# Banking on Trust: How Debit Cards Enable the Poor to Save More*

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**Abstract**

Trust is an essential element of economic transactions, but trust in financial institutions is especially low among the poor. Debit cards provide not only easier access to savings, but also a mechanism to monitor bank account balances and thereby build trust. We study a natural experiment in which debit cards are rolled out to beneficiaries of a Mexican conditional cash transfer program whose benefits are directly deposited into a savings account. Using administrative data on over 340,000 bank accounts over four years, we find that prior to receiving a debit card, beneficiaries do not save in these accounts. After receiving a debit card, beneficiaries do not increase their savings for the first 9–12 months, but after this their savings increase over time. During this initial period, however, they use the card to check their balances frequently; the number of checks decreases over time as their reported trust in the bank increases. Using household survey panel data, we find the observed effect represents an increase in overall savings. After 1–2 years, the debit card causes the savings rate to increase by 3–5 percent of income.

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*Virtually every commercial transaction has within itself an element of trust. . . . It can be plausibly argued that much of the economic backwardness in the world can be explained by the lack of mutual confidence.*

—Kenneth Arrow (1972)

## 1 Introduction

Trust is an essential element of economic transactions and an important driver of economic development (Knack and Keefer, 1997; La Porta et al., 1997; Algan and Cahuc, 2010). It is particularly crucial in financial transactions where people pay money in exchange for promises, and essential where the legal institutions that enforce contracts are weak (McMillan and Woodruff, 1999; Karlan et al., 2009). Given the nature of financial decisions, it is not surprising that trust has been shown to be key to stock market participation (Guiso et al., 2008), use of checks instead of cash (Guiso et al., 2004), and decisions to not withdraw deposits from financial institutions in times of financial crisis (Iyer and Puri, 2012; Sapienza and Zingales, 2012).

Trust in financial institutions, meanwhile, is low. Majorities in close to half of the countries included in the World Values Survey report lack of confidence in banks. Trust is especially low among the poor: in Mexico, the location of our study, 71% of those with less than a primary school education report low trust in banks, compared to 55% of those who completed primary school and 46% of those who completed university (Figure 1).

Lack of trust in financial institutions may not be unfounded. Cohn et al. (2014) provide evidence that the banking industry fosters a culture of dishonesty relative to other industries. In Mexico, bankers have been found to loot money by directing lending to "related parties," i.e. bank shareholders and their firms (La Porta et al., 2003). Mexican newspapers report many instances of outright bank fraud where depositors have lost their savings. For example, an extensively covered scandal involved Ficrea, whose majority shareholder reportedly stole US$ 200 million from savers (CNBV, 2014). Bank fraud is frequently reported in the press, with at least 275 news stories about 32 unique events of savings fraud published in 2014 and 2015 alone.[1] Tellingly, articles that provide financial advice in Mexican newspapers have titles like "How to Save for Your Graduation and Avoid Fraud" and "Retirement Savings Accounts, with Minimal Risk of Fraud."

When fraud is rampant and contract enforcement poor, trust plays an even larger role (Guiso et al., 2004; Karlan et al., 2009) and people are understandably reluctant to use financial institutions (Bohnet et al., 2010). At the country level, trust is strongly associated with the proportion of the population that do save in formal bank accounts (Figure 2). Along with fees and minimum balance requirements, trust is frequently listed by the poor as a primary reason for not saving in formal bank accounts (e.g., Dupas et al., 2016a). Lack of trust could also explain why randomized field experiments in three countries have found that even among people who take up accessible and free

[1] We scraped the online news archives of all electronic newspapers and news websites in Mexico using several keywords, and then filtered the results by hand to keep only relevant stories. The scraping resulted in 1392 stories in 121 newspapers from 2014-2015 that matched our keywords, of which 275 stories from 35 newspapers directly reported on bank fraud.

formal savings products, account use is low (Dupas et al., 2016b).[2] Despite its importance, finding ways to improve trust in financial institutions has not been extensively studied (Karlan et al., 2014).

While trust is important, it is not an innate characteristic but rather can be influenced through experience and information (Hirschman, 1984; Williamson, 1993; Attanasio et al., 2009). Debit cards (and mobile money) provide a low-cost technology to monitor account balances and thereby build trust that a bank is not explicitly stealing deposits or charging unexpectedly large hidden fees.[3] We hypothesize that new debit card clients first use the cards to check balances and thereby establish trust, after which they take advantage of the cards' lower transaction costs to use the services of formal financial institutions. In this sense, we argue that building trust in a financial institution is a necessary condition for the use of formal financial services; i.e., financial inclusion requires trust.

We examine this hypothesis in the context of a natural experiment in which debit cards tied to savings accounts were rolled out geographically over time to beneficiaries of the Mexican conditional cash transfer program Oportunidades. The phased geographic rollout provides plausibly exogenous variation in assignment of debit cards to beneficiaries in a difference-in-differences context. Before the rollout, beneficiaries had been receiving their transfers through savings accounts without debit cards, and very rarely used their accounts to save. Instead, they typically withdrew almost the full amount of the transfer shortly after receiving it. This is consistent with findings from other countries such as Brazil, Colombia, India, Niger, and South Africa, in which cash transfers are also paid through bank or mobile money accounts and recipients generally withdraw the entire transfer amount in one lump sum withdrawal each pay period (Bold et al., 2012; Aker et al., 2016; Muralidharan et al., 2016).

This paper makes four contributions. First, we show that debit cards cause a large and significant increase in savings in formal financial institutions: after a delay, beneficiaries with debit cards save 3–5% more of their income each period. Second, we find that this increase in savings is driven in large part by clients using the debit card to first monitor account balances and thereby build trust that their money is safe. Once trust is established, they take advantage of the reduced transaction costs associated with debit cards and increase the amount of money held in their bank accounts.[4] Third, we find that the observed higher savings in the bank constitute an increase in total savings and not just a substitution from other savings vehicles. Finally, our study uses a much larger sample than most of the literature, with broad geographic coverage across the country. The size of the effect we observe is substantially larger than that of other savings interventions studied in the literature, including offering commitment devices, no-fee accounts, higher interest rates, lower

[2] Trust is hypothesized as one channel through which no-fee accounts led to increased saving in Prina (2015).

[3] Previous studies on debit cards and mobile money have focused on the effect of the lower transaction costs facilitated by these technologies to make purchases (Zinman, 2009), access savings and remittances (Suri et al., 2012; Schaner, forthcoming), and transfer money (Jack et al., 2013; Jack and Suri, 2014), but not their capacity to monitor and build trust in financial institutions.

[4] In our context, debit cards reduce the *indirect* transaction costs of accessing money in the bank account, as savings can be withdrawn at any bank's ATM rather than only at government bank branches, which are often far from beneficiaries. In contrast, Schaner (forthcoming) provides ATM cards that reduce *direct* transaction costs: higher withdrawal fees are charged by bank tellers in her study, and the only ATMs at which the cards can be used are located at bank branches of the corresponding bank.

transaction costs, and financial education. The one exception is Suri and Jack (2016), who study the impact of mobile money—another technology that enables clients to more easily check account balances and build trust—and find that female-headed households increase their savings rate by 3% of income on average, after six years of mobile money exposure.

For the analysis, we use high frequency administrative data on bank transactions for over 340,000 beneficiary accounts in 380 bank branches nationwide over 4 years, as well as several surveys of beneficiaries. We compare three groups of beneficiaries based on the rollout of debit cards: beneficiaries in two treatment waves receive debit cards one year apart, while those in the control group receive a debit card at the end of our study period. Using the administrative data, we find that beneficiaries initially use debit cards to check account balances without increasing their savings. Over time, the frequency of account balance checks falls, and after a delay of 9 to 12 months in wave 1, savings rates rise. We observe a faster effect in wave 2, and find evidence that this faster effect is due to information spillovers, as wave 2 beneficiaries living farther from wave 1 localities still have a delay before they begin saving. We estimate that after 1–2 years with the card, the share of total income saved each payment period increases by 3–5 percentage points. These savings rates closely align with the savings goals individuals set in Breza and Chandrasekhar (2015).

The delayed initiation of saving suggests some kind of learning. We use beneficiary survey data to explore three kinds of learning: (i) learning to trust the bank, (ii) learning to use the ATMs, their location, and associated transaction costs, and (iii) learning that the program will not drop beneficiaries who accumulate savings. We find support for the "learning to trust" hypothesis. Specifically, beneficiaries who have had their debit cards for less time report significantly higher rates of not trusting the bank than beneficiaries who have had their debit cards longer. On the other hand, we find no support for the other forms of learning.

To establish a direct link between trust and increased savings, we merge the administrative data on account balances and transactions with the beneficiary survey reporting trust in the bank. Since trust is both endogenous to the savings decision and susceptible to measurement error, we instrument trust with a set of dummies for timing of debit card receipt. We find that beneficiaries who are induced to trust the bank as a result of having the card longer save an additional 3% of their income. To our knowledge, this provides the first direct causal estimate in the literature of the effect of trust in financial institutions on formal savings.

We then test whether the increase in the bank account balances is an increase in total savings or a substitution from other forms of saving, both formal and informal. Using household survey panel data, we find that after about one year the treatment group increases total savings by 5% of income relative to the control group, which is very close in magnitude to the effect we see in the administrative account data. We find no differential change in income or assets in the treatment group compared to the control, but rather that the increase in savings is financed though reduced current consumption. Hence, the increase in formal bank account savings does not appear to crowd out other forms of saving (consistent with results in Dupas and Robinson, 2013a; Ashraf et al., 2015; Kast et al., 2016).

Given our results, government cash transfer programs could be a promising channel to increase

financial inclusion and enable the poor to save, not only because of the sheer number of the poor that are served by cash transfers, but also because many governments and nongovernmental organizations are already embarking on digitizing their cash transfer payments through bank accounts, debit cards, and mobile money (Aker et al., 2016; Haushofer and Shapiro, 2016; Muralidharan et al., 2016). Furthermore, the technologies of debit cards combined with ATMs or point-of-sale terminals and mobile money are low-cost mechanisms that can be used to check balances and build trust in financial institutions. These technologies are simple, prevalent, and potentially scalable to millions of government cash transfer recipients worldwide.

## 2 Institutional Context

We examine the the rollout of debit cards to urban beneficiaries of Mexico's conditional cash transfer program Oportunidades whose cash benefits were already being deposited directly into formal savings accounts without debit cards. Oportunidades is one of the largest and most well-known conditional cash transfer programs worldwide with a history of rigorous impact evaluation (Parker and Todd, forthcoming). The program provides bimonthly cash transfers to poor families conditional on sending their children to school and having preventive health check-ups. The program seeks to alleviate poverty in the short term and break the intergenerational transmission of poverty by encouraging families to invest in the human capital of the next generation. It began in rural Mexico in 1997 under the name Progresa, and later expanded to urban areas starting in 2002. Today, nearly one-fourth of Mexican households receive benefits from Oportunidades (Levy and Schady, 2013).[5]

As it expanded to urban areas in 2002–2005, Oportunidades opened savings accounts in banks for beneficiaries in a portion of urban localities, and began depositing the transfers directly into those accounts. The original motives for paying through bank accounts were to (i) decrease corruption as automatic payments through banks lower the ability both of local officials to skim off benefits[6] and of local politicians to associate themselves with the program through face-to-face contact with recipients when they receive their transfers, (ii) decrease long wait times for recipients who previously had to show up to a "payment table" on a particular day to receive their benefits, and (iii) decrease robberies and assaults of program officers and recipients transporting cash on known days.

By the beginning of 2005, over one million families received their benefits directly deposited into savings accounts in Bansefi, a government bank created to increase savings and financial inclusion of underserved populations (Figure 3).[7] The Bansefi savings accounts have no minimum balance requirement or monthly fees and pay essentially no interest.[8] Before the introduction of debit cards, beneficiaries could only access their money at Bansefi bank branches. Because there are only about

[5]Oportunidades was recently rebranded as Prospera. We use the name that was in place during our study period.

[6]Consistent with this concern, Muralidharan et al. (2016) find that paying government cash transfers through biometric "smartcards" in India led to a 40% reduction in program leakages to corrupt officials.

[7]Originally Oportunidades partnered with two banks: Bansefi, a government bank, and Bancomer, a commercial bank. However, working with a commercial bank proved to be difficult, and Oportunidades phased out the Bancomer accounts and transferred them to Bansefi by mid-2006.

[8]Nominal interest rates were between 0.09 and 0.16 percent per year compared to an inflation rate of around 5 percent per year during our sample period.

500 Bansefi branches nationwide and many beneficiaries live far from their nearest branch, accessing their accounts involved large transaction costs for many beneficiaries. Overall, the savings accounts were barely used prior to the introduction of debit cards. In 2008, the year before the rollout of debit cards, the average number of deposits per bimester[9] was 11.05 including the deposit from Oportunidades, the average number of withdrawals was 11.02, and 198.9 percent of the transfer was taken during the first withdrawal following payment.

In 2009, the government announced that they would issue Visa debit cards to beneficiaries who were receiving their benefits directly deposited into Bansefi savings accounts. The cards enable account holders to withdraw cash and check account balances at any bank's ATM, as well as make electronic payments at any store accepting Visa. Beneficiaries can make two free ATM withdrawals per bimester at any bank's ATM; additional ATM withdrawals are charged a fee that varies by bank. When Bansefi distributed the debit cards, they also provided beneficiaries with a training session on how and where to use the cards.[10] The training session did not vary over time and did not include a discussion of the importance of saving.

In 275 out of Mexico's 550 urban localities, beneficiaries received their benefits in bank accounts prior to the rollout of debit cards. Debit cards tied to these bank accounts were rolled out to approximately 75,000 beneficiaries in 143 localities in 2009 (wave 1) and to an additional 170,000 beneficiaries in 88 localities in late 2010 (wave 2). Another 100,000 beneficiaries in the remaining localities received cards between November 2011 and February 2012 (control group), immediately after the end date of our study period. The map in Figure B1 shows that the treatment and control waves had substantial geographical breadth.

The sequence with which beneficiaries in localities switched to debit cards was determined as a function of the proportion of households in the locality that were eligible for the program but were not yet receiving benefits. This is because the introduction of debit cards to existing recipients was coupled with an effort to incorporate more beneficiaries. Table 1 compares the means of locality-level variables and account-level variables from the control, wave 1, and wave 2 localities using data from (i) the population census from 2005, (ii) poverty estimates from Oportunidades based on the same census, (iii) Bansefi branch locations from 2008, and (iv) the administrative account data on average balances and transactions from Bansefi in 2008. Column 6 shows the p-value of an F-test of equality of means. Because the rollout was not random, it is not surprising that there are some differences across treatment and control localities: treatment localities are slightly larger and beneficiaries in these localities receive higher transfer amounts. The share of the transfer withdrawn is high, ranging from 97.5 percent to 99.6 percent of the transfer, indicating very low savings in the account prior to receiving the card.

[9] The program is paid in two-month intervals, which we refer to throughout the paper as bimesters. The Spanish word *bimestre* is more common than its English cognate, and is used by Bansefi and Oportunidades.

[10] See Appendix A for a sample of the materials that beneficiaries received together with their cards.

## 3 Data Sources

We use a rich combination of administrative and household survey data sources. To examine the effect of debit cards on savings and account use, we analyze account-level transaction data from Bansefi for 1343,204 accounts at 1380 Bansefi branches over a four-year period, from November 2007 to October 2011. These data include the monthly average savings balance, the date and amount of each transaction made in the account (including Oportunidades transfers), the date the account was opened, and the month the card was awarded to the account holder. Note that the dates that the account was opened and the debit card obtained are determined exogenously by Oportunidades, not endogenously by the beneficiary; the average account had been opened 5.3 years before receiving the card. Figure 3a shows the timing of the administrative Bansefi account balance and transaction data relative to the rollout of debit cards.

To test whether the delayed savings effect can be explained by learning to trust the bank, learning to use the technology, learning the program rules, or other types of learning, we use a combination of the administrative transaction data from Bansefi and two household surveys conducted by Oportunidades. Specifically, we first use the administrative transaction data to examine balance check behavior and test for a within-account correlation between balance checks and saving. We then use the Payment Method Survey, a household survey conducted by Oportunidades in 2012 aimed at eliciting information about beneficiaries' satisfaction with and use of the debit cards, to examine mechanisms behind and outcomes of the different types of learning that could be taking place. This survey asks beneficiaries to report the number of balance checks they make each period, whether they find it hard to use the ATM, get help using the ATM, and know their PIN, and the fees they are charged for balance checks and withdrawals. Finally, we use the Survey of Urban Households' Sociodemographic Characteristics (ENCASDU), conducted by Oportunidades in late 2010, to investigate beneficiaries' self-reported reasons for not saving in their Bansefi accounts. We also merge the ENCASDU with administrative Bansefi data at the beneficiary/account level to study the direct relationship between self-reported trust in the bank and actual savings levels.

Finally, to explore whether the increased savings in the Bansefi accounts is an increase in overall savings or a substitution from other forms of saving, we use the Survey of Urban Household Characteristics (ENCELURB), a panel survey with three pre-treatment waves in 2002, 2003, and 2004, and one post-treatment wave conducted from November 2009 to 2010. This survey has comprehensive modules on consumption, income, and assets. We merge these data with administrative data from Oportunidades on the transfer histories for this sample and on the dates that debit cards were distributed in each locality.[11] Figure 3b shows the timing of the household survey data relative to the rollout of debit cards.

[11] The use of adminstrive transfer data helps us overcome the common misreporting of transfer receipts in households surveys (Meyer et al., 2015)

## 4 Effect of Debit Cards on Savings Behavior and Account Use

In this section, we use the administrative data from Bansefi on average monthly balances and all transactions in 343,204 accounts of Oportunidades beneficiaries to estimate the dynamic effect of debit cards on accumulated savings in these formal bank accounts, on use of the accounts through transactions (deposits and withdrawals), and on the savings rate. We exploit the phased rollout of debit cards to identify the causal effect of debit cards on savings and account use in a difference-in-differences framework.

### 4.1 The Stock of Savings (Account Balances)

The raw data on average account balances display the fundamental patterns of interest. Because Oportunidades payments are made every two months, in Figure 4 we plot the bimonthly average account balances of beneficiaries in the treatment and control groups over time.[12] Panel (a) presents the comparison between wave 1 and the control, and panel (b) between wave 2 and the control. The dashed vertical lines indicate the time at which beneficiaries in treatment localities received debit cards. For both waves, average balances are almost identical in both levels and trends prior to the introduction of debit cards. Strikingly, treatment group savings in wave 1 rise dramatically relative to the control group after an initial period of about 10 months with the debit card. After two years with the card, wave 1 beneficiaries have average balances that are about four times larger than those of the control group. We see a similar pattern in wave 2, but due to the later switch to cards in wave 2, we have fewer periods after treatment to observe the dynamic effect over time. We also observe an effect of the debit cards on savings more quickly in wave 2, which we explore further in Section 4.2.

In order to estimate the causal effect of the debit card on account balances, we estimate a difference-in-differences specification that allows the treatment effect to vary over time. We control for common macro shocks by including time fixed effects, and for time-invariant individual heterogeneity with individual account fixed effects. Specifically, we estimate

$$Balance_{it} = \lambda_i + \delta_t + \sum_k \phi_k T_{j(i)} \times \mathbb{I}(t = k) + \varepsilon_{it} \tag{1}$$

separately for wave 1 and wave 2 (each compared to the control group), where $Balance_{it}$ is the average balance in account $i$ over period $t$, the $\lambda_i$ are account-level (i.e., beneficiary) fixed effects, the $\delta_t$ are time-period fixed effects, $T_{j(i)} = 1$ if locality $j$ in which account holder $i$ lives is a treatment (i.e., wave 1 or wave 2) locality, and $\mathbb{I}(t = k)$ are time period dummies.[13] Thus, the $T_{j(i)} \times \mathbb{I}(t = k)$ terms pick up the difference in balances between treatment and control localities in each period.

[12] These are obtained using the average monthly account balances provided by Oportunidades and averaging within accounts over adjacent months to obtain bimonthly averages.

[13] Following other papers measuring savings (e.g., de Mel et al., 2013; Karlan and Zinman, 2016; Chetty et al., 2014; Dupas et al., 2016a; Kast et al., 2016), we winsorize average balances to avoid results driven by outliers; our main results winsorize at the 95th percentile, and the results are robust to other cut-offs. To avoid truncating a true treatment effect or time trend, we winsorize within each wave and time period.

We average the data within four-month periods because not all beneficiaries receive their payments in each calendar bimester: some payments are shifted to the latter part of the prior bimester in some localities, resulting in some bimesters with double payments and others with no payments.[14] Because we have four years of data, this leaves us with 12 four-month periods. Since one time period dummy and one $T_{j(i)} \times \mathbb{I}(t = k)$ term must be omitted from (1), we follow the standard procedure of omitting the period immediately preceding the change to cards. We estimate cluster-robust standard errors, clustering $\varepsilon_{it}$ by Bansefi branch. The coefficients of interest are the $\phi_k$, which measure the average difference in balances between the treatment and control group in period $k$.

Figure 5 plots the $\phi_k$ coefficients and their 95% confidence intervals. As we saw in Figure 4, it is clear that the pre-treatment levels and trends in account balances are not different between treatment and control groups prior to the debit card rollout. To formally test this, we test the null hypothesis that $\phi_1 = \cdots = \phi_{\ell-1} = 0$, where $\ell$ denotes the period in which debit cards were received; we cannot reject that pre-treatment trends are equal between treatment and control.[15] This is important, as the identifying assumption in our difference-in-differences model is that the beneficiaries who receive the debit card would have had the same trend in average balances as the control group in the absence of treatment. While this assumption is inherently untestable, the fact that the levels and trends of the dependent variable are very similar before treatment makes a strong case for the parallel trends assumption. Having two waves is also helpful for identification as it provides a test of validity for other time periods and populations and suggests that results are not due to a one-time macro shock.

In wave 1, there is no difference in average account balances between the treatment and control groups for about 8 months after receiving debit cards, beyond which balances start to increase significantly. In Section 5 we test three different types of learning that could explain the delayed effect, and find evidence that beneficiaries are using the debit cards to monitor and build trust in the bank; after building trust, they take advantage of the cards' lower transaction costs (due to being able to withdraw at any bank's ATM rather than the nearest Bansefi branch) and use the account to save. After nearly two years with the card, balances are about 11,400 pesos higher for the treatment group than for the control. Furthermore, dynamic savings effects appear to be at play: once beneficiaries begin to save, their balances increase at a decreasing rate. We explore these dynamics further in Section 4.4. In wave 2, savings begin to rise much more quickly after receiving the card; in Section 4.2 we show that this is likely due to information spillovers from wave 1 beneficiaries.

### 4.2 Information Spillovers

In this section, we test whether the faster effect of debit cards on the accumulation of savings in wave 2 compared to wave 1 can be explained by information spillovers: i.e. as beneficiaries in wave 1 learned about the bank's trustworthiness and the value of savings, they may have shared this

[14] This payment shifting happens for various reasons, including for local, state, and federal elections, as a law prohibits Oportunidades from distributing cash transfers during election periods to prevent corruption.

[15] The p-value for the F-test of $\phi_1 = \cdots = \phi_{\ell-1} = 0$ is 1XX for wave 1 and 1YY for wave 2.

information with family members and friends living in nearby wave 2 (and control) localities.[16] Thus, wave 2 beneficiaries may have already had higher levels of trust when they received debit cards than did wave 1 beneficiaries; if trust is a necessary condition for saving but not sufficient when transaction costs are too high, we would then see wave 2 beneficiaries start saving once the debit card lowered their transaction costs. Control beneficiaries would be less likely to be affected as their transaction costs remain high absent a debit card.

To test this, we split wave 2 and control beneficiaries into two groups: those who are closer to and farther from wave 1 localities. We then test whether the effect of debit cards is faster and larger for wave 2 beneficiaries that are close to wave 1 localities 1(relative to close control beneficiaries) than for wave 2 beneficiaries that are far from wave 1 localities (relative to far control beneficiaries). Specifically, we estimate (1) separately for wave 2 beneficiaries and control that are close and for wave 2 beneficiaires and controls who are far.

We measure distance by using the centroid of the block on which each wave 2 or control beneficiary lives to calculate her shortest road distance to the centroid of a wave 1 locality. Because we do not know *ex ante* the distance that information about trustworthiness might travel, we use various road distance cut-offs to separate wave 2 and control beneficiaries that are closer vs. farther from wave 1 localities. Specifically, we use a series of 25-kilometer cut-offs from 25 kilometers to 400 kilometers. We find that, for cut-offs of 150 kilometers or more, the effect of debit cards on the stock of savings is faster and larger for wave 2 beneficiaires who are close to wave 1 localities than for those who are farther. Figure 6 presents the results using a 150 kilometer cut-off.

In all three periods after receiving cards, the effect of the card on savings for close beneficiaries is larger than that for far beneficiaries, and the difference between the effect sizes is statistically significant. This is true for any cut-off of at least 150 kilometers. Furthermore, there is a 4-month delay for far beneficiaries before the card has a savings effect, and no delay for close beneficiaries. Due to the geographic breadth of localities in each wave, the 150 kilometer cut-off in Figure 6 corresponds to about the 79th percentile of the distribution of distances between wave 2 or control beneficiaries and wave 1 localities. As a result, it is not surprising that overall, we see an immediate effect of the debit card on savings in wave 2. Taken together, these results suggest that the faster effect of debit cards in wave 2 can be explained, at least in part, by information spillovers: after wave 1 beneficiaries learn, they share this information with beneficiaries in nearby localities.

### 4.3 Transactions

By lowering indirect transaction costs, debit cards should lead to more account transactions, as predicted by theory (Baumol, 1952; Tobin, 1956) and past empirical evidence (Attanasio et al., 2002; Alvarez and Lippi, 2009; Schaner, forthcoming). This is indeed what we find. Figure 7 presents the distribution of the number of withdrawals per bimester, before and after receiving the

[16] The faster effect is not explained by changes in the information given to beneficiaries during the rollout: we obtained detailed documents on the information given to beneficiaries both when the cards were distributed and during bimonthly chats conducted by Oportunidades, and confirmed that there were no substantial changes in this material over time.

card. Prior to receiving the card, over 90% of beneficiaries made a single withdrawal per bimester. After receiving the card, 65% of beneficiaries continue to make just one withdrawal, but 27% make 2 withdrawals, 6% make 3 withdrawals, and 2% make 4 or more withdrawals.[17] Meanwhile, the distribution of the number of withdrawals in the control group does not change over time.

The effect on withdrawals is *immediate*, as would be expected from the instantaneous change in transaction costs induced by the card. Figure 8 shows the average number of withdrawals per Oportunidades deposit in each bimester.[18] Prior to receiving the card, beneficiaries in both the treatment and control groups average close to 1 withdrawal per bimester. Immediately after receiving the card, this figure jumps to an average of about 1.4 withdrawals the period after receiving the card, then remains relatively constant between 1.3 and 1.4 withdrawals in most periods; in the control group, it remains constant at about 1 withdrawal per bimester.

There is no effect on client deposits: Figure 9 shows that 199% of accounts have zero deposits per bimester before and after receiving the card. Account holders thus do not add savings from other sources of income to their Bansefi accounts. This finding is not surprising, since beneficiaries receive one-fifth of their income from the Oportunidades program on average, so unless the optimal savings rate in a particular period is higher than 20% of income, there is no reason to deposit savings from other income sources in the account.

The fact that debit cards increase the number of withdrawals over a period could lead to a mechanical increase in the average balance within a payment period without increasing savings over time between periods. Suppose, for example, that an individual begins a period with a balance of 0, receives an Oportunidades deposit during the period, and withdraws the full amount on the day the funds are deposited. In this case, the average balance over the period is zero. Compare this to an individual who withdraws half the money the day it is deposited and the other half in the middle of the period. In this case, the average balance would equal one-quarter of the transfer amount (since half of the transfer was left in the account for half of the period). In both cases, however, there is no increase in overall savings, if savings are defined as the balance carried over from one period to the next. The size of this mechanical effect depends on the number, timing, and amounts of the withdrawals.

Using data on the timing and amount of each transfer and withdrawal from the administrative transactions data, we calculate this mechanical effect for each account-period pair (see Appendix C). We then subtract out the mechanical effect from the average balance to obtain a better measure of the stock of savings, which we call "net balance." The results for (1) using net balances—which we denote $Net\ Balance_{it}$—show the same pattern over time; after nearly two years with the

[17] These percentages average over waves 1 and wave 2, combining all account-bimester pairs after the account receives a debit card. Figure 7 shows the effects separately for the two waves, again averaging within each wave over all bimesters that are before or after receiving the card. Recall that the first two withdrawals per bimester are free at any bank's ATM, but subsequent withdrawals are charged a fee, which may explain why so few beneficiaries make more than two withdrawals even after receiving the card.

[18] Because of the shifting of some payments to other bimesters described earlier, we graph the number of withdrawals *per Oportunidades deposit* rather than the absolute number of withdrawals. The latter figure looks similar, but—as expected—with higher numbers of withdrawals in bimesters in which this payment shifting leads to more than one deposit.

card, net balances in wave 1 accounts are approximately 11000 pesos higher than in the control group (Figure B2). The robustness of results to using net balances, as well as the finding that withdrawal patterns change immediately after receiving the card then remain fairly constant over time, confirm that the change in savings over time is not mechanically driven by changes in the pattern of transactions. For the remainder of the paper, we use the net balances measure rather than average balances, in order to net out the mechanical effect of increased withdrawals on savings balances.

### 4.4 Savings Rates

In this section, we examine the impact of debit cards on the savings rate—i.e., the flow of savings as a share of income. There are a number of reasons why households save, including to smooth consumption over the life cycle (Modigliani, 1986), accumulate money for non-divisible purchases of durables in the face of credit constraints (Rosenzweig and Wolpin, 1993), and build a precautionary buffer stock to insure consumption against unexpected shocks (Deaton, 1991). While there is little evidence that life-cycle saving is an important generator of wealth in developing countries, credit constraints make precautionary saving and saving to purchase durables particularly important (Deaton, 1992; Rosenzweig, 2001).[19] The key insight for our purpose is that both the precautionary saving and saving to purchase durables motives lead to a savings target, and as a result, an individual's savings rate is decreasing in her stock of savings as it approaches the target (Carroll, 1997; Fuchs-Schündeln, 2008; Gertler et al., 2016).

Hence, we model the flow of savings in a particular period, denoted $\Delta Savings_{it}$ (where $Savings_{it}$ is beneficiary $i$'s stock of savings in period $t$), as a function of the stock of savings in the previous period and income in the current period. Adding individual and time-period fixed effects, we have

$$\Delta Savings_{it} = \lambda_i + \delta_t + \theta Savings_{i,t-1} + \gamma Income_{it} + \varepsilon_{it}. \tag{2}$$

Models of precautionary saving predict that $\theta < 0$, since the amount of new savings decreases as the stock of savings approaches the target level. In order to identify the effects of the debit card on the savings rate over time, we interact the above terms with a dummy indicating treatment localities and time period dummies.

We are not actually able to implement the above model as specified because we are restricted to using bank account information. Instead, we estimate the change in net account balances as a function of lagged net balances and transfers deposited during the period. Under a set of testable assumptions, we can interpret the estimated coefficients on interactions with the treatment dummy as causal effects of the debit card on the flow of savings. Specifically, we need to assume that (1) there are no deposits into the account other than the transfer, (2) the debit card does not affect other sources of income, and (3) the debit card does not affect other non-account savings. The first two assumptions imply that the debit card can only affect savings out of transfers and not through other sources of income. The last assumption implies that any increase in savings in the

[19] Even in rich countries, Skinner (1988) finds that precautionary savings constitute a large share of overall wealth.

bank account does not substitute for other forms of saving; any increase in savings in the bank would then constitute an increase in total savings. We test each of these three assumptions and find that they hold. First, as we have already shown in Figure 9, there are almost no beneficiaries who deposit any funds in addition to the transfers into their savings accounts in any period. Second, using a household survey panel data set in Section 7, we find that the debit cards do not affect income. Third, using the household survey data, we find the same magnitude effect of the debit card on total savings as we do with administrative bank account data.

Incorporating all of the above changes to (2) and allowing the debit card's effect to vary over time with the card, we obtain the following specification:

$$\begin{aligned} &Net\ Balance_{it} - Net\ Balance_{i,t-1} = \\ &\quad \lambda_i + \sum_k \delta_k \mathbb{I}(k=t) + \sum_k \alpha_k T_{j(i)} \times \mathbb{I}(k=t) \\ &\qquad + \sum_k \theta_k Net\ Balance_{i,t-1} \times \mathbb{I}(k=t) + \sum_k \xi_k Net\ Balance_{i,t-1} \times T_{j(i)} \times \mathbb{I}(k=t) \\ &\qquad + \sum_k \gamma_k Transfers_{it} \times \mathbb{I}(k=t) + \sum_k \psi_k Transfers_{it} \times T_{j(i)} \times \mathbb{I}(k=t) + \varepsilon_{it}. \end{aligned} \tag{3}$$

This specification has three advantages over the reduced form analysis presented in Section 4.1. First, it measures the flow rather than the stock of savings. Second, it controls for the amount of transfers in each period, which varies both across households and within households over time.[20] Third, it allows existing balances to influence the savings rate, enabling us to test the prediction from precautionary saving and saving for durables models that as a beneficiary accumulates savings and approaches her target buffer stock, her rate of saving decreases.

We estimate the effect of the debit card on the savings rate from the above specification as

$$\hat{\Phi}_k \equiv (\hat{\alpha}_k + \hat{\psi}_k \mu_k + \hat{\xi}_k \omega_{k-1})/\overline{Y}, \tag{4}$$

where $\mu_k$ is average transfers in period $k$, $\omega_{k-1}$ is average lagged net balance, and $\overline{Y}$ is average income.[21] The numerator in (4) gives the difference between treatment and control in the flow of savings in pesos; the denominator divides by average income to make the effect sizes easier to interpret.

The right hand side of the specification in equation (3) includes the lagged dependent variable

[20] Transfer amounts vary for a number of reasons. When there is an election, federal law requires Oportunidades to give the transfer in advance so that there is no payment close to the election month. In practice, this means that beneficiaries receive no payment in the bimester of the election and an additional payment in the preceding bimester. If a family does not comply with program conditions such as school attendance and health check-ups, the payment is suspended, but if the family returns to complying with the conditions, the missed payment is added into a future payment. Payments also vary systematically by time of year, as the program includes a school component that is not paid during the summer, and a school supplies component that is only paid during one bimester out of the year. Finally, changes in family structure affect the transfer amount because one child might age into or out of the program, for example.

[21] $\overline{Y}$ is obtained from the 2009–10 wave of the ENCELURB household survey conducted by Oportunidades (described in Section 3). It is scaled to a four-month period to match the estimated effect of the debit card on the flow of savings.

and individual fixed effects. In this case, the assumption that the individual fixed effects are uncorrelated with the error does not hold, and the bias could be significant if the number of time periods is small (Nickell, 1981). To account for this, we use a system GMM estimator proposed by Blundell and Bond (1998) that is consistent for fixed $T$, large $N$ and performs well in Monte Carlo simulations (Blundell et al., 2001; Bun and Kiviet, 2006). We report in panel (a) of Figure 10 the GMM-estimated effects of the debit card on the savings rate from (4).[22] We also present the estimates using a simple fixed effects specification in panel (b).

The results in Figure 10 show that during the pre-treatment period, there is no difference between the treatment and control groups in the savings rate: $\hat{\Phi}_k = 0$ for all $k < \ell$.[23] After receiving the card, there is an initial period of 9–12 months before the beneficiaries begin saving in the account; during this period, we continue to observe $\hat{\Phi}_k = 0$, i.e. no difference between the treatment and control groups in savings rates. In Section 5 we test different types of learning that could explain this delayed effect. After the learning period, however, we observe a substantial increase in the treatment group's savings rate relative to that of the control. Specifically, the difference between the treatment and control groups in savings rates in the account is 5.4% of income after one year with the card. Models of precautionary savings predict that the savings rate should fall once a positive savings balance is acheived, with the savings rate dampened by a negative coefficient on lagged balance. Indeed, this is what we find: in the following period, the effect of the debit card on the savings rate is 3.7% of income, and in the last period before the control group also receives cards, it is 2.7%.[24]

## 5 Learning

The time delay before a beneficiary begins saving after receiving her debit card suggests that learning might be occurring. We explore three specific kinds of learning: (1) *learning to trust* that the bank is a safe place to save; (2) *learning the technology*, i.e., learning how to save in banks or use ATMs, learning where ATMs are located, learning about transaction costs, etc.; and (3) *learning Oportunidades' eligibility rule* that beneficiaries who accumulate savings will not be dropped from the program.[25] In order to test these hypotheses, we complement the administrative Bansefi data with data from two beneficiary surveys: (1) the 2012 Payment Methods Survey and (2) the 2010 ENCASDU. Both are cross-sectional surveys of stratified random samples of urban Oportunidades beneficiaries; the ENCASDU oversampled in localities that received cards in early 2009. The surveys were designed by Oportunidades to learn more about the expansion of debit cards, and in both cases we restrict our analysis to the sample of respondents who received their benefits in savings accounts

[22] The results are presented for wave 1, where—unlike wave 2—we have enough post-treatment periods for learning to take place and afterwards for the flow of savings to change as the stock of savings approaches the savings target.

[23] 1F-test of this

[24] See Appendix D for a full discussion of the estimated coefficients. We show that the estimates align with the predictions from models with a savings target not only for $\hat{\Phi}_k$, as we've shown here, but also for the coefficients governing the dynamic dampening effect of the stock of savings, $\hat{\theta}_k$ and $\hat{\xi}_k$.

[25] These potential learning explanations were conjectured in a meeting with Oportunidades program officials where we shared our savings results.

tied to debit cards at the time of the survey.[26]

### 5.1 Learning to Trust

Beneficiaries might delay starting to save in order to build their trust that the bank is not reducing their account balances by charging hidden fees or through outright stealing. The debit card lowers the cost of checking account balances, leading to an increase in balance checks. Although a beneficiary could check her balance at Bansefi branches prior to receiving the card, the debit card makes it much more convenient since it allows balance checks at any bank's ATM. We hypothesize that by checking her balance and seeing that the amount is as expected, the beneficiary learns that the bank is not stealing any money or applying hidden fees. In turn, the client updates downward her prior about the risk of losing money. With simple Bayesian learning, balance checking has decreasing marginal benefit as she updates her beliefs, which would lead to a decrease in the number of balance checks over time. Hence, over time with the card, we expect balance checks to fall and trust to rise. We test the hypothesis that balance checks fall over time with both the administrative and survey data. We then examine whether higher savings balances are correlated with the number of balance checks within accounts in the administrative account data and use the survey data to test whether self-reported trust in the bank increases over time with the card.

#### Balance Checks Over Time with the Debit Card

We first use the Bansefi transactions data to test whether, as we hypothesize, balance checks fall over time with the card.[27] Figure 11 plots the average number of times clients check their balances over time in wave 1, with the dashed vertical line indicating the timing of card receipt. In the first period after receiving debit cards, beneficiaries check their balances 3 times on average, and the number of balance checks falls to 1.4 after two years with the card.

A possible explanation for this pattern is that clients are not checking if their existing balances remain, but if a new transfer has arrived. To reject this alternative explanation, we also plot in Figure 11 the average number of balance checks, conditional on these checks occurring after the transfer was received in that same bimester, and on a different day than a withdrawal. Since this subset of balance checks happens after the transfer is received, and no money is withdrawn on that day (as would be expected if they were checking if the transfer had been deposited and discovered that it had), we posit that these checks occur precisely to monitor the bank and verify that existing balances remain in the account. We observe that immediately after receiving the card, recipients check their balances in this fashion 2.2 times on average per period, and only 0.4 times per period

[26] The questions we use were not asked to those who had not yet received a debit card.

[27] We do not observe balance checks at Bansefi branches in our transactions data since these are not charged a fee; hence, we do not observe balance checks prior to receiving the card. Nevertheless, it is unlikely that beneficiaries used this mechanism to monitor the bank prior to receiving a debit card due to the relatively high indirect costs of traveling to the nearest Bansefi branch. The median household lives 5.2 kilometers (using the shortest road distance) from the nearest Bansefi branch, compared to 1.1 kilometers from an ATM.

after two years with the card.[28]

A final concern is that for learning to occur, clients must have a positive balance. Since we do not have daily account balances, we take the conservative approach of defining a balance as positive if the cumulative transfer amounts minus cumulative withdrawal amounts in the bimester is positive at the time of the balance check (which is a sufficient but not necessary condition for the balance to be positive). Among the subset of balance checks that occur after the transfer is deposited and not on the same day as a withdrawal, 89% of accounts have a positive balance at the time of checking. Focusing on the first period, when such balance checks are very frequent, we observe that many checks occur on small but positive balances: the 25th percentile of balances at the time of a balance check is 20 pesos, the median is 55 pesos, and the 75th percentile is 110 pesos. This supports the hypothesis that a beneficiary would initially leave a small balance in her account in order to be able to check her balance and confirm that it is as expected.

We cross-validate the above results from the administrative data by applying a similar test using the self-reported number of balance checks per period from the Payment Methods Survey of beneficiaries. We exploit variation in length of time with the debit card to test whether those who have had the card longer make less balance checks. Specifically, we split the sample by the median time with the card and estimate the following model:

$$y_i = \alpha + \gamma \mathbb{I}(\text{Card} \leq \text{median time})_i + u_i, \tag{5}$$

where $y_i$ is either (i) the self-reported number of balance checks over the past bimester; or (ii) the self-reported number of balance checks over the past bimester without withdrawing any money. Figure 12a shows the results: both the number of balance checks and the number of balance checks without withdrawing decrease over time with the card. Those who have had the card for more than the median time (12 months) make 31 percent fewer trips to the ATM to check their balances without withdrawing money than those who have had the card for less time.

### Correlation of Savings Balances and Balance Checks

We now test whether a negative correlation between balance checks and savings exists *within* accounts. We hypothesize that initially, when trust is low, beneficiaries do not yet save in the account, but do use the card to make a high number of balance checks. As they build trust, they make fewer balance checks, and this is followed by an increase in the stock of savings. Specifically, we estimate

$$Net\ Balance_{it} = \lambda_i + \sum_{c \neq 0} \eta_c \mathbb{I}(Checks_{it} = c) + \varepsilon_{it} \tag{6}$$

[28] Clients were given calendars with exact transfer dates and hence should know the transfer dates (see Figure A3). We remain conservative by excluding all balance checks that *could be* interpreted as checking if a new transfer has arrived, i.e. excluding all balance checks that occurred prior to the transfer being deposited or after the transfer being deposited on the same day that money is withdrawn.

where $Net\ Balance_{it}$ is the net balance in account $i$ at time $t$, the $\lambda_i$ are account-level (i.e., beneficiary) fixed effects, and $Checks_{it}$ is the number of balance checks in account $i$ over period $t$, which we top code at 5 to avoid having many dummies for categories of high numbers of balance checks with few observations.[29] The $\eta_c$ coefficients thus measure the within-account correlation between the stock of savings and number of balance checks, relative to the 0 balance checks ($c = 0$) category. Our hypothesis that beneficiaries increase their savings as they decrease their number of balance checks predicts that $\eta_c < 0$, and that $\eta_c$ is decreasing (i.e., becoming more negative) in $c$.

Figure 13 shows the results from equation (6). As predicted, account balance is negatively correlated with number of balance checks. Although there is no difference (precisely estimated) in balances when beneficiaries make 0 vs. 1 balance check, all coefficients for the categories corresponding to more than one balance check are negative, large, and statistically significant. For example, in periods where beneficiaries make two balance checks, their savings average 180 pesos less than in periods when they make zero or one balance checks. Furthermore, for every additional balance check, net balances decrease, and this decrease is statistically significant. For example, in periods in which beneficiaries make 5 or more balance checks, their balances are on average 422 pesos lower than periods in which those same beneficiaries make zero or one check. This shows that as a beneficiary checks her balance less, she increases her savings balance.

### Trust Over Time with the Debit Card

In this section we test the hypothesis that the longer beneficiaries have had the debit card, the higher their trust in the bank. We measure trust as follows. The ENCASDU survey asks: "Do you leave part of the monetary support from Oportunidades in your bank account?" If the response is no, the respondent is then asked: "Why don't you keep part of the monetary support from Oportunidades in your Bansefi savings account?" *Lack of trust* is captured by the answer "because if I do not take out all of the money I can lose what remains in the bank" (one of the pre-written responses to the question) or a similar open-ended response related to not trusting the bank.[30] If the respondent provides a different reason for not saving in the account, or answers the first question "Yes" (i.e., saves in the account), we code *lack of trust* as 0. We then estimate (5) with lack of trust as the dependent variable, again exploiting the exogenous variation in the length of time beneficiaries have had the card. Because ENCASDU was conducted in the second half of 2010 before any beneficiary from wave 2 received cards, those with less and more time with the card in this comparison all received cards during wave 1.[31]

Figure 14 shows the results. Lack of trust is cited as the reason for not saving by 24 percent

[29] We do not include time fixed effects because the within-account changes in the stock of savings over time constitute precisely the variation we are exploiting. As always, $\varepsilon_{it}$ are clustered at the bank branch level.

[30] The survey question allows the beneficiary to select one of the pre-written responses, or answer "other" and provide an open-ended response. 5% use the open-ended option. Examples of open-ended responses that were coded as lack of trust include "because I don't feel that the money is safe in the bank"; "distrust"; and "because I don't have much trust in leaving it."

[31] In Table B1, we show balance between those who have had the card for more vs. less than the median time. We find no statistically significant differences at the 5% level and one statistically significant difference (out of 10 variables) at the 10% level, as would be expected by chance.

of those who have had the card for less than the median time. Trust increases over time, however, and beneficiaries with more than the median time with the card are 33 percent less likely to report not saving due to low trust.[32]

### 5.2 Learning the Technology

During the period of delay in starting to save, beneficiaries could over time be learning how to use their debit cards, learning that they can save in the account, learning where ATMs are located, or learning the transaction costs of using the account. The Payment Methods Survey includes various questions about use of the accounts after receiving debit cards: specifically, each respondent is asked whether (i) it is hard to use the ATM; (ii) she gets help using the ATM; and (iii) she knows her PIN. Thus, we estimate (5) with each of these three dependent dummy variables. Figure 12b shows that there is no statistically significant difference between the group that has had the card for less than the median time compared to the group that has had the card more than the median time.

In the ENCASDU, we use the same direct survey question from Section 5.1 on self-reported reasons for not saving to test whether beneficiaries don't save due to lack of knowledge about how to save in the account. Lack of this type of knowledge, however, is rarely cited as a reason for not saving in the ENCADSU survey: less than 2 percent of beneficiaries cite not saving due to lack of knowledge, and there is no difference between who that have had the card for less than and more than the median time (Figure 14).

In addition to finding little evidence of increased knowledge of the technology, we find that use of the accounts and ATMs increases immediately after receiving the card, then remains fairly stable over time. This is inconsistent with the hypothesis of learning to use the technology. Using the administrative data, we saw this pattern for withdrawals in Figure 8; we can also test if clients immediately start using the card to withdraw at ATMs and convenience stores rather than bank branches. Figure 15 shows the percentage of clients in each wave who use their debit card to make at least one withdrawal at an ATM or convenience store instead of going to the bank branch: depending on the bimester, 93–95% of clients use them to withdraw at ATMs and convenience stores and the adoption rate appears nearly instantaneous.[33]

The *learning the technology* hypothesis is also inconsistent with the evolution of balance checks over time. As a beneficiary learns the technology, it should become easier (i.e., less costly) for her to check her balance. The fall in the marginal cost of using the ATM should then increase the number of balance checks over time. As shown in Section 5.1, however, we find the opposite trend in balance checks: the number of balance checks falls over time.

Finally, beneficiaries might be learning about ATM transaction costs, and start saving once

[32] Note that because of the timing of the ENCASDU survey, those with the card for less than the median time have nevertheless had the card for at least 9 months, meaning that some of them would have likely developed trust in the bank prior to being surveyed. Those with more than the median time with the card have had it for 5 months longer on average.

[33] In the period in which cards are received, 82% of beneficiaries make a withdrawal at an ATM; this figure is lower than in subsequent periods, as expected, since some beneficiaries received the card mid-period after already receiving and withdrawing their benefits.

they learn that these are sufficiently low. We test this alternative story directly using two questions from the Payment Methods Survey asking beneficiaries if they know how much the bank charges them for each (i) balance check and (ii) withdrawal after the initial free withdrawals. We find that self-reported cost of transactions is not different for beneficiaries with more than the median time with the card compared to those with less: Figure 12c displays, by time with the card, beneficiaries' self-reported estimates of these fees to check balances and withdraw. There is no difference in beneficiaries' self-reported estimates of transaction costs based on time with the card.[34]

### 5.3 Learning the Program Rules

The last type of learning conjectured by Oportunidades program officials was that beneficiaries may have initially thought that saving in the account would make them be viewed as less poor and thus ineligible for the program, but learned over time that this was not the case. Due to this salient concern among program officials, the Payment Methods Survey includes the following pre-written response to the question about reasons for not saving: "because if I save in the account, they can drop me from Oportunidades."

We thus estimate (5) with the dependent variable equal to 1 if respondents do not save for this reason (which we call *fear of ineligibility* in Figure 14), or a related reason listed in response to the optional open-ended response to the same survey question.[35] All other beneficiaries with savings accounts and debit cards are coded as 0 (including if they reported saving in the account in response to the previous survey question). The first thing to note from Figure 14 is that the fear of being dropped from the program due to having savings in the bank is rarely cited as a reason for not saving, accounting for less than 4 percent of the sample who have had the card for less than the median amount of time. Furthermore, there is no statistically significant difference for those who have had the card for more than the median amount of time. This is consistent with information from our meetings with Oportunidades program officials, in which they reported that during the implementation they emphasized to beneficiaries that saving in the account would not disqualify them from future benefits.

## 6 The Relationship between Trust and Saving

In this section, we directly estimate the relationship between reported trust in the bank and the savings rate. In Section 5 we found that time with the card increases trust, but does not affect knowledge of how to use the technology, transaction costs, or program rules regarding saving in the account. Consistent with these results, we assume in this section that time with the card affects

[34] Beneficiaries are also fairly accurate. The median *actual* balance check fee in the transactions data is 10.4 pesos, while the median fee *estimated by beneficiaries* is 11 pesos; more importantly, these estimates do not vary by how long beneficiaries have had the card, as shown in Figure 12. The median withdrawal fee is 40 pesos while the median estimated withdrawal fee is 24 pesos. While beneficiaries underestimate withdrawal fees (which are only charged after the second withdrawal in the bimester), the estimates do not differ by time with the card.

[35] Examples of open-ended responses coded as *fear of ineligibility* include "because they say that the card gets canceled if we don't withdraw the entire benefit" and "because they told me that if I don't take my benefit in a single withdrawal, the account would be frozen."

savings only through its effect on trust. If this assumption holds, we can express the reduced form effect of time with the card on the savings rate as

$$\frac{\mathrm{d}\Delta Savings}{\mathrm{d}Time\ with\ card} = \frac{\partial \Delta Savings}{\partial Trust} \cdot \frac{\partial Trust}{\partial Time\ with\ card.} \tag{7}$$

In Section 4.4 we estimated the left-hand side of equation 7, and found that after one to two years with the card, beneficiaries save 2.7 to 5.4% more of their income than the control group.[36]

Our goal here is to directly estimate the relationship between reported trust in the bank and the savings rate, i.e., the first term of the right-hand side of (7). To do this, we merge the administrative data on net balances (from Section 4.4) with the ENCASDU survey data on trust (from Section 5.1). Everyone in this sample has had the card for between 9 and 18 months; we exploit variation in time with the card for identification. Since all of the beneficiaries in this sample have the card, all benefit from the lower transaction costs that debit cards engender. Using administrative identifiers provided by Oportunidades, we are able to merge 1330 of the 1694 beneficiaries in the survey with their corresponding administrative savings data; among this sample, we restrict the administrative savings data to the two bimesters that overlap with the timing of the survey.

To estimate the effect of trust on saving, we regress the flow of savings $\Delta Savings_{it}$ on a trust dummy (which is the complement of the *lack of trust* dummy used in Section 5.1):

$$Net\ Balance_{it} - Net\ Balance_{i,t-1} = \zeta Trust_{it} + \varepsilon_{it}. \tag{8}$$

In an OLS regression, we find no relationship between reported trust and the flow of savings. This is not surprising, as trust is endogenous: in the cross-section, those with initially high trust prior to the card or who developed trust in the bank quickly may have already reached their savings targets and thus not be adding additional savings. Since trust is endogenous, we instrument it with the date of debit card assignment; this isolates the variation in trust that can be explained exogenously by time with the card. We already know from Section 5 that this instrument has a strong first stage.

Three pieces of evidence suggest that the instrument should satisfy the exclusion restriction. First, time with the card is uncorrelated with sociodemographic characteristics. Second, time with the card does not affect other types of learning, as shown in Section 5. Third, *time* with the card (as opposed to the card itself) does not affect transaction costs, which are immediately reduced upon receiving the card: indeed, beneficiaries immediately react to these reduced transaction costs by increasing the number of withdrawals (Figure 8) and switching to withdrawing at ATMs (Figure 15). After they make these immediate behavioral changes upon receiving the card, withdrawals per bimester and the proportion of withdrawals made at ATMs are constant over time. Recall that, in the sample used in this section, everyone has had a card for at least 9 months; transaction costs do not change as a result of having the card for additional months.

[36] The range 2.7–5.4% is a function of the time period at which we measure the savings rate increase in the treatment compared to the control group, restricted to time periods after beneficiaries have had the card for at least one year.

Table 2 reports the OLS and IV results from estimating (8), where in the IV regression trust is instrumented with a set of dummy variables for the timing of debit card receipt. Coefficients are expressed as a proportion of average income (from the survey) and standard errors are clustered at the locality level. The first stage, i.e. the effect of timing of debit card receipt on trust, has an F-statistic of 40. Taking a weighted average of the coefficients on each debit card timing dummy, the first stage shows that an average of six additional months with the card leads to a 10.3 percentage point increase in the probability of trusting the bank. The IV coefficient in column 2 shows that beneficiaries who report trusting the bank as a result of having the card for an additional six months save an additional 2.8% of their income, statistically significant at the 5% level.[37]

The IV coefficient corresponds to the effect of being induced to trust the bank by virtue of having the debit card for a longer period of time (and hence having sufficient time to build trust in the bank). We find that a beneficiary who switches from not trusting the bank to trusting it as a result of having the card longer increases her savings rate by 2.8%. If this sample were identical to that of Section 4.4 and the relationship between time with the card and trust were linear (i.e., if $\frac{\partial Trust}{\partial Time\ with\ card}$ were constant), we could compare this coefficient to the estimates from Section 4.4 of the effect of having the card for different lengths of time on savings. However, the samples are not identical and the evolution of average balances suggests a non-linear relationship between time with the card and trust. While the results in Section 4.4 are based on a treatment group that has the card for up to two years versus a control group that has not yet received a card, in the ENCASDU sample everyone has had the card for at least 9 months. Indeed, among the ENCASDU sample, the weighted average of the reduced-form effect shows that having the card for an additional 6 months on average (relative to a group that has nevertheless had the card for at least 9 months) increases saving by just 0.3% of income. This is consistent with the IV estimate, which equals the reduced-form estimate (0.3% of income) divided by the first stage (time with the card increases trust by 10.3 percentage points).

To conclude, assuming that time with the card affects savings only through trust, we find a direct effect of trust (more precisely, the portion of trust explained by having the debit card for a longer period time) on the flow of savings at the beneficiary level. This is to our knowledge the first direct causal estimate in the literature of the effect of trust in formal financial institutions on

[37] Our results are robust to estimating a specification more analogous to (3) based on models of precautionary saving, controlling for the amount of income received through transfers and the lagged stock of savings, and interacting these controls with trust:

$$\begin{aligned} Net\ Balance_{it} - Net\ Balance_{i,t-1} = \zeta Trust_{it} + \theta Net\ Balance_{i,t-1} + \xi Trust_{it} \times Net\ Balance_{i,t-1} \\ + \gamma Transfers_{it} + \psi Trust_{it} \times Transfers_{it} + \varepsilon_{it}. \end{aligned}$$

In this specification, because $Trust_{it}$ is interacted with lagged net balance and transfers, we also include the interactions of card-receipt dummies with lagged net balance and with transfers as instruments. The instruments are again strong: the Sanderson and Windmeijer (2016) multivariate F-test for IV models with multiple endogenous variables (in this case, trust and its interactions) gives F-statistics of 18 for trust, 147 for trust interacted with lagged net balance, and 38 for trust interacted with transfers. The effect of trust on the flow of savings in this specification is estimated as $\hat{\Phi} \equiv (\hat{\zeta} + \hat{\psi}\mu + \hat{\xi}\omega_{-1})/\overline{Y}$, where $\mu$ is average transfers, $\omega_{-1}$ is average lagged net balance, and $\overline{Y}$ is average income within this sample and period. The result, shown in Table 2 column 3, is that the part of trust that can be explained by the exogenous timing of debit card receipt accounts for a savings rate increase of 2.9% of income (statistically significant at the 10% level), consistent with the results from the simpler specification (8).

formal saving.

## 7 Increase in Overall Savings vs. Substitution

The increase in formal savings in beneficiaries' Bansefi accounts might represent a shift from other forms of saving, such as saving under the mattress or in informal saving clubs, with no change in overall saving. This section investigates whether the observed increase in Bansefi account savings crowds out other savings. We take advantage of Oportunidades' ENCELURB panel survey, conducted in urban and semi-urban localities in four waves during the years 2002, 2003, 2004 and November 2009 to 2010. This survey is conducted by Oportunidades and has comprehensive modules on consumption, income, and assets for 6272 households.[38] Of these, 2942 households live in urban areas and received their benefits in a savings account prior to the rollout of debit cards; these households make up our sample.

We use a simple difference-in-differences identification strategy where we examine changes in consumption, income, saving, purchases of durables, and the stock of assets across beneficiaries, exploiting the differential timing of debit card receipt. Because the ENCELURB was conducted after wave 1 localities had received cards but before wave 2 or control localities had received cards, we compare those with cards (wave 1) to those who had not yet received cards (wave 2 and control), respectively referring to them as "treatment" and "control" localities in this section. The identification assumption is that in the absence of the debit card, treatment and control groups would have experienced similar changes in consumption, income, saving, and assets. We formally test for parallel pre-treatment trends for each of our dependent variables in Table B4 and fail to reject the null hypothesis of parallel trends.

Having established that the identification assumption is plausible, we estimate

$$y_{it} = \lambda_i + \delta_t + \gamma D_{j(i)t} + \nu_{it}, \tag{9}$$

separately for five dependent variables: consumption, income, flow of savings (constructed as income minus consumption), purchase of durables, and an asset index.[39] All variables except the asset index are measured in pesos per month, $i$ indexes households, and $t$ indexes survey years.[40] Variables are winsorized at the 5% level to avoid results driven by outliers. Time-invariant differences in household observables and unobservables are captured by the household fixed effect $\lambda_i$, common time shocks are captured by the time fixed effects $\delta_t$, and $D_{j(i)t} = 1$ if locality $j$ in which household $i$ lived prior

[38] The 2002, 2003, and 2004 waves had around 17,000 households, but due to budget constraints the number of localities was cut for the 2009–10 wave. We restrict our sample to those included in the 2009–10 wave; not every household was surveyed in every baseline wave, resulting in an unbalanced sample. The consumption, income, and assets modules of Oportunidades' analogous survey for rural areas have been used by Angelucci and De Giorgi (2009), Attanasio et al. (2013), de Janvry et al. (2015), Gertler et al. (2012), and Hoddinott and Skoufias (2004), while these modules from the ENCELURB have been used by Angelucci and Attanasio (2013) and Behrman et al. (2012).

[39] Our measure of the flow of savings is imperfect, but is commonly used in the literature (e.g., Dynan et al., 2004).

[40] The asset index dependent variable is constructed as the first principal component of dummy variables indicating ownership of the assets that are included in all rounds of the survey questionnaire: car, truck, motorcycle, TV, video or DVD player, radio, washer, gas stove and refrigerator.

to treatment has received debit cards by time $t$. We use the locality of residence prior to treatment to avoid confounding migration effects, and estimate cluster-robust standard errors clustered by locality.

If the increase in formal savings is merely a substitution away from other forms of saving, we expect to find $\gamma = 0$ when the dependent variable is the flow of total savings (defined as income minus consumption). And if the form of savings that beneficiaries substituted away from was durable assets, we expect $\gamma < 0$ for the stock of assets, and potentially also for the purchase of durables. If, on the other hand, the formal savings increase constitutes an increase in total savings, then we expect $\gamma > 0$ for the flow of total savings; if there is partial crowding out, we expect the magnitude of $\gamma$ to be less than the magnitude found in the administrative Bansefi data, while if there is no crowding out, we expect the magnitude to be similar. Furthermore, one of the assumptions in Section 4.4 was that the debit card does not affect income, so we test $\gamma = 0$ for income. After confirming there is no effect on income, we expect $\gamma < 0$ for consumption, since consumption must decrease if total savings increases and income does not change. Furthermore, if there is no substitution of savings from assets (and if they are not using the formal savings accounts to save up for assets, at least in the short run), we expect $\gamma = 0$ for the purchase of durables (which measures a flow) and the asset index (which measures a stock).

Our findings indicate that the increase in formal savings shown in Section 4 represents an increase in overall savings. Figure 16 shows that consumption decreased by about 138 pesos per month on average (statistically significant at the 5% level). We do not find any effect on income.[41] Purchases of durables and the stock of assets do not change, ruling out a crowding out of these forms of saving. The increase in the flow of savings, measured as income minus consumption, is estimated at 236 pesos per month, and is statistically significant at the 5% level. These results are robust to the extent of winsorizing and to allowing time trends to differ more flexibly as a function of household characteristics.[42]

These results mean that total savings—not just account savings—increase, and that this increase in being funded by lower consumption today. A back-of-the-envelope calculation reveals that the magnitude of the increase in monthly saving from the household survey data is about the same in magnitude as the increase in the flow of savings in the Bansefi account. In Section 4.4 we estimate that after 1 year with the card, beneficiaries who received cards in wave 1 save 5.4% more of their income than the control group. In our survey data, the effect we find on the flow of total savings

[41] We also test the difference in the coefficients of consumption and income using a stacked regression (which is equivalent to seemingly unrelated regression when the same regressors are used in each equation, as is the case here); although both are noisily measured, the difference in the coefficients is significant at the 10% level ($p = 0.092$).

[42] Table B2 shows that the effects are robust to using the raw data without winsorizing (column 1) and to winsorizing at 1% (column 2) or 5 percent (column 3, which are our main results presented in Figure 16); we follow Kast and Pomeranz (2014) who show the robustness of results to these three possibilities for their savings measures. They are also robust to including baseline characteristics interacted with time fixed effects (column 4). The baseline characteristics that we interact with time fixed effects in column 4 include whether the household head worked, a quadratic polynomial in years of schooling, and a quadratic polynomial in age, whether the household has a bank account, the proportion of household members with health insurance, the proportion aged 15 or older that are illiterate, the proportion aged 6 to 14 that do not attend school, the proportion aged 15 or older with incomplete primary education, the proportion aged 15 to 29 with less than 9 years of schooling, whether the house has a dirt floor, no bathroom, no water, no sewage, and the number of occupants per room.

is 236 pesos per month; dividing by average household income in the post-treatment survey wave, 4,629 pesos per month, equates to 5.1% of income. We cannot reject that the effect sizes in the administrative data and survey data are equal; since the point estimate from the survey data has a large confidence interval, however, the more convincing result is that the point estimate of the savings effect from the survey data is within the tight $[0.045, 0.062]$ confidence interval of the effect size from the more precise administrative data. These results suggests that most or all of the increase in savings in the account is new saving and that there is no crowd-out of other types of saving.

These results are consistent with Dupas and Robinson (2013a), Ashraf et al. (2015), and Kast et al. (2016), who find that offering formal savings accounts does not crowd out other forms of saving. This may be because saving informally is difficult for a number of reasons, so access to a trusted formal savings account allows households to achieve a higher level of overall savings. First, informal savings can be stolen (Banerjee and Duflo, 2007; Schechter, 2007; Alvarez and Lippi, 2009). Second, intra-household bargaining issues may prevent women from saving at home (Anderson and Baland, 2002; Ashraf, 2009; Schaner, 2015). Third, money saved at home could be in demand from friends and relatives (Baland et al., 2011; Dupas and Robinson, 2013b; Jakiela and Ozier, 2016). Finally, it may be tempting to spend money that had been intended to be saved if it is easily accessible, especially if the beneficiary has easy access to money saved informally at times when she is more financially constrained (Carvalho et al., 2016). Once the bank is trusted, the account might form a soft commitment device that overcomes these self-control problems (Ashraf et al., 2006b; Bryan et al., 2010).

## 8 Alternative Explanations

We have argued that the card allows beneficiaries to build trust in the bank by monitoring the bank's activity through balance checks. We now explore alternative explanations for the observed delayed effect, followed by a gradual increase in the savings balance and a change in the savings rate that adheres to predictions from models of precautionary saving and saving to purchase durables.

### 8.1 Supply-Side Expansion

An alternative explanation for the delayed effect and increase in savings over time is that banks gradually expanded complementary infrastructure (e.g., the number of ATMs) in localities where treated beneficiaries live, potentially as an optimal response to an increase in demand for financial services from the new cardholders in those localities. More ATMs would decrease the transaction cost of accessing funds, which could boost savings once the transaction cost is low enough that the bank becomes a desirable place to save (since, in precautionary savings models, a shock forces the client to incur the cost of an additional trip to the ATM). This explanation, which would imply a delayed decrease in transaction costs for some beneficiaries, is inconsistent with the immediate increase in the number of withdrawals we observe in Figure 8.

We nevertheless directly test this hypothesis using quarterly data on the number of ATMs at the municipality level to see if there was a contemporaneous expansion of infrastructure that was

correlated geographically with Oportunidades debit card expansion. Specifically, we estimate

$$y_{mt} = \lambda_m + \delta_t + \sum_{k=-8}^{8} \beta_k D_{m,t+k} + \varepsilon_{jt}, \tag{10}$$

where $y_{mt}$ is the number of total ATMs, total bank branches, Bansefi ATMs, or Bansefi branches in municipality $m$ in quarter $t$, and $D_{mt}$ equals one if at least one locality in municipality $m$ has Oportunidades debit cards in quarter $t$. The error term $\varepsilon_{jt}$ is clustered by municipality. We include two years (eight quarters) of lags to test whether the supply of ATMs or bank branches responds to the rollout of debit cards, which from the perspective of banks can be thought of as a discrete jump in the number of potential users. We also include two years of leads to test whether the rollout of debit cards instead followed an expansion of bank infrastructure, which would be a threat to validity.

We use data on the number of ATMs and bank branches by bank by municipality by quarter from the Comisión Nacional Bancaria y de Valores (CNBV), from the last quarter of 2008—the first quarter for which data are available—through the last quarter of 2012, which includes one year before wave 1 received cards and two years after wave 2 received cards. We separately test whether lags of debit card receipt predict banking infrastructure (i.e., whether there is a supply-side response to the rollout of debit cards) by testing $\beta_{-8} = \cdots = \beta_{-1} = 0$, and whether leads of debit card receipt predict banking infrastructure (i.e., whether debit cards were first rolled out in municipalities with a recent expansion of banking infrastructure) by testing $\beta_1 = \cdots = \beta_8 = 0$. We find evidence of neither relationship, failing to reject the null hypotheses of zero correlation between the rollout of debit cards and the expansion of banking infrastructure for each of the four dependent variables (Table B3).[43]

### 8.2 Local Income Shocks

A second alternative explanation is that the increase in savings is due to local macro shocks to incomes at the locality level. Given the geographical breadth of the treatment and control groups throughout Mexico, however, this is unlikely. Furthermore, if this were the case we would expect to find a differential change in income between the treatment and control groups after treatment; we directly test this hypothesis in Section 7 and find no differential change in income after treatment.

### 8.3 Time with the Bank Account

Finally, we investigate whether individuals learn about banks in general the longer they have their savings account itself (regardless of whether they have a debit card). This could only explain the differential savings trends in treatment and control groups if the debit card were a necessary

[43] This lack of a supply-side response by private banks is not illogical: the banks would have to make sufficient profit off of the new cardholders to justify the cost of installing new ATMs. Oportunidades beneficiaries with debit cards only make 1.4 withdrawals per bimester on average, and may not constitute a large enough share of the population in urban localities to justify the cost of installing new ATMs.

condition for saving, but learning about the bank through having the *account* (not card) for a sufficient amount of time were also a necessary condition.

There are a number of reasons why experience with the savings account rather than time with the debit card itself cannot explain the savings effect. First, because the savings accounts were rolled out between 2002 and 2004 (Figure 3), most beneficiaries had already accumulated several years with the account by 2009, when debit cards were first introduced. Indeed, the median month of account opening is October 2004, and less than 5 percent of accounts had existed for less than two years before they received debit cards. Second, both treatment and control accounts are accumulating time with their savings accounts simultaneously, and they have had accounts for the same amount of time on average. Third, our results from Section 4.4 include account fixed effects, so any time-invariant effect of having the account for a longer period of time would be absorbed. Fourth, we test whether results on savings rates vary when we split the sample based on whether the account was opened before or after the median date. We find very similar results across the two subsamples (Figure B3).

## 9 Conclusion

Trust in financial institutions is low, especially among the poor, and this may be a barrier to financial inclusion. A lack of trust in banks could explain why a number of studies offering the poor savings accounts with no fees or minimum balance requirements have found low take-up and, even among adopters, low use of the accounts (e.g., Dupas et al., 2016b). We show that the trust barrier is not insurmountable: it can be overcome by debit cards, a scalable existing technology. Our first important result is to show that debit cards have a large causal effect on savings in the account. Our second result is that increasing trust plays an important role in the savings effect. Once beneficiaries build trust in banks by using their debit cards to repeatedly check account balances, they begin to save and their savings increase over time. Our third result is to show that the savings in the account are new savings, rather than a substitution from other forms of saving.

It is worth noting that beneficiaries with the debit card voluntarily use the technology and build savings in the account (whereas they could continue withdrawing all of their benefits from the bank branch, as they did prior to receiving the card), indicating a revealed preference for saving in formal financial institutions after building trust. Because the formal accounts pay no interest, this action also reveals an unmet demand for savings among program beneficiaries.

The size of the savings effect—between 3 and 5% of income after 1–2 years with the card—is larger than that of various other savings interventions, including offering commitment devices, no-fee accounts, higher interest rates, lower transaction costs, and financial education. Figure 17 compares the effect size we find to the effect of various savings interventions studied in the literature on the savings rate.[44] One other intervention found to have a similarly large effect is mobile money, which is another technology that enables clients to more easily check account balances and build

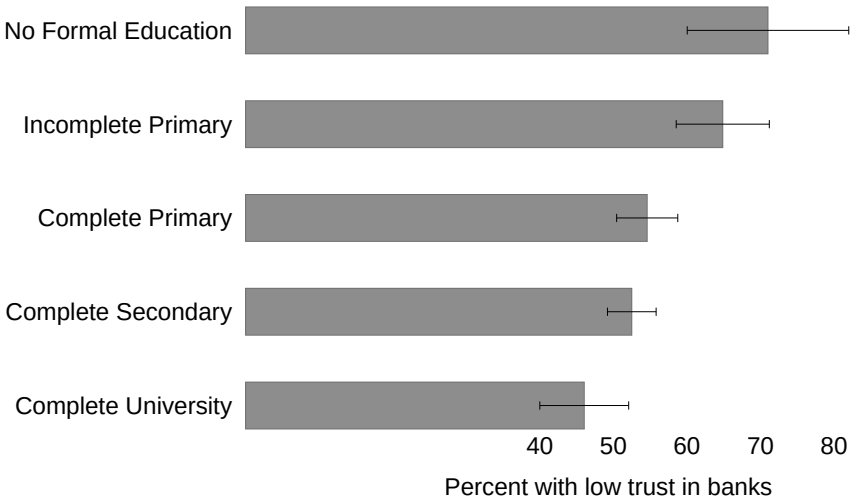
[44] See Appendix E for details on each study and how we use the results from the study's tables or replication data to estimate the impact of the savings intervention on the savings rate.

trust. Suri and Jack (2016) find that female-headed households increase their savings rate by 3% of income on average, after six years of mobile money exposure.

These results are important for public policy, as building savings in formal financial institutions has been shown to have positive welfare effects for the poor by enabling them to decrease consumption volatility (Chamon et al., 2013; Prina, 2015), accumulate money for microenterprise investments (Dupas and Robinson, 2013a), invest in preventative health products and pay for unexpected health emergencies (Dupas and Robinson, 2013b), invest in children's education (Prina, 2015), increase future agricultural/business output and household consumption (Brune et al., 2016), and decrease debt (Atkinson et al., 2013; Kast et al., 2016). For these reasons, Mullainathan and Shafir (2009) conclude that access to formal savings services "may provide an important pathway out of poverty."

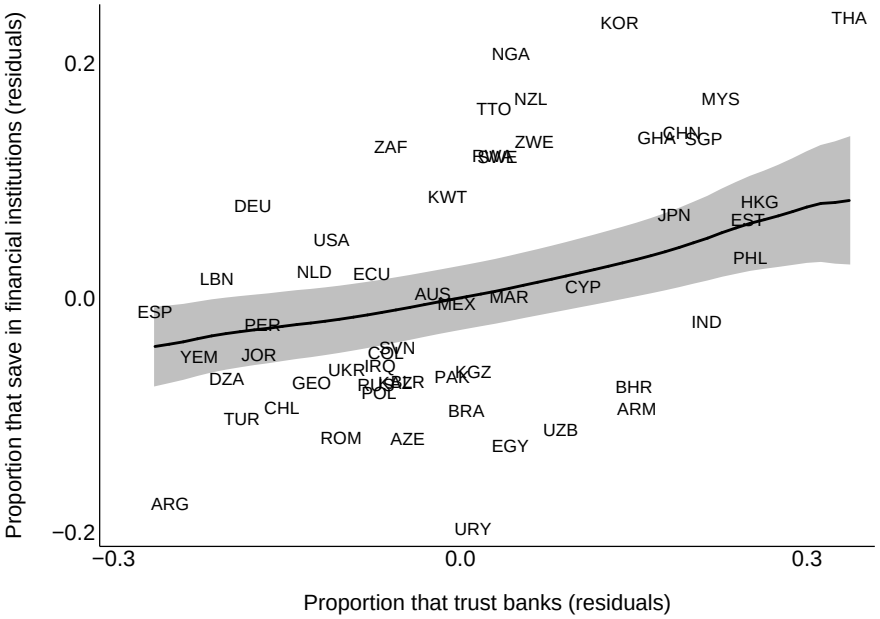
Interventions that enable account holders to monitor banks and increase their trust in financial institutions may be a promising avenue to enable the poor to save in the formal financial sector. These interventions take advantage of prevalent technologies—such as debit cards, ATMs, point of sale terminals, and mobile phones. Governments and non-governmental organizations are increasingly using these technologies to digitize their social cash transfer programs, providing the opportunity to rapidly scale these trust-building technologies and enable the poor to save more.

Figure 1: Low Trust in Banks by Education Level in Mexico




Source: World Values Survey, Mexico, Wave 6 (2012).
Notes: $N = 1993$ individuals. Low trust in banks is defined as "not very much confidence" or "none at all" for the item "banks" in response to the following question: "I am going to name a number of organizations. For each one, could you tell me how much confidence you have in them: is it a great deal of confidence, quite a lot of confidence, not very much confidence or none at all?" Whiskers denote 95 percent confidence intervals.

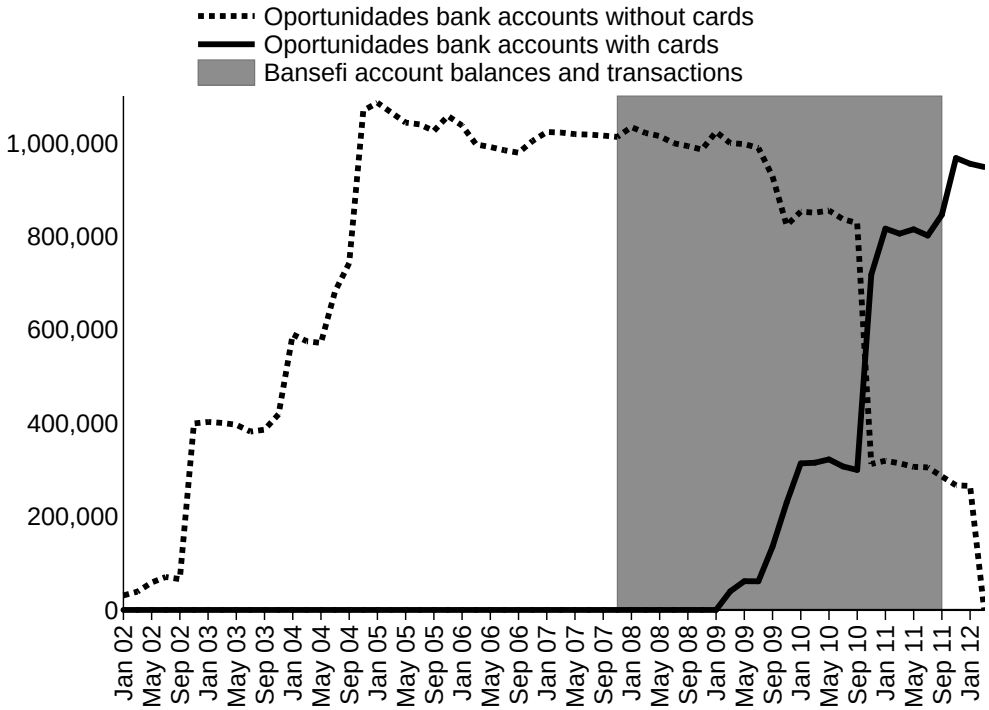
Figure 2: Cross-Country Comparison of Trust in Banks and Saving in Financial Institutions



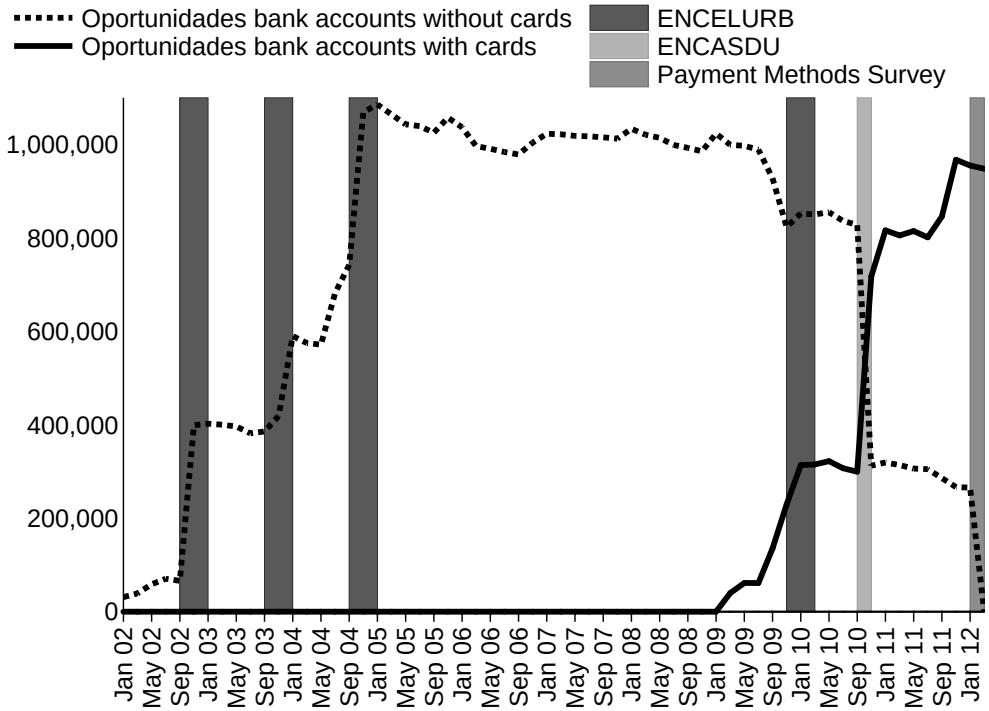

Sources: World Values Survey (WVS), Wave 6 (2010–2014); Global Findex; World Development Indicators (WDI).
Notes: $N = 56$ countries. The y-axis plots residuals from a regression of the proportion that save in financial institutions (from Global Findex) against controls (average age, education, and perceived income decile from WVS, GDP per capita and growth of GDP per capita from WDI). The x-axis plots residuals from a regression against the same controls of the proportion that respond "a great deal of confidence" or "quite a lot of confidence" in response to the WVS question "could you tell me how much confidence you have in banks: a great deal of confidence, quite a lot of confidence, not very much confidence or none at all?" The solid line shows a kernel-weighted local polynomial regression, while the gray area shows its 95% confidence interval.

Figure 3: Timing of Roll-out and Data

(a) Administrative Bank Account Data

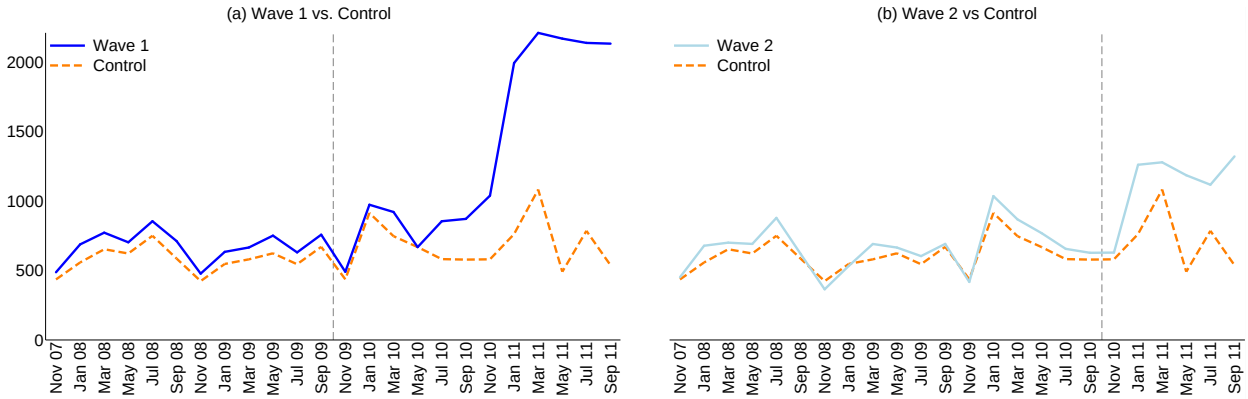



(b) Household Survey Data



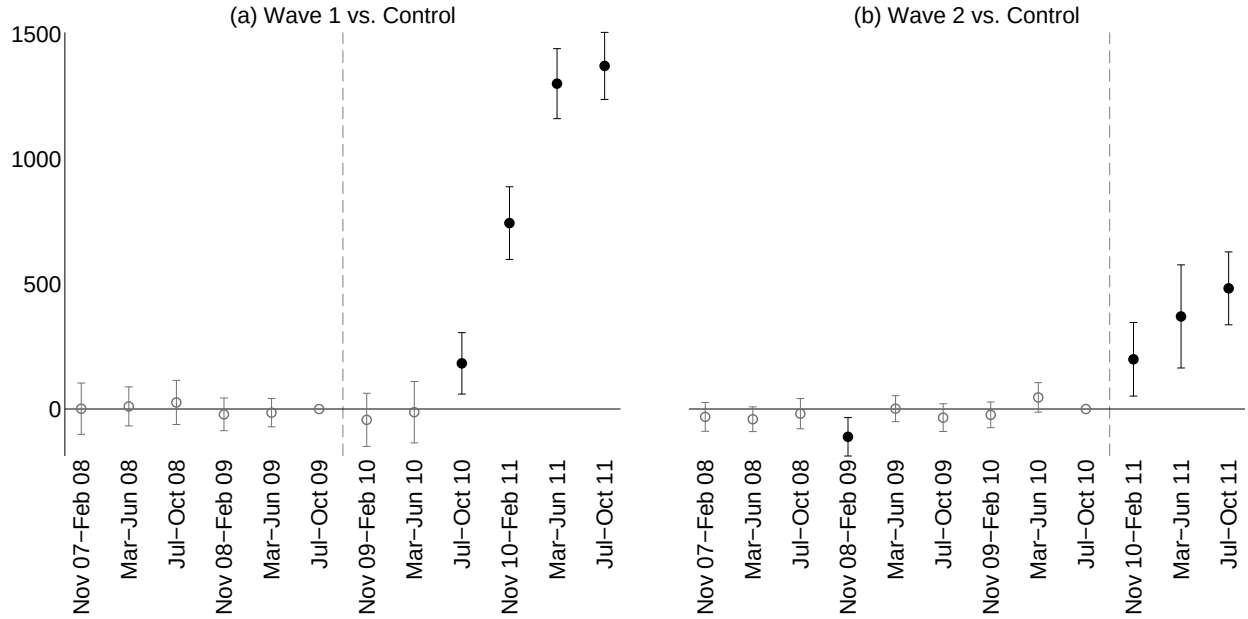

Source: Number of Oportunidades bank accounts with cards and without cards by bimester is from administrative data provided by Oportunidades.

Figure 4: Evolution of Average Balances



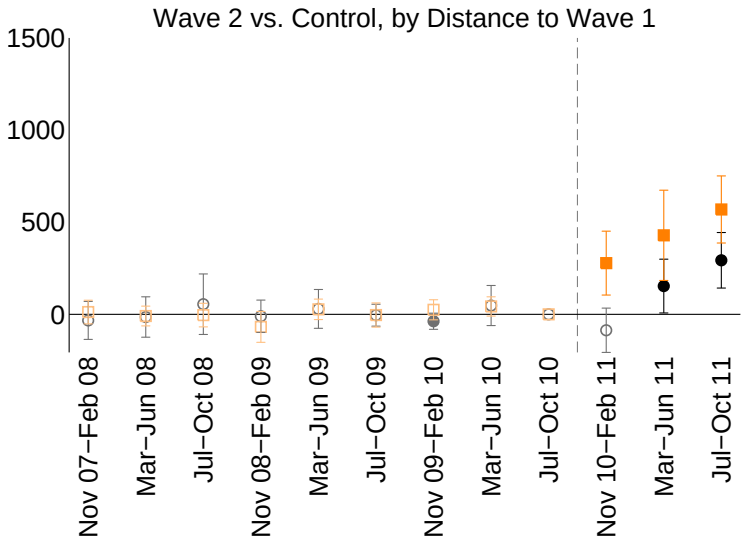

Sources: Administrative data from Bansefi on average account balances by bimester and timing of card receipt.
Notes: $N = 5{,}834{,}468$ account-bimester observations from 343,204 accounts. Average balances are winsorized at the 95th percentile.

Figure 5: Effect of Debit Cards on Stock of Savings



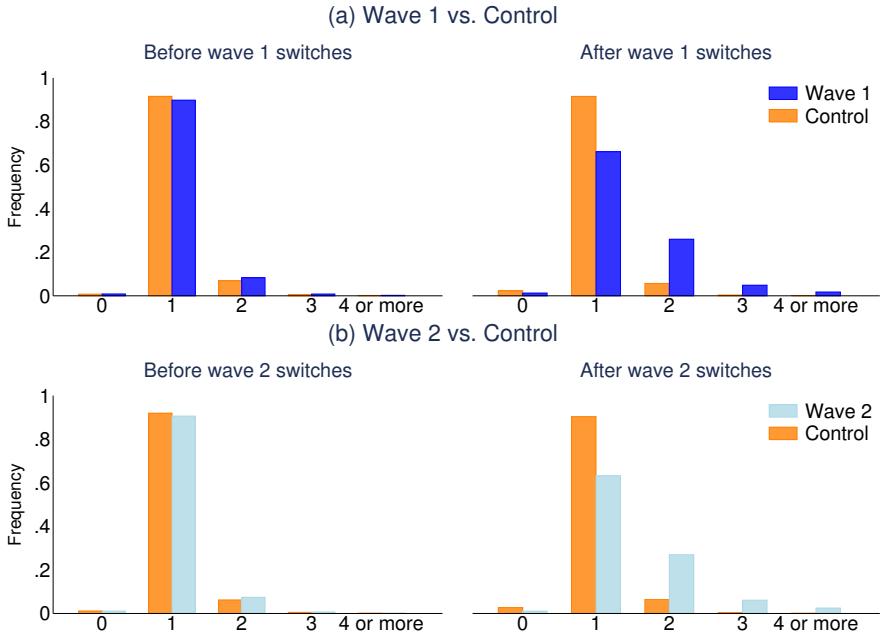

Sources: Administrative data from Bansefi on account balances and timing of card receipt.
Notes: (a) $N = 2{,}023{,}862$ from 171,441 accounts. (b) $N = 3{,}086{,}749$ from 270,046 accounts. The figure plots $\phi_k$ from (1). Average balance over each four-month period is the dependent variable, and is winsorized at the 95th percentile. Whiskers denote 95 percent confidence intervals. Black filled in circles indicate results that are significant at the 5 percent level, gray filled in circles at the 10 percent level, and hollow circles indicate results that are statistically insignificant from 0. The period prior to receiving the card is the omitted period, which is why its point estimate is 0 with no confidence interval.

Figure 6: Effect of Debit Cards on Stock of Savings by Distance to Wave 1 Localities



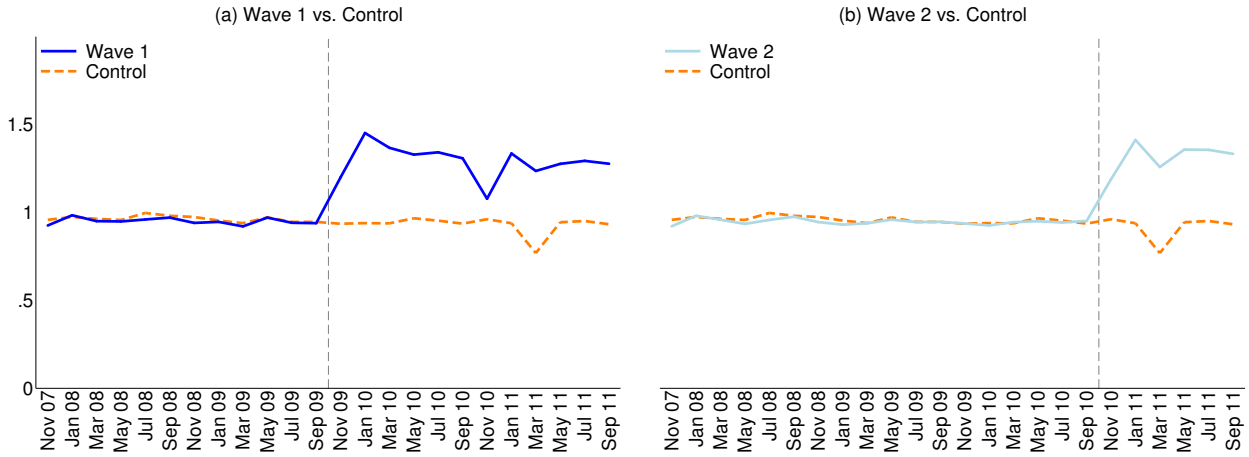

Sources: Administrative data from Bansefi on monthly average account balances and timing of card receipt.
Notes: The figure plots $\phi_k$ from (1), from separate regressions for wave 2 and control beneficiaries closer to a wave 1 locality (orange squares) and farther from a wave 1 locality (black circles). Average balance over each four-month period is the dependent variable, and is winsorized at the 95th percentile. Whiskers denote 95 percent confidence intervals. Black filled in circles or orange filled in squares indicate results that are significant at the 5 percent level, gray filled in circles or light orange filled in squares at the 10 percent level, and hollow circles or squares indicate results that are statistically insignificant from 0. The period prior to receiving the card is the omitted period, which is why its point estimate is 0 with no confidence interval.

Figure 7: Distribution of Withdrawals



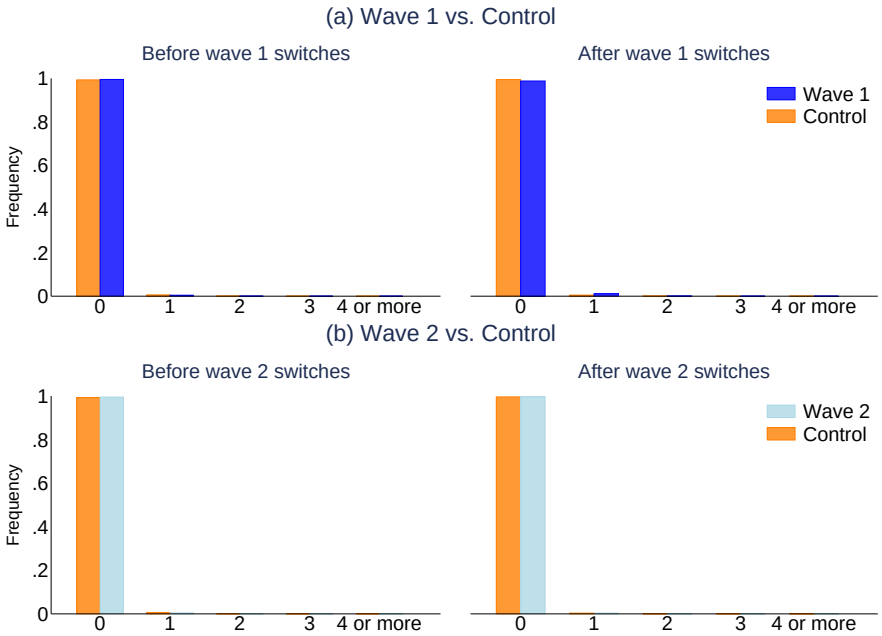

Sources: Administrative data from Bansefi on transactions and timing of card receipt.
Notes: $N$ = 14,594,799 transactions from 343,204 accounts. This figure shows the distribution of withdrawals from the accounts, before and after the switch to debit cards, for each of wave 1 and wave 2, compared to the control group.

Figure 8: Withdrawal to Deposit Ratio Over Time



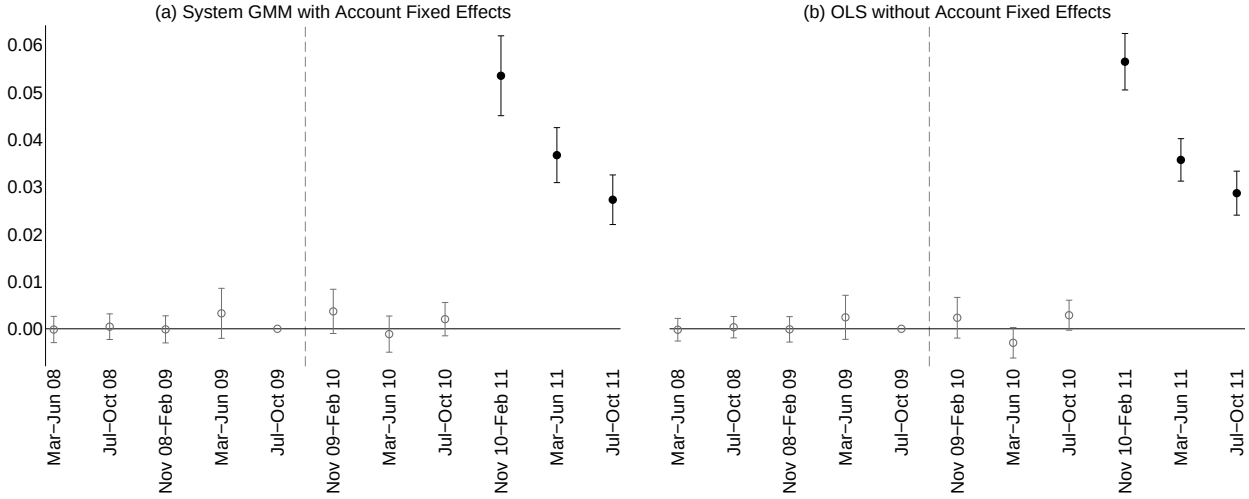

Sources: Administrative data from Bansefi on transactions and timing of card receipt.
Notes: $N = 14{,}594{,}799$ transactions from 343,204 accounts. The figure shows that the withdrawal to deposit ratio is equal to one before receiving the debit card: beneficiaries get a bi-monthly deposit from Oportunidades, which they withdraw with one transaction. Immediately after the reception of the card, beneficiaries increase their number of withdrawals, which stays constant thereafter.

Figure 9: Distribution of Client Deposits




Sources: Administrative data from Bansefi on transactions and timing of card receipt.
Notes: $N = 14{,}594{,}799$ transactions from 343,204 accounts. This figure shows the distribution of client deposits in the accounts, before and after the switch to debit cards, for each of wave 1 and wave 2, compared to the control group.

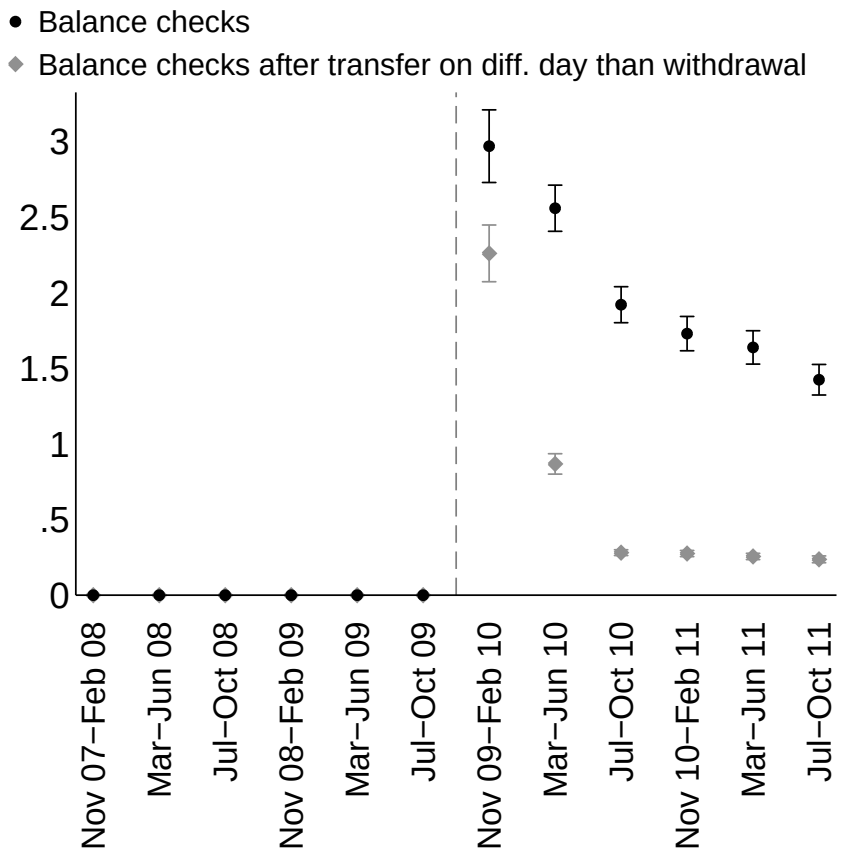
Figure 10: Effect of Debit Cards on Savings Rate (as Proportion of Income), Wave 1 vs. Control




Sources: Administrative data from Bansefi on average account balances by bimester, account transactions, and timing of card receipt.

Notes: $N = 1{,}852{,}416$ account-time observations from 171,441 accounts. The figure plots $\hat{\Phi}_k$ from (4). Panel (a) is from (3) estimated by Blundell and Bond (1998) two-step system GMM, while panel (b) is from (3) estimated using a standard fixed effects specification. Net balances and transfer amounts are winsorized at the 95th percentile. The variance of $\hat{\Phi}_k$ is estimated using the delta method. Whiskers denote 95 percent confidence intervals. Black filled in circles indicate results that are significant at the 5 percent level, gray filled in circles at the 10 percent level, and hollow circles indicate results that are statistically insignificant from 0. The period prior to receiving the card is the omitted period, which is why its point estimate is 0 with no confidence interval.

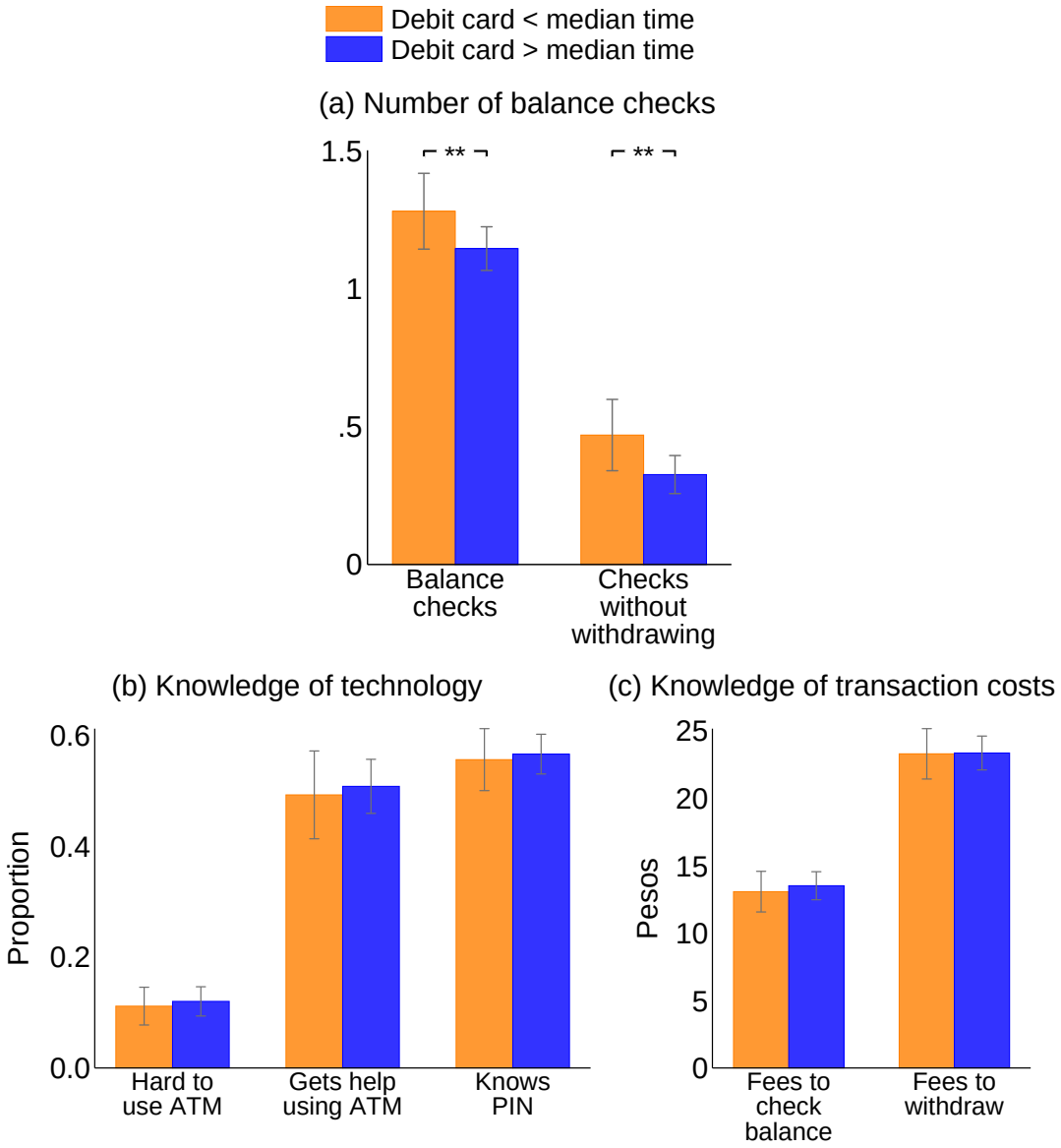
Figure 11: Number of Balance Checks Over Time in Administrative Data (Wave 1)




Source: Administrative transactions data from Bansefi.

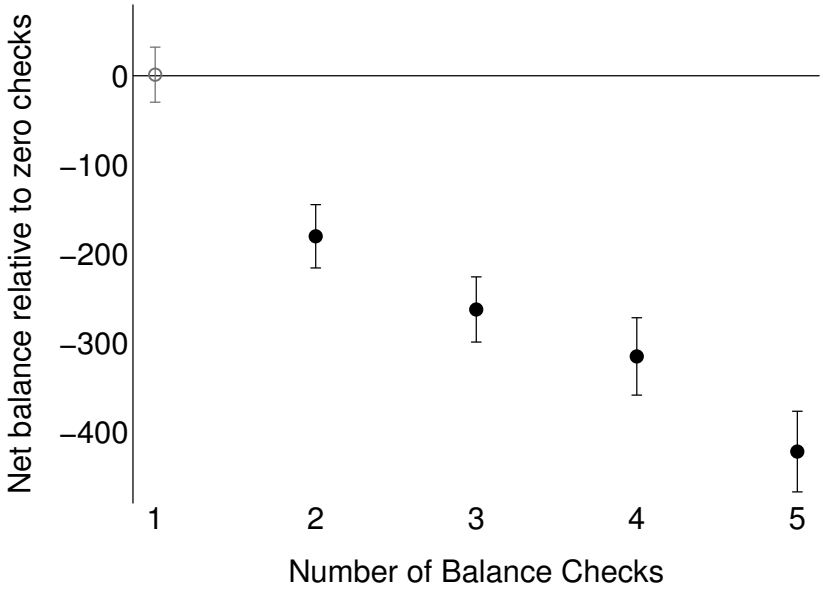
Notes: $N = 73{,}070$ accounts in Wave 1. This figure plots the number of balance checks per account tied to a debit card. The black dots shows the total number of balance checks. The grey diamonds show the subset of balance checks which occur after the transfer was received in a bimester, and on a different day than a withdrawal. This subset are balance checks that must occur to verify if existing balances remain in the account, instead of checking if a new transfer has arrived. The balance check data is not available prior to receiving the card, since it was only possible to check balances at Bansefi branches, which are not recorded in our transactions data. Balance check patterns in Wave 2 follow a similar decreasing pattern over time, though starting from lower average levels. Whiskers denote 95 percent confidence intervals.

Figure 12: Self-Reported Balance Checks and Knowledge



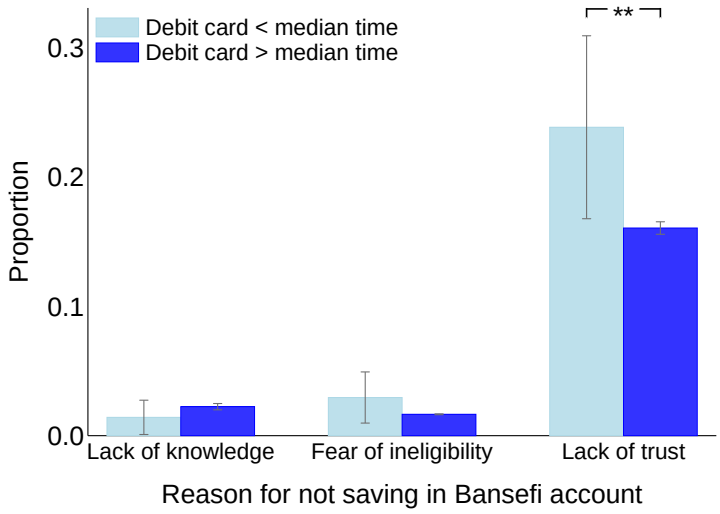

Source: Payment Methods Survey 2012.
Notes: $N = 1,617$, or less in some regressions if there were respondents who reported "don't know" or refused to respond. Balance checks are measured over the past bimester. Whiskers denote 95 percent confidence intervals. * indicates statistical significance at $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Figure 13: Within Account Relation Between Balance Checks and Net Balances (Wave 1)



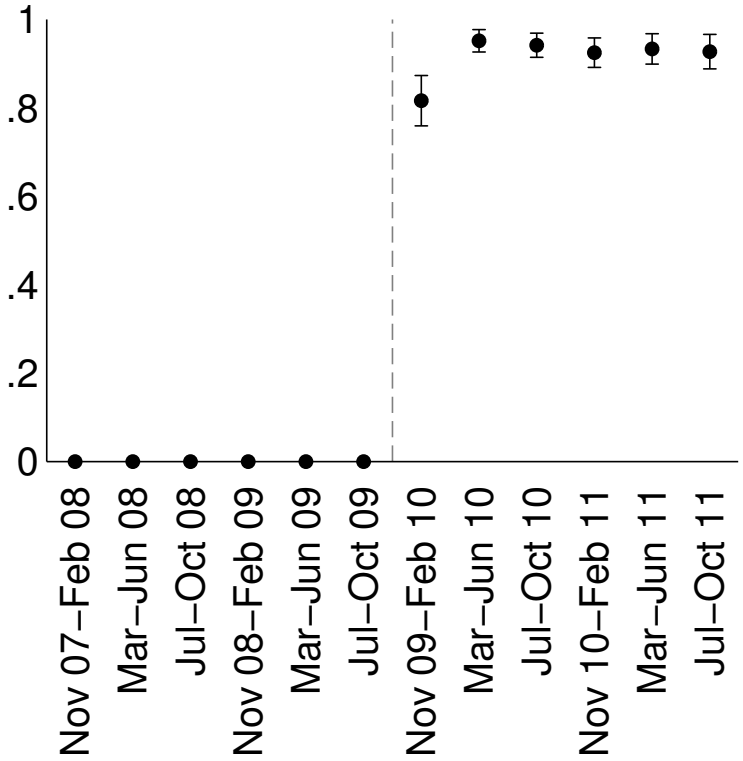

Source: Administrative data from Bansefi on transactions and average balances.
Notes: $N$ = 73,070 accounts in Wave 1. Standard errors are clustered at the locality level. The figure plots $\phi_k$ from (6). These coefficient show the within account net balance difference in pesos, relative to zero balance checks. Net balances are significantly lower when beneficiaries check balances more than once per bimester and the difference increases in the number of balance checks, providing support that balance checks are used to monitor the account and build trust. Whiskers denote 95 percent confidence intervals. Black filled in circles indicate results that are significant at the 5 percent level, gray filled in circles at the 10 percent level, and hollow circles indicate results that are statistically insignificant from 0.

Figure 14: Self-Reported Reasons for Not Saving in Bansefi Account



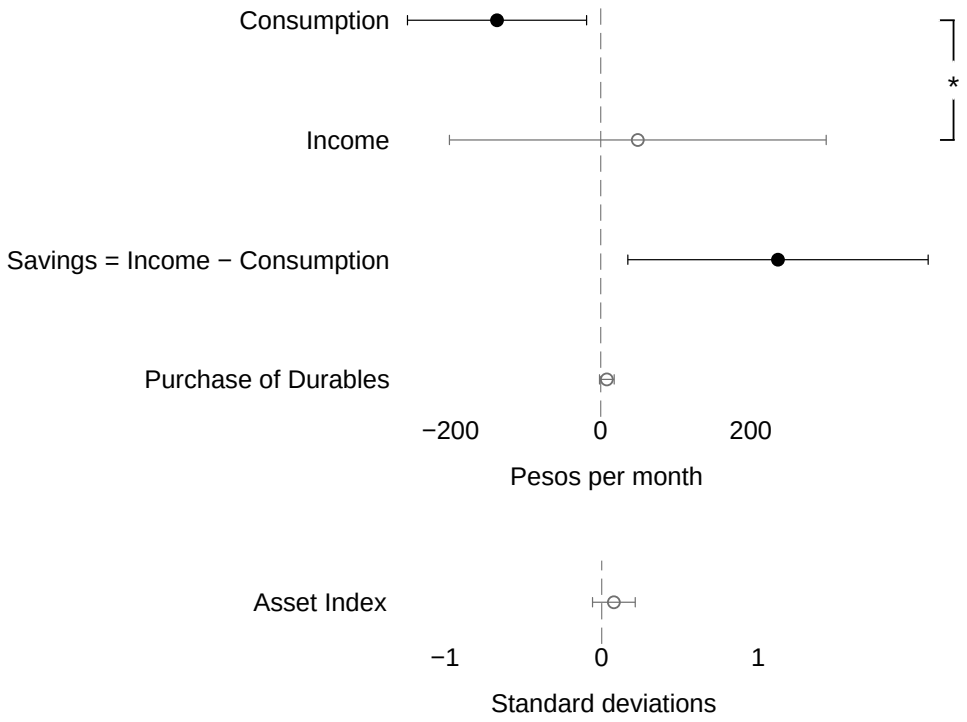

Source: ENCASDU 2010.
Notes: $N = 1,674$. Whiskers denote 95 percent confidence intervals. * indicates statistical significance at $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Figure 15: Share of Clients Using Debit Cards to Withdraw at ATMs or Convenience Stores over Time (Wave 1)



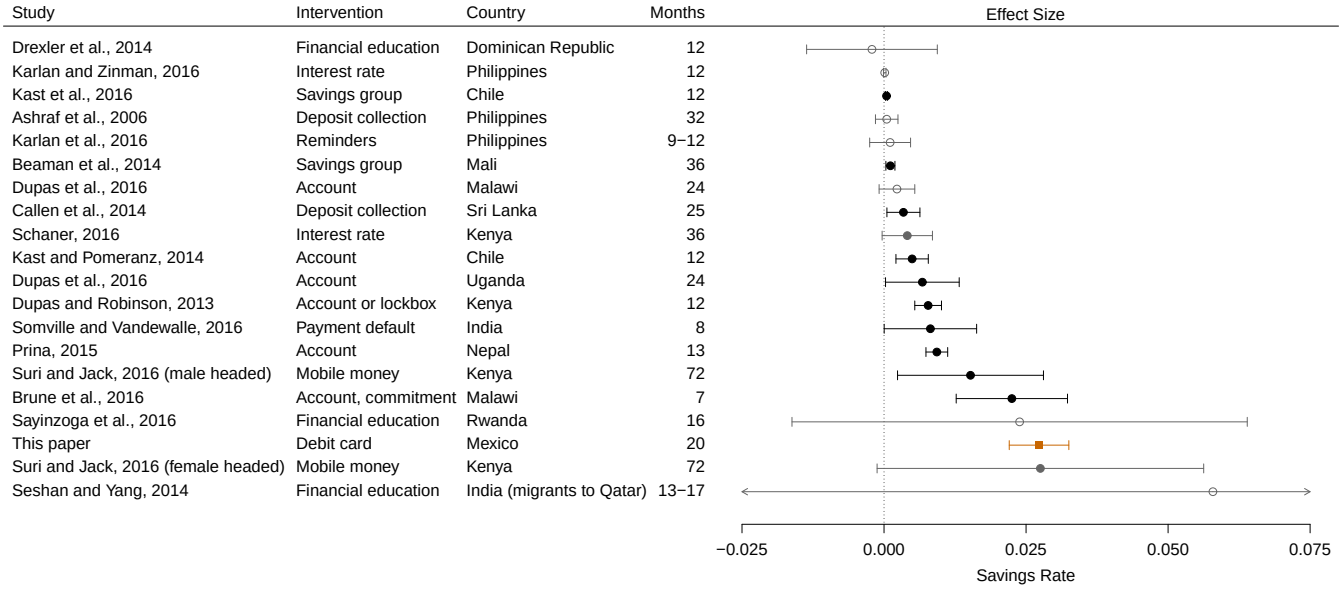

Source: Administrative transactions data from Bansefi.
Notes: The figure shows the share of clients using their debit card for at least one withdrawal or payment during a four month period. It shows that beneficiaries immediately adopt the new technology and use their cards to withdraw their transfers, instead of going to the Bansefi bank branch. Wave 2 displays the same pattern of immediate use of the cards to withdraw. Whiskers denote 95 percent confidence intervals.

Figure 16: Effect of the Debit Card from Household Survey Panel Data




Sources: ENCELURB panel survey combined with administrative data on timing of card receipt and transfer payment histories for each surveyed beneficiary household.
Notes: $N = 9,496$ (number of households $= 2,942$). Dependant variables are measured in pesos per month, with the exception of the asset index. Asset index is the first principal component of assets that are included in both the early (2002, 2003, 2004) and post-treatment (2009–2010) versions of the survey: car, truck, motorcycle, television, video or DVD player, radio or stereo, washer, gas stove, and refrigerator. Whiskers denote 95 percent confidence intervals. Black filled in circles indicate results that are significant at the 5 percent level, gray filled in circles at the 10 percent level, and hollow circles indicate results that are statistically insignificant from 0. The * linking consumption and income denotes that a test of equal coefficients from the consumption and income regressions is rejected at the 10 percent level using a stacked regression. Results are from the preferred specification of winsorizing variables at the 95th percentile (and 5th percentile for variables that do not have a lower bound of 0). Raw results, winsorized at 1 percent, winsorized at 5 percent, and winsorized at 5 percent with baseline household characteristics interacted with time fixed effects are available in Appendix Table B2. All regressions include household and time fixed effects, and standard errors are clustered at the locality level, using pre-treatment (2004) locality.

Figure 17: Comparison with Other Studies




Notes: For details on how we obtained the effect of savings interventions on the savings rate in each of these studies, as well as additional details about the studies, see Appendix E. Whiskers denote 95 percent confidence intervals. Black filled in circles indicate results that are significant at the 5 percent level, gray filled in circles at the 10 percent level, and hollow circles indicate results that are statistically insignificant from 0. Estimates from this study are orange filled-in squares.

Table 1: Comparison of Baseline Means

| Variable | Control | Wave 1 | Wave 2 | Diff. W1–C | Diff. W2–C | F-test p-value |
|---|---|---|---|---|---|---|
| *Panel A: Locality-level data* | | | | | | |
| Log population | 10.57 | 11.18 | 11.48 | 0.60*** | 0.91*** | 0.000*** |
| | (0.11) | (0.10) | (0.16) | (0.14) | (0.19) | |
| Bansefi branches per 100,000 | 1.27 | 1.23 | 1.58 | −0.03 | 0.32 | 0.411 |
| | (0.28) | (0.13) | (0.23) | (0.30) | (0.36) | |
| % HHs in poverty | 15.93 | 13.20 | 12.23 | −2.73 | −3.71* | 0.177 |
| | (1.67) | (0.75) | (1.09) | (1.82) | (1.99) | |
| Occupants per room | 1.18 | 1.11 | 1.12 | −0.07 | −0.06 | 0.260 |
| | (0.04) | (0.01) | (0.02) | (0.04) | (0.04) | |
| Number of localities | 44 | 143 | 88 | | | |
| *Panel B: Administrative bank account data* | | | | | | |
| Average balance | 581.25 | 670.32 | 614.29 | 89.07 | 33.05 | 0.112 |
| | (12.46) | (56.24) | (21.26) | (55.33) | (23.95) | |
| Number of deposits | 1.06 | 1.05 | 1.06 | −0.02 | −0.01 | 0.907 |
| | (0.01) | (0.04) | (0.03) | (0.04) | (0.03) | |
| Size of transfer | 1506.55 | 1809.50 | 1761.26 | 302.96*** | 254.71*** | 0.000*** |
| | (12.73) | (20.16) | (17.47) | (23.67) | (21.15) | |
| Number of withdrawals | 1.03 | 1.01 | 1.02 | −0.01 | −0.01 | 0.757 |
| | (0.01) | (0.03) | (0.02) | (0.03) | (0.02) | |
| Percent withdrawn | 98.56 | 97.50 | 99.64 | −1.06** | 1.08 | 0.021** |
| | (0.18) | (0.45) | (0.71) | (0.46) | (0.72) | |
| Years with account | 5.31 | 5.49 | 5.21 | 0.17 | −0.10 | 0.510 |
| | (0.08) | (0.15) | (0.25) | (0.17) | (0.26) | |
| Number of accounts | 97,922 | 73,070 | 171,717 | | | |

Sources: Census (2005), Bansefi branch locations (2008), poverty estimates from Oportunidades (based on 2005 Census), timing of card receipt by locality from Oportunidades, and administrative data from Bansefi.
Notes: HHs = households, W1 = wave 1, W2 = wave 2, C = control, Diff. = difference. For the administrative data from Bansefi, baseline is defined as January 2009 to October 2009 (prior to any accounts receiving cards in the data from Bansefi).

Table 2: Relationship between Trust and Savings Rates

| | (1) OLS | (2) 2SLS | (3) 2SLS |
|---|---|---|---|
| Coefficient | 0.001 | 0.028** | 0.029* |
| | (0.002) | (0.013) | (0.014) |
| First stage F-test for $Trust_{it}$ | | 40.0 | 18.1 |
| First stage F-test for $Trust_{it} \times Net\ Balance_{i,t-1}$ | | | 147.3 |
| First stage F-test for $Trust_{it} \times Transfers_{it}$ | | | 38.3 |
| Number of observations | 1330 | 1330 | 1330 |
| Lagged balance and transfers | No | No | Yes |

Sources: ENCASDU survey data merged with administrative bank account balance and transactions data from Bansefi.
Notes: $N = 1330$ beneficiary households merged with accounts. The specification for column 1 is (8) with OLS; the specification for column 2 is (8) with 2SLS, instrumenting trust with a set of dummies for timing of card receipt; column 3 is the specification from footnote 37 with 2SLS, instrumenting trust and its interactions with lagged net balance and transfers with a set of dummies for timing of card receipt and their interactions with lagged net balance and transfers. Coefficients are expressed as a proportion of income.

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# Online Appendices (NOT FOR PUBLICATION)

## Appendix A Sample of Materials Received by Beneficiaries

Figure A1: Flyer provided with the Debit Card (Front)

Bienvenido al mundo de tu *Tarjeta de Débito...*

0100, PRUEBA
0100, ALPINA
Río Magdalena 115, 01090
Alvaro Obregon, DF
0100, PRUEBA

010001000200

L@Red de la Gente
Un mundo que crece para ti

bansefi
Banco del Ahorro Nacional y Servicios Financieros S N C
4380 9910 0000 0200
VENCE 11/07
LUISA ARCOS DURAN
USO ELECTRÓNICO SOLAMENTE
VISA Electron

IMPORTANTE

1.- DESPRENDE tu Tarjeta de Débito.

2.- FÍRMALA en el espacio que se encuentra al reverso de tu Tarjeta de Débito donde se indica: **Firma Autorizada.**

3.- ACTÍVALA llamando al Tel.:
01 800 821 3822

4.- CONSERVA este documento. Contiene información importante que puedes utilizar en el futuro.

¡¡¡CUIDADO!!!

* Memoriza tu NIP (Número de Identificación Personal), que es tu clave secreta para hacer algunas operaciones.

* No proporciones tu NIP a nadie ni lo guardes junto con tu Tarjeta de Débito.

*** No le des tu número de Tarjeta de Débito a gente que te lo solicite sin razón.**

* Si no vas a utilizar tu tarjeta, consérvala en un lugar seguro.

* Reporta de inmediato el robo o extravío de tu Tarjeta de Débito, al Tel.:
01 800 821 3844

BANCO DEL AHORRO NACIONAL Y SERVICIOS FINANCIEROS, S. N. C., INSTITUCIÓN DE BANCA DE DESARROLLO, RÍO MAGDALENA No. 115, COL. TIZAPÁN SAN ÁNGEL, DELEG. ÁLVARO OBREGÓN, C. P. 06030, MÉXICO, D. F. CONMUTADOR 5481-3300

Notes: This flyer is provided by Oportunidades together with the debit card. The front of the flyer provides activation instructions and security tips regarding the PIN number and debit card.

Figure A2: Flyer provided with the Debit Card (Back)

L@Red de la Gente
Un mundo que crece para ti

USO EN CAJERO AUTOMÁTICO

Puedes realizar operaciones en cualquier cajero con logotipos RED PLUS

1. Introduce o desliza tu Tarjeta de Débito como lo indica el cajero automático.

2. Teclea tu NIP (Número de Identificación Personal) que te ha sido entregado.

3. Selecciona la operación que deseas realizar: Retiro, Consulta de Saldo, Cambio de NIP, Venta Genérica (tiempo aire para teléfonos celulares), etc.

4. Una vez que has realizado la operación, no olvides retirar tu Tarjeta de Débito y el comprobante de la operación realizada.

COMPRA O RETIRO DE EFECTIVO EN ESTABLECIMIENTO

Puedes realizar compras en cualquier establecimiento afiliado a VISA ELECTRON.

1. Al pagar en un establecimiento con Tarjeta de Débito, no la pierdas de vista.

2. Cuando te entreguen el voucher (comprobante de pago), verifica que la cantidad impresa sea la misma de tu compra.

3. Firma tu voucher. No permitas que impriman más de un voucher.

4. Conserva tus vouchers para confirmar las operaciones que has realizado con tu Tarjeta de Débito.

5. Con tu Tarjeta de Débito puedes retirar efectivo de tu cuenta en Gigante, Comercial Mexicana y WalMart. Entrega tu tarjeta al cajero (a) y solicita la cantidad que deseas retirar.

Paga con tu tarjeta y gana de Boletazo

Notes: The back of the flyer provides instructions on using the card to withdraw money at ATMs and to make purchases. It clarifies that the card can be used to withdraw money at any ATM within the networks "RED" and "PLUS" (which cover almost all ATMs in Mexico) and at major grocery stores.

Figure A3: Sample Calendar of Transfer Dates Given to Beneficiaries

Oportunidades

**Calendario Fijo de Retiro de Apoyos Monetarios**

GOBIERNO FEDERAL SEDESOL

Entidad: **15 MEXICO**

Zona de Atención: **150303**

Municipio: **33 ECATEPEC DE MORELOS**

Localidad: **1 ECATEPEC DE MORELOS**

AGEB: [redacted] Código Postal: **55450**

Domicilio: [redacted]

Folio Titular: [redacted]

Nombre Titular: [redacted]

Identificador de Familia: [redacted]

Fase de Incorporación: **35**

Esquema de Apoyos: **Urbano 1**

Colonia: [redacted]

**Estimada Titular:**

| Los apoyos del bimestre de corresponsabilidad | los puede retirar a partir del |
|---|---|
| Noviembre - Diciembre del 2008 | Lunes 20 de Abril del 2009 |
| Enero - Febrero del 2009 | Lunes 1 de Junio del 2009 |
| Marzo - Abril del 2009 | Lunes 13 de Julio del 2009 |
| Mayo - Junio del 2009 | Lunes 14 de Septiembre del 2009 |
| Julio - Agosto del 2009 | Lunes 16 de Noviembre del 2009 |
| Septiembre - Octubre del 2009 | Lunes 11 de Enero del 2010 |

Bimestre de Generación de Calendario: **Corresponsabilidad Noviembre - Diciembre del 2008**

**Titular beneficiaria: Usted podrá retirar sus apoyos con su Tarjeta de Débito a partir de la fecha indicada en cajeros automáticos ó establecimientos autorizados (que aceptan tarjetas VISA).**

**Recuerde que en cajeros automáticos podrá realizar dos operaciones (retiros ó consultas) gratuitas al bimestre, también puede utilizar su Tarjeta para comprar en establecimientos que aceptan Tarjetas de Débito VISA.**

Para mayor Información, consultas, dudas ó quejas, comunicarse al 01800 500 50 50 de lunes a viernes de 9 de la mañana a 6 de la tarde.

"Este programa es público, ajeno a cualquier partido político. Queda prohibido el uso para fines distintos al desarrollo social."

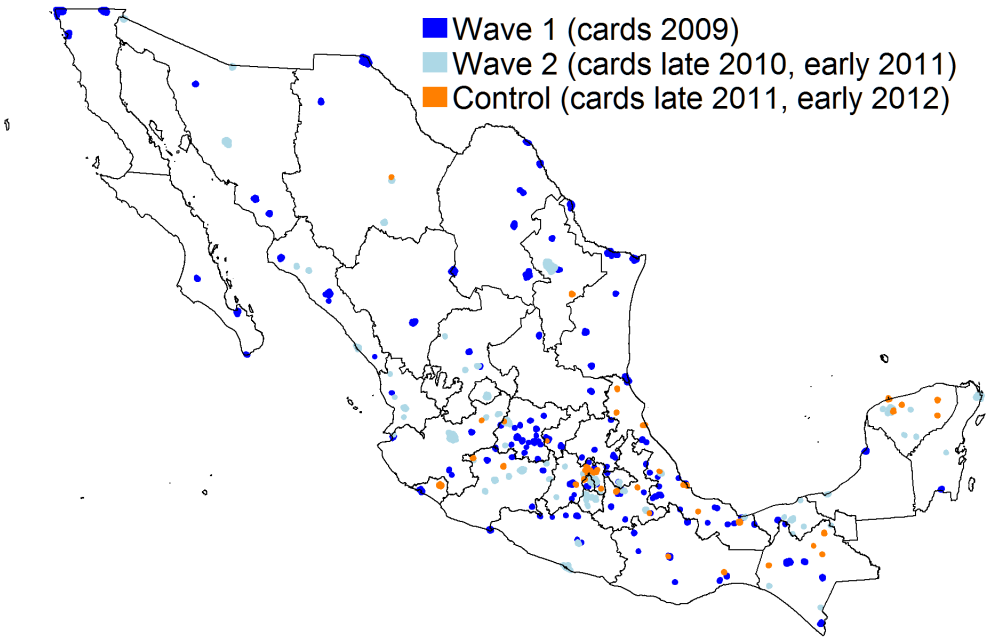
"Este programa es de carácter público, no es patrocinado ni promovido por partido político alguno y sus recursos provienen de los impuestos que pagan todos los contribuyentes. Está prohibido el uso de este programa con fines políticos, electorales, de lucro y de otros distintos a los establecidos. Quien haga uso indebido de los recursos de este programa deberá ser denunciado y sancionado de acuerdo a la ley aplicable y ante la autoridad competente."

Consecutivo: **1005**

Notes: The sample calendar provides the transfer dates to recipients. For each bimester in the following year, it states the corresponding payment date. It reminds recipients that they should use their debit cards after the indicated date at ATMs or establishments accepting VISA. It also reminds them that they are allowed two free transactions per bimester at ATMs.

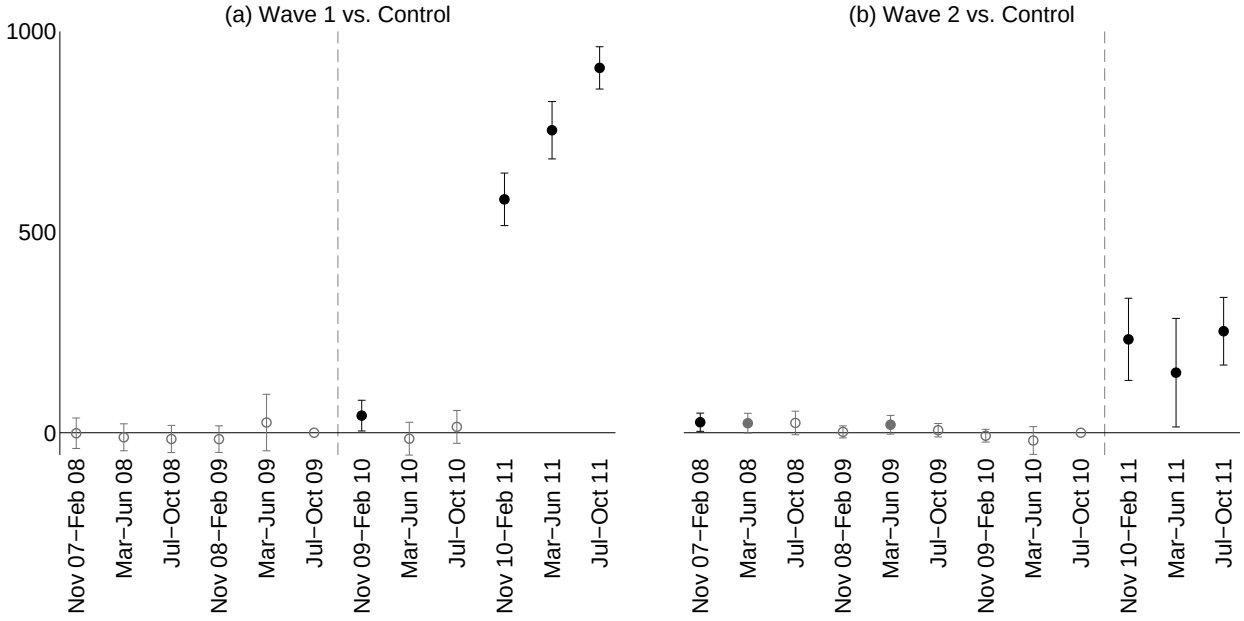
## Appendix B Additional Figures and Tables

Figure B1: Geographic Coverage and Expansion of Debit Cards



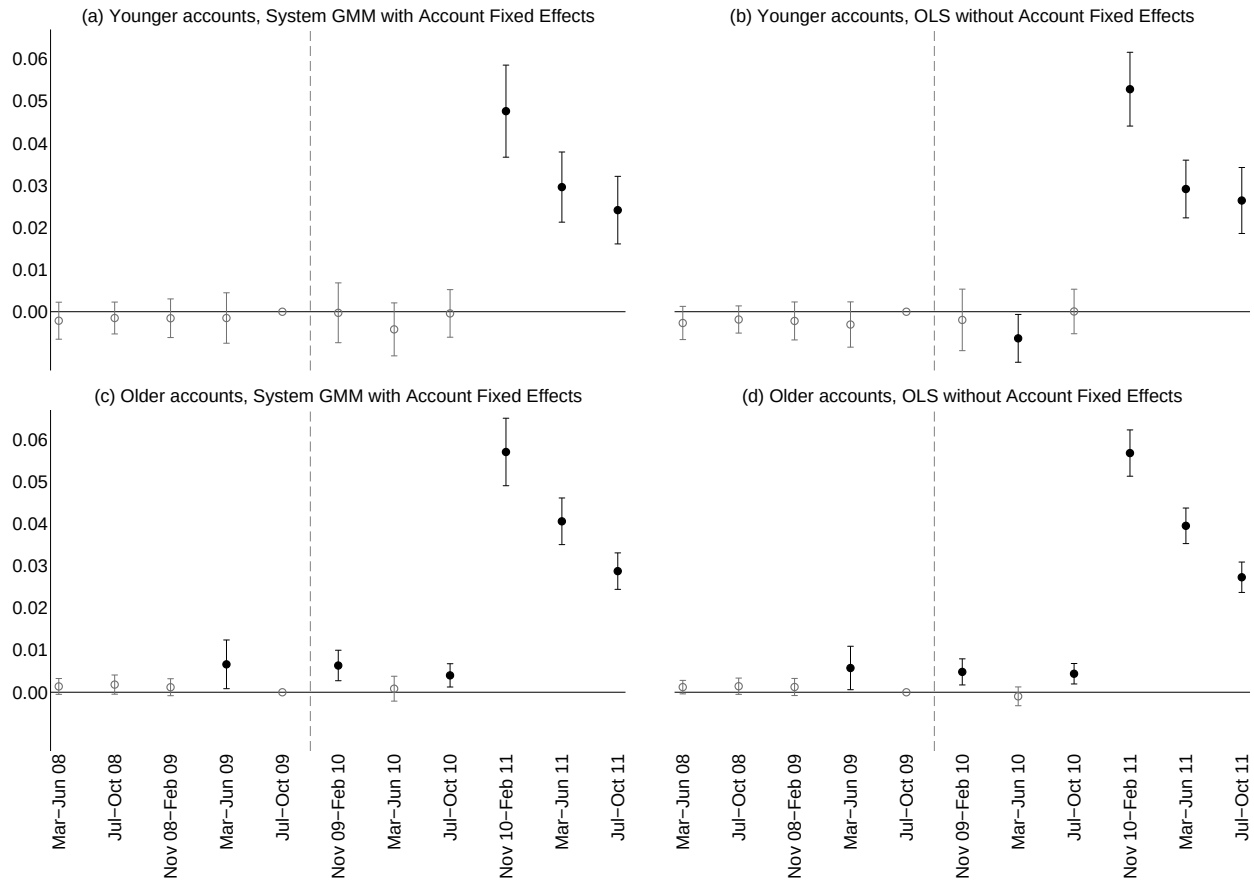

Sources: Administrative data from Oportunidades on timing of debit card receipt by locality and shape files from INEGI.
Notes: $N = 275$ localities (44 in control, 143 in wave 1, 88 in wave 2). The area of each urban locality included in the study is shaded according to its wave of treatment. Urban localities that were not included in the Oportunidades program at baseline or were included in the program but did not pay beneficiaries through Bansefi savings accounts are not included in the figure or in our study.

Figure B2: Difference between Treatment and Control in Net Balances




Sources: Administrative data from Bansefi on average account balances by bimester, timing and amount of transfer payments, timing and amount of withdrawals, and timing of card receipt.
Notes: (a) $N = 2{,}023{,}862$ from 171,441 accounts. (b) $N = 3{,}086{,}749$ from 270,046 accounts. Net balances refer to average balances minus the mechanical effect on average balance of leaving a portion of the deposit in the account for a certain number of days before withdrawing it. The figure plots $\phi_k$ from (1). Average balance over each four-month period is the dependent variable, and is winsorized at the 95th percentile. Whiskers denote 95 percent confidence intervals. Black filled in circles indicate results that are significant at the 5 percent level, gray filled in circles at the 10 percent level, and hollow circles indicate results that are statistically insignificant from 0. The period prior to receiving the card is the omitted period, which is why its point estimate is 0 with no confidence interval.

Figure B3: Separated by Time with Account: Effect of Debit Cards on Savings Rate (as Proportion of Income), Wave 1 vs. Control




Sources: Administrative data from Bansefi on average account balances by bimester, transfer payments, and timing of card receipt.

Notes: (a) $N = 743{,}776$ from 99,362 accounts; (b) $N = 905{,}335$ from 118,228 accounts; (c) $N = 455{,}172$ from 79,511 accounts; (d) $N = 1{,}088{,}677$ from 157,717 accounts. See the notes to Figure 10 for the specification. Accounts are split into older accounts and younger accounts based on the median account opening date, which is October 19, 2004.

Table B1: Balance test in ENCASDU

| | (1) Mean for Card > Median Time | (2) Difference for Card < Median Time | (3) P-value of Difference |
|---|---|---|---|
| Number of household members | 5.18 | 0.26 | 0.114 |
| | (0.08) | (0.15) | |
| Number of children | 2.19 | 0.03 | 0.743 |
| | (0.08) | (0.10) | |
| Age of household head | 44.73 | 0.96 | 0.246 |
| | (0.08) | (0.80) | |
| Household head is male | 0.67 | 0.02 | 0.603 |
| | (0.03) | (0.03) | |
| Household head is married | 0.70 | 0.02 | 0.459 |
| | (0.04) | (0.03) | |
| Education level of head | 9.30 | −0.33 | 0.092* |
| | (0.16) | (0.18) | |
| Occupants per room | 3.50 | −0.03 | 0.801 |
| | (0.07) | (0.11) | |
| Access to health insurance | 0.59 | 0.05 | 0.165 |
| | (0.02) | (0.03) | |
| Asset index | 0.04 | −0.04 | 0.605 |
| | (0.04) | (0.08) | |
| Income | 3190.32 | 222.69 | 0.150 |
| | (47.40) | (146.67) | |

Table B2: Change in Savings and Assets After Receiving Card

| | (1) | (2) | (3) | (4) | Mean |
|---|---|---|---|---|---|
| Consumption | –178.11** | –153.96** | –138.09** | –143.63** | 2731.20 |
| | (80.15) | (69.49) | (60.86) | (62.11) | (82.81) |
| Income | 78.98 | 85.09 | 49.44 | 46.28 | 3148.28 |
| | (168.11) | (149.46) | (128.00) | (130.40) | (89.02) |
| P-value Consumption vs. Income | [0.058]* | [0.055]* | [0.092]* | [0.103] | |
| Savings = Income – Consumption | 257.09* | 243.20** | 236.16** | 243.75** | 412.17 |
| | (132.50) | (118.50) | (102.04) | (108.26) | (103.32) |
| Purchase of durables | 9.77 | 8.64 | 8.20 | 7.54 | 32.98 |
| | (12.41) | (8.61) | (4.99) | (4.98) | (3.32) |
| Asset index | 0.06 | 0.06 | 0.08 | 0.07 | 0.48 |
| | (0.08) | (0.08) | (0.07) | (0.07) | (0.10) |
| Number of households | 2,942 | 2,942 | 2,942 | 2,929 | |
| Number of observations | 9,496 | 9,496 | 9,496 | 9,469 | |
| Time fixed effects | Yes | Yes | Yes | Yes | |
| Household fixed effects | Yes | Yes | Yes | Yes | |
| Household characteristics × time | No | No | No | Yes | |
| Winsorized | No | 1% | 5% | 5% | 5% |

Notes: Each row label is the dependent variable from a separate regression; each column is a different specification. The "Mean" column shows the mean of the dependent variable for the control group, winsorized at 5%. * indicates statistical significance at $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Standard errors are clustered at the locality level, using pre-treatment (2004) locality. Dependant variables are measured in pesos per month, with the exception of the asset index. Asset index is the first principal component of assets that are included in both the early (2002, 2003, 2004) and post-treatment (2009–2010) versions of the survey: car, truck, motorcycle, television, video or DVD player, radio or stereo, washer, gas stove, and refrigerator. Household characteristics are measured at baseline (2004, or for households that were not included in the 2004 wave, 2003). They include characteristics of the household head (working status, a quadratic polynomial in years of schooling, and a quadratic polynomial in age), whether anyone in the household has a bank account, a number of characteristics used by the Mexican government to target social programs (the proportion of household members with access to health insurance, the proportion age 15 and older that are illiterate, the proportion ages 6-14 that do not attend school, the proportion 15 and older with incomplete primary education, the proportion ages 15-29 with less than 9 years of schooling), and dwelling characteristics (dirt floors, no bathroom, no piped water, no sewage, and number of occupants per room). The number of households in column (4) is slightly lower because 13 households have missing values for one of the household characteristics included (interacted with time fixed effects) in that specification.

Table B3: Supply-Side Response

| | Total | | Bansefi | |
|---|---|---|---|---|
| | ATMs | Branches | ATMs | Branches |
| Current quarter | −1.52 | 0.03 | −0.01 | −0.01 |
| | (4.14) | (0.30) | (0.01) | (0.02) |
| 1 quarter lag | 0.01 | 0.02 | −0.02 | 0.02 |
| | (4.11) | (0.34) | (0.01) | (0.02) |
| 2 quarter lag | −10.83 | 0.08 | 0.01 | 0.01 |
| | (5.64) | (0.36) | (0.03) | (0.01) |
| 3 quarter lag | −5.42 | 0.08 | −0.03 | 0.02 |
| | (2.98) | (0.26) | (0.02) | (0.02) |
| 4 quarter lag | −0.74 | 0.42 | 0.00 | −0.03 |
| | (5.97) | (0.50) | (0.01) | (0.03) |
| 1 quarter lead | −1.10 | −0.12 | −0.01 | 0.00 |
| | (3.66) | (0.36) | (0.00) | (0.02) |
| 2 quarter lead | −6.09 | 0.25 | 0.00 | 0.01 |
| | (4.90) | (0.34) | (0.02) | (0.01) |
| 3 quarter lead | −7.84 | 0.25 | −0.03 | −0.01 |
| | (8.00) | (0.65) | (0.01) | (0.03) |
| 4 quarter lead | 7.58 | 0.59 | −0.01 | −0.06 |
| | (10.32) | (0.94) | (0.03) | (0.05) |
| Mean control group | 198.29 | 36.87 | 0.49 | 1.41 |
| F-test of lags | 1.26 | 0.20 | 0.68 | 0.96 |
| [p-value] | [0.29] | [0.94] | [0.61] | [0.43] |
| F-test of leads | 0.69 | 0.44 | 0.79 | 0.67 |
| [p-value] | [0.60] | [0.78] | [0.53] | [0.62] |
| Municipality fixed effects | Yes | Yes | Yes | Yes |
| Quarter fixed effects | Yes | Yes | Yes | Yes |

Notes: * indicates statistical significance at $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. The table shows $\beta_k$ from

$$y_{jt} = \lambda_j + \delta_t + \sum_{k=-4}^{4} \beta_k D_{j,t+k} + \varepsilon_{jt}$$

where $y_{jt}$ is the number of ATMs or bank branches of any bank or of Bansefi in municipality $j$ during quarter $t$, $D_{jt} = 1$ if municipality $j$ has at least one locality with Oportunidades debit cards in quarter $t$. The F-test of lags tests $\beta_{-4} = \cdots = \beta_{-1} = 0$; the F-test of leads tests $\beta_1 = \cdots = \beta_4 = 0$.

Table B4: Parallel Trends in Consumption, Income, Savings, Assets

| Dependent variable | p-value |
|---|---|
| Consumption | 0.322 |
| Income | 0.159 |
| Savings = Income – Consumption | 0.176 |
| Purchase of Durables | 0.269 |
| Asset Index | 0.398 |
| Number of households | 2,942 |
| Number of observations | 9,496 |
| Household fixed effects | Yes |
| Time fixed effects | Yes |
| Winsorized | 5% |

Notes: Table shows p-values from an F-test of $\omega_k = 0 \; \forall \; k < 2009$ (where $k = 2002$ is the reference period and is thus omitted) from

$$y_{it} = \lambda_i + \delta_t + \sum_k \omega_k T_{j(i)} \times \mathbb{I}(k = t) + \eta_{it}$$

Dependant variables are measured in pesos per month, with the exception of the asset index. Asset index is the first principal component of assets that are included in both the early (2002, 2003, 2004) and post-treatment (2009–2010) versions of the survey: car, truck, motorcycle, television, video or DVD player, radio or stereo, washer, gas stove, and refrigerator.

## Appendix C Mechanical Effect

This appendix defines the "mechanical effect", which we use to compute net average balances. We explain the logic behind the mechanical effect, provide a step by step guide for its computation, summarized in Table C1, and present an example.

### C.1 Logic of the Mechanical Effect

The mechanical effect is the contribution to average balances from the transit of transfers in recipients' accounts. Since the mechanical effect does not represent net (long-term) desired savings, our goal is to net it out from average balances and construct our measure of Net Balances. Changes in the mechanical effect can arise due to changes in the *frequency* of withdrawals. For example, if client A receives \$2,000 in his account, and withdraws \$1,000 on the first day of the term, and the other \$1,000 midway through the term, his average balance will equal $1,000 * 0 + 1,000 * \frac{1}{2} = \$500$. Compared to client B who withdrew the entire \$2,000 on the first day of the term, client A's average balance is \$500 higher, but their net average balance, after subtracting the mechanical effect, are both equal to zero. Changes in the mechanical effect can also arise from changes in the *timing* of withdrawals, compared to the deposit dates. The deposit date is usually known by the recipients: Oportunidades disburses transfers within the first week of the bimester, and Oportunidades distributes calendars, stating the dates when accounts are due to be credited. In some instances, Oportunidades deposited the transfers to the accounts several days in advance, in order to avoid backlogs. In our data the mechanical effect increases for debit card recipients as a result of increased withdrawal frequency of smaller amounts and increased time between the deposit and first withdrawal. Finally we need to compare not only the timing of deposits and withdrawals, but also their relative sizes. Although the calculation is simple there are several cases to consider depending on the number of withdrawals, when they occur and whether they outsize deposits. We think the steps are best exemplified with an example of one of those cases.

### C.2 Example:

1. Select a pattern where clients received a single deposit
2. Select a pattern with one deposit followed by two withdrawals (DWW)
3. The pattern with one deposit and two withdrawals (DWW), must fit in one of the following three scenarios: (a) the deposit is less than the first withdrawal ($W_1 \geq D$), (b) the deposit is larger than the first withdrawal but smaller than the sum of the two withdrawals ($W_1 < D$ & $W_1 + W_2 \geq D$), (c) the deposit is larger than the sum of withdrawals ($W_1 + W_2 < D$).
4. Compute the mechanical effect, at the individual level, for each of the three scenarios discussed above:

- (a) The deposit is less than the first withdrawal ⇒ The mechanical effect is just the time lapse between the deposit and the first withdrawal times the deposit amount ($ME = lapse_{DW_1} * Deposit$ ).
- (b) The deposit is larger than the first withdrawal but smaller than the sum of the two withdrawals ⇒ The mechanical effect is the time lapse between the deposit and the first withdrawal times the amount of the first withdrawal, plus the time lapse between the deposit and the second withdrawal times the remaining deposit amount after subtracting the first withdrawal ($lapse_{DW_1} * Withdrawal_1 + lapse_{DW_2} * (Deposit - Withdrawal_1)$).
- (c) The deposit is larger than the sum of the withdrawals ⇒ The mechanical effect is the time lapse between the deposit and the first withdrawal times the amount of the first withdrawal, plus the time lapse between the deposit and the second withdrawal times the amount of the second withdrawal ($lapse_{DW_1} * Withdrawal_1 + lapse_{DW_2} * (Withdrawal_2)$).

Table B1 shows the cases we considered as well as their prevalence in the data.

Table C1: Computation of Mechanical Effect

| | Pattern | % Total | Conditions | Mechanical Effect |
|---|---|---|---|---|
| *Panel A. Regular patterns: single deposit into account in the bimester* | | | | |
| (1) | DW | 73.4% | $W \leq D$ | $lapse_{DW} * Withdrawal$ |
| | | | $W > D$ | $lapse_{DW} * Deposit$ |
| (2) | DWW | 9.1% | $W_1 \geq D$ | $lapse_{DW_1} * Deposit$ |
| | | | $W_1 < D$ & $W_1 + W_2 \geq D$ | $lapse_{DW_1} * Withdrawal_1 + lapse_{DW_2} * (Deposit - Withdrawal_1)$ |
| | | | $W_1 + W_2 < D$ | $lapse_{DW_1} * Withdrawal_1 + lapse_{DW_2} * (Withdrawal_2)$ |
| (3) | DWWW | 1.7% | $W_1 \geq D$ | $lapse_{DW_1} * Deposit$ |
| | | | $W_1 < D$ & $W_1 + W_2 \geq D$ | $lapse_{DW_1} * Withdrawal_1 + lapse_{DW_2} * (Deposit - Withdrawal_1)$ |
| | | | $W_1 + W_2 < D$ & $W_1 + W_2 + W_3 \geq D$ | $lapse_{DW_1} * Withdrawal_1 + lapse_{DW_2} * Withdrawal_2$ |
| | | | | $+lapse_{DW_3} * (Deposit - Withdrawal_1 - Withdrawal_2)$ |
| *Panel B. Irregular patterns: multiple deposits into account in the bimester* | | | | |
| (4) | DDWW | 3.1% | $W_1 \leq D_1$ & $W_2 \leq D_2$ | $lapse_{D_1W_1} * Withdrawal_1 + lapse_{D_2W_2} * Withdrawal_2$ |
| | | | $W_1 > D_1$ & $W_2 \leq D_2$ | $lapse_{D_1W_1} * Deposit_1 + lapse_{D_2W_2} * Withdrawal_2$ |
| | | | $W_1 \leq D_1$ & $W_2 < D_2$ | $lapse_{D_1W_1} * Withdrawal_1 + lapse_{D_2W_2} * Deposit_2$ |
| | | | $W_1 > D_1$ & $W_2 > D_2$ | $lapse_{D_1W_1} * Deposit_1 + lapse_{D_2W_2} * Deposit_2$ |
| (5) | DWD | 3.0% | $W \leq D$ | $lapse_{DW} * Withdrawal$ |
| | | | $W > D$ | $lapse_{DW} * Deposit$ |
| (6) | DDW | 2.7% | $W \geq D_1 + D_2$ | $lapse_{D_1W} * Deposit_1 + lapse_{D_2W} * Deposit_2$ |
| | | | $W < D_1 + D_2$ & $W \leq D_2$ | $lapse_{D_1W} * (Withdrawal - Deposit_2) + lapse_{D_2W} * Deposit_2$ |
| | | | $W < D_2$ | $lapse_{D_2W} * Withdrawal$ |
| (7) | DWDW | 1.6% | $W_1 \leq D_1$ & $W_2 \leq D_2$ | $lapse_{D_1W_1} * Withdrawal_1 + lapse_{D_2W_2} * Withdrawal_2$ |
| | | | $W_1 > D_1$ & $W_2 \leq D_2$ | $lapse_{D_1W_1} * Deposit_1 + lapse_{D_2W_2} * Withdrawal_2$ |
| | | | $W_1 \leq D_1$ & $W_2 < D_2$ | $lapse_{D_1W_1} * Withdrawal_1 + lapse_{D_2W_2} * Deposit_2$ |
| | | | $W_1 > D_1$ & $W_2 > D_2$ | $lapse_{D_1W_1} * Deposit_1 + lapse_{D_2W_2} * Deposit_2$ |

$D_i$ indicate the $i^{th}$ deposit and $W_i$ indicate the $i^{th}$ withdrawal within a bimester. $lapse_{D_iW_j}$ measures the number of days between the $i^{th}$ deposit and the $j^{th}$ withdrawal, divided by the number of days in the bimester. The patterns listed here represent 95% of all bimonthly patterns, but all patterns containing above 0.01% have been coded to obtain an estimate of the mechanical effect.

### C.3 Steps

More generally we follow the steps below:

1. We separate the sample based on the number of transfers received by Opportunidades' beneficiaries: 85% of beneficiaries received a single transfer in the bimester and 15% received two transfers in the same bimester. Multiple transfers per bimester can result from administrative decisions impacting the entire program[1], and from beneficiaries' behavior. For example, transfers are withheld for beneficiaries who fail to fulfill the eligibility conditions in a specific period; once the conditions are met again, they receive transfer backload as a separate deposit.

2. We determine the pattern of transactions: for example, a beneficiary who first received a deposit and then performed two withdrawals has a sequence $(Deposit, Withdrawal_1, Withdrawal_2)$.

3. We compare the size of the deposit to the withdrawals, and generate different scenarios. These scenarios depend on the relative size of the deposit and withdrawals: each withdrawal could be larger than the deposit, their sum might be larger, or the deposit is always larger than the sum of withdrawals.

4. We compute the mechanical effect. To this purpose we measure the lapse of time, in days, which passes between the deposit and each withdrawal, and multiply the time lapses by the amount of the transfer which only transited through the account, and was not kept on the account the whole bimester.

## Appendix D Details on the GMM estimation

In this section we clarify some details of the GMM estimation. First, standard errors of the parameters in (3) are clustered at the bank branch level and corrected for finite sample bias following Windmeijer (2005); the formula for the variance of $\Phi_k$ is then approximated using the delta method.

Because lags of lagged net balance and its interactions are used as GMM-style instruments in the system GMM estimator, including $Net\ Balance_{i,t-1} \times \mathbb{I}(k = t)$ and $Net\ Balance_{i,t-1} \times T_{j(i)} \times \mathbb{I}(k = t)$ terms leads to a proliferation of instruments and an over-parameterized model. As a result, for the lagged balance terms we create three groups of time dummies (focusing on wave 1, since we do not have enough post-treatment periods in wave 2 to explore dynamic effects): pre-treatment, post-treatment learning period, and post-learning period, and interact these (rather than $\mathbb{I}(k = t)$ time dummies for each $k$) with lagged net balance. We thus estimate the dampening effect of lagged net balance in the control group during these three broad periods ($\theta_1$, $\theta_2$, and $\theta_3$), and the difference in the dampening effect of lagged net balance between the treatment and control group during the three broad periods ($\xi_1$, $\xi_2$, and $\xi_3$).

Following the best reporting practices outlined in Roodman (2009a), the details of our two-step system GMM estimation are as follows. Lagged balance is used as an endogenous GMM-style

[1] For example, payments that are due during election periods are shifted to earlier bimesters.

instrument; because bias can increase in finite samples as $T$ increases (since this leads to more lags and, hence, more instruments: see Ziliak, 1997), to reduce the number of instruments we collapse instruments (as in Beck and Levine, 2004), following the procedure described in Roodman (2009b). Because $Transfers_{it}$ is predetermined but not strictly exogenous, variables on the right hand side of (3) interacted with $Transfers_{it}$ are valid instruments in the system's equation in levels, but not the equation in differences; as a result, we include time dummies and all interaction terms on the right-hand side of (3) as IV-style instruments in the system's equation in levels, and time dummies and interaction terms excluding those interacted with $Transfers_{it}$ in the equation in differences. These specification choices result in a total instrument count of 88. Because our panel does not include gaps, we use first differencing—as in Blundell and Bond (1998)—rather than the sample-maximizing forward orthogonal deviations—as in Arellano and Bover (1995)—to eliminate fixed effects in the transformed equation to be estimated.

Our motivating precautionary saving model makes a number of testable predictions about the dynamic effect of lagged balance on the flow of savings, i.e., the $\theta_k$ and $\xi_k$ terms from (3). As described above, including $Net\ Balance_{i,t-1} \times \mathbb{I}(k = t)$ and $Net\ Balance_{i,t-1} \times T_{j(i)} \times \mathbb{I}(k = t)$ terms for all $k$ leads to a proliferation of instruments in the GMM estimation, s o we estimate the dampening effect of lagged net balance during three broad periods: pre-treatment, post-treatment pre-learning, and post-learning. For these three broad periods, the coefficients $\theta_p$ for $p \in \{1, 2, 3\}$ give the dampening effect for the control group, and $\xi_p$ give the differential dampening effect for the treatment group relative to control.

In the control group, the households have not been given a technology to monitor and build trust in the bank, so we expect their precautionary savings target *in the account* to be 0. Thus, if the household's balance increases by a bit in the previous period, the balance would exceed the savings target, and the household would fully offset this increase in $Net\ Balance_{i,t-1}$ by dissaving in period $t$, resulting in a strong negative correlation between lagged balance and the flow of savings, and thus $\theta_p \approx -1$ for all $p$. This prediction is fairly accurate: we find $\hat{\theta}_1$, $\hat{\theta}_2$, and $\hat{\theta}_3$ ranging from $-0.76$ to $-0.59$. In the treatment group, we expect a similar effect in the pre-treatment period and the learning period since treatment beneficiaries do not yet trust the bank in these periods, i.e. $\xi_1 = 0$, $\xi_2 = 0$; the prediction is again fairly accurate, with $\hat{\xi}_1 = 0.10$, $\hat{\xi}_2 = 0.12$.

Once households have built trust in the bank through balance checks, the savings target *in the account* would no longer be 0, but some positive number less than or equal to the household's *overall* precautionary savings target. Once the precautionary savings target in the account has increased, the household would no longer fully offset increases in $Net\ Balance_{i,t-1}$, so the dampening effect of net balance would shift from close to $-1$ toward 0, so (with $\theta_3 \approx -1$) we expect $0 < \xi_3 < 1$; because precautionary savings models predict that the flow of saving is decreasing in lagged assets as the level of precautionary savings approaches its target, we nevertheless expect $\theta_3 + \xi_3 < 0$. This is indeed what we find: $\hat{\xi}_3 = 0.58$, and $\hat{\theta}_3 + \hat{\xi}_3 = -0.18$.

## Appendix E Comparison with Other Studies

The savings rates in Figure 17 are drawn form papers which met the following four criteria. First, we tried to include all studies measuring the impact of savings interventions. This includes offering accounts or other savings devices, deposit collection, financial education, and savings group interventions, as well as sending reminders, changing the interest rate, and defaulting payments. We excluded studies which measure the impact of income shocks and cash transfers on savings, since these are not savings interventions. Second, we focused on interventions in developing countries aimed at adults. Third, we include papers published in peer-reviewed journals, NBER working papers, and other working papers listed as "revise and resubmit" on authors' websites as of January 2017. This filter intends to avoid using preliminary results. Finally, to estimate the savings rate we need to divide the change in savings by income. We therefore only include studies that include average income (or, at a minimum, consumption or profits) in their tables, or an income (or consumption or profits) variable in the replication data.

Most papers report the impact of savings interventions on savings balances, which we divide by total income over the relevant period to obtain a savings rate. We use intent-to-treat estimates. In the cases that replication data are available, we use the replication data to replicate the studies' findings and compute the intent-to-treat impact of the intervention on the savings rate. When possible, we use total savings; when this is not available, we use savings in the savings intervention being studies (e.g., in the bank). For studies with results for multiple periods in time, we select results for the longer time period. This appendix provides more detail on how the savings rates in Figure 17 were computed for each study.

**Ashraf et al. (2006a).** This study looks at the effect of a deposit collection service in the Philippines. The authors find an effect of the deposit collection service on bank savings after 12 months that is statistically significant at the 10% level, but that dissipates and is no longer significant after 32 months; the effect on total savings after 12 months is of similar magnitude to that of bank savings, but is noisier and not statistically significant. We use the effect on bank savings after 32 months (since the effect on total savings after 32 months is not available). The effect on bank savings after 32 months is 163.52 pesos (Table 6), which we divide by total income over 32 months, which was obtained by dividing annual household income (129,800 pesos; Table 1, column 2 of the December 2015 version but not included in the final version) by 12 months to get monthly income, then multiplying by the 32 month duration of the study.

**Beaman et al. (2014).** This study looks at the effect of introducing rotating savings and credit association (ROSCA) groups in Mali to new techniques in order to improve their flexibility, namely allowing members to take out loans from the group savings rather than waiting for their turn to take home the whole pot. We use the impact of treatment on total savings (Table 4A, column 2) of $3.654. Because income was not included in their survey, we calculate expenditure over the three year period to use as the denominator by adding monthly food consumption (weekly food

consumption from Table 5, column 6, times the average number of weeks per month) and monthly non-food consumption (Table 5, column 5), and multiplying this by the 36 months of the study. Since these consumption figures are expressed in per adult equivalent rather than per household amounts, we then multiply by the average number of adult equivalents—which we conservatively estimate as 1+0.7+(average number of household members−2), where average number of household members, 7.55, is from Table 3. This estimate uses the OECD adult equivalence scale and assumes that only two household members are adults, thus leading us to likely underestimate its true value and thereby overestimate the savings rate.

**Brune et al. (2016).** This study looks at the effect of allowing farmers in Malawi to channel profits from their harvests into formal bank accounts; some farmers are also offered a commitment account. We use the intent-to-treat impact of any account on bank savings after 7 months, 1863 kwacha (Table 5, column 1). We divide by average monthly expenditure in the treatment group from Table 6, multiplied by the 7 months in the study.

**Callen et al. (2014).** This study looks at the effect of offering deposit collection to rural households in Sri Lanka. We use the estimate of the impact of treatment on savings from Table 1 (of the February 2016 version of the paper, which is more recent than the NBER version), or 883 Rupees. Since this effect pools results from surveys conducted 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 19, and 25 months post-treatment, we divide the savings effect by total household income over the previous month from Table A3 times the average number of post-treatment months (9 months).

**Drexler et al. (2014).** This study looks at the effect of financial literacy training in the Dominican Republic. In the study, neither the standard accounting nor rules of thumb treatment arms have a statistically significant impact on savings. We use the replication microdata to replicate their results from Table 2 of the impact of training on savings; we then estimate the pooled treatment effect. Because the paper and data set do not include income or expenditures, we use microenterprise sales in the denominator (the sample consisted entirely of microenterpreneurs). We calculate average sales among the treatment group at endline in the microdata, and multiply this by the 12-month duration of the study.

**Dupas and Robinson (2013b).** This study looks at the effect of providing different savings tools to ROSCA members in Kenya: a savings box, locked savings box, health savings pot, and health savings account. We used replication data to replicate the results in the paper and estimate a pooled treatment effect for the three interventions in which savings could be directly measured: the savings box, lockbox, and health savings account. We divide the savings effect by average weekly income among the treatment group (which we calculate using the replication data) multiplied by the 52 week duration of the study.

**Dupas et al. (2016b).** This study looks at the impact of providing access to formal savings accounts to households in three countries: Chile, Malawi, and Uganda. In Chile, an endline survey was not conducted due to low take-up, so we cannot include results for this country. For Malawi and Uganda, we use the intent-to-treat impact of treatment on totel monetary savings of $1.39 in Uganda and $4.98 in Malawi (Talbe 5, column 7). Because these effects pool data from three surveys conducted 12, 18, and 24 months post-treatment, we divide the savings effect by monthly household income (Table 10) multiplied by the average post-treatment time (18 months).

**Karlan et al. (2016).** This study looks at the effect of text message reminders to save in Bolivia, Peru, and the Philippines. Because the Philippines is the only country for which income data was collected, it is the only country from the study for which we estimate the effect of treatment on the savings rate. We use replication data to estimate the effect of treatment on the level of savings. (The paper uses a log specification, but for consistency with the other studies we use levels; in both cases, the effect is statistically insignificant for the Philippines.) Because savings was measured between 9 and 11 months after treatment, we divide by average weekly income of the treatment group (estimated using the replication data) times the average number of weeks per month times the midpoint number of months (10 months).

**Karlan and Zinman (2016).** This study looks at the effect of increased interest rates offered by a bank in the Philippines. Using the replication data, we replicate the results in Table 3 for the effect in the various treatment arms; the results for both the unconditional high interest rate and commitment "reward" interest rate treatment arms are statistically insignificant from 0. We then estimate the pooled treatment effect, using the variable for savings winsorized at 5% (since this is consistent with the winsorizing we perform in this paper). We divide by average weekly income of the treated (estimated using the replication data) times the 52 week duration of the study.

**Kast et al. (2016).** This study looks at the effects of participating in a self-help peer group savings program in Chile. We use the intent-to-treat estimate of self-help peer groups on average monthly balance, 1817 pesos (Table 3, column 7). Although we would prefer to use the effect on ending balance, Figure 3b shows that average monthly balance is similar to ending balance. We use the estimate winsorized at 5% (since this is consistent with the winsorizing we perform in this paper). We divided the savings effect by average number of household members times average per capita household income in the treatment group (Table 1) times the 12 month duration of the study.

**Kast and Pomeranz (2014).** This study looks at the effects of removing barriers to opening savings accounts for low-income members of a Chilean microfinance institution, with a focus on the impacts on debt. Because of the focus on debt, we estimate the effect of treatment on *net* savings, or savings minus debt. To obtain estimates of the intent-to-treat effect, we multiply the average savings balance of active account users, 18,456 pesos by the proportion of the treatment group who are active users (39%) and add the minimum balance of 1000 pesos times the proportion who take up but

leave only the minimum balance (14%), all from Table 2. We then subtract the intent-to-treat effect on debt, $-12,931$ pesos. This gives an effect of $18,456 \cdot 0.39 + 1000 \cdot 0.14 - (-12,931) = 20,251.76$ pesos. We used the estimates of the effects of the different treatments on savings given in Table 3 as the numerator. We divide this by the average number of household members times average per capita monthly income (Table 1) times the 12 month duration of the study.

**Prina (2015).** This study looks at the effects of giving female household heads in Nepal access to savings accounts. We use the replication data to estimate the intent-to-treat effect on savings account balances after 55 weeks, the duration of the study. While the paper shows average savings among those who take up accounts, to estimate the intent-to-treat effect we take the bank savings variable and recode missing values (assigned to those who do not take up the account or are in the control group) as zero, then regress this variable on a treatment dummy. We divide by average weekly income among the treatment group from the endline survey (available in the replication data) times the 55 week duration of the study.

**Sayinzoga et al. (2016).** This study looks at the effects of offering financial literacy training to small farm owners in Rwanda. We use the replication data to estimate the intent-to-treat effect on the level of savings. We divide this by mean monthly expenditure for the treatment group multiplied by the mean time between baseline and endline surveys, which we calculate directly in the replication data using survey date variables.

**Schaner (2016).** This study looks at the effects of offering very high, temporary interest rates in Kenya. We use the effect on bank savings (Table 3, column 2) and divide it by average monthly income of the treatment group (Table 4, column 6) times the 36 month duration of the study.

**Seshan and Yang (2014).** This study looks at the effects of inviting migrants from India working in Qatar to a motivational workshop that sought to promote better financial habits and joint decision-making with their spouses in India. The intent-to-treat effect on the level of savings comes from Table 3, column 1. We divide this by total monthly household income (constructed by adding the migrant's income and wife's household's income from Table 1, column 3) times the average duration of the study (averaging between 13 and 17 months).

**Somville and Vandewalle (2016).** This study looks at the effects of defaulting payments into an account for rural workers in India. We use the effect of treatment on savings balances 23 weeks after the last payment, or 33 weeks after the beginning of the study (Table 5, column 3). We divide this by average weekly income (p. 20) times 33 weeks.

**Suri and Jack (2016).** This study looks at the effects of mobile money access in Kenya. The authors find that an increase in the penetration of mobile money agents within 1 kilometer of a

household increases their log savings by 0.021 for male-headed households and 0.032 for female-headed households (Table 1). Using average savings of 286,752 shillings and the average change in agent density between 2008 and 2010 of 4.68 agents (Table S1), we thus calculate the effect of the average increase in access to mobile money on the level of savings as $(\exp(0.021) - 1) \cdot 286,752 \cdot 4.68$ for male-headed houseolds, and $(\exp(0.032) - 1) \cdot 286,752 \cdot 4.68$ for female-headed households. We divide this by average daily per capita income (Table S1) times 365 days times 6 years times the average number of household members. We obtain the average number of household members from the replication data for Jack and Suri (2014) and estimate it separately for male- and female-headed households.