BANKING ON TRUST:
HOW DEBIT CARDS ENABLE THE POOR TO SAVE MORE*

Pierre Bachas
Paul Gertler
Sean Higgins
Enrique Seira

Abstract

Trust is an essential element of economic transactions, but trust in financial institutions is low. Debit cards provide a mechanism to monitor bank account balances and build trust. We study a natural experiment in which debit cards are rolled out to beneficiaries of a conditional cash transfer program whose benefits are deposited into a savings account. Using administrative data on 350,000 bank accounts over five years, we find that prior to receiving a debit card beneficiaries do not save in these accounts, but begin saving once they have the card, with a delay. During the initial stagnant period they use the card to check their balances frequently, and the number of checks decreases over time as their reported trust in the bank increases. After 1–2 years, the debit card causes the savings rate to increase by 3–4 percent of total household income. This effect represents an increase in overall savings; consumption of temptation goods and entertainment decrease.

JEL codes: D14, D83, G21, O16

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.

<div align="right">—Kenneth Arrow (1972)</div>

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, Sapienza, and Zingales, 2008), use of checks instead of cash (Guiso, Sapienza, and Zingales, 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 I).

Lack of trust in financial institutions may not be unfounded. Cohn, Fehr, and Maréchal (2014) provide evidence that the banking industry fosters a culture of dishonesty relative to other industries. In Mexico, bankers loot money by directing lending to "related parties," i.e. bank shareholders and their firms (La Porta, López-de-Silanes, and Zamarripa, 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

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[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.

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, Sapienza, and Zingales, 2004; Karlan et al., 2009) and people are understandably reluctant to use financial institutions (Bohnet, Herrmann, and Zeckhauser, 2010). At the country level, trust is strongly associated with the proportion of the population that do save in formal bank accounts (Figure II). 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., 2016). 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 formal savings products, account use is low (Dupas et al., forthcoming).[2] Despite its importance, finding ways to improve trust in financial institutions has not been extensively studied (Karlan, Ratan, and Zinman, 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, Pellerano, and Reyes, 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 an event study 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

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[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 in developing countries have focused on the effect of the lower transaction costs facilitated by these technologies to make purchases (Zinman, 2009), access savings and remittances (Suri, Jack, and Stoker, 2012; Schaner, forthcoming), and transfer money (Jack, Ray, and Suri, 2013; Jack and Suri, 2014), but not their capacity to monitor and build trust in financial institutions.

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, Porteous, and Rotman, 2012; Aker et al., 2016; Muralidharan, Niehaus, and Sukhtankar, 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 3–4% more of their total household income each period. The size of the effect we observe is larger than that of other savings interventions studied in the literature, including offering commitment devices, savings reminders, no-fee accounts, higher interest rates, lower transaction costs, and financial education (Figure III compares the effect of debit cards on the accumulated savings stock, expressed as a percent of annual household income for comparison with other studies). 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.[4] 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.[5] 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.

For the analysis, we use high frequency administrative data on bank transactions for over 340,000 beneficiary accounts in 357 bank branches nationwide over 5 years, as well as several surveys of beneficiaries. Beneficiaries received debit cards on a rolling basis between January 2009 and April 2012; we thus study the impact of the debit cards on saving in a generalized differences-in-differences or event study framework. Because our data end in October 2011, beneficiaries who received cards between November 2011 and April 2012 serve as a control group throughout the duration of our study. Using the administrative data, we find that while about 10% of beneficiaries increase their

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[4]Another study in which a large savings effect was observed—but that we do not include in the comparison because the study does not include a measure of total household income—is Suri and Jack (2016), who study the impact of mobile money. Like debit cards, mobile money enables clients to easily observe account balances; hence, building trust could explain in part Suri and Jack's (2016) results.

[5]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.

savings immediately after receiving the card, the majority do not increase their savings initially. After a delay, however, savings balances rise substantially for the majority of beneficiaries with cards. We find that after 1–2 years with the card, the share of total income saved each payment period increases by 2–3 percentage points relative to the control group.

Exploring mechanisms behind the effect of debit cards on savings, we find that after receiving a card, a beneficiary tends to use it to check her balance frequently; over time, her frequency of balance checks falls. We also find that beneficiaries who have had their debit cards for less time report significantly lower rates of trusting the bank than beneficiaries who have had their debit cards longer. To establish a direct link between trust and increased saving, we merge the administrative data on account balances and transactions with survey data on reasons for not saving. Since trust is endogenous to the flow of savings (for example, someone who already trusted the bank may have already reached their savings target and thus not be adding new savings), 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 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 close to one year with the card, there is no difference in income between the treatment and control groups, but the treatment group consumes about 3% less of income, suggesting that savings has risen by about 3% of income—very close to the magnitude of the effect we see in the administrative account data. We also find no differential change in the stock of assets or purchase of durables in the treatment group compared to the control. 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, Karlan, and Yin, 2015; Kast, Meier, and Pomeranz, 2016). Analyzing categories of consumption, we find a statistically significant decrease in two consumption categories: temptation goods and entertainment.

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, Niehaus, and Sukhtankar, 2016). Furthermore, debit cards combined with ATMs or point-of-sale terminals and mobile phones combined with mobile money platforms are low-cost technologies that can be used to check balances and build trust in financial institutions. These technologies are simple, prevalent, and potentially scalable to millions of cash transfer recipients worldwide.

2 Institutional Context

We examine 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. 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).[6]

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 of local officials to skim off benefits[7] 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, all families in over half of Mexico's urban localities received their benefits directly deposited into savings accounts in Bansefi, a government bank created to increase savings and financial inclusion among underserved populations. The Bansefi savings accounts have

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

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

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 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. Prior to the rollout of debit cards, the average number of deposits per bimester[9] is almost exactly one—including the deposit from Oportunidades—and over 90% of clients make one withdrawal each bimester, withdrawing on average 100% of the transfer.

In 2008, 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 encourage recipients to save.

In 290 out of Mexico's 550 urban localities, beneficiaries received their benefits in bank accounts prior to the rollout of debit cards. As shown in Figure IV, beginning in January 2009 debit cards tied to these bank accounts were rolled out to beneficiaries by locality. In January-February 2009, about 8,000 beneficiaries received cards. By the end of 2009, about 75,000 beneficiaries had received debit cards tied to their pre-existing savings accounts, while 275,000 beneficiaries with savings accounts had not yet received cards. Another 172,000 beneficiaries in the remaining localities received cards in late 2010. By the last month for which we have account balance and transaction data from Bansefi—October 2011—256,000 beneficiaries had received debit cards tied to their pre-existing savings accounts. Another 93,000 beneficiaries received cards between November 2011 and April 2012 (at which point all beneficiaries previously receiving their benefits in savings accounts had received debit cards). These 93,000 beneficiaries who received cards shortly after the end date of

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

[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.

our study period thus form a pure control group throughout the duration of our study, although as we describe in Section 4, for identification we use all the variation across time in which beneficiaries have debit cards and for how long. The map in Figure B.1 shows that the card expansion had substantial geographic breadth throughout the rollout.

The sequence by which localities switched to debit cards was determined as a function of the proportion of households in the locality that were eligible for the cash transfer 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 into the program. Table II compares the means of locality-level variables and account-level variables from the treatment and control localities using (i) measures of social development constructed by the Mexican government's Consejo Nacional de Evaluación de la Política de Desarrollo Social (CONEVAL) based on the 2005 Census, (ii) Bansefi branch locations from 2008, and (iv) the administrative account data on average balances and transactions from Bansefi at "baseline," defined as the period prior to any beneficiary receiving a card, i.e. 2007–2008.

Because the rollout was not random, it is not surprising that there are some differences between treatment and control branches. Treatment and control branches are nevertheless fairly similar; furthermore, time-invariant differences between treatment and control localities are not a threat to identification since we include account fixed effects. As in other studies exploiting a rollout over time (e.g., Galiani, Gertler, and Schargrodsky, 2005; de Janvry et al., 2015), our identifying assumption is one of parallel trends: that *changes* in characteristics over time are uncorrelated with the rollout. We provide evidence of parallel trends from both administrative and survey data in Section 4. Comparing means, we find that treatment branches are slightly larger and that beneficiaries receive slightly larger transfer amounts (significant at the 1% level). On all other variables—concentration of Bansefi branches, literacy rates, school attendance, dwelling characteristics (dirt floors, piped water, electricity, occupants per room), number of client deposits, number of withdrawals, percent of transfer withdrawn, net savings balance, and years with the card—treatment and control localities are balanced.

Since our empirical specification exploits variation over time in who has received debit cards (even within the treatment group), we conduct an analogous balance test of whether the rollout over time is correlated with baseline characteristics. Following Galiani, Gertler, and Schargrodsky

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(2005), we estimate a discrete-time hazard model of the probability of receiving cards. Among accounts that have not yet received cards by period $t-1$, we model the probability that the account receives a card in period t as a function of baseline locality and account characteristics.[11] Table II, column 4 shows the results. We again find that larger localities receive cards earlier; there is no longer a correlation with Oportunidades transfer size, but we now find that localities treated earlier are slightly more literate and have had savings accounts for longer (i.e., were originally given bank accounts earlier in Oportunidades' expansion to urban areas).

3 Data Sources

We use four main data sources: administrative data on account balances and transactions from Bansefi and three surveys of Oportunidades beneficiaries (two cross-sections and one panel). These data sources, the number of beneficiaries in each, time period, main variables we use, and variation we exploit are summarized in Table I.

3.1 Administrative Data

To examine the effect of debit cards on savings and account use, we analyze account-level balance and transactions data from Bansefi for the universe of accounts that received transfers in a savings account prior to receiving a debit card, then received a debit card tied to this account. These data consist of accounts at 357 Bansefi branches over almost five years, from January 2007 to October 2011. These data include the monthly average savings balance, the date, amount and type of each transaction made in the account (including Oportunidades transfers), the date the account was opened, and the month the card was given to the account holder. Note that the date of debit card receipt was determined exogenously by Oportunidades, not endogenously by the beneficiary. Figure IVa shows the timing of the administrative Bansefi account balance and transaction data relative to the rollout of debit cards.

Table II, panel B shows summary statistics from this dataset. Using pre-treatment data averaged across all bimesters from 2007–2008, the accounts in our sample make 0.01 client deposits and 0.97 withdrawals per bimester, and the amount withdrawn is 100 percent of the Oportunidades transfer

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[11]The estimation method is described in Jenkins (1995). As in Galiani, Gertler, and Schargrodsky (2005) we include a fifth-order polynomial in time, but all coefficients on the terms in this polynomial are insignificant from zero.

on average, indicating very low use of the account and savings prior to receiving the card.[12] Net balances are 151 pesos or about US$11 on average; the distribution of net balances is skewed: the 25th percentile is less than 13 pesos (US$1) and the median is 77 pesos (US$6). The average amount transfered by Oportunidades in 2007–2008 is 1194 pesos, or about US$92, per bimester; using survey data we find that Oportunidades income represents between one-fifth and one-fourth of beneficiaries' total income on average. The average account had already been open for 4.3 years by January 2009, so beneficiariaries in our study had substantial experience with a savings account prior to receiving the debit card.

3.2 Survey data

Since its inception in 1997, Oportunidades has a long history of collecting high quality surveys. Here we use three distinct household-level surveys conducted with Oportunidades beneficiaries: (1) a household panel survey conducted in urban and semi-urban localities, (2) a cross-sectional survey including questions on reasons for saving or not saving; and (3) a cross-sectional survey focused on Oportunidades' payment methods (i.e., bank accounts with or without a debit card or—in rural areas—cash payments).[13] The first surveys a panel of households which we use to measure the debit cards' effects on total savings and consumption in a differences-in-differences specification. The other two surveys are used to explore the mechanisms through which receiving the card leads to higher savings. For these surveys we only have a cross-section of households with a debit card and exploit variation across households in how long clients have had the debit card resulting from the rollout of cards over time. Figure IVb shows the timing of each survey relative to the rollout of debit cards. Figures B.2–B.4 shows the the distribution of exposure time in the sample included in each survey.

A. ENCELURB Household Panel Survey

The most comprehensive survey data we use is the Encuesta de las Características de los Hogares Urbanos (ENCELURB), a household panel survey with data on consumption, income, and assets. We merge these data with administrative data from Oportunidades on beneficiary status and the

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[12] Amount withdrawn can exceed 100% since the account could have a positive balance prior to the Oportunidades payment.

[13]The first and third surveys are public and can be found at https://evaluacion.prospera.gob.mx/es/eval_cuant/p_bases_cuanti.php.

dates that debit cards were distributed in each locality to explore whether the increased savings in the Bansefi accounts is an increase in overall savings or a substitution from other forms of saving.

The survey includes three pre-treatment waves in 2002, 2003, and 2004, and one post-treatment wave conducted between November 2009 and February 2010. It is a high-quality survey whose original objective was to measure the impact of the urban expansion of Oportunidades.[14] 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 since this is the only post-treatment wave for our study. Not every household was surveyed in every baseline wave, resulting in an unbalanced sample.

The fourth wave of the survey included 6,272 households. Of these, 2942 households spanning 49 localities live in urban (as opposed to semi-urban) areas, received their benefits in a savings account prior to the rollout of debit cards, and had non-missing values for income and consumption in the post-treatment and at least one pre-treatment wave; these households make up our sample. Localities that switched to debit cards in early 2009 were oversampled in the fourth wave (relative to localities that switched to debit cards later in 2009), as can be seen in Figure B.4. As a result, the treatment group in this survey—beneficiaries who received cards prior to the fourth wave of the survey—had the card for close to one year when surveyed.

The ENCELURB survey has comprehensive consumption, income and asset modules. For instance for consumption we observe spending on food items (like tomatos, tobacco, alcohol, tortillas, etc.) for 48 items, as well as spending on transport, clothes, rent, utilities, among others. We observe ownership of assets, as well as house dwelling characteristics, employment status, and income from work and from many kinds of transfers.

The main variables we use are aggregates from these survey questions, and include total consumption over the past month, total income over the past month, purchase of durables over the past month, and an asset index defined as the principal component of indicator variables for each asset.[15] The questionnaires and data sets for this survey are publicly available from Oportunidedes.

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[14]The consumption, income, and assets modules of Oportunidades' analogous survey for rural areas have been used by Angelucci and De Giorgi (2009), Gertler, Martinez, and Rubio-Codina (2012), Attanasio et al. (2013), and de Janvry et al. (2015), while these modules in ENCELURB have been used by Behrman et al. (2012) and Angelucci and Attanasio (2013), among others.

[15] While the 2009–10 wave of the survey includes 21 assets, only 9 of these assets are also included in the pre-treatment waves of the survey. To construct the asset index we thus restrict to assets included in all waves of the survey.

B. ENCASDU with Questions on Reasons for Saving or Not Saving (2010)

The Encuesta de Características Sociodemográficas de los Hogares Urbanos (ENCASDU) is a stratified random sample of 9,931 Oportunidades beneficiaries from 47 urban localities. Since one of its objectives was to understand beneficiaries' satisfaction with the new payment method of debit cards, the survey oversampled from localities that switched to debit cards at the beginning of the rollout (early 2009). We restrict our analysis to beneficiaries who had already received debit cards by the time of the survey since the module with questions we use about reasons for not saving were only asked to those who had already received debit cards. This leaves us with a sample of 1694 households in 16 localities. The median exposure with the card in this sample is 14 months.

This survey gives us direct self-reported reasons for not saving in the Bansefi bank account. "Do you leave part of the monetary support from Oportunidades in your bank account?" 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 pre-written response "because if I do not take out all of the money I can lose what remains in the bank" or similar open-ended responses related to not trusting the bank.[16]

Using program identification numbers provided by Oportunidades, we also merge this survey to the administrative data to measure the direct effect of trust in the bank induced by more time with the card on the flow of savings in the account. We are able to successfully merge 1,330 of the 1,694 observations in the ENCASDU.

We also use the same ENCASDU survey question to test two alternative reasons for not saving in the account that could explain the delayed savings effect we observe in the administrative data. The first is a lack of knowledge ("They didn't explain the process for saving" and other open-ended responses). The second is fear that by accumulating savings in the account, they will be deemed not poor enough to be eligible for continued program benefits ("Because if I save in that account they can remove me from the Oportunidades program" and open-ended responses).

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[16]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."

C. Payment Methods Survey (2012)

The Encuesta Medios de Pago is a cross-sectional survey conducted in mid-2012, and consists of a stratified random sample of 5,388 Oportunidades beneficiaries in 133 localities in 2012. It has two strata: urban/rural localities, and within those by conglomerate zone, were conglomerates coincide with Oportunidades zones covering 500 households. Since only some urban localities received debit cards, we restrict the analysis to the 1,641 surveyed beneficiaries across 55 localities who receive their benefits by debit card.

The Payment Methods Survey was specifically fielded to measure operational details of the payment method. In particular it asks about use of cards and beneficiaries' experience using ATMs. We use it to measure the self-reported number of balance checks and withdrawals with the card, as well as whether beneficiaries find it hard to use the ATM, whether they get help using the ATM, and know their PIN. We also use it to compare beneficiaries' knowledge of the fees they are charged to use ATMs and to withdraw cash at other banks' ATMs after the first 2 free withdrawals per month. The median time with the card in this sample is 12 months. We will provide some summary statistics of for this survey in the next section. The questionnaire and data set for this survey is publicly available from Oportunidedes.

D. Auxiliary data sources

Finally, to assess balance of covariates across localities that received cards at different times during the rollout, in addition to using administrative data from Bansefi we use locality-level data related to local development and poverty (e.g., the percent of households without piped water) from the Consejo Nacional de Evaluación de la Política de Desarrollo Social (CONEVAL), an independent government institution tasked with measuring poverty; these measures were computed by CONEVAL using the 2005 Census.

To test another alternative explanation for the delayed savings effect, namely that there was a supply-side response by banks to the debit card expansion—installing more ATMs in response to increased concentrations of cardholders—we use data on the number of bank ATMs and branches by municipality by quarter from the Comisión Nacional Bancaria y de Valores (CNBV), a government banking council.

4 Empirical Strategy and Identification

This section describes our main empirical specifications, which all exploit variation generated by the staggered expansion of debit card delivery by Oportunidades. Additional specifications will be described later in the paper.

When we there is a panel dimension, like in the administrative data or the household survey panel data, we estimate a differences in differences specification. In the case of the administrative data, we estimate a generalized differences-in-differences specification that allows the timing of treatment to vary and allows the treatment effect to vary dynamically over time since receiving the card. When we only have a cross section of cardholders (the ENCASDU and Payment Methods surveys), we exploit variation in the length of time beneficiaries have been exposed to the card. Note that in each data set all beneficiaries eventually receive cards (by April 2012), and are drawn from the population present in the administrative data, which includes the universe of beneficiaries who were already receiving benefits in a savings account and then received a debit card tied to the account. Regardless of the data source, we always rely on the exogeneity of the geographical expansion of debit cards for identification.

4.1 Generalized Difference-in-Differences (Event Study) with Administrative Data

For the administrative data we have a pure control group and we estimate an event study specification that allows the treatment effect to vary over time (as in Jacobson, LaLonde, and Sullivan, 1993). The gradual rollout of debit cards over time is helpful for identification as it rules out treatment effects being driven by some other event occurring at the same time as treatment (unless the other event also occurs gradually following the same timing pattern as the rollout of debit cards, which is unlikely).

We control for common macro shocks by including calendar time fixed effects, and for time-invariant individual heterogeneity with individual account fixed effects. Specifically, we estimate the following equation:

$$y_{it} = \lambda_i + \delta_t + \sum_{k=a}^{b} \phi_k D_{it}^k + \varepsilon_{it} \tag{1}$$

where y_{it} is the outcome of interest (e.g., average number of withdrawals per bimester) in account i over period t, the λ_i are account-level (i.e., beneficiary) fixed effects, and the δ_t are calendar time

fixed effects. $D_{it}^k = D_i \cdot \mathbb{I}(t = \tau_i + k)$, where $D_i = 1$ if individual i is ever treated during the study period (i.e., switches to a debit card by October 2011) and τ_i denotes the period in which individual i receives a debit card. $a < 0 < b$ are periods relative to the switch to debit cards; we omit the dummy for the $k = -1$ relative period. In other words, D_{it}^k is a dummy variable indicating that account i has had a debit card for exactly k periods (or for $k < 0$, will receive a debit card in $|k|$ periods). This event study specification is a generalized difference-in-differences framework that takes advantage of the staggered rollout of debit cards, and also allows us to estimate treatment effects that vary dynamically over time. Since we have a control group that does not receive cards until after the study period ends (as in McCrary, 2007), we can pin down the calendar time fixed effects without facing the under-identification problems described in Borusyak and Jaravel (2016).[17]

As in any differences-in-differences model, to interpret each ϕ_k as causal effects of having the card for k periods, we need to invoke a parallel trend identification assumption: that in the absence of the card, early vs. late recipients would have had the same savings behavior. While this is untestable, we test for parallel pre-intervention trends by showing that $\phi_k = 0$ for all $k < 0$ whenever we use specification 1. Figures VI, VII, and VIII show parallel pre-treatment trends in the number of withdrawals, stock of savings, and the savings rate.

When using the administrative data, we average time in four-month periods since 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, which would distort the number of withdrawals and net balance within a bimester.[18] We estimate cluster-robust standard errors, clustering ε_{it} by Bansefi branch.

4.2 Difference-in-Differences with Survey Data

Because we only have one post-treatment wave in the household survey panel data, instead of the generalized difference-in-differences approach above, we use a standard difference-in-differences

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[17]We set a and b as the largest number of periods before or after receiving the card that are possible in our data. The estimates for periods furthest from the time of card receipt are based on a smaller sample (as only the earliest switchers have the card for more than two years in our data, and only the latest switchers have more than three years of pre-card data). We thus include coefficients for all possible values of k in the regression but only graph the coefficients representing three years before receiving the card and two years after. This differs slightly from McCrary (2007) who "bins" relative periods below or above extreme cut-offs. However, Borusyak and Jaravel (2016) show that this can bias all ϕ_k estimates if treatment effects do not "level off," and thus recommend including all relative period dummies in event study regressions.

[18]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.

approach. We compare trends in consumption, income, purchase of durables, and stock of assets for those who have already received the card by the fourth survey wave to those who had not yet received cards. Specifically, we estimate

$$y_{it} = \lambda_i + \delta_t + \gamma D_{j(i)t} + \nu_{it}, \tag{2}$$

where y_{it} is the outcome of interest for individual i at time t. Time-invariant differences in household observables and unobservables are captured by the household fixed effect λ_i, common time shocks are captured by the time fixed effects δ_t, and $D_{j(i)t} = 1$ if locality j in which household i lived prior 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.

The identifying assumption is one of parallel trends. Table B.4 uses the three pre-treatment rounds (2002, 2003, 2004) of the ENCELURB household panel survey to estimate pre-treatment trends in total consumption, total income, purchase of durables, and assets. We estimate $y_{it} = \lambda_i + \delta_t + \sum_k \omega_k T_{j(i)} \times \mathbb{I}(k = t) + \eta_{it}$, where k indexes survey round ($k = 2002$ is the reference period and is thus omitted), $T_{j(i)}$ is an indicator of whether beneficiary i lives in locality j, and $\mathbb{I}(k = t)$ are time dummies. ω_k measures the difference in differences before treatment (which can be thought of as placebo effects if we change the timing of treatment). We show p-values from an F-test of $\omega_k = 0 \ \forall \ k < 2009$ and fail to reject the null of parallel trends.

4.3   Cross-Section Exploiting Variation in Time with Card

The ENCASDU and the Means of Payment Survey are cross sections of beneficiaries with cards, each with less than 2,000 observations. This poses two challenges. First, we have to rely on exposure time as the identifying variation. Second, to economize on power, we split the beneficiaries into two equal-sized groups based on how long they have had the card. Concretely, we run a regression of the outcome variable—such as self-reported trust or balance checks—on a dummy of whether the exposure to the card of beneficiary i is below the median exposure to the card.

$$y_i = \alpha + \gamma \mathbb{I}(\text{Card} \leq \text{median time})_i + u_i, \tag{3}$$

There are two other issues with this specification. First, since all households in this specification have been treated, if the effect manifests itself immediately after getting the card, we would be biased towards not finding an effect. Second, the identification assumption is stronger than with the panel, as we requiere orthogonality between the error term u_i and exposure time. That is, beneficiaries would have been the same in trust and withdrawal behavior regardless of when they received the card. The first issue is not a significant concern in our case, because we do find significant effects, and we show that that effect on savings manifests with substantial delay, suggesting that the underlying mechanism is slowly evolving. We deal with the second issue by conducting a balance test across the samples who have had the card for more and less time. In , we show balance between those who have had the card for more vs. less than the median time in the ENCASDU sample. 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.

5 Effect of Debit Cards on Account Use and Savings

In this section, we use the administrative data from Bansefi on all transactions and average monthly balances in 343,204 accounts of Oportunidades beneficiaries to estimate the dynamic effect of debit cards on use of the accounts through transactions (deposits and withdrawals), on accumulated savings in these formal bank accounts, and on the savings rate.

5.1 Transactions

By lowering indirect transaction costs, debit cards should lead to more transactions, as predicted by theory (Baumol, 1952; Tobin, 1956) and past empirical evidence (Attanasio, Guiso, and Jappelli, 2002; Alvarez and Lippi, 2009; Schaner, forthcoming). This is indeed what we find. Figure V panel (a) presents the distribution of the number of withdrawals per bimester, before and after receiving the card. Prior to receiving the card, 90% of beneficiaries made a single withdrawal per bimester. The distribution of withdrawals in the control group is nearly identical to that of the treatment group prior to receiving a debit card. In contrast, after receiving the card, 67% of beneficiaries continue to make just one withdrawal, but 25% make 2 withdrawals, 5% make 3 withdrawals, and

2% make 4 or more withdrawals.[19] Meanwhile, the distribution of the number of withdrawals in the control group does not change over time (Figure B.5).

On the contrary, there is no effect on client deposits: Figure V panel (b) shows that 99% of accounts have zero client 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.

In order to examine the evolution of the debit card's effect on withdrawals over time, we estimate an event study specification in equation 1, with account level period withdrawals as the dependent variable. Figure VI plots the ϕ_k coefficients of average withdrawals by bimester for each four-month period, compared to the period just before the switch. Prior to receiving the card, pre-trends are indistinguishable between treatment and control: we cannot reject the null of $\phi_k = 0$ for any $k < 0$. In addition to having parallel trends, pre-treatment levels of the number of withdrawals are also the same between treatment and control. (Although this cannot be determined from (1) since any difference in levels would be absorbed by the account fixed effects, it is obvious from Figure V.) The effect on withdrawals is *immediate*, as would be expected from the instantaneous change in transaction costs induced by the card. The coefficients on the post-periods imply that beneficiaries perform 0.3 more withdrawals per bimester following receipt of the card.   Prior to receiving the card, beneficiaries in both the treatment and control groups average just above 1 withdrawal per bimester. Immediately after receiving the card, about one-third of beneficiaries begin making an additional withdrawal each bimester, so 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 the control group, it remains constant at about 1 withdrawal per bimester.

5.2   The Stock of Savings (Account Balances)

Next, we explore whether debit cards cause an increase in account balances and savings from period to period. The increased use of the accounts shown in Section 5.1 does not necessarily mean

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[19]After receiving the card, store purchases can also be made on the debit card; these are grouped together with withdrawals. 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 few beneficiaries make more than two withdrawals even after receiving the card.

beneficiaries are saving in the account *across* periods; they could just be leaving some money in the account after the first withdrawal in the bimester, but withdraw the remaining money later in the same bimester. This would show up as higher average daily balances (which is the variable we observe) even though all the savings is being depleted within period.

Since we are more interested in a measure of saving across periods and because we do not observe end-of-period balance, we will adjust the average balance measure to remove mechanical effect owing to more and lower amount withdrawals.[20] Because we observe the timing and amount of each transaction, we can calculate and subtracct off the mechanical effect for each account-bimester to obtain a measure of "net balance" (see Appendix C for more details) which is a good proxy for period-to-period savings.

We estimate (1) with account i's net balance in period t as the dependent variable. Following other papers measuring savings (e.g., de Mel, McIntosh, and Woodruff, 2013; Dupas et al., 2016; Kast, Meier, and Pomeranz, 2016; Karlan and Zinman, 2016), we winsorize savings balances to avoid results driven by outliers.[21] The ϕ_k terms thus measure the causal effect of debit cards on the stock of savings k periods after receiving the card. Figure VII plots the ϕ_k coefficients and their 95% confidence intervals.

First note that the parallel trends assumption is satisfied as pre-treatment ϕ_k coefficients are statistically zero. In the first few periods after receiving a card, there is a small savings effect of about 200 pesos (about US$15). This increased effect reflects both an increase over time in the proportion of individuals using their accounts to save, as well as an increase in the balance conditional on saving. Figure uses as dependent variable a dummy equal to one if the individual has positive savings and zero otherwise and shows that of individuals start saving immediately after receiving a card (presumably because they already trust the bank), a further save in during the first year, and by the time they have had the card for two years, 60 percent of beneficiaries save. The savings effect begins increasing one year (three periods) after receiving a debit card, and

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[20]An example of the mechanical effect is this. Suppose 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.

[21]Our main results winsorize at the 95th percentile, and the results are robust to other cut-offs.

continues increasing. Two years after receiving the debit card, beneficiaries save on average about 800 pesos (US$62) more than the control group, whose savings do not change over the period.

5.3   Saving Rate

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).[22] 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

$$\Delta Savings_{it} = \lambda_i + \delta_t + \theta Savings_{i,t-1} + \gamma Income_{it} + \varepsilon_{it}. \tag{4}$$

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 event study time.

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

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

(i) there are no deposits into the account other than the transfer, (ii) the debit card receipt does not affect other sources of income, and (iii) 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 bank account does not substitute for other forms of saving; an increase in bank savings constitutes an increase in total savings. Empirically we find that all three assumptions hold. First, as we have already shown in Figure V, almost no beneficiaries deposit any funds in addition to the transfers into their savings accounts in any period. Second, using household survey panel data in Section 7, we find that the debit cards do not affect income. Third, using the household survey data, we find a effect of the debit card on total savings as we do with administrative bank account data.

Incorporating all of the above changes to (4) and allowing the debit card's effect to vary over time with the card, we obtain the following specification:

$$\Delta Savings_{it} = \lambda_i + \delta_t + \sum_{k=a}^{b} \alpha_k D_{it}^k + \theta Savings_{i,t-1} + \sum_{k=a}^{b} \xi_k D_{it}^k \times Savings_{i,t-1} \qquad (5)$$

$$+ \gamma Transfers_{it} + \sum_{k=a}^{b} \psi_k D_{it}^k \times Transfers_{it} + \varepsilon_{it},$$

where $Savings_{it}$ refers to the stock of savings and $\Delta Savings_{it} \equiv Savings_{it} - Savings_{i,t-1}$ refers to its flow. D_{it}^k is a dummy variable that equals 1 if account i has had the debit card for k periods, i.e. $D_{it}^k = D_i \cdot \mathbb{I}(t = \tau_i + k)$. For those in the control group who receive cards after our study period ends, $D_{it}^k = 0$ for all k.

The main advantage of this specification over the reduced-form analysis presented in Section 5.2 is that it allows existing balances to influence the savings rate, enabling us to test the prediction from precautionary saving models that as a beneficiary accumulates savings and approaches her target buffer stock, her rate of saving decreases. An additional advantage is that it controls for the amount of transfers in each period, which varies both across households and within households over

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time.[23]

We estimate the effect of the debit card on the savings rate from the above specification, allowing it to vary over time with the card, as

$$\hat{\Phi}_k \equiv (\hat{\alpha}_k + \hat{\xi}_k \omega_{k-1} + \hat{\psi}_k \mu_k)/\overline{Y}, \tag{6}$$

where ω_{k-1} is average lagged net balance and μ_k is average transfers k periods after receiving the card; $\overline{Y}$ is average income. The numerator in (6) gives the difference between treatment and control in the flow of savings in pesos; the denominator divides by average income to obtain the savings rate.[24]

The right-hand side of the specification in (5) includes individual both, fixed effects and lagged net balance. As (Nickell, 1981) has shown, this may generate bias if the number of time periods is small (Nickell, 1981). To avoid this bias, in practice we do not include the individual fixed effects λ_i and instead include a simple treatment dummy in their place. Because individuals in both treatment and control were not saving prior to receiving the card, excluding the fixed effects does not change the estimates much. [25]

The results in Figure VIII 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 $k < 0$.[26] After receiving the card, some beneficiaries start savings immediately, and in the first year after receiving a card (relative periods 0 to 2) we thus see an average savings effect of between 0 and 1.5% of

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[23]Results are robust to excluding the $Transfer_{it}$ interaction terms; see Figure B.6. Because transfer amounts vary for a number of reasons, we control for them in the preferred specification. 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.

[24]Average income is obtained from the 2009–10 wave of the Consumption, Income and Asset Panel Survey conducted by Oportunidades (described in Section 3). It is scaled to a four-month period to match the time period of the estimated effect of the debit card on the flow of savings.

[25]To assess the robustness of our results to including the individual fixed effects without biasing our estimates, we also 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 (Bun and Kiviet, 2006). For the system GMM, we restrict the pre-card trend to be equal to 0 to reduce the number of coefficients to be estimated and instruments needed.

[26] In 8 of the 9 pre-treatment periods, there is no statistically significant difference between the savings rate of the treatment and control groups.

income. In the second year after receiving the card, more individuals save and we see a savings effect between 3 and 4% of income.

Models of precautionary savings predict that the savings rate should fall once a positive savings balance is achieved, with the savings rate dampened by a negative coefficient on lagged balance. We do find a decreasing pattern of savings: after a large average savings effect of close to 4% of income one year after receiving the card, the effect of the debit card on the savings rate falls to about 3% of income.[27]

Ideally, we would also like to measure the equilibrium buffer stock that beneficiaries accumulate. Since many beneficiaries are still accumulating savings after two years with the card, we may not have sufficient time periods to measure their equilibrium buffer stock. Assuming that they have reach a steady state, we can use the equation $Savings_{it} = Savings_{i,t-1}$ (where "savings" refers to the stock of savings) and plug it into (5) to solving for the equilibrium saving stock for those with a card and obtain $Savings = (\delta + \alpha + (\gamma + \psi)Transfers)/(-\theta - \xi)$. Using averages for these coefficients from the periods after the vast majority of beneficiaries have started saving, we predict that the average equilibrium buffer stock is 1156 pesos (US$89); to put this quantity in context, it equals 36% of beneficiaries' monthly income. After two years with the card, beneficiaries have on average accumulated about two-thirds of their desired buffer stocks.

6 The Trust Mechanism

The time delay before a beneficiary begins saving after receiving her debit card suggests that learning might be occurring. In this section, we explore learning mechanisms and provide evidence from a variety of data sources that beneficiaries use their debit cards to monitor the bank and ensure that their account balance is as expected; over time they build trust in the bank. In order to test the trust hypotheses, we complement the administrative Bansefi data with data from two beneficiary surveys: (1) the 2012 Payment Methods Survey and (2) the 2010 ENCASDU described above. In both cases, we restrict our analysis to the sample of respondents who received their benefits in savings accounts tied to debit cards at the time of the survey (since the questions we use were not asked to those who had not yet received debit cards) and exploit exogenous variation in amount

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[27]See Appendix D for a full discussion of the estimated coefficients. We show that the estimates align with the predictions from models of precautionary saving 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 $\hat{\xi}_k$.

of time with the card. Note that exposure time to the card is the same variation we used in the previous sections with administrative data

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. It also increases the value of the account and may make it worthwhile to risk losing money by leaving it in the account in order to learn to trust. 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.[28] 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 first test the hypothesis that balance checks fall over time with both the administrative and survey data. Secondly, we examine whether higher savings balances are negatively correlated with the number of balance checks *within* accounts in the administrative account data. Thirdly, we use the survey data to test whether self-reported trust in the bank increases over time with the card. Finally, we merge survey data with self-reported trust in the bank with administrative data and find a direct relationship between self-reported trust and savings in the account.

6.1 Balance Checks Fall 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.[29] We only observe balance checks once beneficiaries have debit cards which restricts our analysis to the treatment group and to periods after the card is received. On average beneficiaries check their balances 1.8 times per four-month period. To test the hypothesis of a decreasing time trend in balance checking, we run a regression of the number of balance checks on

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[28]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. Most households do not have cards.

[29]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 costs of traveling to the nearest Bansefi branch.

event study time dummies, with calendar time and account fixed effects:

$$Balance\ Checks_{it} = \lambda_i + \delta_t + \sum_{k=0}^{4} \phi_k D_{it}^k + \varepsilon_{it}. \qquad (7)$$

We measure the number of balance checks relative to the last period for which we observe the recipient by omitting this period. The ϕ_k coefficients graph the number of balance checks k periods after receiving the card relative to the last period in the sample (October 2011), which depending on beneficiaries corresponds to one to two year after reception of the card. We thus expect ϕ_k to be greater than 0 and decreasing in k.

Panel (a) of Figure XI plots the coefficients of this regression which measure the number of extra balance checks compared to the last sample period. The number of balance checks in the periods following the reception of the debit card is significantly higher than in the last period. For example, in the period that the debit card is received, about 0.75 more balance checks are made than two years after receiving the card. After having the card for about one year, this falls to about 0.2 more checks. One objection could be that this pattern does not reflect beneficiaries checking to monitor their balances, but instead beneficiaries checking if a new transfer has arrived. To address this concern we look at the subset of balance checks which occur after the beneficiary knows that the transfer has arrived. Specifically, we first restrict a sample to balance checks which occur after the transfer has been disbursed and on a different day than a withdrawal.[30] Panel (b) of Figure XI shows that balance checks after reception of the transfer exhibit the same decrease over time than all balance checks and explain most of the time variation in the pattern of all balance checks. A second concern is that beneficiaries might not exactly know the transfers' dates (even though they have calendars with these dates) and therefore some checks post-transfer could still occur to verify that the transfer has indeed arrived. To address this, we further restrict balance checks to those happening after clients have already withdrawn once in a bimester and on a different day than a withdrawal. Panel (c) of Figure XI shows that the results remain unchanged.

Of course for learning to occur beneficiaries need a positive balance in their account at the

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[30]This is a conservative measure that may go against our hypothesis. 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. We should also mention that Beneficiaries were given calendars with exact transfer dates and hence should know the dates on which transfers are deposited (see Figure A.3).

time of checking. Although many have not yet started saving strongly shortly after receiving the card, we find that they do have small positive balances in their accounts. Since we do not have daily account balances, we take the conservative approach of defining a balance as positive if the cumulative transfer amount minus the cumulative withdrawal amount in the bimester is positive at the time of the balance check (this is a sufficient but not necessary condition for the balance to be positive). 89% of accounts have a positive balance at the time of a balance check post-transfer reception. Focusing on the first four-month period, when such balance checks are most 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 validate the above results using survey data from the Payment Methods Survey. 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 equation 3 where the dependent variable 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 XII 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% fewer trips to the ATM to check their balances without withdrawing money than those who have had the card for less time. Because the survey data is self-reported, this not only confirms the findings from the administrative data, but also shows that balance checking behavior is salient for beneficiaries.

6.2 Correlation of Savings Balances and Balance Checks

We then test whether balance checks and savings are negatively correlated *within* accounts. We hypothesize that initially, when trust is low, beneficiaries do not yet save in the account, but do use the card to frequently monitor their account. As they build trust, they make fewer balance checks,

and this is followed by an increase in the stock of savings. Specifically, we estimate

$$Savings_{it} = \lambda_i + \sum_{c \neq 0} \eta_c \mathbb{I}(Checks_{it} = c) + \varepsilon_{it}, \tag{8}$$

where $Savings_{it}$ is the net balance in account i at time t, the λ_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.[31] The η_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 prediction is that $\eta_c < 0$, and that η_c is decreasing (i.e., becoming more negative) in c.

Figure XIII shows the results from (8). We find that account balances are 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 and statistically significant. For example, in periods where beneficiaries make two balance checks, their savings average 110 pesos less than in periods when they make zero or one balance checks. This decreases a further 250 pesos for those making five balance checks. So we confirm that as a beneficiary checks her balance less, she increases her savings balance.[32]

6.3 Trust Increases with Time with the Debit Card

In this section we test the hypothesis that a longer tenure with the debit card induces higher trust in the bank. We measure trust using the ENCASDU. As described above this survey first asks if the beneficiary saves in the Bansefi bank account, and if the answer is in the negative, it asks why. Lack of trust is captured by the pre-written answer "because if I do not take out all of the money I can lose what remains in the bank". Note that 5% of beneficiaries answer "other" and provide an open-ended response. 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

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[31]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, ε_{it} are clustered at the bank branch level.

[32]In Appendix Figure B.5 we show that the correlation between net savings and balance checks is even larger when restricting the definition of balance checks to those occurring post-transfer receipt and on a different day than a withdrawal, which we argued are precisely the checks used to monitor the account.

trust in leaving it." 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 (3) with lack of trust as the dependent variable, again exploiting the exogenous variation in the length of time beneficiaries have had the card.

Of those who have had the card for less than the median time, 24% don't save and report lack of trust as the reason. Trust increases over time, however, and beneficiaries with more than the median time with the card are 33% less likely to report not saving due to low trust.[33]

As explained in section 4, to interpret γ in equation 3 as a causal effect we need to assume that time with the card is orthogonal to our potential outcomes of interest. That is that card rollout is uncorrelated with trust in the bank. All the tests conducted in Section 4 support this assumption. Figure XIV shows our estimates of γ from equation 3. Figure XIV also shows results for two alternative forms of learning—learning to use the technology and learning that the program will not drop beneficiaries who accumulate savings. Few beneficiaries report these as reasons for not saving. More importantly, the proportion of beneficiaries reporting these as reasons for not saving does not change over time. We discuss these alternative explanations in more detail in Section 8.

6.4   The Direct Relationship between Trust and Saving

Finally, we directly estimate the relationship between reported trust in the bank and the savings rate. In Section 6.3 we found that time with the card increases trust, and in Section 8 we will show that it 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 saving only through its effect on trust. If this assumption holds, we can express the reduced-form effect of time with the card on the flow of savings as

$$\frac{\mathrm{d}\Delta Savings}{\mathrm{d}Time\ with\ card} = \frac{\partial \Delta Savings}{\partial Trust} \cdot \frac{\partial Trust}{\partial Time\ with\ card.} \tag{9}$$

So far we have been analyzing the left hand side term. 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-

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[33]Note that because of the timing of the ENCASDU, 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.

hand side of (9). To do this, we merge the administrative data on net balances (from Section 5.3) with the ENCASDU survey data on trust (from Section 6.3). Everyone in this sample has had the card for between 9 and 18 months; we exploit this 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. Because we use the intersection of the two datasets, we also need to restrict the administrative savings data to the that overlap with the timing of the survey.

To estimate the effect of trust on saving, we regress the flow of savings on a trust dummy (which is the complement of the *lack of trust* dummy used in Section 6.3):

$$\Delta Savings_{it} = \zeta Trust_{it} + \varepsilon_{it}. \tag{10}$$

In the 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. Furthermore, self-reported trust is likely measured with error. Since trust is endogenous and potentially measured with error, 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 6.3 that this instrument has a strong first stage.

Three pieces of evidence suggest that the instrument satisfies the exclusion restriction. First, time with the card is uncorrelated with sociodemographic characteristics (shown in Section 4). Second, time with the card does not affect other types of learning, as shown in Figures XII and XIV. Third, *time* with the card (as opposed to the card itself) does not affect transaction costs, which are immediately reduced upon receiving the card: beneficiaries react *instantaneously* to the reduction in transaction costs by increasing the number of withdrawals (Figure VI) and switching to withdrawing at ATMs (Figure XV). 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.

Table III reports the OLS and IV results from estimating (10), 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[34] 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.[35] 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).

To conclude, assuming that *additional* time with the card affects saving only through trust, we find a direct effect of trust on the flow of savings at the beneficiary level. 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% of income. This is, to our knowledge, the first direct causal estimate in the literature of the effect of trust in financial institutions on 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' Consumption, Income and Assets 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

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[34]Income measures are taken from the survey.

[35]The results are robust to estimating a specification analogous to (5) based on models of precautionary saving, controlling for the lagged stock of savings and current transfers, interacted with trust. 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 result in Table III column 3 shows that, using this alternative specification, the part of trust explained by the timing of debit card receipt accounts for a savings rate increase of 2.9% of income (significant at the 10% level), consistent with the results from the simpler specification.

comprehensive modules on consumption, income, and assets for 6272 households.

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. We compare those with cards at the time of the survey to those who had not yet received cards, respectively referring to them as "treatment" and "control" beneficiaries 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. Section 4 formally tested for parallel pre-treatment trends for each of our dependent variables in this survey and failed to reject the null hypothesis of parallel trends. Having established that the identification assumption is plausible, we estimate

$$y_{it} = \lambda_i + \delta_t + \gamma D_{j(i)t} + \nu_{it}, \tag{11}$$

separately for five dependent variables: consumption, income, flow of savings (constructed as income minus consumption), purchase of durables, and an asset index.[36] All variables except the asset index are measured in pesos per month, i indexes households, and t indexes survey rounds.[37] Variables are winsorized at the 5% level to avoid results driven by outliers.

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 γ 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 . Furthermore, one of the assumptions in Section 5.3 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

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[36]Our measure of the flow of savings is imperfect, but is commonly used in the literature (e.g., Dynan, Skinner, and Zeldes, 2004).

[37]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.

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 5 represents an increase in overall savings. Figure XVI 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.[38] 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 flexible time trends as a function of household characteristics.[39]

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 the flow of savings from the household survey data is about the same as that of the increase in the flow of savings in the Bansefi account. In Section 5.3 we estimate that after 1 year with the card, beneficiaries save 3.0% more of their income than the control group. In our survey data, we find a decrease in consumption of 138 pesos per month, while we find an increase in the flow of total savings (a noisier measure) of 236 pesos per month; dividing by average household income in the post-treatment survey wave, 4,629 pesos per month, these equate to effects of 3.0 and 5.1% of income. We cannot reject that the effect sizes in the administrative data and survey data are equal; these results suggest that the increase in savings in the account is new savings, and that there is no crowd-out of other types of saving. These results are consistent with Dupas and Robinson (2013a), Ashraf, Karlan, and Yin (2015), and Kast, Meier, and Pomeranz (2016), who find that increased formal savings in bank accounts does not crowd out other forms of

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[38]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 ($p = 0.092$).

[39]Table B.2 shows that the effects are robust to using the raw data without winsorizing (column 1) and to winsorizing at 1% (column 2) or 5% (column 3, which are our main results presented in Figure XVI); 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 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).

saving.

7.1 Why Does the Debit Card Increase Total Savings?

What savings constraint is the debit card relaxing? In this section we present suggestive evidence consistent with the hypothesis that saving informally is difficult, so that access to a trusted formal savings account allows households to achieve a higher level of overall savings.

It may be tempting to spend money that had been intended to be saved if it is easily accessible, especially at times when she is more financially constrained (Carvalho, Meier, and Wang, 2016). Once the bank is trusted, the account might form a soft commitment device that overcomes these self-control problems (Ashraf, Karlan, and Yin, 2006b; Bryan, Karlan, and Nelson, 2010). Under this hypothesis, one would expect that card receipt would cause consumption to fall more in categories where temptation is the greatest.

To shed some light on this issue, we estimate the following difference-in-differences specification separately for each consumption category g. The dependent variable is the proportion of income spent on consumption category g, and D_{jt} is an indicator of locality j getting the card in period t.

$$\frac{Consumption_{gijt}}{Income_{ijt}} = \lambda_{gi} + \delta_{gt} + \gamma_g D_{jt} + \nu_{gijt} \tag{12}$$

Figure XVII shows that the only two categories for which we find a statistically significant reduction in spending are temptation goods (alcohol, tobacco, and sugar) and entertainment.

Other explanations in the literature that might explain why saving informally is difficult are that intra-household bargaining issues may prevent women from saving at home (Anderson and Baland, 2002; Ashraf, 2009; Schaner, 2015), money saved at home could be in demand from friends and relatives (Baland, Guirkinger, and Mali, 2011; Dupas and Robinson, 2013b; Jakiela and Ozier, 2016), and that that informal savings can be more easily stolen (Banerjee and Duflo, 2007; Schechter, 2007; Alvarez and Lippi, 2009).

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 Learning the Technology

During the period of delay before starting to save, beneficiaries could be learning over time 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. To address the first two of these possibilities, 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 regression (3) with each of these three dependent dummy variables. Figure XIIb 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 6.3 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 survey: less than 2% of beneficiaries cite not saving due to lack of knowledge, and there is no difference between those who have had the card for less than and more than the median time (Figure XIV).

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 where ATMs are located. Using the administrative data, we saw this pattern for withdrawals in Figure VI; we can also test if clients immediately start using the card to withdraw at ATMs and convenience stores rather than bank branches. Figure XV shows the percentage of clients who use their debit card to make at least one withdrawal at an ATM or convenience store instead of going to the bank branch: the adoption rate appears nearly instantaneous, since 85% of beneficiaries make a withdrawal at an ATM in the first period after receiving the card. After that, depending on the four-month period, 89–93% of clients use them to withdraw at ATMs and convenience stores.

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 6.1, 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 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 the self-reported cost of transactions is not different for beneficiaries who have had the card for less vs. more than the median time: Figure XIIc 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.[40]

8.2 Learning the Program Rules

Another type of learning conjectured by Oportunidades program officials when we shared our savings results 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 (3) with the dependent variable equal to 1 if respondents do not save for this reason (which we call *fear of ineligibility* in Figure XIV), or a related reason listed in response to the optional open-ended response to the same survey question.[41] 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 XIV is that fear of being dropped from the program due to having savings in the bank is rarely cited as a reason

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[40]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 XII. 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.

[41]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."

for not saving, accounting for less than 4% of the sample who have had the card for less than the median amount of time. Furthermore, there is no statistically significant difference comparing these beneficiaries to 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 when initially providing bank accounts, they emphasized to beneficiaries that saving in the account would not disqualify them from future benefits. We want to highlight however that Oportunidades did not encouraged beneficiaries to save in Bansefi. If Anything they remarked that the money is theirs and they can withdraw it at any time.

8.3 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. 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 VI.

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

$$y_{mt} = \lambda_m + \delta_t + \sum_{k=-6}^{6} \beta_k D_{m,t+k} + \varepsilon_{jt}, \tag{13}$$

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 ε_{jt} is clustered by municipality. We include one and a half years (six 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 six quarters 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 2011, which is the end of our study period. 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 $\beta_{-6} = \cdots = \beta_{-1} = 0$, 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 $\beta_1 = \cdots = \beta_6 = 0$. 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 B.3).[42]

8.4 Local Income Shocks

Another 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.

8.5 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). 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 2005, beneficiaries had already experienced 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% of accounts had existed for less than two years before they received debit cards. Second, both

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[42]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.

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 5 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 in Figure B.9. We find similar results across the two subsamples.

9 Conclusion

An important literature has documented that is very difficult to get the poor to save in formal accounts. We document that debit cards are a promising avenue to facilitate formal savings and find large effects between 2 and 3% of income after 1–2 years with the card. This effect 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. Extrapolating our estimates from the precautionary savings model to future periods, we predict that beneficiaries are saving towards an equilibrium buffer stock of 2470 pesos on average, which corresponds to 55% of their monthly income.

We explored mechanisms for this effect and find that trust is playing an important role and that it could potentially 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., forthcoming). We show that the trust barrier is not insurmountable: it can be overcome by debit cards, a scalable existing technology. 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.

Finally we documented that savings in the bank account are new savings, rather than a substitution from other forms of saving, and that the increased savings seems to come from disproportionately from reductions in temptation good spending.

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

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also reveals an unmet demand for savings products among program beneficiaries.

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, Liu, and Prasad, 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, Meier, and Pomeranz, 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.

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Figure I: Low Trust in Banks by Education Level in Mexico
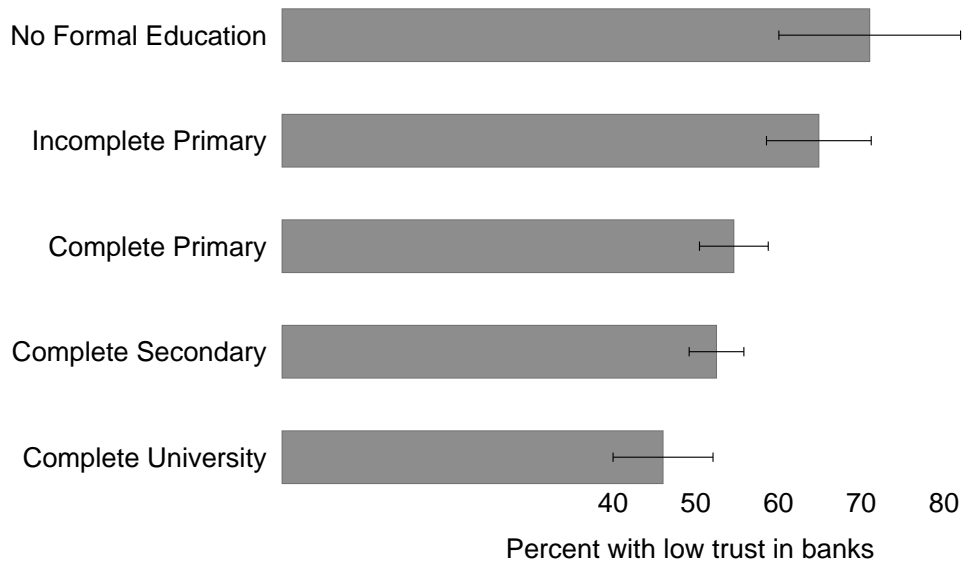
Source: World Values Survey, Mexico, Wave 6 (2012).
Notes: $N = 1993$ individuals. Low trust in banks is defined as "not very much confidence" or "none at all" for the item "banks" in response to the following question: "I am going to name a number of organizations. For each one, could you tell me how 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% confidence intervals.

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

Figure III: Comparison with Other Studies

Study	Intervention	Country	Months	Effect Size
Panel A: Studies with about 1 year duration				
Drexler, Fischer, and Schoar, 2014	Financial education	Dominican Republic	12	
Karlan and Zinman, 2016	Interest rate	Philippines	12	
Kast, Meier, and Pomeranz, 2016	Savings group	Chile	12	
Karlan et al., 2016	Reminders	Philippines	9–12	
Kast and Pomeranz, 2014	Account	Chile	12	
Somville and Vandewalle, 2017	Payment default	India	8	
Dupas and Robinson, 2013	Account or lockbox	Kenya	12	
Prina, 2015	Account	Nepal	13	
This paper (1 year)	Debit card	Mexico	12–15	
Seshan and Yang, 2014	Financial education	India (migrants to Qatar)	13–17	
Panel B: Studies with longer duration				
Ashraf, Karlan, and Yin, 2006	Deposit collection	Philippines	32	
Dupas et al., 2017	Account	Malawi	24	
Karlan et al., 2017	Savings group	Ghana, Malawi, Uganda	22–30	
Dupas et al., 2017	Account	Uganda	24	
Schaner, 2016	Interest rate	Kenya	36	
This paper (2 years)	Debit card	Mexico	20–23	

Stock of Savings as Proportion of Annual Income

Notes: For details on the selection criteria to determine which studies could be included in the figure and how we obtained the effect of savings interventions on the stock of savings as a proportion of annual income in each of these studies, as well as additional details about the studies, see Appendix E. Whiskers denote 95% confidence intervals. Black filled in circles indicate results that are significant at the 5% level, gray filled in circles at the 10% level, and hollow circles indicate results that are statistically insignificant from 0. The estimates from this study are represented by orange squares.

Figure IV: Timing of Rollout and Data

(a) Administrative Bank Account Data
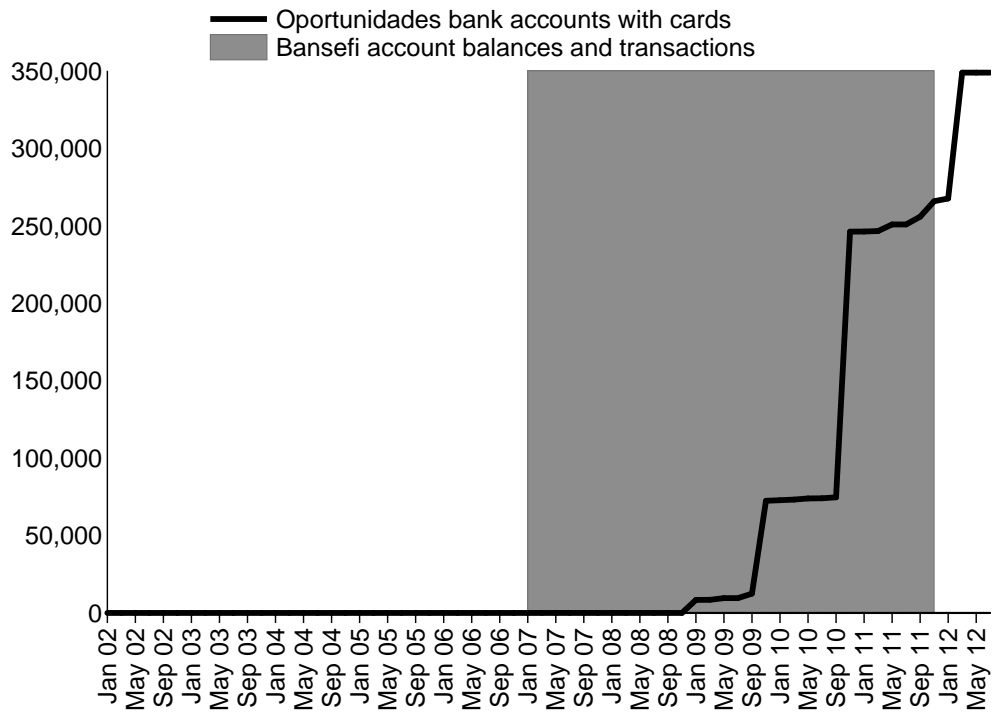
(b) Household Survey Data
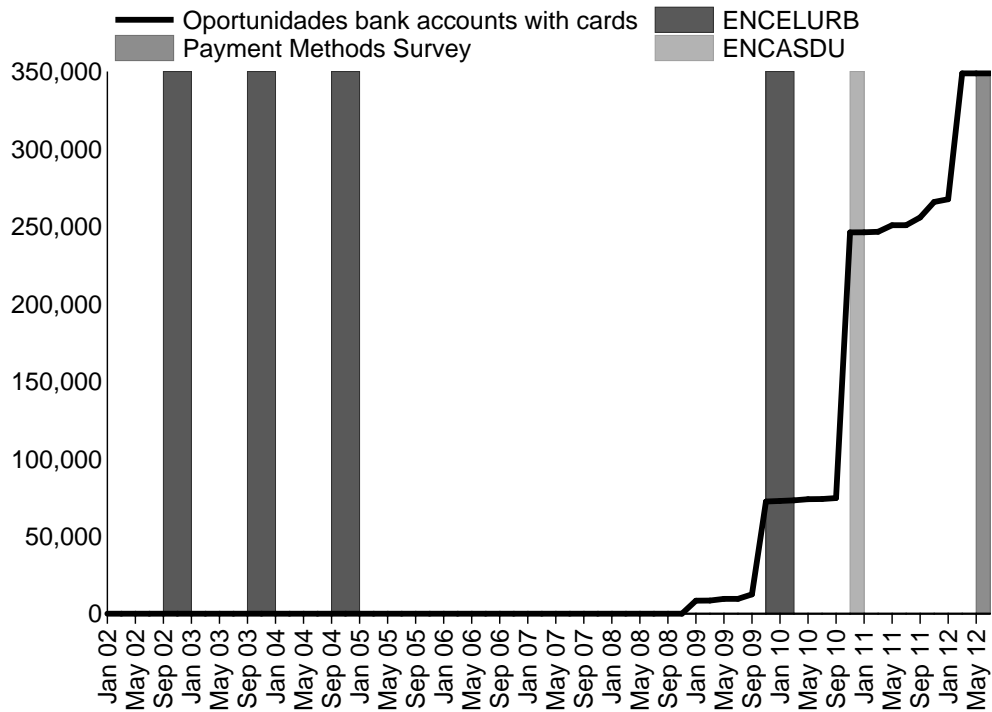
Source: Number of Oportunidades bank accounts with cards by bimester is from administrative data provided by Bansefi.

Figure V: Distribution of Withdrawals and Client Deposits per Bimester
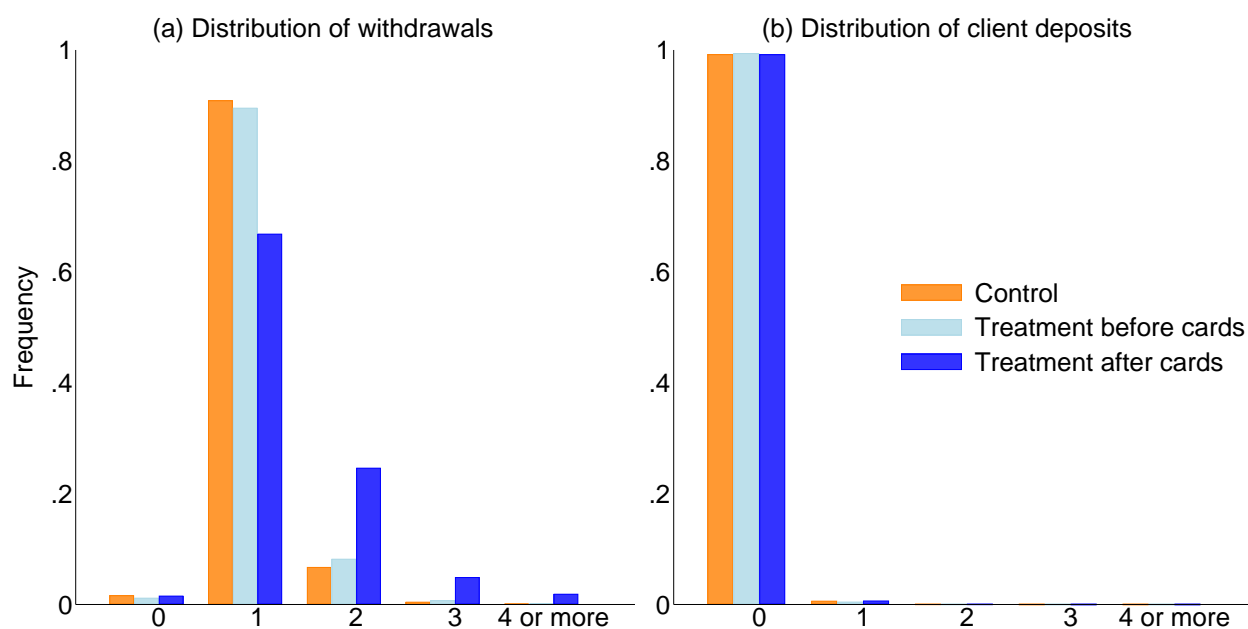
Sources: Administrative data from Bansefi on transactions and timing of card receipt.
Notes: Based on $N = 16{,}787{,}160$ transactions from 343,204 accounts over 4 years. This figure plots the distribution of withdrawals per bimester (panel a) and client made deposits per bimester (panel b). The three categories represent accounts in the control group, the treatment group before receiving the cards and the treatment group after receiving the card. In order to do so we take the mean across all bimesters in the relevant category.

Figure VI: Effect of Debit Card on Number of Withdrawals

Four–month periods relative to switch to cards

Figure VII: Effect of Debit Cards on Savings Balances (Pesos)
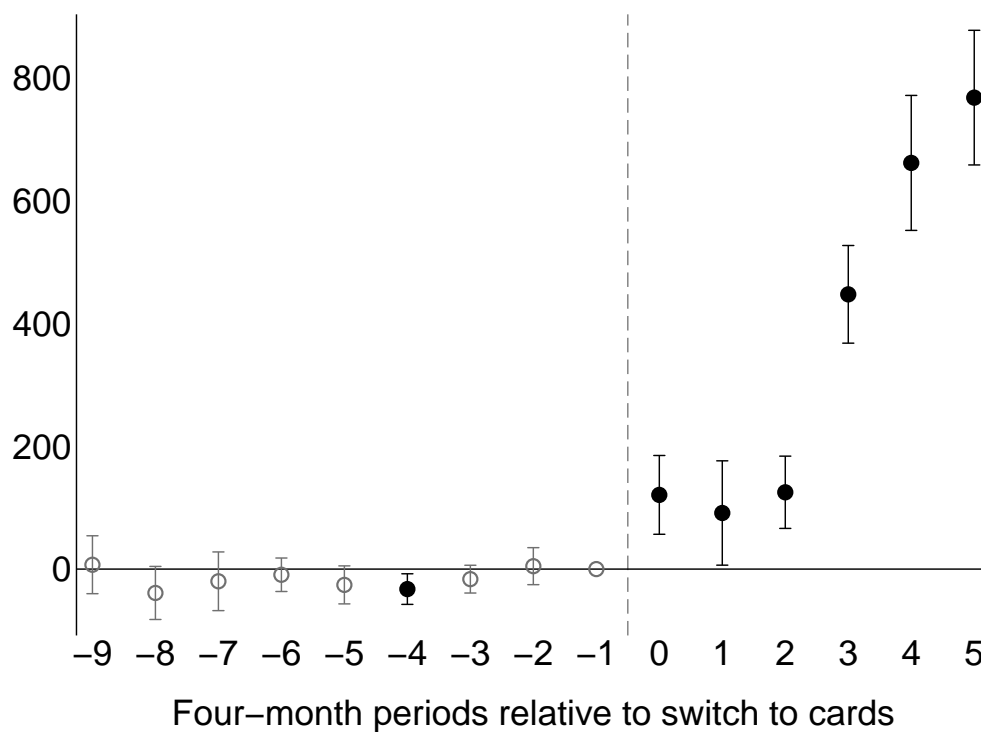
Sources: Administrative data from Bansefi on account balances and timing of card receipt.

Notes: $N = 4{,}664{,}772$ account-period observations from 348,802 accounts. This figure plots ϕ_k from (??). Average balance over each four-month period is the dependent variable, and is winsorized at the 95th percentile. Standard errors are clustered at the bank branch level. Whiskers denote 95% confidence intervals. Black filled in circles indicate results that are significant at the 5% level, gray filled in circles at the 10% 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. Dashed vertical lines indicate timing of debit card receipt.
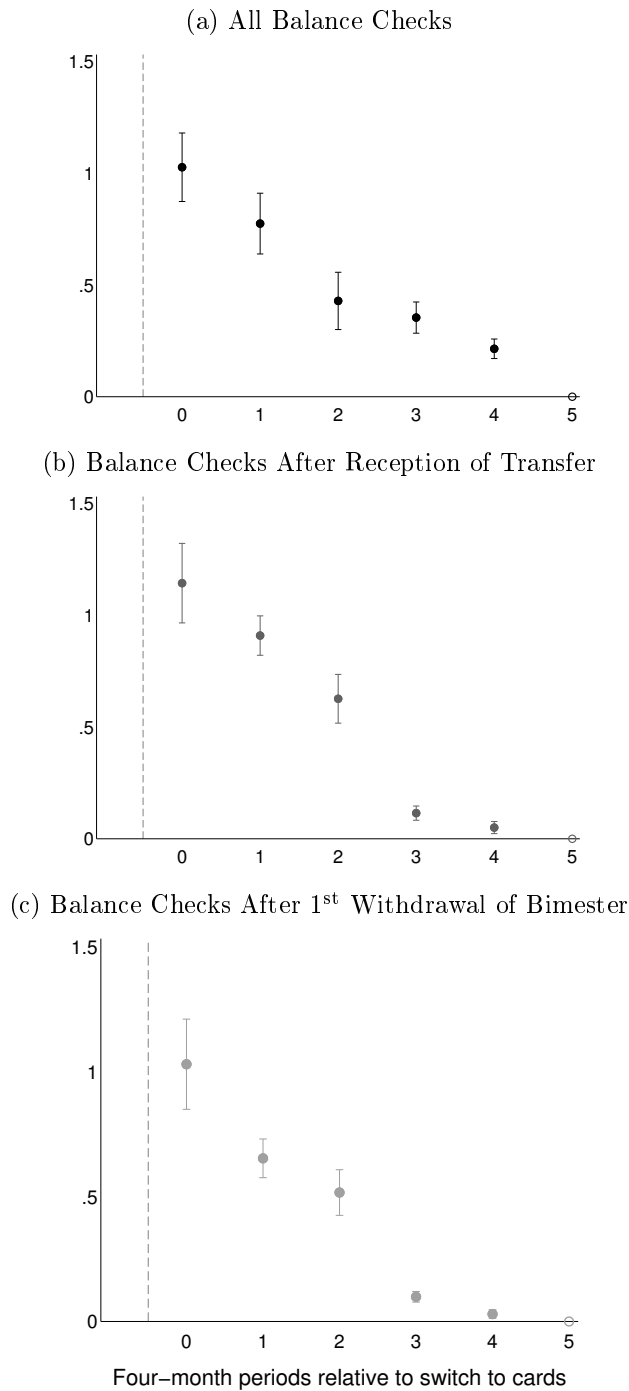
Figure VIII: Effect of Debit Cards on Savings Rate (as Proportion of Income)

Four–month periods relative to switch to cards
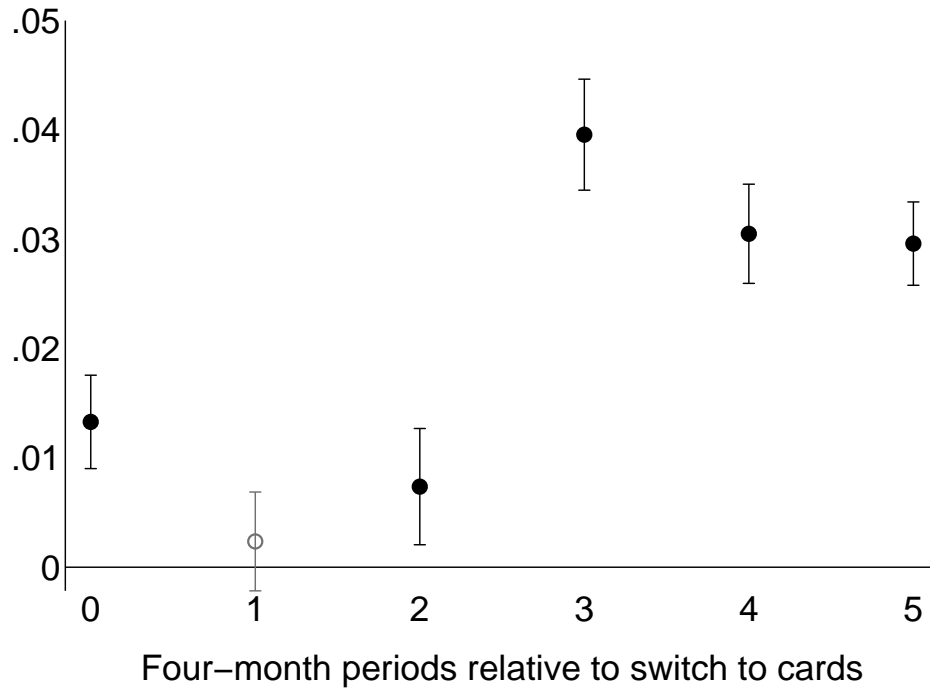
Sources: Administrative data from Bansefi on account balances by bimester, transactions, and timing of card receipt. Notes: $N = 1{,}852{,}416$ account-period observations from 171,441 accounts over 11 periods. This figure plots $\hat{\Phi}_k$ from (6). Panel (a) is from (5) estimated by Blundell and Bond (1998) two-step system GMM, while panel (b) is from (5) but replacing account fixed effects with a treatment dummy, estimated using OLS. Net balances and transfer amounts are winsorized at the 95th percentile. The variance of $\hat{\Phi}_k$ is estimated using the delta method. Standard errors are clustered at the bank branch level. Whiskers denote 95% confidence intervals. Black filled in circles indicate results that are significant at the 5% level, gray filled in circles at the 10% 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. Dashed vertical lines indicate timing of debit card receipt.

Figure IX: Effect of Debit Cards on Probability of Saving
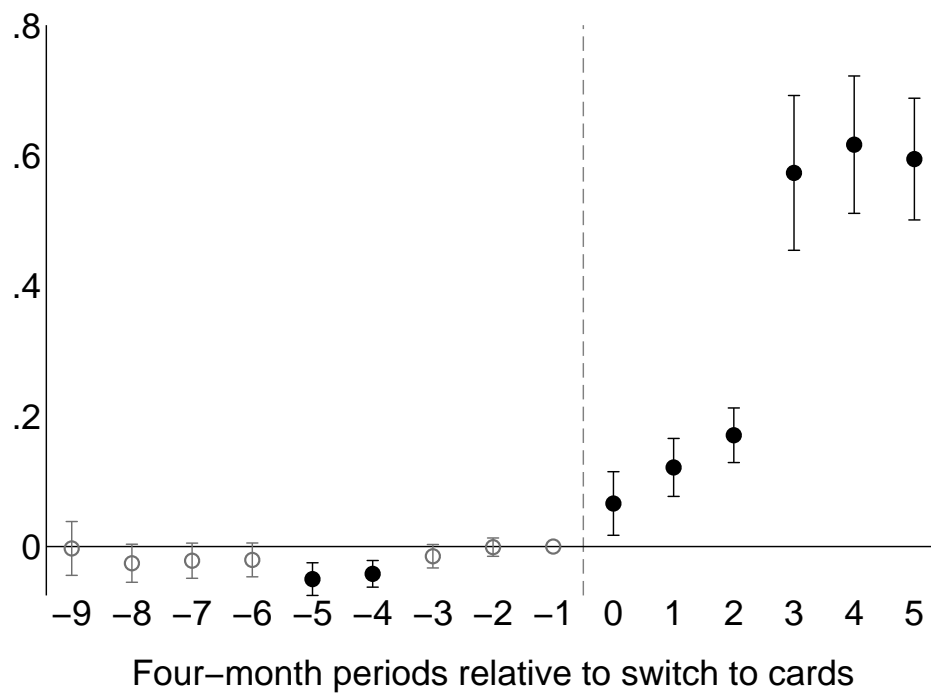
Four–month periods relative to switch to cards

Figure X: Savings Rate Relative Since Starting to Save

Four−month periods relative to **starting to save**
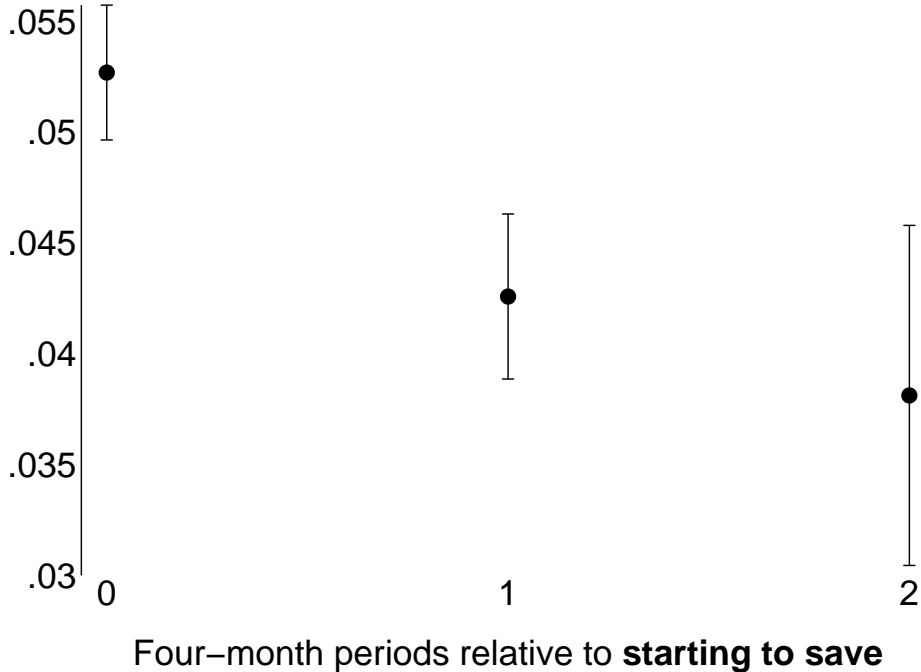
Figure XI: Balance Checks Over Time in Admin. Data (Relative to 5 Periods after Switch to Cards)

(a) All Balance Checks

(b) Balance Checks After Reception of Transfer

(c) Balance Checks After 1st Withdrawal of Bimester

Four–month periods relative to switch to cards

Source: Administrative transactions data from Bansefi.
Notes: There are 1,597,141 balance checks performed by 233,755 unique beneficiaries, which corresponds to 848,664 four month period-beneficiary observations. This figure plots the number of balance checks compared to the last period for which we observe the beneficiary, following equation 7. Balance checks are zero prior to receiving the card since it was only possible to check balances at Bansefi branches, which were not recorded in our data. Each panel corresponds to a narrower definition of balance checks: Panel (a) all balance checks, Panel (b) balance checks after the transfer was received and on a different day than a withdrawal, and Panel (c) after the first withdrawal occurred in the bimester and on a different day than a withdrawal. Standard errors are clustered at the bank branch level. Whiskers denote 95% confidence intervals. Dashed vertical line indicates timing of debit card receipt.

Figure XII: Self-Reported Balance Checks and Knowledge
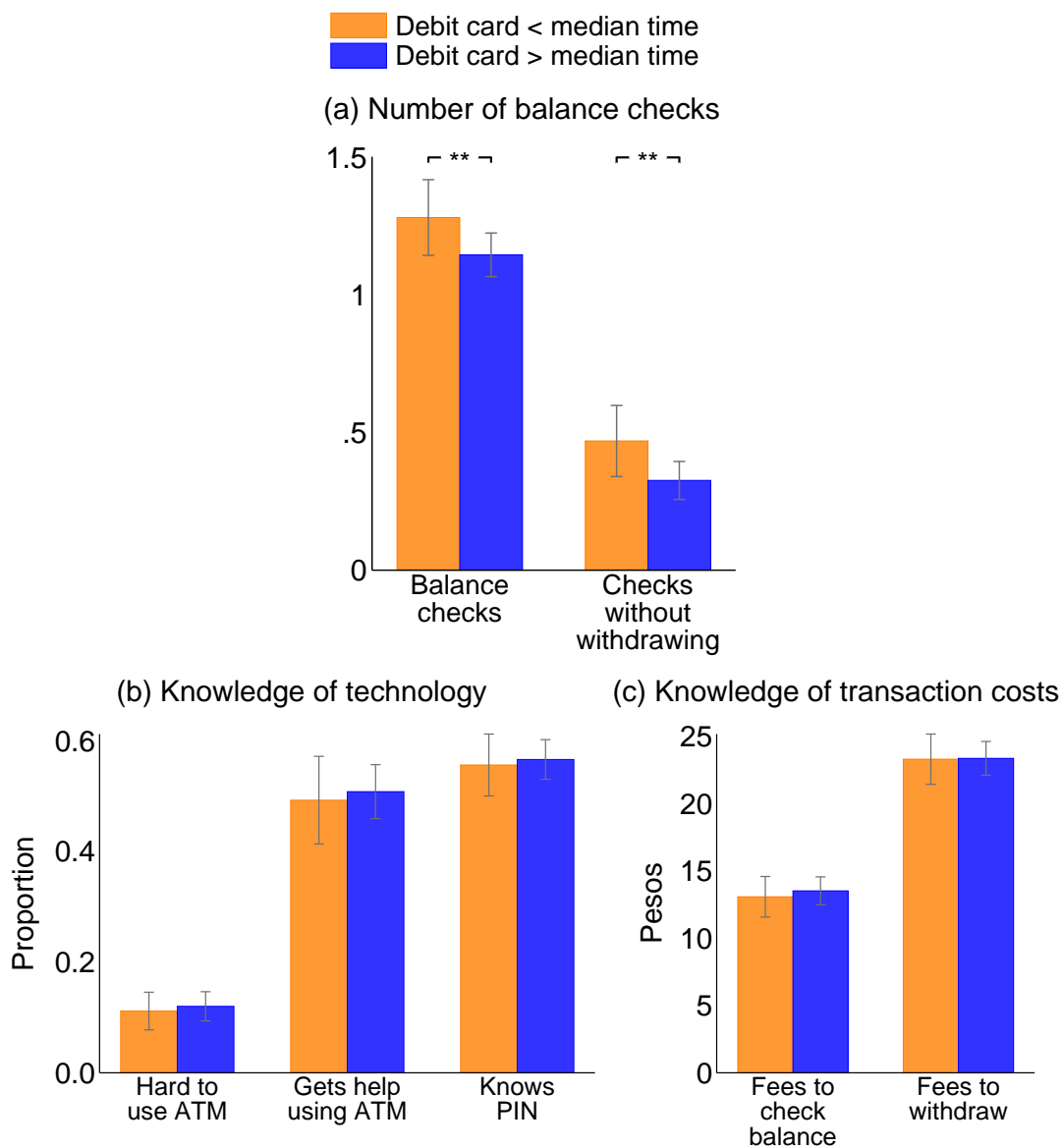
Source: Payment Methods Survey 2012.

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. Standard errors are clustered at the locality level, using pre-treatment (2004) locality. Whiskers denote 95% confidence intervals. * indicates statistical significance of the difference between those with the card for less vs. more than the median time at $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Figure XIII: Within-Account Relation Between Balance Checks and Net Balances
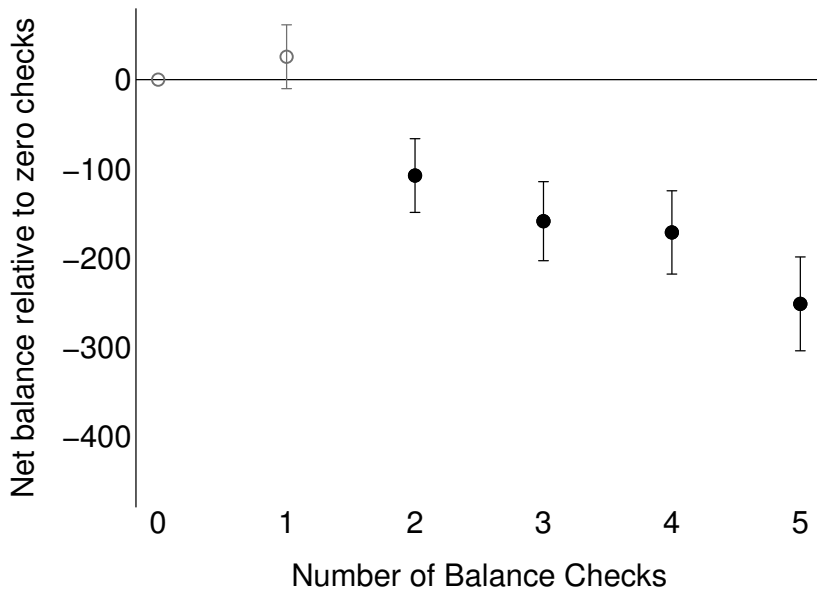
Source: Administrative data from Bansefi on transactions and average balances.
Notes: $N = 1,691,095$ account-bimester observations from 233,747 accounts. This figure plots η_c from (8). These coefficients show the within-account net balance difference in pesos, relative to zero balance checks. Standard errors are clustered at the bank branch level. Whiskers denote 95% confidence intervals. Black filled in circles indicate results that are significant at the 5% level, gray filled in circles at the 10% level, and hollow circles indicate results that are statistically insignificant from 0. This figure shows that 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.

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

Figure XV: Share of Clients Using Debit Cards to Withdraw at ATMs or Convenience Stores

Four month periods relative to switch to cards

Figure XVI: Effect of the Debit Card from Household Survey Panel Data
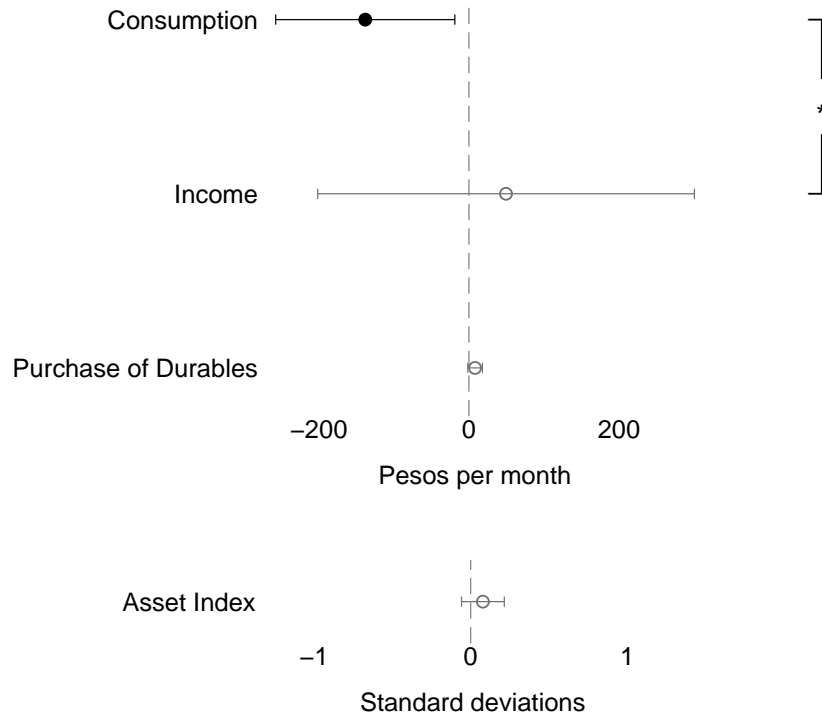
Sources: ENCELURB panel survey combined with administrative data on timing of card receipt and transfer payment histories for each surveyed beneficiary household.

Notes: $N = 9,496$ (number of households $= 2,942$). Dependent 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. Standard errors are clustered at the locality level, using pre-treatment (2004) locality. Whiskers denote 95% confidence intervals. Black filled in circles indicate results that are significant at the 5% level, gray filled in circles at the 10% 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%, winsorized at 5%, and winsorized at 5% with baseline household characteristics interacted with time fixed effects are available in Appendix Table B.2. All regressions include household and time fixed effects, and standard errors are clustered at the locality level, using pre-treatment (2004) locality.

Figure XVII: Effect of the Debit Card on Consumption by Category
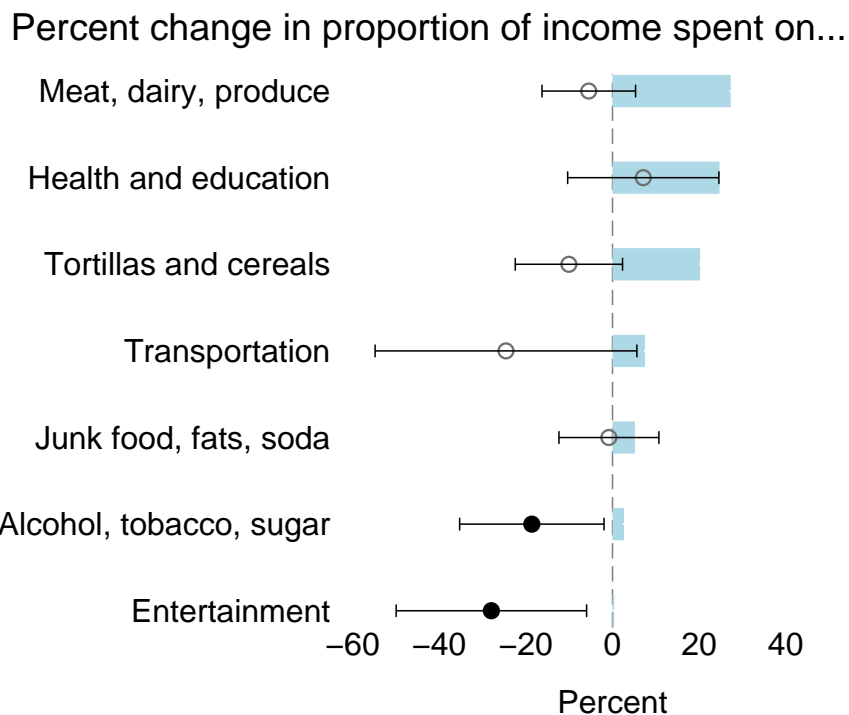
Percent change in proportion of income spent on...

Sources: ENCELURB panel survey combined with administrative data on timing of card receipt and transfer payment histories for each surveyed beneficiary household.

Notes: $N = 9496$ (number of households $= 2942$). Each plotted coefficient is from a separate regression using (??), and shows the percent change in the proportion of income spent on that category of consumption. In other words, the graph plots γ_g/μ_g, where μ_g is the mean proportion of income spent on consumption category g by the control group at baseline. Categories are sorted in descending order of the percent of income spent on each consumption category at baseline, i.e. $100\mu_g$, which is shown by the thick horizontal bars. The whiskers show 95% confidence intervals with no adjustment for multiple hypothesis testing. After adjusting for multiple hypothesis testing using the sharpened false discovery rate (Benjamini, Krieger, and Yekutieli, 2006; Anderson, 2008), the result for the "alcohol, tobacco, and sugar" category is significant at the 10% rather than 5% level ($p = 0.023, q = 0.086$).

Table I: Summary of Data Sources and Identification

Data Source	# Beneficiaries	Period	Main Variables	Variation Used
Administrative bank account data from Bansefi	343,204	Continuous panel: Jan 07-Oct 11	Balances, Transactions, Balance checks	Event study with control (generalized Dif-in-Dif) using phased in geographical roll-out
ENCELURB survey from Oportunidades	2,942	Panel: 02,03,04, Feb 10	Consumption, Income, Assets, Purchase of durables	Dif-in-Dif: received card in 2009 versus received card later
ENCASDU survey from Oportunidades	1,694	Cross-section: Dec 10	Self-reported reasons for not saving: e.g. Lack of trust, Lack of knowledge	Tenure with card below /above median (= 14 months)
Medios de Pagos survey from Oportunidades	1,617	Cross-section: Jun 12	Self-reported number of balance checks, Knowledge of technology	Tenure with card below /above median (= 12 months)

Table II: Comparison of Baseline Means

Variable	Control	Treatment	Difference T–C	Discrete Time Hazard
Panel A: Locality-level data				
Log population	10.58	11.13	0.55***	0.36***
	(0.11)	(0.08)	(0.13)	(0.04)
Bansefi branches per 100,000	1.39	1.19	−0.20	−0.02
	(0.30)	(0.10)	(0.32)	(0.01)
% illiterate	8.03	6.69	−1.35	−0.07***
	(0.79)	(0.24)	(0.83)	(0.02)
% attending school	4.30	4.23	−0.07	0.00
	(0.32)	(0.09)	(0.34)	(0.03)
% with dirt floors	6.28	5.84	−0.44	0.01
	(0.98)	(0.30)	(1.03)	(0.01)
% without piped water	8.31	6.64	−1.67	0.00
	(1.58)	(0.49)	(1.65)	(0.00)
% without electricity	4.12	4.10	−0.01	−0.01
	(0.30)	(0.10)	(0.32)	(0.02)
Average occupants per room	1.22	1.14	−0.07*	−0.54
	(0.04)	(0.01)	(0.04)	(0.36)
Panel B: Administrative bank account data				
Number of client deposits	0.01	0.01	0.00	1.23
	(0.00)	(0.00)	(0.00)	(0.93)
Number of withdrawals	0.98	0.96	−0.02	0.03
	(0.01)	(0.01)	(0.01)	(0.35)
% withdrawn	100.01	100.02	0.00	−0.01
	(0.02)	(0.02)	(0.03)	(0.01)
Size of Oportunidades transfer	1077.91	1241.62	163.72***	0.00
	(10.56)	(12.90)	(15.54)	(0.00)
Net balance	144.07	153.66	9.59	0.00
	(6.22)	(8.61)	(10.09)	(0.00)
Years with account by Jan 2009	4.22	4.39	0.17	0.09***
	(0.08)	(0.12)	(0.15)	(0.03)

Sources: Locality-level data is from CONEVAL based on the 2005 Census. Administrative bank account data is account balance and transactions data from Bansefi, averaged over all baseline (2007–2008) periods.
Notes: T = treatment; C = control. Control refers to accounts that received cards between November 2011 and April 2012 (after our study period). 260 localities are in control and 30 in treatment. The discrete time hazard model uses the timing of debit card receipt throughout the rollout (January 2009 to April 2012) and includes a 5th-order polynomial in time, where time is measured by bimester.

Table III: 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: $N = 1,330$ beneficiary households merged with accounts. The specification for column 1 is $\hat{\zeta}/\overline{Y}$ from (10) with OLS; column 2 is $\hat{\zeta}/\overline{Y}$ from (10) with 2SLS, instrumenting trust with a set of dummies for timing of card receipt; column 3 is $\hat{\Phi} = (\hat{\zeta} + \hat{\xi}\omega_{-1} + \hat{\psi}\mu)/\overline{Y}$ from

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

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 average income.

Supplementary Material (FOR ONLINE PUBLICATION ONLY)

Appendix A   Sample of Materials Received by Beneficiaries

Figure A.1: Flyer Provided with the Debit Card (Front)

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 A.2: Flyer Provided with the Debit Card (Back)

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 A.3: 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: ▬▬▬ Código Postal: **55450**

Domicilio: ▬▬▬

Folio Titular: ▬▬▬

Nombre Titular: ▬▬▬

Identificador de Familia: ▬▬▬

Fase de Incorporación: **35**

Esquema de Apoyos: **Urbano 1**

Colonia: ▬▬▬

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: This is a sample of the calendars that provide 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 B.1: Geographic Coverage and Expansion of Debit Cards across Time and Space

Notes: 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 B.2: Distribution of Months with the Card at Time of Survey, Payment Methods Survey
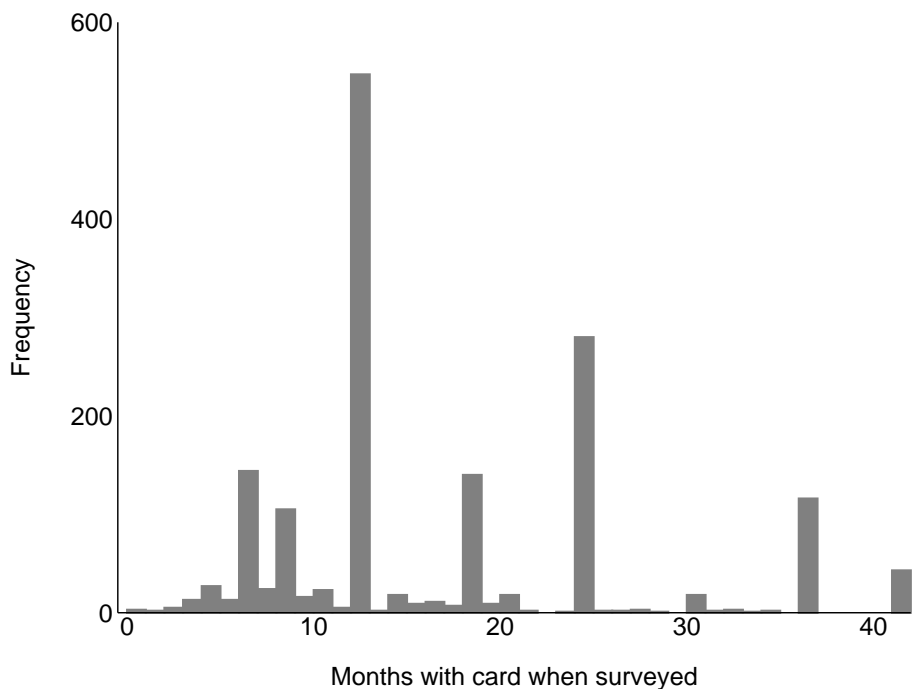
Notes: We use self-reported months with the card in the Payment Methods Survey.

Figure B.3: Distribution of Card Expansion in ENCASDU Sample
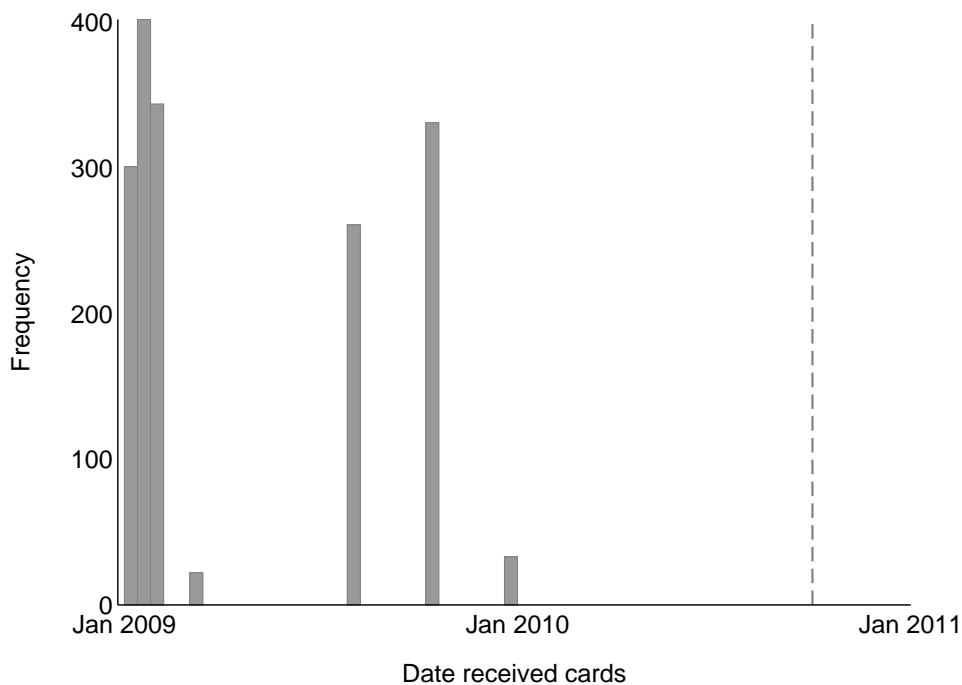
Notes: Dashed vertical line indicates timing of survey.

Figure B.4: Distribution of Card Expansion in ENCELURB Sample

Figure B.5: Number of Withdrawals Over Calendar Time in the Control Group

Figure B.6: Effect of Debit Card on Savings Rate without Transfer Interactions

Figure B.7: Proportion of Treatment Accounts Saving over Time

Four−month periods relative to switch to cards
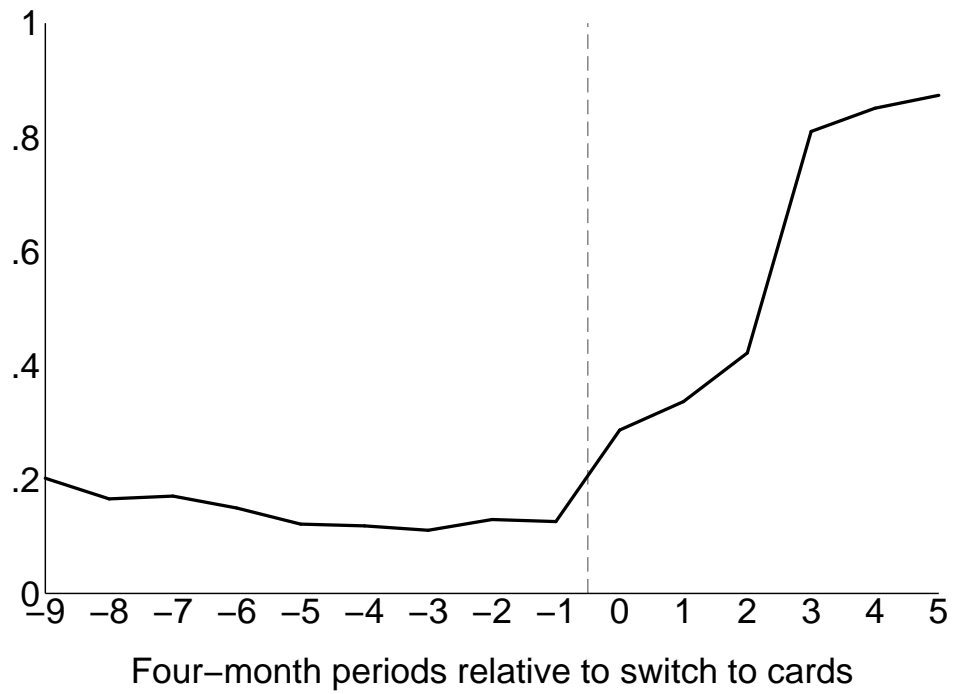
Source: Administrative data from Bansefi on transactions and average balances.
Notes:

Figure B.8: Savings Balance Relative Since Starting to Save

Four−month periods relative to **starting to save**
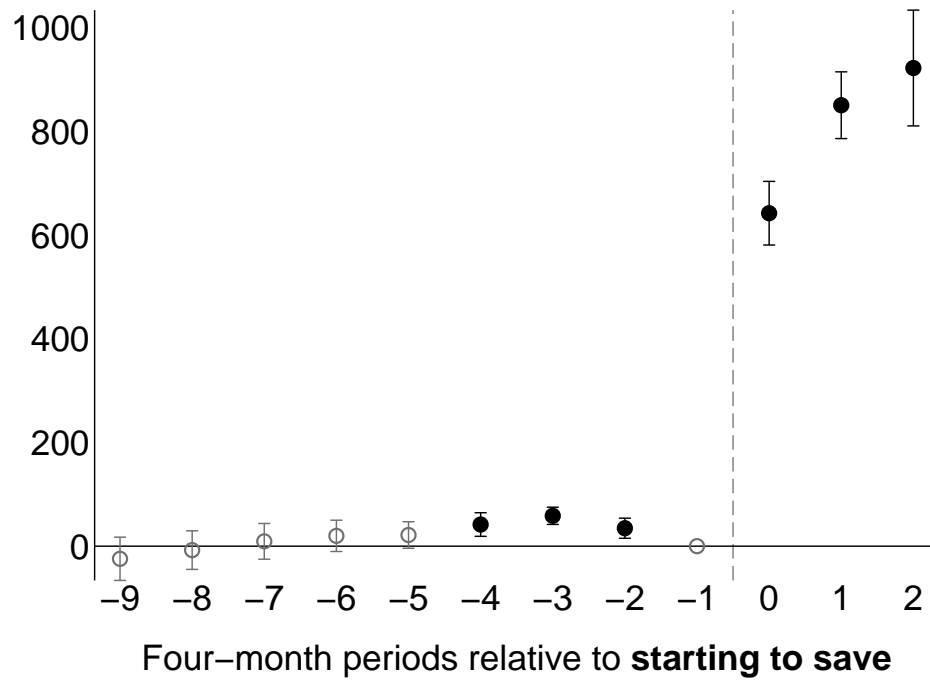
Sources:
Notes:

Figure B.9: Separated by Time with Account: Effect of Debit Cards on Savings Rate (as Proportion of Income), Wave 1 vs. Control

Figure B.10: Within-Account Relation Between Balance Checks (Non-same day and after transfer) and Net Balances
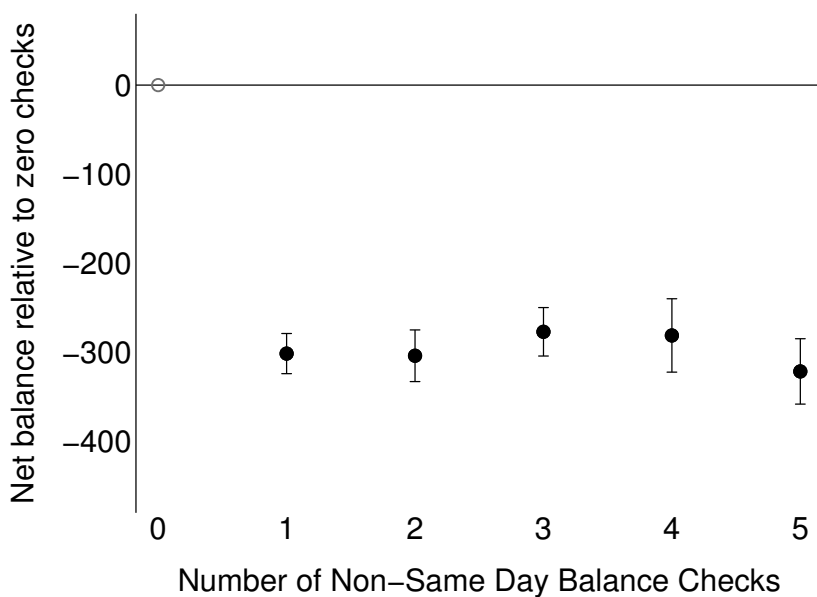
Source: Administrative data from Bansefi on transactions and average balances.

Notes: This figure plots η_c from (8). These coefficients show the within-account net balance difference in pesos, relative to zero balance checks. Balance checks are restricted as occurring post-transfer reception and on a different day than a withdrawal, which we argued are precisely the checks used to monitor the account and build trust. Standard errors are clustered at the bank branch level. Whiskers denote 95% confidence intervals. Black filled in circles indicate results that are significant at the 5% level, gray filled in circles at the 10% level, and hollow circles indicate results that are statistically insignificant from 0. This figure shows that 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.

Table B.1: 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)	

Source: ENCASDU 2010.
Notes: $N = 1,694$, or less for variables that were missing for some observations. Standard errors clustered at the locality level.

Table B.2: 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%

Sources: ENCELURB panel data merged with administrative data on beneficiary status and timing of debit card receipt.

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. Dependent 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 B.3: Supply-Side Response

| | Total | | Bansefi | |
	ATMs	Branches	ATMs	Branches
Current quarter	−0.37	−0.01	0.00	−0.01
	(1.51)	(0.34)	(0.00)	(0.02)
1 quarter lag	−1.79	0.10	−0.01	0.02
	(2.49)	(0.37)	(0.01)	(0.02)
2 quarter lag	2.04	0.12	0.01	0.01
	(3.72)	(0.39)	(0.01)	(0.02)
3 quarter lag	−0.57	−0.01	−0.01	0.02
	(1.11)	(0.29)	(0.01)	(0.02)
4 quarter lag	2.29	−0.28	0.00	−0.04
	(2.54)	(0.64)	(0.00)	(0.03)
5 quarter lag	−1.13	0.08	0.00	0.00
	(2.56)	(0.81)	(0.00)	(0.02)
6 quarter lag	−0.31	0.94	0.00	0.02
	(3.60)	(0.67)	(0.00)	(0.02)
1 quarter lead	0.66	−0.25	0.00	−0.01
	(1.74)	(0.40)	(0.00)	(0.02)
2 quarter lead	3.96	0.11	0.01	0.00
	(3.65)	(0.40)	(0.01)	(0.02)
3 quarter lead	−0.06	0.26	−0.01	−0.01
	(4.18)	(0.65)	(0.02)	(0.03)
4 quarter lead	−2.50	0.83	0.00	−0.04
	(4.04)	(0.78)	(0.01)	(0.05)
5 quarter lead	3.97	0.27	0.00	0.01
	(3.19)	(0.40)	(0.00)	(0.02)
6 quarter lead	5.18*	−0.98	0.01	−0.04
	(3.03)	(0.97)	(0.01)	(0.03)
Mean control group	46.08	37.13	0.09	1.42
F-test of lags	0.59	0.60	0.73	1.15
[p-value]	[0.74]	[0.73]	[0.63]	[0.33]
F-test of leads	0.87	1.00	1.24	0.79
[p-value]	[0.52]	[0.42]	[0.29]	[0.58]
Municipality fixed effects	Yes	Yes	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes

Source: Data obtained from CNBV.
Notes: $N = 2,491$ municipality-quarter observations from 199 municipalities. This table shows β_k from (13). The F-test of lags tests $\beta_{-6} = \cdots = \beta_{-1} = 0$; the F-test of leads tests $\beta_1 = \cdots = \beta_6 = 0$. * indicates statistical significance at $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table B.4: 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%

Sources: ENCELURB panel data merged with administrative data on beneficiary status and timing of debit card receipt.

Dependent 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. Dependent variables are winsorized at the 5% level.

Appendix C   Mechanical Effect

This appendix defines the "mechanical effect," which we use to compute net balances. We explain the logic behind the mechanical effect, present an example, and provide a step by step guide for its computation, summarized in Table C.1.

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) savings, or even saving from one period to the next, our goal is to net it out from average balances and construct a measure of net balances, $Net\ Balance_{it}$. Changes in the mechanical effect can arise due to changes in the *frequency* of withdrawals. For example, if client A begins the period with 0 balance, receives 2,000 pesos in her account, and withdraws 1,000 pesos on the first day of the period, and the other 1,000 pesos midway through the period, her average balance will equal $1,000 * 0 + 1,000 * \frac{1}{2} = 500$ pesos. Compared to client B who withdrew the entire 2,000 pesos on the first day of the period, client A's average balance is 500 pesos higher, but both end the period with a balance of zero. Their net balances, constructed as average balance minus 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 generally disburses transfers within the first week of the bimester, and the program distributes calendars stating the dates when accounts will be credited. Nevertheless, beneficiaries may not withdraw their benefits on the day they are deposited, which also leads to a mechanical effect that contributes to the average balance. In our data, the mechanical effect can thus change for debit card recipients relative to the control group as a result of increased withdrawal frequency of smaller amounts and changes in 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 exceed the amount deposited that period. We use an example to exemplify the steps involved.

C.2   Example:

1. Select a pattern where clients received a single deposit (the most common, although as explained previously, beneficiaries receive more than one Oportunidades deposit in some bimesters)

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 $\Rightarrow$ the mechanical effect is just the time lapse between the deposit and the first withdrawal times the deposit amount ($lapse_{DW_1} * D$).

   (b) The deposit is larger than the first withdrawal but smaller than the sum of the two withdrawals $\Rightarrow$ 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 ($lapse_{DW_1} * W_1 + lapse_{DW_2} * (D - W_1)$).

   (c) The deposit is larger than the sum of the withdrawals $\Rightarrow$ 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 ($lapse_{DW_1} * W_1 + lapse_{DW_2} * (W_2)$).

Table C.1 shows the cases we considered as well as their prevalence in the data.

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 beneficiary-bimester pairs receive a single transfer in the bimester and 15% received two transfers in the same bimester. See footnote 23 for a description of the reasons some benefeciary-bimester pairs include more than one transfer.

2. We determine the pattern of transactions: for example, a beneficiary who first received a deposit and then performed two withdrawals has a sequence (D, W_1, W_2), or DWW for short.

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 larger than the sum of withdrawals.

4. We compute the mechanical effect. To do this, 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 in the account through the end of and into the next bimester.

Table C.1: Computation of Mechanical Effect

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

Notes: D_i indicates the ith deposit and W_i indicates the ith withdrawal within a bimester. $lapse_{D_iW_j}$ measures the number of days between the ith deposit and the jth withdrawal, divided by the number of days in the bimester. The patterns listed here represent 95% of all bimonthly patterns, but all patterns representing at least 0.01% of all account-bimester pair patterns have been coded to obtain an estimate of the mechanical effect.

Appendix D   Details on the GMM estimation

Appendix E   Comparison with Other Studies

The savings rates in Figure III are drawn form papers which meet the following five criteria.

1. We try to include all studies measuring the impact of savings interventions on the stock of savings. 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 exclude studies which measure the impact of income shocks and cash transfers on savings, since these are not savings interventions.

2. We only include studies with a duration of at least 6 months.

3. We focus on interventions in developing countries aimed at adults.

4. Finally, to estimate the savings rate we need to divide the change in savings by total household income. We therefore only include studies that include average household income in their tables, or a household income variable in the replication data. We exclude studies that only provide labor income of the respondent rather than total household income.

5. We include papers published or accepted for publication in peer-reviewed journals, NBER working papers, and other working papers listed as "revise and resubmit" on authors' websites as of July 2017. This filter intends to avoid using preliminary results.

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 studied (e.g., in the bank). This appendix provides more detail on how the savings rates in Figure III were computed for each study. We also provide details about some studies that were excluded because they did not meet all of the above criteria.

Ashraf, Karlan, and Yin (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 annual household income (129,800 pesos; Table 1, column 2 of the December 2005 version but not included in the final version).

Beaman, Karlan, and Thuysbaert (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 exclude this study from the comparison because it does not include a measure of total household income.

Blumenstock, Callen, and Ghani (2017).   This study looks at the effect of default savings contributions out of salary payments in Afghanistan. We exclude this study from the comparison because it includes a measure of salary, but not a measure of total household income.

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 exclude this study from the comparison because it does not include a measure of total household income.

Callen et al. (2014).   This study looks at the effect of offering deposit collection to rural households in Sri Lanka. We exclude this study from the comparison because it measures the effect of the intervention on the *flow* of savings, but not on the stock. (Note that the flow of savings is self-reported and has a minimum of 0 in the replication data, which means that using the estimate on the flow of savings to estimate the stock could be inaccurate if the flow of savings is negative in some accounts during some months.)

Drexler, Fischer, and Schoar (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 total household income, we use microenterprise sales in the denominator (the sample consisted entirely of microentrepreneurs). We calculate average annual sales among the treatment group at endline in the microdata.

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 income among the treatment group (which we calculate using the replication data).

Dupas et al. (forthcoming).   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 total monetary savings of $1.39 in Uganda and $4.98 in Malawi (Table 4, column 7). We divide by the sum of income of the respondent and income of the spouse (to approximate total household income), which is given in footnote 27.

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.) We divide by average annual income of the treatment group (estimated using the replication data).

Karlan et al. (2017). This study looks at the effect of savings groups on financial inclusion, microenterprise outcomes, women's empowerment, and welfare. Using the replication data, we replicate the results in Table S3 on the effect of savings groups on total savings balance, and divide this by endline average annual income for the treatment group (estimated using the replication data).

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 annual income of the treated (estimated using the replication data).

Kast, Meier, and Pomeranz (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 divide the savings effect by average number of household members times average per capita household monthly income in the treatment group (Table 1) times 12 months.

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 divide this by the average number of household members times average per capita household monthly income (Table 1) times 12 months.

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 bank 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 annual income among the treatment group from the endline survey (available in the replication data).

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 12 months.

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 12 months.

Somville and Vandewalle (forthcoming). 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 (given in the text of the 2016 working paper version, p. 20) times 52 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 per agent for male-headed households and 0.032 per agent for female-headed households (Table 1). We exclude this study from the comparison because it does not include a measure of total household income.