Increasing Financial Inclusion and Attracting Deposits through Prize-Linked Savings

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Abstract

Despite the benefits of saving in formal financial institutions, take-up and use of savings accounts are low among the poor. In a randomized experiment across 110 bank branches throughout Mexico, we provide a temporary incentive to both open and use a savings account: saving earns raffle tickets for cash prizes. We find that 41% more accounts are opened in treatment branches than control branches during the incentive months, and the temporary two-month incentive has a lasting three-year impact on the number of deposits made at treatment branches. Prize-linked savings can thus benefit both poor households and banks.

1 Introduction

There are a number of well-documented positive effects when the poor save in formal financial institutions, including increased investment in children's education, increased ability to cope with shocks, increased investment in microenterprises, and reduced debt (Dupas and Robinson, 2013; Kast and Pomeranz, 2014; Prina, 2015). In spite of this, "uptake and active usage of formal savings devices remain puzzlingly low" (Karlan et al., 2016, p. 2), even when accounts are offered without fees (Dupas et al., forthcoming). Only 55% of adults worldwide have a bank or mobile money account (Demirgüç-Kunt et al., 2015), and most households do not have sufficient savings to cope with relatively small shocks (Dercon, 2002). For example, more than 40% of Americans report that they "either could not pay or would have to borrow or sell something" to finance a $400 emergency (Federal Reserve, 2017).

Why is take-up of formal savings devices low despite the benefits found by numerous studies? More puzzling, why is use after opening a savings account also low? Models of procrastination (O'Donoghue and Rabin, 1999; Carroll et al., 2009), high fixed costs to open the account (Cole et al., 2011), or a lack of trust in banks (Bachas et al., 2018) could explain the lack of account opening. Once accounts are open, indirect transaction costs—for example, the cost of traveling to the bank to access savings—could remain high (Bachas et al., forthcoming). Savings accounts may also be an experience good, where even after opening the account, gaining experience by using the account over time is crucial to fully understanding the costs and benefits of account use; indeed, Giné and Goldberg (2017) find that account holders randomly induced to gain more experience with their account behave more rationally.

One solution developed and implemented by both the public and private sectors around the world has been prize-linked savings (PLS) accounts (Cole et al., 2007; Kearney et al., 2011). These accounts offer lottery tickets as an incentive to save: often in lieu of paying a regular fixed interest rate, large prizes are awarded each month in a lottery. The number of lottery tickets a client receives is a function of the amount of new savings she generated recently. Like a traditional lottery, PLS offers a small chance at winning a large prize, and could thus be appealing to potential savers who overweight small probabilities (Filiz-Ozbay et al., 2015). Unlike a traditional lottery, PLS customers keep the principal that they stored in the savings account. PLS accounts could thus overcome the barriers to saving by providing not only a nudge to open accounts—due to the exciting possibility of winning a large prize—but also an incentive to begin saving and gain experience using the account, because the number of lottery tickets a saver earns is based on new savings.

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). They are also a common financial product in places like Latin America, the Middle East, Europe, and South Africa (Cole et al., 2007; Kearney et al., 2011). PLS accounts were legalized in the United States in December 2014 by the American Savings Promotion Act, but must still be legalized by state legislatures; in November 2017, for example, Texas added a state constitutional amendment to allow banks and credit unions to offer PLS. A number of PLS products have now been introduced in various US states, with high demand, generally among consumers who did not have a prior bank account.[1]

While only legalized recently in the United States, Tufano et al. (2011) report high potential demand for PLS products among lower-income consumers in the US, based on a survey. PLS accounts appear to not only be in high demand, but also nudge those who previously do not save to begin saving (Atalay et al., 2014). This might be due to overweighting small probabilities: Filiz-Ozbay et al. (2015) show theoretically and in a lab experiment that people who overweight small probabilities save more when offered a PLS account, and Dizon and Lybbert (2017) replicate this result in a lab-in-the-field experiment in Haiti. Likewise, 18 months after a South African bank began offering a PLS product, there were more PLS accounts at the bank than regular savings accounts; many of the PLS account holders had no prior savings accounts (Cole et al., 2016).

In this paper, we conduct the first randomized control trial of PLS, offering lotteries only in the first two months of the experiment so that they serve as a short-term incentive to both open an

[1] `http://freakonomics.com/podcast/say-no-no-lose-lottery-rebroadcast/`

account and gain experience saving in the account. We find that short-term incentives can increase voluntary account openings, and that new account holders use the accounts substantially over the next five years.

We make three main contributions to the literature. First, we provide the first evidence from a randomized control trial (RCT) on the effectiveness of PLS products at increasing account opening and use. PLS accounts have previously been studied only in surveys (Tufano et al., 2011), laboratory and lab-in-the-field experiments (Atalay et al., 2014; Filiz-Ozbay et al., 2015; Dizon and Lybbert, 2017), or real-world but non-randomized settings (Cole et al., 2016). Our study also contrasts with RCTs of other savings interventions in the literature, which usually provide either an extensive-margin incentive to open an account *or* an intensive-margin incentive to save more in an existing account: our experiment provides an incentive to both open and save in the account. Second, rather than randomizing the offer of savings accounts at the individual level, we randomize at the branch level and allow individuals to self-select into opening accounts. Thus, we are studying a population likely at the margin of opening accounts and can study impacts of PLS on both the extensive margin (account opening by new clients) and intensive margin (increases in saving by existing clients). Third, we follow users for five years, making this one of the longest-run studies of savings in a developing country.

Among 110 branches of a government bank in Mexico (Bansefi), we randomly assign 40 branches to conduct a lottery in the months of October and November 2010. The lottery was advertised through posters in bank branches and, in September 2010, by loud-speaker cars. To participate in the lottery, people would have to open or already have an account and generate new savings in the account. The client gets one lottery ticket for each 50 pesos of *new* savings accumulated in the account that month. Bansefi then 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, and paid the winners in their accounts. After November 2010 the lotteries cease, so that the benefits of opening an account or saving in treatment and control branches no longer differ, allowing us to study the long term effects of this temporary incentive.

We find that offering PLS causes a 41% increase in the opening of savings accounts in treatment branches relative to control branches. As expected, there is no difference in the number of accounts opened between treatment and control branches prior to offering PLS. The treatment effect on new account openings steadily increases over the two months that we offer lotteries; in the second month of lotteries, offering PLS causes a 68% increase in the number of accounts opened. After the final lottery, the daily treatment effect abruptly falls to zero, which also allows us to rule out that PLS account openers would have opened an account anyway a few months in the future, and simply change the timing of account opening in response to the lottery incentive.[2]

We compare savings and transactions in accounts opened during the PLS experiment in treatment and control branches, focusing on accounts opened during the second lottery month when the extensive margin treatment effect is larger.[3] Although new accounts in treatment branches may start with a lower savings balance than new accounts in control branches during the first two months they are open, their savings balances catch up after three months. Importantly, survival of accounts opened during lottery months at treatment branches is the same as that of accounts opened during the same months at control branches. After 5 years, 35% of accounts are still being used (defined as having a minimum balance above 50 pesos) in both groups. Based on various deposit-based measures of active use (Schaner, 2017; Dupas and Robinson, 2013; Dupas et al., forthcoming), we also cannot reject that behavior by treatment and control account openers are equal.

A central question about any savings product that leads to an increase in formal savings is whether it crowds out other forms of saving. While we do not have survey data to answer this question in our context, both Cole et al. (2016) and Dizon and Lybbert (2017) find that savings in PLS accounts do not substitute other savings, but rather that they crowd out gambling expenditures.

[2]If this were the case, we would expect a negative treatment effect in months after the lotteries end.

[3]Since treatment led to a 68% increase in the number of accounts opened in the second month of lotteries, under the standard assumption that openings at treatment branches would have been the same as those in control in the absence of treatment, we know that .68/1.68 = 40% of account openers in treatment branches are "compliers" induced to open accounts by the lotteries, while the other 60% are "always takers" who would have opened accounts anyway. For an upper bound on the treatment effect on compliers, we can assume that always takers would have used the account the same way as those who opened accounts at control branches during lottery months, and hence multiply treatment effects by 1/.4 = 2.5 to obtain the upper bound.

This is consistent with Herskowitz's (2016) finding that gambling and savings technologies are both used to save for durables in Uganda, and that there is substitution from gambling to saving when a secure savings device is provided. It is also consistent with findings from the broader financial inclusion literature, which find that new savings in formal financial institutions are often explained by increases in overall savings rather than a substitution from other forms of saving (Dupas and Robinson, 2013; Ashraf et al., 2015; Bachas et al., 2018; Kast et al., forthcoming).

Policy interest in the impact of PLS accounts on savings has increased recently, in particular after they became legal in the US following the passage of the American Savings Promotion Act in December 2014. In both developed and developing countries, PLS products have been popular (Cole et al., 2007; Kearney et al., 2011). We provide evidence from a large-scale RCT that people are induced by lotteries to open and save in formal bank accounts, and that temporary lotteries can have a lasting impact on their savings behavior.

2 Experimental Design and Data

2.1 Partner Bank

Mexico's financial market is dominated by five large banks which have 90% market share (Ponce et al., 2017), and these banks have little interest in serving the poor. 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 (Demirgüç-Kunt et al., 2015). While microfinance institutes like Compartamos Banco have rapidly expanded access to credit (Angelucci et al., 2015), they have not aggressively pursued microsavings.

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 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 does not carry loan products, but 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 carried these accounts from 2005–2007 but, due to a change of management and a lack of evidence to evaluate the effectiveness of PLS, discontinued them in 2008. In 2010, we partnered with Bansefi and the Inter-American Development Bank (IADB) to test if these lotteries actually attract new clients and generate more savings.

2.2 Experimental Design

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.[4]

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 2 shows the locations of treatment and control branches.

Lotteries. 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 1 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." 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, in the October lottery the probability of a particular ticket winning a small prize of 400 pesos (US\$32) was 1 in 713, while the probability of a particular ticket winning the large prize

[4]Two notable exceptions are the recent multi-country savings experiments in Dupas et al. (forthcoming) and Karlan et al. (2017).

of 10,000 pesos (US$809) was 1 in 350,000. The median saver earned 27 tickets.

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 *debicuenta* accounts (see Bachas et al., 2018, 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.

2.3 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.

3 Results

We now show our estimates for the effect of the lotteries on new account openings, as well as transactions and savings in both newly opened and existing accounts.

3.1 New Accounts

3.1.1 Account Openings

Did the lotteries induce people to open savings accounts at treatment branches? To answer this question, we use a simple experimental comparison of treated vs. control branches and estimate an average treatment effect. Because there is variation in branch size across branches, to increase power we use an analysis of covariance (ANCOVA) specification where we control for baseline account openings (McKenzie, 2012). We restrict the analysis to the two-month period during which lotteries were offered (September 13, 2010 to November 12, 2010) and estimate

$$y_j = \alpha + \gamma T_j + \theta y_{j0} + \varepsilon_j, \tag{1}$$

where y_j is the total number of accounts opened at branch j, T_j is a dummy variable indicating that branch j was randomly assigned to treatment, and y_{j0} is baseline account openings.[5] In addition, we conduct placebo tests using the same specification for other two-month periods, from eight months prior to the lotteries through eight months after the lotteries.

We plot these results in Figure 3. During the lottery months, there is an increase of 2.99 account openings in treatment branches ($p = 0.06$), compared to a control mean of 7.39 account openings over the two-month period, or a 40.5% increase. 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

[5]We 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 e:extensive.

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 4.2.

In pre-lottery periods, we find point estimates close to 0 in magnitude and statistically insignificant from 0 when we estimate e:extensive. In post-lottery periods, we find results that are statistically insignificant from 0 in three out of the four periods, and in one period a positive but fairly small effect—about half of the treatment effect during lottery periods—that is statistically significant at the 10% level.

Next, we explore how account openings evolve over time during the two-month lottery period. We plot the treatment effect *by day* in Figure 4, once again shading the period during which lotteries are offered in gray. 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 opeend 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.[6]

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 et al.,

[6] More 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 et al. (2014).

2016). Indeed, if we estimate e:extensive using one-month periods (again ending at the 12th of each month since these are the lottery dates), we find that the treatment effect is statistically insignificant from 0 in the first month that the lotteries were offered, but is significant and large (relative to accounts opened at control branches) in the second month. Figure 5 shows the results: in the second month from October 13 to November 12, 2010, an additional 2.13 accounts per branch per month are opened in treatment branches ($p < 0.01$); this represents a 68% increase compared to control branches. In all 8 pre-lottery months, there is no statistically significant difference between the number of accounts opened in treatment vs. control branches, and there is a significant difference at the 10% level ($p = 0.08$) in just one post-lottery month.

Are new account openers previously unbanked individuals? While we cannot directly test this, there is evidence for conjecturing that this may be the case for most new account openers at Bansefi. First, Bansefi is a government social bank that strives to increase financial inclusion and hence deliberately places its branches in underserved areas where there is a high concentration of unbanked individuals. Second, according to the 2009 wave of the Mexican Family Life Survey, only 12% of households had a bank account in a formal financial institution. Furthermore, only 13% of the households with at least one bank account had more than one account. Third, if the new Bansefi account openings were among households who already had at least one bank account, we would expect to see the account openings concentrated in areas with a higher concentration of other banks. We 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

$$y_j = \alpha + \theta y_{j0} + \gamma T_j + \xi Density\ of\ banks_j + \phi T_j \times Density\ of\ banks_j + \varepsilon_j \qquad (2)$$

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 ($t =$ Oct. 2010 and $t =$ Nov. 2010). For both measures of density and both months, we cannot reject that $\phi = 0$.[7]

3.1.2 Account Survival

A concern is that new account holders induced to open accounts by the lottery did so only for the purpose of participating in the lottery, and would close the account or leave it unused after the lotteries end. If this were the case, temporary lotteries would likely not be a successful financial inclusion tool. In the next three subsections we show that the accounts opened at treatment branches during lottery months are comparable to those opened during the same months at control branches in account survival, the levels of savings, and active use of the account. Based on the result from Section 3.1.1 that the lotteries only had a statistically significant effect on account openings in the second lottery month, we focus on accounts opened between the first and second lottery dates at treatment and control branches.

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.

[7] In 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.

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 6 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.[8] 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.

3.1.3 Savings

We now compare savings stocks (measured by end-of-period balance, where months are measured relative to lottery dates) of those who open accounts in treatment branches in the second lottery month to those who open accounts in the same month in control branches. Because clients in treatment branches may have won prizes from the lottery—which could in turn affect the amount they save in the account—we control for the amount won. We estimate the following equation separately for each month t:

$$Balance_{ij} = \alpha + \gamma T_j + \psi Winnings_{ij} + \varepsilon_{ij} \tag{3}$$

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

where $Balance_{ij}$ is end of month balance of account i in branch j (in month t), T_j is a dummy that equals 1 for treatment branches, and and we control for the amount won $Winnings_{ij}$ (which equals 0 in pre-lottery months and the first lottery month, the amount won in the first lottery for the following month's regression, and the sum of winnings in the two lotteries for all post-lottery months).[9] There is no control for baseline account balance since these accounts did not exist prior to the lottery months. Standard errors are clustered at the branch level. The γ coefficients for each month t are shown in Figure 7.

We see that there is no statistically significant difference between the savings levels of accounts opened in treatment and control (except in three months toward the end of the five-year period). In the first couple of months, point estimates are large but noisy, indicating that accounts induced to open accounts by the lottery might initialy save less. By three months after the lotteries, however, point estimates are close to 0 and largely remain around 0 for the remainder of the five-year period.

3.1.4 Transactions

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 (Dupas et al., forthcoming). Figure 8 shows the proportion of accounts with a balance of at least 50 pesos that are actively used by these criteria.

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.[10] 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

[9]Following other papers measuring savings (e.g., de Mel et al., 2013; Dupas et al., 2016; Kast et al., forthcoming; Karlan and Zinman, forthcoming), we winsorize savings balances to avoid results driven by outliers. Our main results winsorize at the 95th percentile, and the results are robust to other cut-offs.

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

active users after five years. Finally, using Dupas et al.'s (forthcoming) 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 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.

3.2 Existing accounts

We use existing accounts at treatment and control branches to measure the effect of the lottery on the intensive margin. We measure savings using end-of-period balance, and again use an ANCOVA specification that controls for baseline savings balance of each account in the past by averaging over the baseline months (from January 2008 to February 2010). We restrict the analysis to accounts that had been opened prior to October 2009 so that baseline savings is defined, and we control for the amount won. Separately for each month, we estimate

$$Balance_{ij} = \alpha + \gamma T_j + \psi Winnings_{ij} + \beta Balance_{ij0} + \varepsilon_{ij}, \tag{4}$$

where $Balance_{ij}$ 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.

4 Alternative Explanations

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

4.1 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 4. 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 4) provides strong evidence against this alternative explanation.

4.2 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

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

separately for each month t, where *Distance to control*$_j$ is either a dummy indicating whether the geodesic distance between Bansefi 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.[11] 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 = Oct. 2010 and t = Nov. 2010). We do not find evidence of substitution across branches.

5 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, thus validating in a randomized control trial a result conjectured by Tufano et al. (2011) based on survey data and confirmed in the lab by Atalay et al. (2014). 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 (consistent with the long-run impact of a large but temporary incentive to save in Schaner (forthcoming)).

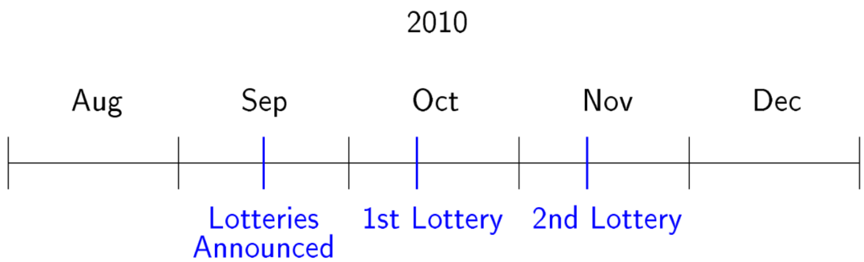
Taken together, our results suggest that prize-linked savings accounts can encourage the unbanked to open bank accounts. Nevertheless, a minority of new account openers remain active account users (both in the treatment and control groups) in the long term, suggesting that if savings accounts are an experience good, the benefit of saving in a formal account is higher than antici-

[11] We measure *Distance 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.

pated only for some new account-holders. Alternatively, Bansefi might be a gateway to financial inclusion if, after opening and using a Bansefi account, a fraction of clients open accounts in closer commercial bank branches.

Figure 1: Details on Experiment

(a) Timeline of Experiment



(b) Example Advertisement

Figure 2: Treatment and Control Branches

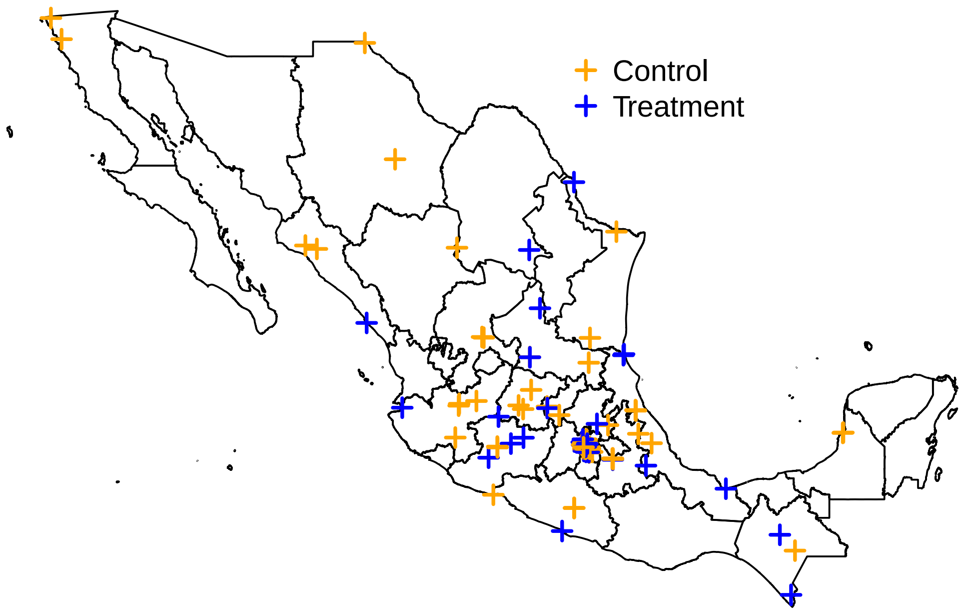
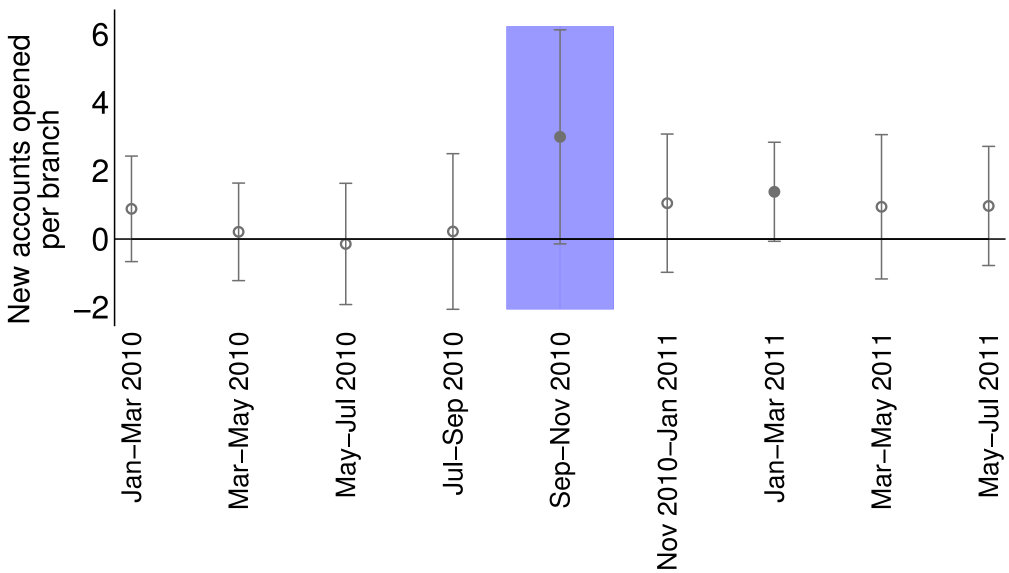
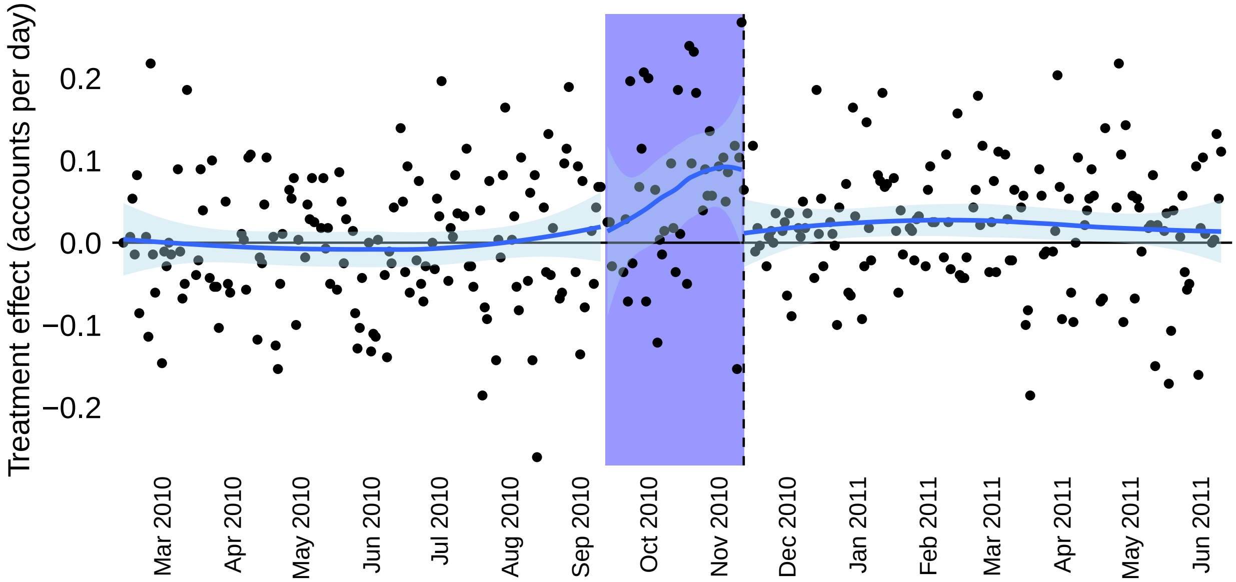


Figure 3: Impact of Treatment on Number of Accounts Opened



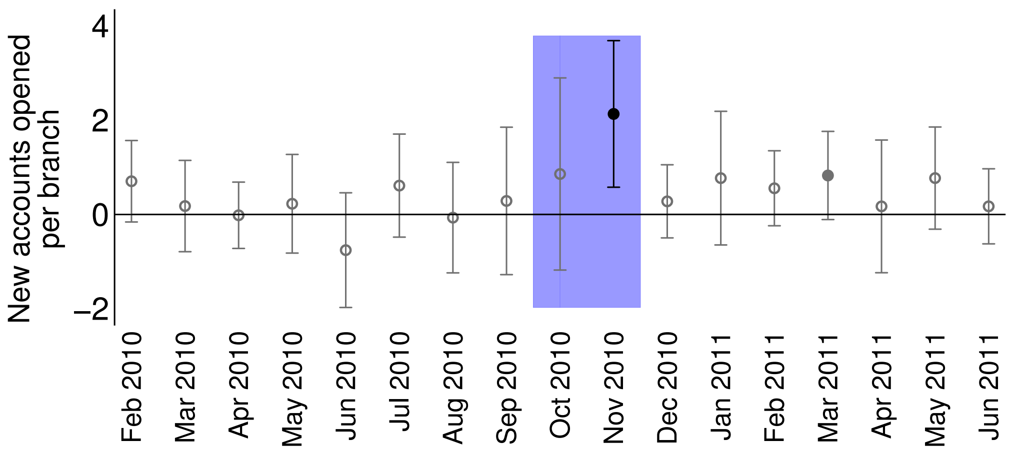
Notes: This figure shows the treatment effect of offering prize-linked savings accounts on the total number of account openings per branch over two-month periods. The two-month period during which lotteries were offered is shaded in gray. Because the lotteries occurred on October 12 and November 12, 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 range corresponding to the lottery months, marked “Sep–Nov 2010,” refers to September 13, 2010 to November 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 4: Impact of Treatment on Number of Accounts Opened per Day



Notes: This figure shows the daily treatment effect of offering prize-linked savings 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 in gray. The final lottery on November 12, 2010 is represented by a dashed vertical line.

Figure 5: Impact of Treatment on Number of Accounts Opened



Notes: This figure shows the treatment effect of offering prize-linked savings 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 in gray. 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 6: Proportion of Accounts Opened during Second Lottery Month Remaining Used in Treatment vs. Control Branches

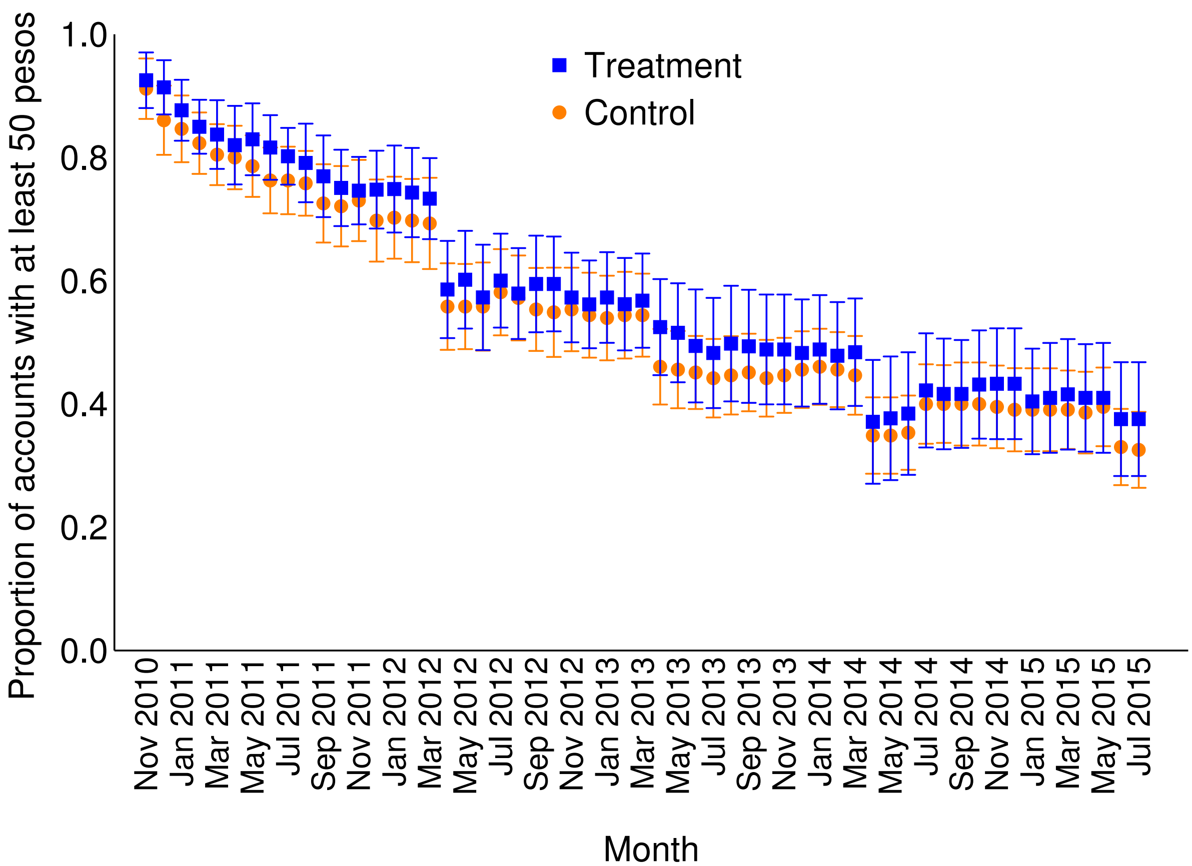


Figure 7: Savings in Accounts Opened during Second Lottery Month in Treatment vs. Control Branches

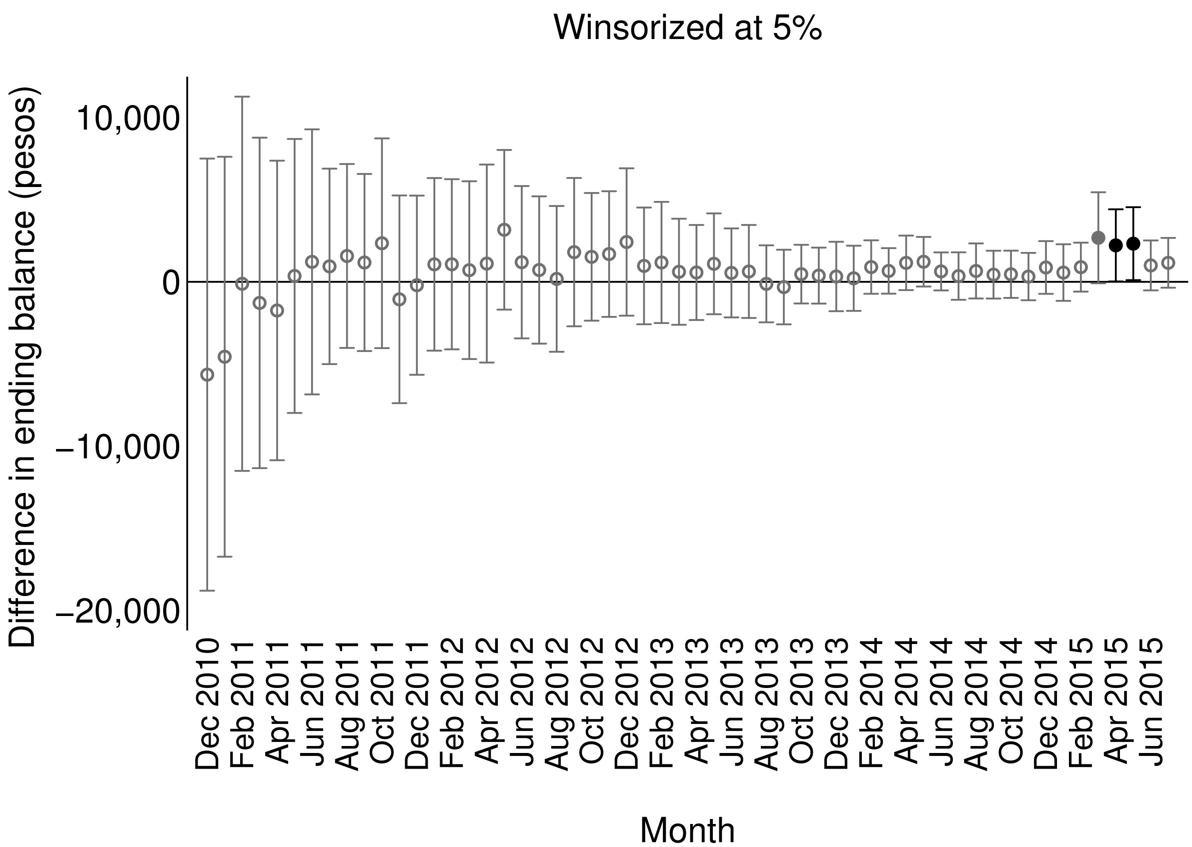


Figure 8: Active Users, Accounts Opened during Second Lottery Month

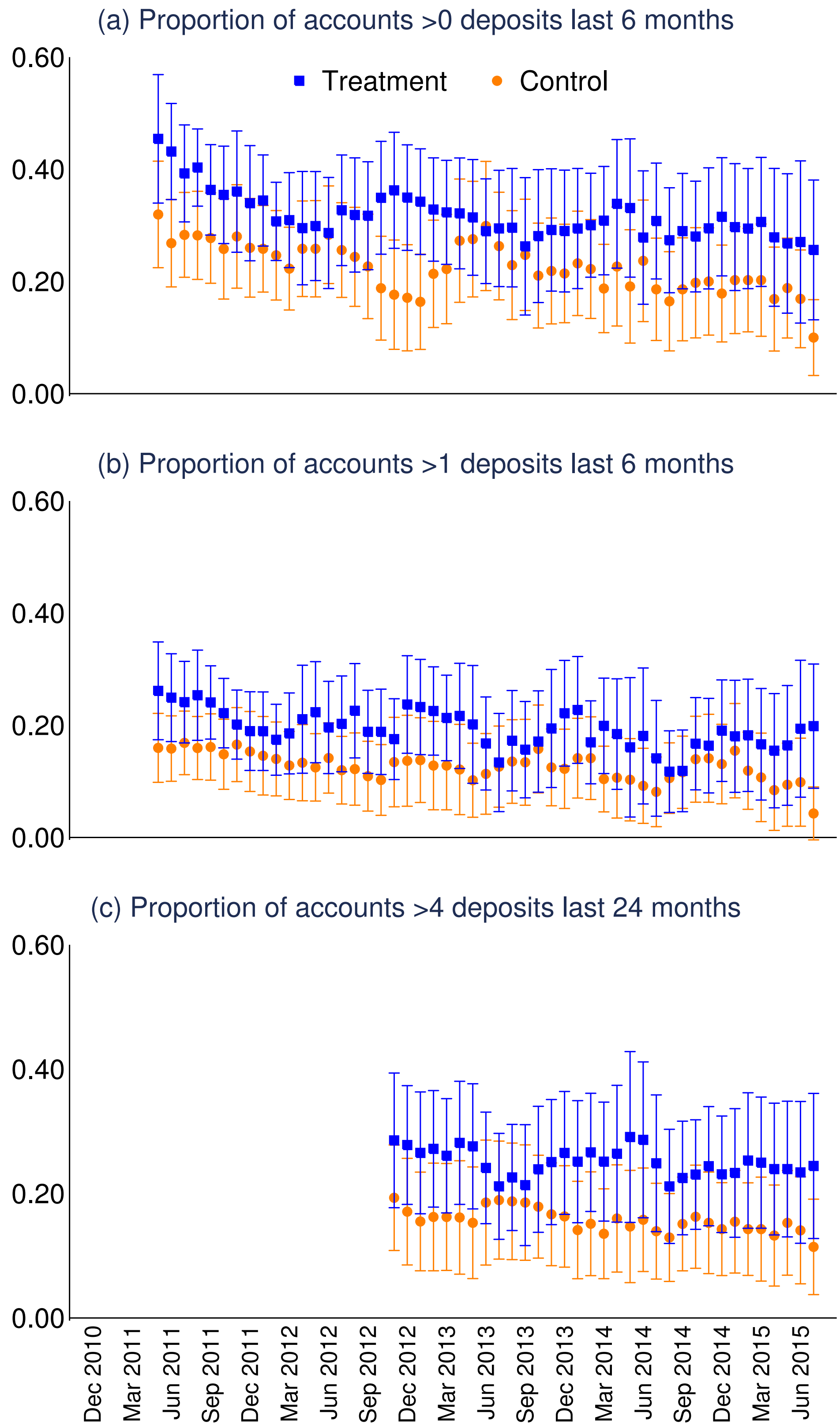


Figure 9: Distribution of Transactions, Accounts Opened October 2010

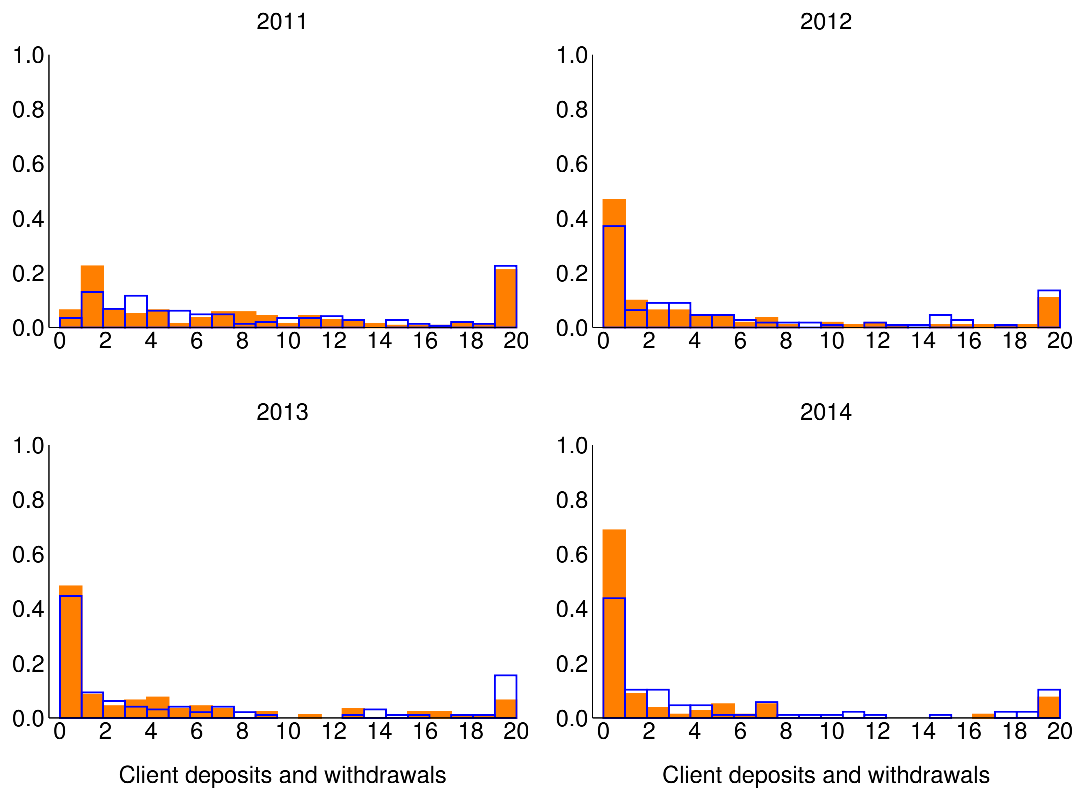


Table 1: Summary Statistics and Balance

Variable	Control	Treatment	Difference
Panel A: Locality-level Data (2005)			
Log population	12.75	12.91	0.16
	(0.13)	(0.14)	(0.19)
Bansefi branches per 100,000	0.93	0.79	-0.14
	(0.08)	(0.09)	(0.12)
% illiterate	3.84	3.76	-0.07
	(0.23)	(0.42)	(0.48)
% attending school	3.38	3.43	0.05
	(0.14)	(0.24)	(0.28)
% with dirt floors	3.11	2.87	-0.24
	(0.41)	(0.62)	(0.74)
% without piped water	3.11	3.03	-0.08
	(0.57)	(0.66)	(0.87)
% without electricity	4.87	5.25	0.38
	(0.23)	(0.30)	(0.38)
Average occupants per room	1.00	0.97	-0.03
	(0.02)	(0.03)	(0.03)
Panel B: Bank Administrative Data *Averages over baseline (2008 and 2009)*			
Ending Balance	1112.40	1160.94	48.54
	(86.32)	(108.80)	(138.21)
Total Number of Accounts per Branch	57.91	49.15	-8.76
	(4.99)	(5.27)	(7.25)
Total Value Deposited at Branch	64423.81	57060.32	-7363.49
	(5988.20)	(5542.10)	(8146.18)
Accounts Opened (per month)	4.50	3.89	-0.61
	(0.45)	(0.38)	(0.59)
Deposits (per account per month)	0.26	0.29	0.03
	(0.02)	(0.02)	(0.03)
Withdrawals (per account per month)	1.05	1.18	0.14
	(0.05)	(0.07)	(0.09)
Amount Withdrawn (per account per month)	664.26	785.88	121.62
	(46.50)	(77.87)	(90.16)
Amount Deposited (per account per month)	522.14	646.64	124.50
	(39.43)	(67.83)	(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**	0.315**	0.503**	0.315**
	(0.201)	(0.125)	(0.208)	(0.130)
Treatment branch	1.610*	0.419**	2.382	0.144**
	(0.987)	(0.644)	(1.072)	(0.772)
Density of commercial banks	–0.124	–0.377	0.002	–0.014*
	(0.622)	(0.580)	(0.026)	(0.039)
Treatment × density	0.173	1.254	–0.054	0.071
	(1.547)	(0.986)	(0.070)	(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***	3.878***	4.278***	3.710***
	(1.318)	(1.038)	(1.284)	(1.027)
Treatment branch	–0.270	1.566	0.482	2.046**
	(1.316)	(0.958)	(1.165)	(0.780)
Distance to control branch	0.515	0.637	–0.002	0.000
	(1.109)	(0.753)	(0.004)	(0.003)
Treatment × distance to control	1.659	0.625	0.004	–0.001
	(1.996)	(1.552)	(0.011)	(0.010)

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