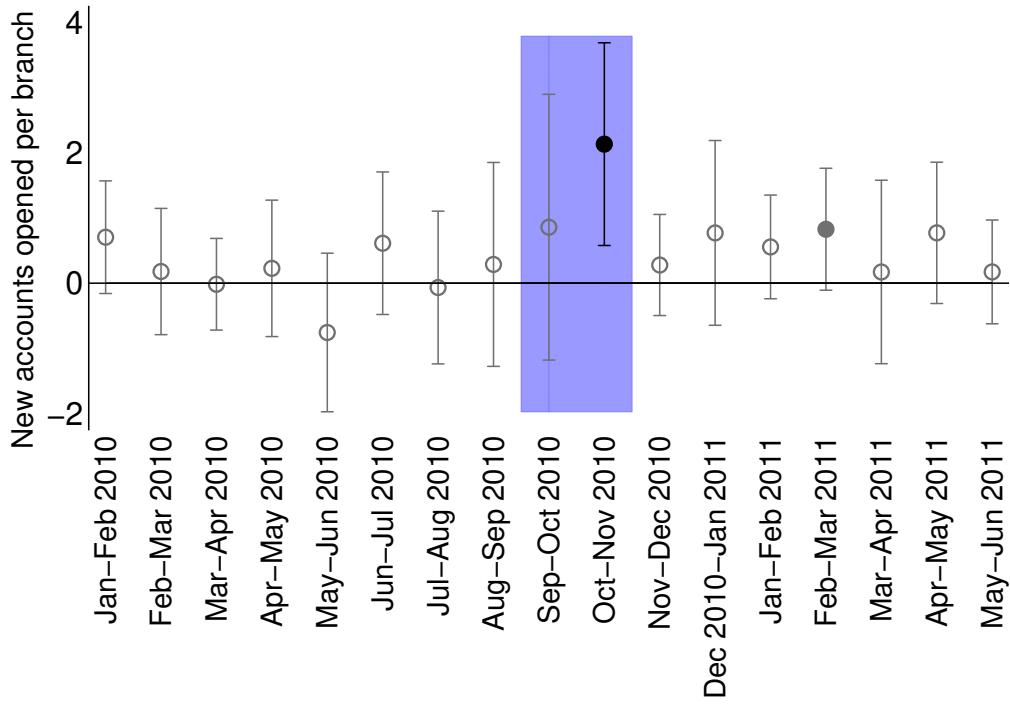
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Figure 1: Treatment and Control Branches



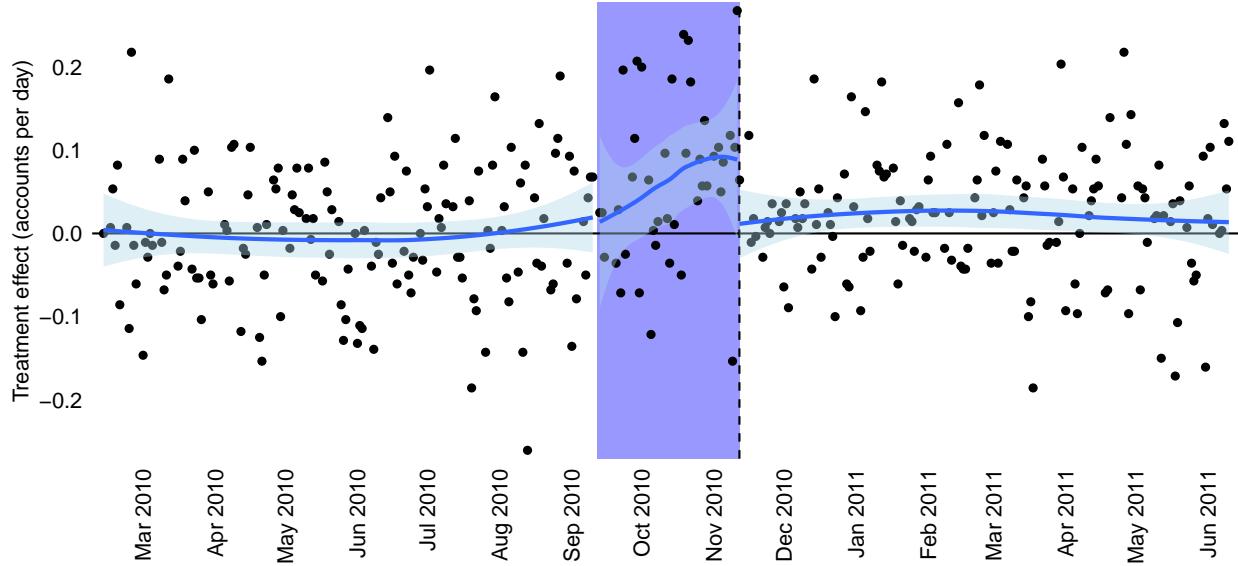
This figure shows the geocoordinates of the 40 treatment and 70 control Bansefi branches in our experiment.

Figure 2: Impact of PLS on Number of Accounts Opened per Branch



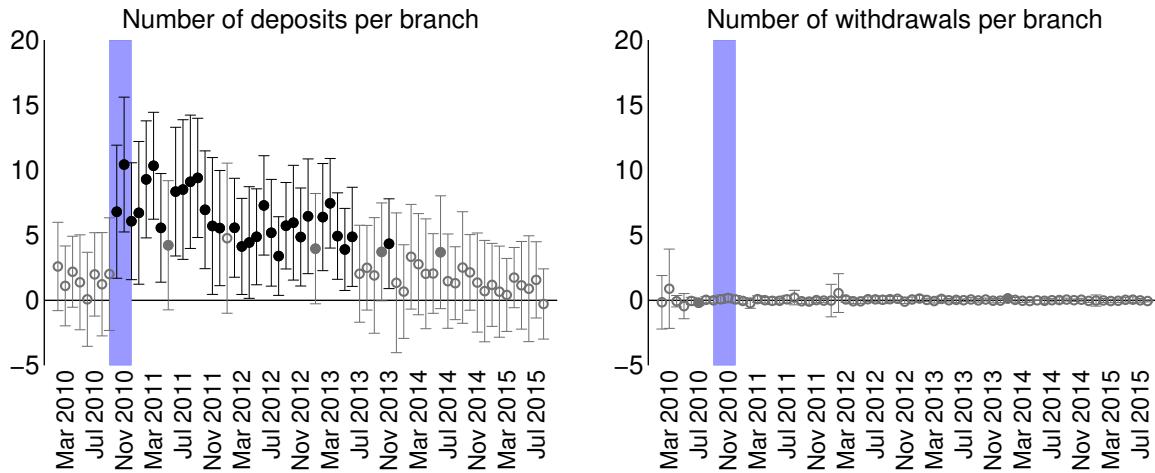
This figure shows the treatment effect of offering PLS accounts on the total number of account openings per branch over one-month periods. The one-month periods during which lotteries were offered is shaded. Each point estimate is from a separate regression for that month, where the outcome is regressed on a treatment dummy and baseline levels of the outcome. Because the lotteries occurred on October 12 and November 12, the month ranges on the x-axis refer to the 13th day of the first month in the range to the 12th day of the second month in the range. For example, the first range corresponding to the lottery months, marked “Sep–Oct 2010,” refers to September 13, 2010 to October 12, 2010. Black circles indicate results that are significant at the 5% level, gray circles at the 10% level, and hollow circles statistically insignificant from 0.

Figure 3: Impact of PLS on Number of Accounts Opened per Branch per Day



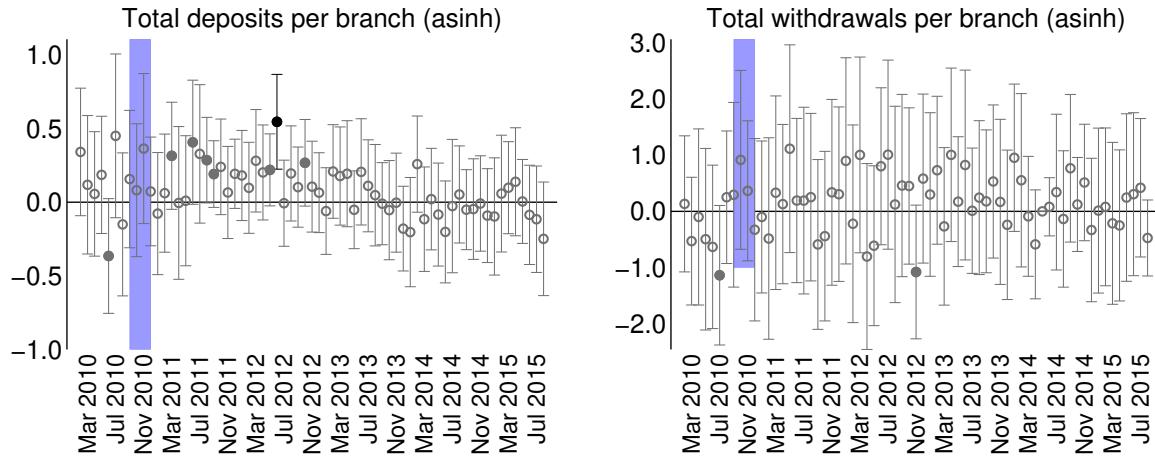
This figure shows the daily treatment effect of offering PLS accounts. Each point in the graph represents one day, and shows the treatment effect for that day, i.e. the difference in the average number of accounts opened between treatment and control branches. The blue line is a local linear regression, estimated separately for days before, during, and after the lotteries. The light blue area shows the 95% confidence interval. Lottery months (September 12 to November 12, 2010) are shaded. The final lottery on November 12, 2010 is represented by a dashed vertical line.

Figure 4: Impact of PLS on Number of Deposits per Branch



This figure shows the treatment effect of offering PLS accounts on the total number of deposits and withdrawals per branch per month. Each point estimate is from a separate regression for that month, where the outcome is regressed on a treatment dummy and baseline levels of the outcome.. Lottery months are shaded. Black circles indicate results that are significant at the 5% level, gray circles at the 10% level, and hollow circles statistically insignificant from 0.

Figure 5: Total deposits: noisier but appears consistent



This figure shows the treatment effect of offering PLS accounts on an asinh transformation of the total volume (in pesos) of the flow of pesos each month. Each point estimate is from a separate regression for that month, where the outcome is regressed on a treatment dummy and baseline levels of the outcome.. Lottery months are shaded.

Figure 6: Existing accounts: Number of deposits

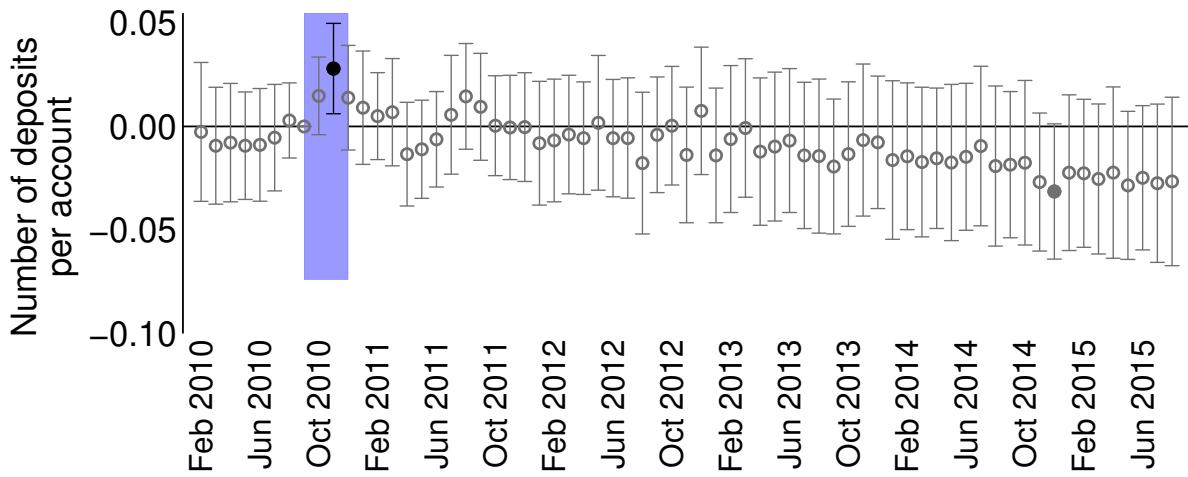


Figure 7: New accounts: Number of deposits

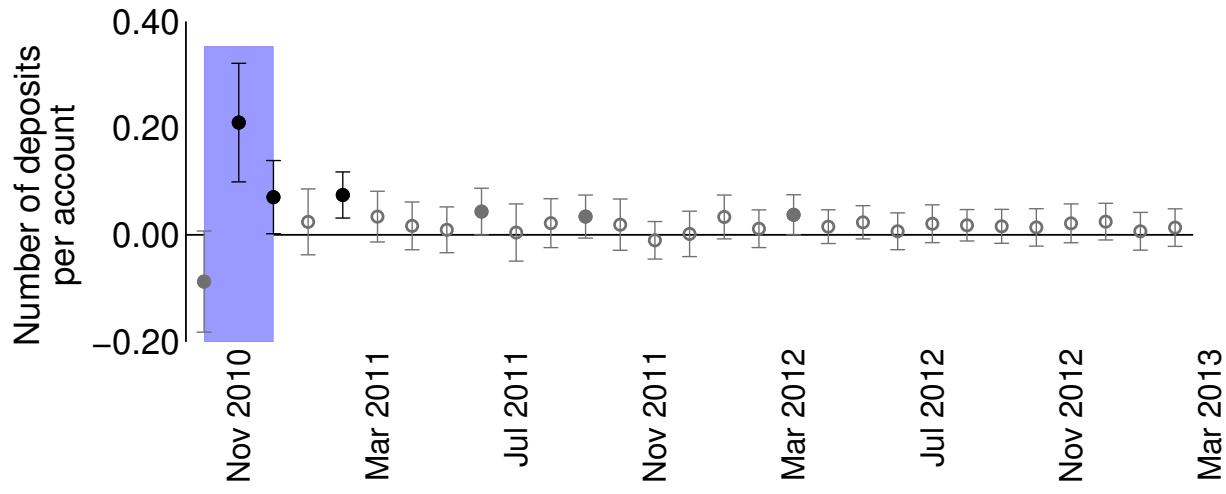


Figure 8: Proportion of Accounts Opened during Lottery Months Remaining Used in Treatment vs. Control Branches

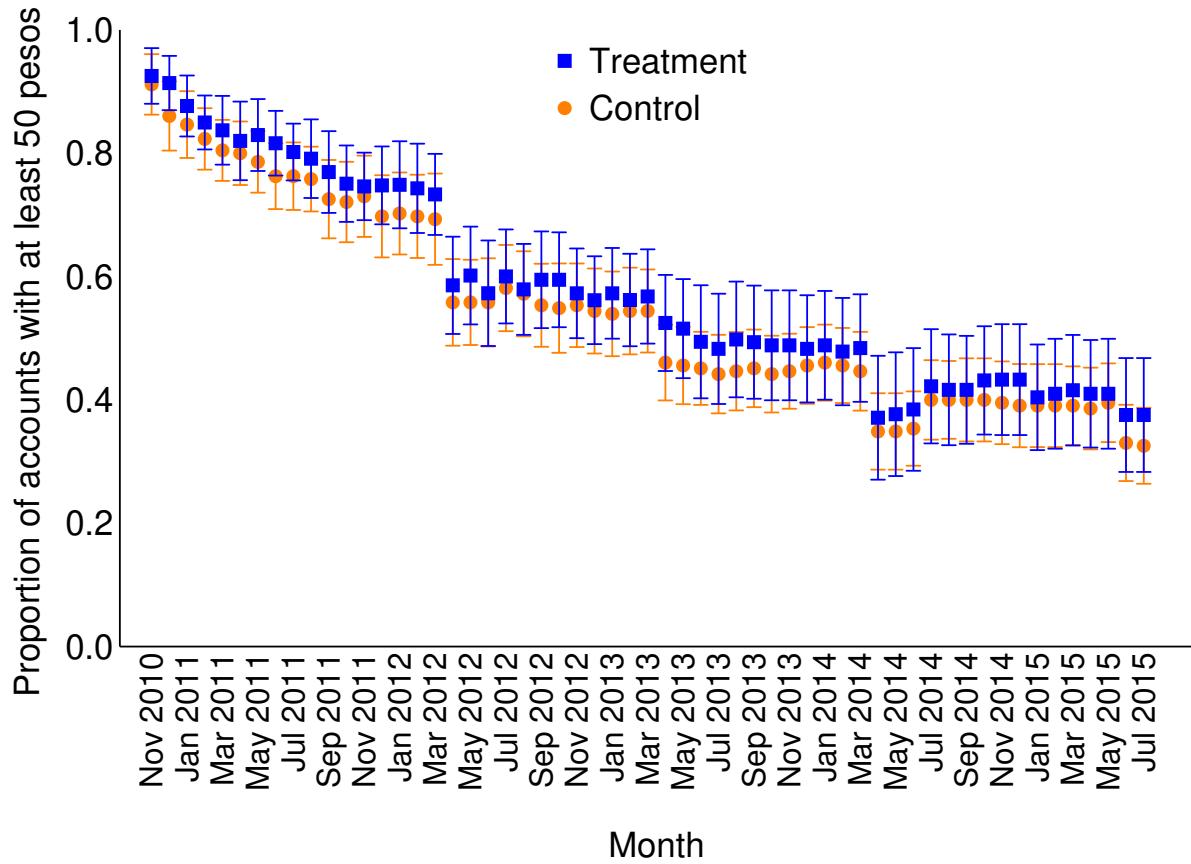


Figure 9: Distribution of Transactions, Accounts Opened during Lottery Months

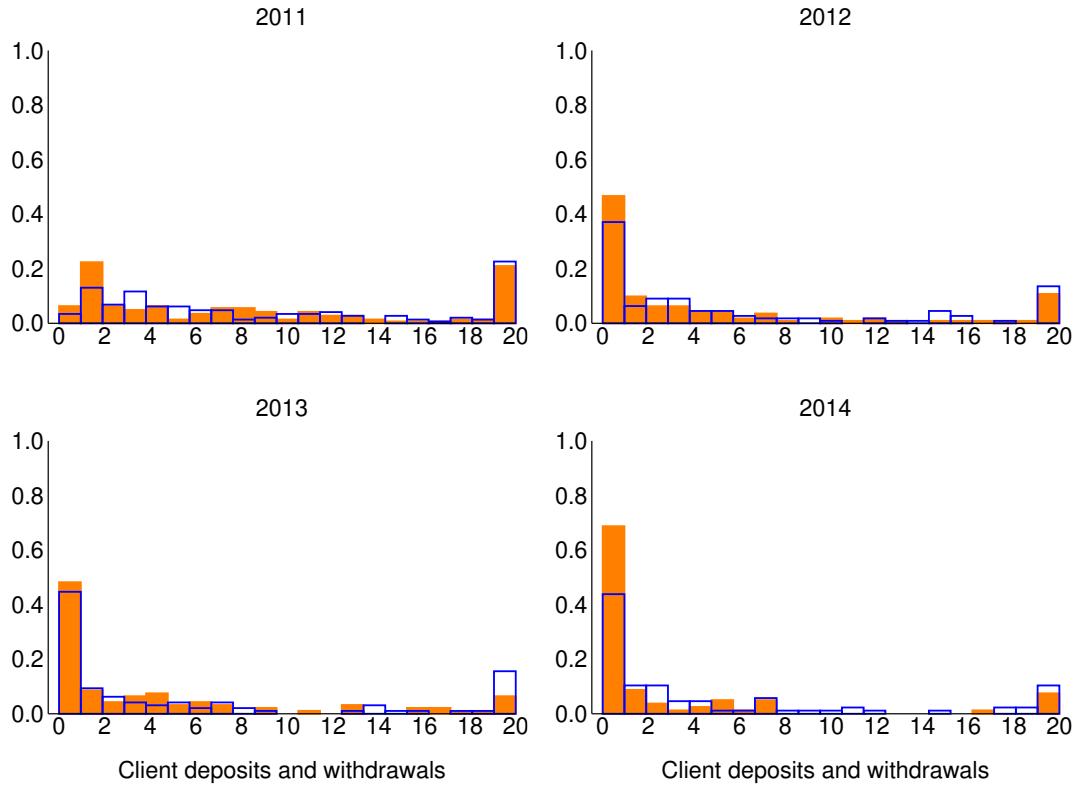


Figure 10: Comparison with Other Studies of Deposits

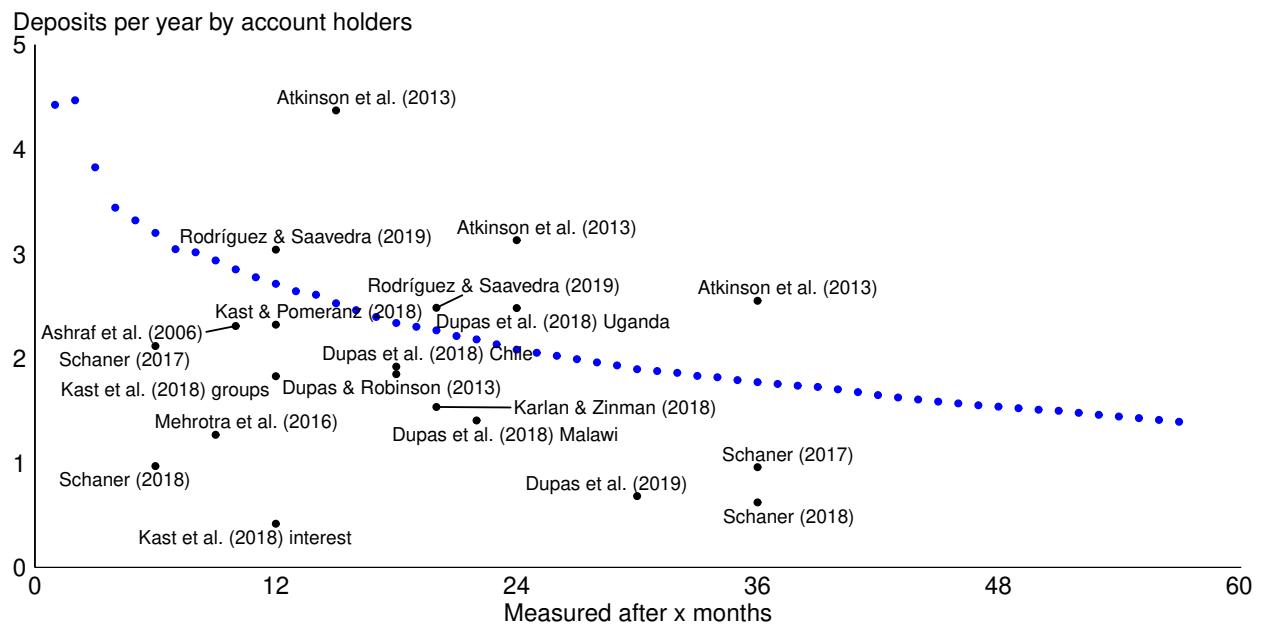


Table 1: Summary Statistics and Balance

Variable	Control	Treatment	Difference
<i>Panel A: Locality-level Data (2005)</i>			
Log population	12.75 (0.13)	12.91 (0.14)	0.16 (0.19)
Bansefi branches per 100,000	0.93 (0.08)	0.79 (0.09)	-0.14 (0.12)
% illiterate	3.84 (0.23)	3.76 (0.42)	-0.07 (0.48)
% attending school	3.38 (0.14)	3.43 (0.24)	0.05 (0.28)
% with dirt floors	3.11 (0.41)	2.87 (0.62)	-0.24 (0.74)
% without piped water	3.11 (0.57)	3.03 (0.66)	-0.08 (0.87)
% without electricity	4.87 (0.23)	5.25 (0.30)	0.38 (0.38)
Average occupants per room	1.00 (0.02)	0.97 (0.03)	-0.03 (0.03)
<i>Panel B: Bank Administrative Data</i>			
<i>Averages over baseline (2008 and 2009)</i>			
Ending Balance	1112.40 (86.32)	1160.94 (108.80)	48.54 (138.21)
Total Number of Accounts per Branch	57.91 (4.99)	49.15 (5.27)	-8.76 (7.25)
Total Value Deposited at Branch	64423.81 (5988.20)	57060.32 (5542.10)	-7363.49 (8146.18)
Accounts Opened (per month)	4.50 (0.45)	3.89 (0.38)	-0.61 (0.59)
Deposits (per account per month)	0.26 (0.02)	0.29 (0.02)	0.03 (0.03)
Withdrawals (per account per month)	1.05 (0.05)	1.18 (0.07)	0.14 (0.09)
Amount Withdrawn (per account per month)	664.26 (46.50)	785.88 (77.87)	121.62 (90.16)
Amount Deposited (per account per month)	522.14 (39.43)	646.64 (67.83)	124.50 (77.99)

Table 2: Account openings: treatment interacted with density of commercial banks

	Dummy: greater than median density		Branches within 1 kilometer	
	Oct. 2010 (1)	Nov. 2010 (2)	Oct. 2010 (3)	Nov. 2010 (4)
Baseline account openings	0.496** (0.201)	0.315** (0.125)	0.503** (0.208)	0.315** (0.130)
Treatment branch	1.610* (0.987)	0.419** (0.644)	2.382 (1.072)	0.144** (0.772)
Density of commercial banks	-0.124 (0.622)	-0.377 (0.580)	0.002 (0.026)	-0.014* (0.039)
Treatment \times density	0.173 (1.547)	1.254 (0.986)	-0.054 (0.070)	0.071 (0.052)

Table 3: Account openings: treatment interacted with distance to control branches

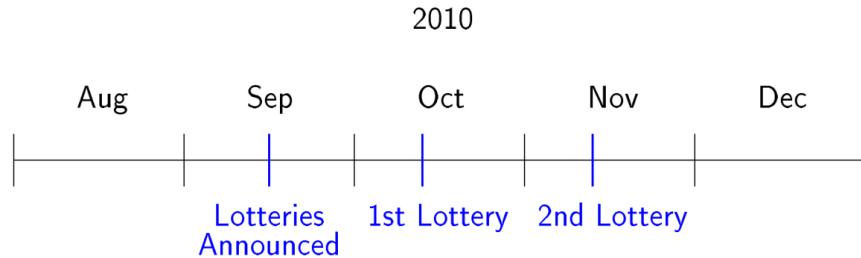
	Dummy: greater than median distance		Distance in kilometers	
	Oct. 2010 (1)	Nov. 2010 (2)	Oct. 2010 (3)	Nov. 2010 (4)
Baseline account openings	4.501*** (1.318)	3.878*** (1.038)	4.278*** (1.284)	3.710*** (1.027)
Treatment branch	-0.270 (1.316)	1.566 (0.958)	0.482 (1.165)	2.046** (0.780)
Distance to control branch	0.515 (1.109)	0.637 (0.753)	-0.002 (0.004)	0.000 (0.003)
Treatment \times distance to control	1.659 (1.996)	0.625 (1.552)	0.004 (0.011)	-0.001 (0.010)

Internet Appendix

A Appendix Figures and Tables

Figure A1: Experiment description

(a) Timeline of the experiment

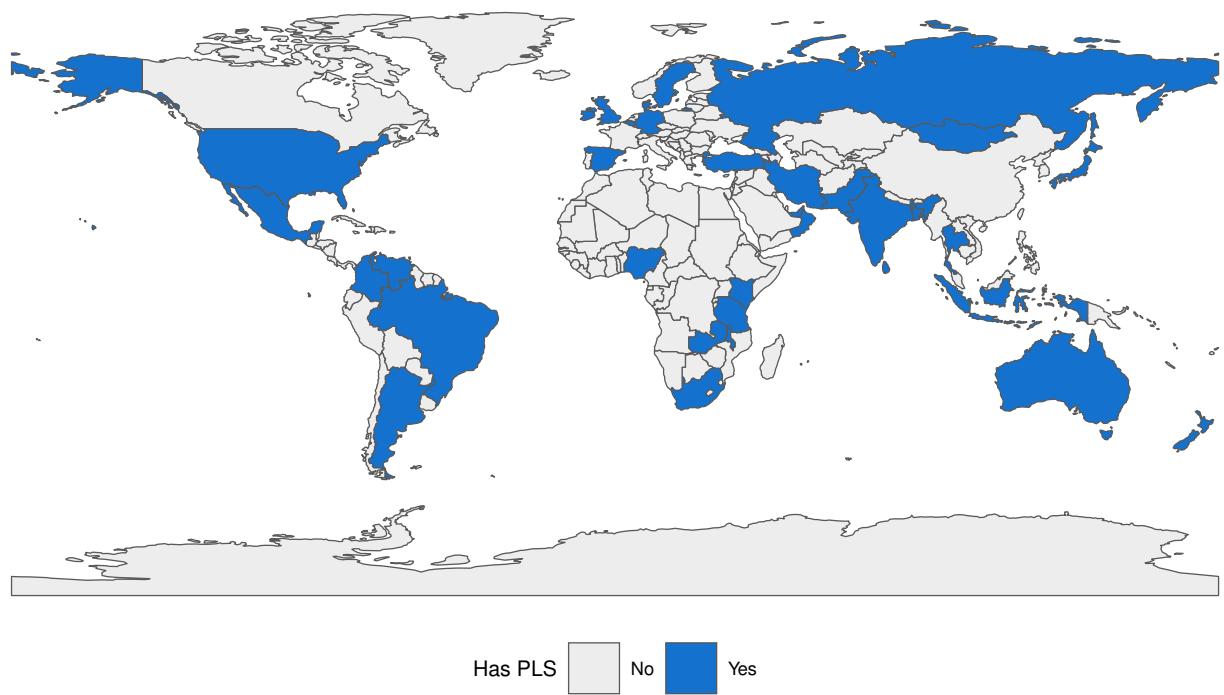


(b) Flier



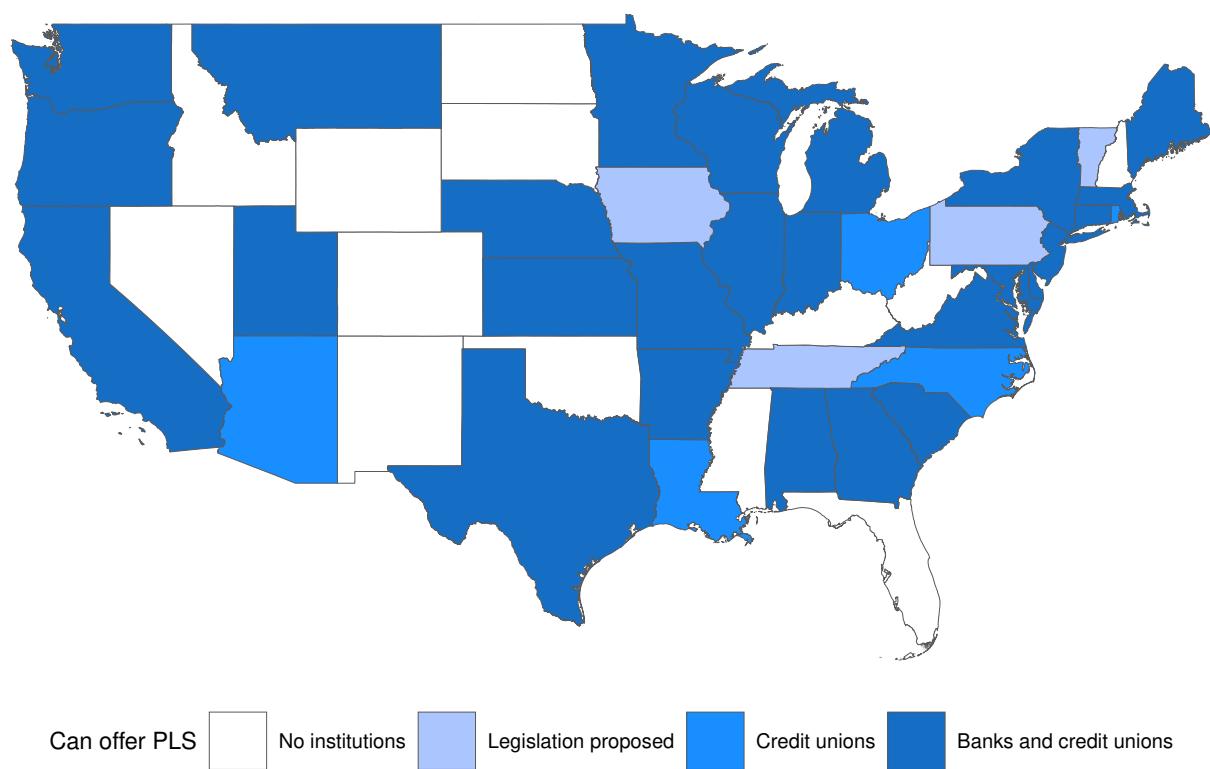
This figure shows the timeline of our RCT (panel a) and the flier that was hung in treatment branches describing the PLS product (panel b).

Figure A2: PLS Products Around the World



This figure shows which countries currently offer or have previously offered a PLS product. To create this figure, 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.

Figure A3: PLS Products Around the World



This figure shows which states allow credit unions and banks to offer PLS products. In all states in which credit unions are allowed to offer PLS products, these products are available.