How Debit Cards Enable the Poor to Save More

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We study a natural experiment in which debit cards are rolled out to beneficiaries of a cash transfer program, who already received transfers directly deposited into a savings account. Using administrative account data and household surveys, we find that before receiving debit cards, few beneficiaries used the accounts to make more than one withdrawal per period, or to save. With cards, beneficiaries increase their number of withdrawals and check their balances frequently; the number of checks decreases over time as their reported trust in the bank and savings increase. Their overall savings rate increases by 3–4 percent of household income. (*JEL*: D14, D83, G21, O16)

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1 Introduction

A remarkably large number of households worldwide do not have sufficient savings to cope with relatively small shocks (Alderman, 1996; Dercon, 2002). For example, more than 40% of Americans report that they "either could not pay or would have to borrow or sell something" to finance a $400 emergency (Federal Reserve, 2017). Some hypothesize that this is due to a lack of access to low-cost, convenient formal savings devices (Karlan, Ratan and Zinman, 2014). When the poor do save in formal financial institutions, there are a number of well-documented causal impacts including increased investment in agriculture, microenterprises, and children's education, increased ability to cope with shocks, and reduced debt.[1] These positive impacts motivated Mullainathan and Shafir (2009, p. 126) to posit that access to formal financial services "may provide an important pathway out of poverty."

Nevertheless, "uptake and active usage remain puzzlingly low" (Karlan et al., 2016, p. 2), even when accounts are offered without fees (Dupas et al., forthcoming). In fact, over 40% of adults worldwide do not have a formal bank or mobile money account (Demirgüç-Kunt et al., 2015). Similarly, cash transfer recipients paid through direct deposit into bank accounts generally withdraw the entire transfer amount in one lump sum each pay period (e.g., Aker et al., 2016; Muralidharan, Niehaus and Sukhtankar, 2016).

We study a natural experiment in which debit cards tied to existing savings accounts were rolled out geographically over time to beneficiaries of the Mexican conditional cash transfer program Oportunidades. Debit cards alleviate two important barriers to using formal financial institutions. First, debit cards lower the indirect transactions costs of accessing money in an account by facilitating more convenient access via a network of ATMs.[2] Second, debit cards also reduce the indirect cost of checking balances, which is a mechanism that individuals can use to monitor that banks are not unexpectedly reducing balances. Through monitoring, individuals build trust that money deposited in a bank account will be there when wanted. In fact, a lack of trust in banks to not "steal" their savings—often through hidden and unexpected fees—is frequently listed as a primary reason why the poor are hesitant to use banks (Dupas et al., 2016; FDIC, 2016). Among Oportunidades beneficiaries, "repeated balance checking is common, usually out of anxiety to confirm that their

[1] See Dupas and Robinson (2013*a*); Kast and Pomeranz (2014); Prina (2015); Brune et al. (2016).

[2] In our context, debit cards reduce the *indirect* time and transport transaction costs of accessing money in the bank account, as savings can be withdrawn at any bank's ATM, rather than only at bank branches of a particular bank. In contrast, Schaner (2017) 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.

money is still there" (CGAP, 2012, p. 20).

The phased geographic rollout of debit cards to Oportunidades recipients provides plausibly exogenous variation in the timing of assignment of debit cards, allowing us to estimate the causal impact of having a debit card on saving in a difference-in-differences event study framework. Before the rollout, beneficiaries had been receiving their transfers through savings accounts without debit cards, but rarely used their accounts to save: they typically withdrew the full transfer amount shortly after receiving it.[3]

Using high-frequency administrative data from nearly 350,000 beneficiary bank accounts in 359 bank branches nationwide over five years, we find that debit cards caused a large and significant increase in the active use of the accounts. The number of transactions (withdrawals) jumped immediately, while the proportion of beneficiairies holding significant positive savings in their bank account increased more slowly from 13% to 87% over a two-year period. After two years, beneficiaries with debit cards save an additional 3–4% of income more each month than those without debit cards.

We also estimate a model of precautionary savings; these models predict that an individual's savings rate is decreasing in her stock of savings as it approaches the equilibrium buffer stock or savings target (Carroll, 1997). We confirm this prediction in our data, and use the model to estimate the equilibrium buffer stock to be 5% of annual income. After saving in the account for one year, beneficiaries accumulate half of the equilibrium buffer stock on average; after two years, they reach two-thirds of the target.

Using a rich, high-quality household panel survey of a subsample of the beneficiaries, we then test whether the increase we observe in formal savings is an increase in *total* savings or a substitution from other forms of saving, both formal and informal. We find that after one year with the card, while there is no effect on income, there is a significant reduction in consumption equal to about 4% of income—suggesting that the total savings rate rose by a similar amount to what we observe in the administrative bank account data. We also find no differential change in the stock or flow of assets in the treatment group compared to the control. Hence, the increase in formal bank account savings appears to be fully financed by a reduction in consumption and does not appear to crowd out other forms of saving (consistent with results in Dupas and Robinson, 2013*a*; Ashraf, Karlan and Yin, 2015; Kast, Meier and Pomeranz, 2016).

[3]Prior to receiving cards, 13% of beneficiaries saved in the bank accounts. This is consistent with findings from other countries such as Brazil, Colombia, India, Niger, and South Africa, in which cash transfers are also paid through bank or mobile money accounts and recipients generally withdraw the entire transfer amount in one lump sum withdrawal each pay period (CGAP, 2012; Aker et al., 2016; Muralidharan, Niehaus and Sukhtankar, 2016).

Exploring mechanisms, we find that the reduction in transaction costs by itself does not fully explain the increase in savings. The number of withdrawals made per month jumps immediately after receiving the card, and 16% of beneficiaries begin saving immediately, likely due to the immediate reduction in transaction costs. For the majority of beneficiaries, however—who begin saving only after a delay—the increase in savings is likely driven by a combination of reductions in both transaction and monitoring costs. Upon receiving a debit card, most beneficiaries do not begin saving immediately, but instead appear to first use the card to 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 savings held in their bank accounts.

Three main pieces of evidence support the mechanism of using the card to monitor balances and thereby build trust. First, using the high-frequency administrative data on bank account transactions, we observe that upon receipt of the debit card, clients initially use the card to check their account balances frequently, but reduce balance check frequency over time. Simultaneously, the proportion of beneficiaries who save in the account and the amount that they save rises over time with the card. We confirm this relationship statistically by testing for a negative *within*-account correlation between balance checks and savings. Second, in survey data from a subsample of the beneficiaries, those who have had their debit cards for a short period of time report significantly lower rates of trusting the bank than beneficiaries who have had their debit cards longer. Finally, linking the survey data on self-reported trust with the corresponding cross-section of administrative data on account balances, we establish a direct link between trust and increased saving: we instrument trust with length of time since card receipt and find that beneficiaries who trust the bank save an additional 3% of their income. We also rule out a number of alternative mechanisms including falling transaction costs over time and learning the banking technology, among others.

We thus make four main contributions to the literature. First, we show that debit cards caused a large and significant increase in the number of active account users in terms of both transactions and savings. We show that the savings effect comes from an increase in total savings achieved by reducing consumption, rather than a substitution from other forms of saving. The magnitude of the savings effect is larger than that of most other

[4] 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. In addition, the reduced indirect transaction costs of accessing money in the account increase the potential benefit of saving formally, which would increase the beneficiary's desire to learn whether the bank is trustworthy.

interventions studied in the literature. Comparing the stock of savings accumulated after 1–2 years in our study (relative to total household income) with estimates from other savings interventions—including offering commitment devices, no-fee accounts, higher interest rates, lower transaction costs, and financial education—we find that debit cards have a substantially larger effect (Figure 1). Two other studies that also find a large effect on savings are Suri and Jack (2016), who study the impact of mobile money, and Callen et al. (2014), who study the impact of weekly home visits by a deposit collector equipped with a point-of-sale terminal. Like debit cards, these technologies both lower transaction costs and enable clients to more easily monitor account balances.[5]

Second, we directly investigate two barriers to saving: indirect transaction costs and trust. We find ample evidence that the immediate increase in the number of transactions is due to the decreased transaction costs of accessing the account, while the delayed increase in the proportion of beneficiaries who save is due to allowing clients to more easily monitor the bank by checking account balances, thereby increasing their trust in the bank over time. While studies have explored the role of trust in stock market participation, use of checks instead of cash, and take-up of insurance products (Guiso, Sapienza and Zingales, 2004, 2008; Cole et al., 2013), there are few studies that rigorously explore the role of trust in banks (Karlan, Ratan and Zinman, 2014).[6]

Third, we provide estimates of equilibrium buffer stock savings and how the marginal savings rate evolves over time for a poor population as they progress toward their savings target. Finally, we study an at-scale policy change affecting hundreds of thousands of households across the country; our study thus uses a much larger sample with broader geographic coverage than most of the literature.

In summary, debit cards combined with ATMs or point-of-sale terminals (and, in other contexts, mobile phones combined with mobile money platforms) are low-cost technologies that reduce the indirect transaction costs of both accessing funds in an account and checking balances to build trust in financial institutions. These technologies are simple,

[5]Mobile money clients can easily check account balances from their phones, and Callen et al.'s (2014) deposit collection includes a receipt printed in real-time with the deposit amount and new account balance after each weekly deposit—a feature that the bank viewed as crucial to establish trust in the deposit collectors. We were unable to include these studies in the comparison for reasons explained in Appendix A.

[6]Previous studies on debit cards and mobile money have focused on the effect of the lower transaction costs facilitated by these technologies to make purchases, access savings and remittances, and transfer money (Zinman, 2009; Jack and Suri, 2014; Schaner, 2017), but not their capacity to monitor and build trust in financial institutions. Two studies on trust and savings are Osili and Paulson (2014), who study the impact of past banking crises on immigrants' use of banks in the US, and Mehrotra, Vandewalle and Somville (2016), who promote interactions with bankers and find that account savings is strongly associated with trust in one's own banker.

prevalent, and potentially scalable to millions of cash transfer recipients worldwide. Combining these technologies with 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 or mobile money accounts (e.g., Aker et al., 2016; Muralidharan, Niehaus and Sukhtankar, 2016).

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, 2017). The program provides 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, recently rebranded as Prospera.

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. By 2005, beneficiary families in over half of Mexico's urban localities were receiving their transfer 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 no minimum balance requirement or monthly fees and pay essentially no interest.[7] No debit or ATM cards were associated with the accounts, so 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. Overall, the savings accounts were barely used prior to the introduction of debit cards: over 90% of clients made one withdrawal each bimester, withdrawing 100% of the transfer on average (Table B.1).[8]

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

[8]A bimester is a two-month period; Oportunidades payments are paid every two months. Our measure of percent withdrawn can exceed 100% of the transfer since the account could have a positive balance prior to the Oportunidades payment.

In 2009, the government began issuing 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 to 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 (Appendix C). The training sessions did not vary over time and did not discuss savings, nor encourage recipients to save.

Our sample consists of urban beneficiaries who received their transfer benefits in bank accounts prior to the rollout of debit cards. As shown in Figure 2, beginning in January 2009 debit cards tied to these existing bank accounts were rolled out to beneficiaries by locality. By the end of 2009, about 75,000 beneficiaries had received debit cards tied to their pre-existing savings accounts. Another 172,000 beneficiaries received cards by late 2010. By October 2011, the last month for which we have administrative data from Bansefi, a total of 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, shortly after the end date of our study period. We use this last group as a "pure" control group throughout the duration of our study, although as we describe in Section 4, we take advantage of all the variation in exposure time generated by the staggered rollout of cards over time. The map in Figure B.1 shows that the card expansion had substantial national geographic breadth throughout the rollout.

The introduction of debit cards to existing recipients was coupled with an effort to incorporate new beneficiaries into the program.[9] Because of this, the sequence by which localities switched to debit cards was not random: Oportunidades went first to localities that had a large eligible but not-yet incorporated populations. Table B.1 columns 1–3 show that treatment and control localities are quite similar overall, but treatment localities have slightly larger population and beneficiaries receive larger transfer amounts in treatment localities.[10] For 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, withdrawals, percent of transfer withdrawn, net savings balance, and years with the card—we cannot reject equality of means. We

[9]New beneficiaries are excluded from our sample.

[10]For this comparison, treatment localities are localities that received cards between January 2009 and October 2011, and control localities between November 2011 and April 2012.

also show using a discrete time hazard that the timing of the rollout is unrelated to locality and account characteristics except for locality population, the proportion of the population that is illiterate, and years with the account (Table B.1, column 4).[11] We view Table B.1 as descriptive of the implementation, not as an identification test; as detailed below, our identification does not rely on perfect balance in levels, but rather on parallel trends.

3 Data Sources

We use four main sources of data. The first is administrative data on account balances and transactions from Bansefi on the universe of beneficiaries who already received benefits in a savings account and were then awarded a debit card. We also use three surveys of Oportunidades beneficiaries. Table 1 displays the number of beneficiaries, time periods, main variables, and variation we exploit for each of these data sources.

3.1 Administrative Data

To examine the effect of debit cards on savings and account use, we exploit 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. These data consist of 348,802 accounts at 359 Bansefi branches over almost five years, from January 2007 to October 2011. They include 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. Figure 2a shows the timing of the administrative data and the rollout of debit cards.

Table B.1, 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 on average, and the average amount withdrawn is 100% of the Oportunidades transfer, indicating very low use of the account for saving prior to receiving the card. 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 1,194 pesos, or about US$92, per bimester; using survey data we find that Oportunidades income represents about one-fourth of beneficiaries' total income on average. The average account had already been open for 4.3 years by January 2009, so

[11] We model the probability of receiving cards in period t among accounts that have not yet received cards by period $t-1$ as a function of baseline locality and account characteristics using a discrete-time hazard model. As in Galiani, Gertler and Schargrodsky (2005), we include a fifth-order polynomial in time, but all coefficients on the polynomial are insignificant from zero.

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 from their beneficiaries, and these surveys have been used extensively by researchers (Parker and Todd, 2017). We use three distinct Oportunidades household-level surveys, described below. Figure 2b shows the timing of each survey relative to the rollout of debit cards, and Figures B.2–B.4 show when survey respondents received cards.

3.2.1 Household Panel Survey (ENCELURB)

The most comprehensive survey data we use is the Encuesta de las Características de los Hogares Urbanos (ENCELURB), a household panel survey with comprehensive modules on consumption, income, and assets. The survey includes three pre-treatment waves in 2002, 2003, and 2004, and one post-treatment wave conducted between November 2009 and February 2010. The surveys were originally collected for the evaluation of the program on the urban population. Localities that switched to debit cards in early 2009 were oversampled in the fourth wave (which did not return to all localities from the original sample for budgetary reasons). 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. We merge the survey with administrative data from Oportunidades on the debit card expansion to study the effect of the card on consumption and saving in a difference-in-differences model.

3.2.2 Trust Survey (ENCASDU)

The Encuesta de Características Sociodemográficas de los Hogares Urbanos (ENCASDU), conducted in 2010, is a stratified random sample of 9,931 Oportunidades beneficiaries. We refer to this survey as the Trust Survey since it gives us our main measure of trust in the bank. 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 was only asked to those who had already received debit cards. This leaves us with a sample of 1,694 households, with a median exposure to the card of 14 months.

Our main trust measure comes from this survey. The 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 the open-ended question, "Why don't you keep part of the monetary support from Oportunidades in your Bansefi savings account?" *Lack of trust* is captured by responses such as "because if I do not take out all of the money I can lose what remains in the bank"; "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."[12] We also merge this survey with administrative account data to relate savings and reported trust measures directly.

3.2.3 Payment Methods Survey

The Encuesta de Medios de Pago (Payment Methods Survey) is a cross-sectional survey of a stratified random sample of 5,388 beneficiaries, conducted in 2012. This survey was fielded to measure operational details of the payment method. In particular, it asks about use of the debit cards and beneficiaries' experiences using ATMs. We use it to measure the self-reported number of balance checks and withdrawals with the card, whether beneficiaries get help using an ATM, and if they know their card's PIN by heart. We restrict the analysis to the 1,617 surveyed beneficiaries who responded to the relevant module of the survey from the sampled urban localities that received cards; median exposure time to the card is 12 months.

4 Empirical Strategy and Identification

We exploit variation generated by the staggered rollout of debit cards to different localities by Oportunidades. When the data has a panel dimension—i.e., the administrative data and the Household Panel Survey—we estimate a difference-in-differences specification. When we only have a cross-section of cardholders—i.e., the Trust Survey and Payment Methods Survey—we exploit variation in the length of time beneficiaries have been exposed to the card. In both cases the underlying variation we use stems from the exogenous rollout of debit cards over time. In this section, we present the main empirical models we use and verify the plausibility of the identification assumptions needed for a causal interpretation.

4.1 Generalized Difference-in-Differences (Event Study)

The large sample over a long period of time in the administrative data allows us to estimate a generalized difference-in-differences specification where the treatment effect is allowed to vary dynamically over time and is measured in "event time" relative to each beneficiary's treatment date. In other words, we use an event study specification with a pure control

[12] We also use this question to define alternative reasons for not saving, including *lack of knowledge* (e.g., "they didn't explain the process for saving") and *fear of ineligibility* (e.g., "because if I save in that account they can remove me from the Oportunidades program").

group throughout the study period. Specifically, we estimate

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

where y_{it} is the outcome of interest, i and t index account and period respectively, the λ_i are account-level fixed effects, and the δ_t are calendar-time fixed effects. D_{it}^k is a dummy variable indicating that account i has had a debit card for exactly k periods at time t, while $a < 0 < b$ are periods relative to the switch to debit cards; we measure effects relative to the period before getting the card, so we omit the dummy for $k = -1$. For those in the control group who receive cards after our study period ends, $D_{it}^k = 0$ for all k.[13] We use this specification to study withdrawals and savings in the account. We average time over four-month periods since payments are sometimes shifted to the end of the previous bimester.[14] We estimate cluster-robust standard errors, clustering ε_{it} by Bansefi branch.

As in any difference-in-differences model, to interpret each ϕ_k as the causal effect of having the card for k periods, we need to invoke a parallel trend assumption: in the absence of the card, early and late recipients would have had the same account use and 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 4–6 show parallel pre-treatment trends in the number of withdrawals, stock of savings, and savings rate. Parallel pre-treatment trends also hold for client deposits, which are virtually zero in all accounts.

4.2 Difference-in-Differences with Survey Data

With the household survey panel data, we estimate a standard difference-in-differences model since we observe just one time period after treatment. We estimate

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

[13] 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). We set a and b as the largest number of periods before or after receiving the card that are possible in our data, but only graph the coefficients representing three years before receiving the card and two years after (see Borusyak and Jaravel, 2016, on why this is better than "binning" periods below some $\underline{k}$ or above $\overline{k}$.).

[14] This could cause an artificially large end-of-bimester balance if the recipient had not yet withdrawn their transfer. Payment shifting happens for various reasons, including local, state, and federal elections, as a law prohibits Oportunidades from distributing cash transfers during election periods.

where y_{it} is consumption, income, purchase of durables, or stock of assets for household i at time t. Time-invariant differences in household observables and unobservables are captured by the household fixed effects 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 beneficiary 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 again parallel trends. We verify parallel pre-treatment trends by estimating $y_{it} = \lambda_i + \delta_t + \sum_k \omega_k T_{j(i)} \times \mathbb{I}(k = t) + \eta_{it}$, where k indexes survey round ($k = 2002$ is the reference period and is thus omitted), $T_{j(i)} = 1$ if locality j in which beneficiary i lives is a locality that received cards before the post-treatment survey wave, and $\mathbb{I}(k = t)$ are time dummies. Thus, the ω_k for $k < 2009$ estimate placebo difference-in-differences effects for the pre-treatment years. For each variable, we fail to reject the null of parallel trends using an F-test of $\omega_k = 0$ for all $k < 2009$ (Table 2b, column 4).

4.3 Cross-Section Exploiting Variation in Time with Card

The Trust Survey and Payment Methods Survey are cross-sections of beneficiaries with cards (hence there is no pure control group), and each survey has less than 2,000 observations. This poses constraints: we have to rely on exposure time to the card as the identifying variation, and to economize on power, we split the beneficiaries into two equal-sized groups based on how long they have had the card. Concretely, we regress the outcome variable—such as self-reported trust—on a dummy of whether beneficiary i's exposure to the card is below median exposure:

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

where u_i is clustered at the locality level.

This specification requires orthogonality between the error term u_i and timing of card receipt for a causal interpretation of γ—a stronger identification assumption than parallel trends.[15] We thus conduct balance tests using (3) with characteristics that should not be affected by debit card receipt as the dependent variable, such as number of household members, age, gender, status, and education level, as well as variables unaffected by debit card receipt in the Household Panel Survey, such as assets and income. Table 2b shows

[15] An additional issue with this specification is that, to the extent that treatment has immediate effects, we may be biased against finding an effect since all our observations here are treated.

that in our survey samples, those with the card for less and more than the median time are balanced.[16]

It is worth emphasizing that the beneficiaries in the household surveys are a strict subset of the beneficiaries in the administrative data, and that the underlying variation in all specifications stems from exposure time to the card, which was determined exogenously by Oportunidades' rollout of debit cards.

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 348,802 accounts of Oportunidades beneficiaries to estimate the dynamic effect of debit cards on the use of accounts (deposits and withdrawals), stock of savings in these accounts, and 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 empirical evidence (Attanasio, Guiso and Jappelli, 2002; Schaner, 2017). This is indeed what we find. Figure 3a shows 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.[17] Although the debit cards can be used at any store that accepts card payments, the majority of transactions on the card are made at ATMs: including card purchases in the definition of withdrawals, 11% of the total withdrawn and 22% of withdrawals are made at stores. Meanwhile, the number of withdrawals in the control group does not change over time (Figure B.5).

On the other hand, there is no effect on client deposits: Figure 3b shows that 99% of accounts have zero client deposits per bimester before and after receiving the card. Account

[16]In the Trust Survey, outcomes are balanced for 9 out of 10 variables; 1 of 10 variables has a statistically significant difference at the 10% significance level, as would be expected by chance. The Payment Methods Survey includes fewer measures of household characteristics since the survey was focused on experience with the debit cards and ATMs. We find no statistically significant differences in the 5 variables on household characteristics included in the Payment Methods Survey.

[17]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.

holders thus do not add savings from other sources of income to their Bansefi accounts. This finding is not surprising, since beneficiaries receive about one-fourth of their total income from the Oportunidades program on average, so unless the optimal savings rate in a particular period is higher than 25% of total income, there is no reason to deposit more into the savings account from other income sources.

In order to examine the evolution of the debit card's effect on withdrawals over time, we estimate the generalized difference-in-differences or event study specification from (1), with withdrawals per bimester as the dependent variable. Figure 4 plots the ϕ_k coefficients of average withdrawals per bimester for each four-month period, compared to the period just before receiving cards. Prior to receiving the card, pre-trends are indistinguishable between treatment and control: we cannot reject the null of $\phi_k = 0$ for all $k < 0$. In addition to having parallel trends, pre-treatment levels of the number of withdrawals are also the same between treatment and control (this cannot be determined from Figure 4 since any difference in levels would be absorbed by the account fixed effects, but it can be seen in Table B.1). The effect on withdrawals is *immediate*, as would be expected from the instantaneous change in transaction costs induced by the card. Prior to receiving the card, beneficiaries in both the treatment and control groups average about 1 withdrawal per bimester, but immediately after receiving the card, treated beneficiaries begin making an additional 0.4 withdrawals per bimester on average.

5.2 The Stock of Savings (Account Balances)

Next, we explore whether debit cards cause an increase in savings from period to period. The increased number of withdrawals shown in Section 5.1 will lead to a mechanically higher average balance *within* each period, but this does not necessarily mean beneficiaries are accumulating saving in the account over time, i.e., *across* periods. They could just be leaving some money in the account after the first withdrawal in the pay period, but withdrawing the remaining money later in the same period thereby leaving the account balance close to zero by the end of that period.

Since we are interested in a measure of saving across periods but do not observe end-of-period balance, we adjust the average balance measure to remove the mechanical effect resulting from making more (lower-amount) withdrawals after receiving the card.[18] Using the timing and amount of each transaction, we calculate and subtract off the mechanical

[18] We use this measure rather than forcing initial balance in January 2007 to zero and constructing end-of-period balance using the transactions data since the average balance data reveal that a small portion of beneficiaries do save in their accounts prior to 2007, as we discuss in Section 5.4.

effect for each account-bimester observation to obtain a measure of "net balance" to study period-to-period savings (see Appendix D for more details).

We estimate (1) with account i's net balance in period t as the dependent variable.[19] The ϕ_k terms thus measure the causal effect of debit cards on the stock of savings k periods after receiving a card. Figure 5a plots the ϕ_k coefficients and their 95% confidence intervals. First, note the parallel trends for $k < 0$.[20] In the first few periods after receiving a card, there is a small savings effect of about 100 pesos (about US$8). The initial effect is small because only some beneficiaries begin saving shortly after receiving a card—we explore this further in Section 5.4. Savings increase substantially after about one year with the card: three periods after card receipt, the savings effect is 450 pesos, while it is 750 pesos after two years with the card. These effect sizes are equal to 1.2 and 2.0% of annual income, respectively, and are larger than the effect sizes found in other studies of savings interventions (Figure 1).

The effect of debit cards on the average stock of savings from Figure 5 combines two effects: the impact of debit cards on the probability of saving and savings conditional on saving. Figure 5b shows the proportion of treated beneficiaries who save each period. While just 13% of beneficiaries saved in their account in the period before receiving cards, Figure 5b shows that an additional 16% of beneficiaries start saving immediately after receiving a card. For these beneficiaries, it is likely that the reduction in the transaction costs of accessing savings provided by the cards was a sufficient condition to save in a formal bank account. The proportion of beneficiaries who save in their Bansefi accounts increases over time: after nearly one year with the card, 42% of beneficiaries save in the account, and after two years nearly all beneficiaries (87%) save in their Bansefi account.

5.3 Savings 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 precaution-

[19]Following other papers measuring savings (e.g., Kast, Meier and Pomeranz, 2016), we winsorize savings balances at the 95th percentile to avoid results driven by outliers.

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

ary saving and saving to purchase durables particularly important (Deaton, 1992).[21] 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 $\Delta Savings_{it} = \lambda_i + \delta_t + \theta Savings_{i,t-1} + \gamma Income_{it} + \varepsilon_{it}$. Models of precautionary saving predict that $\theta < 0$, since the amount of new savings decreases as the stock of savings approaches the target level.

We are not actually able to implement the above model as specified because we are restricted to using bank account information rather than data on overall savings and income. Instead, we estimate the *change in net account balances* (rather than change in total savings) as a function of lagged *net balances* (instead of lagged total savings) and *transfers deposited during the period* (instead of total income). In order to identify the effects of the debit card on the savings rate over time, we then interact the terms from the above model with event-time dummies that describe time since receipt of the card. Incorporating all of the above changes

$$\begin{aligned}\Delta Savings_{it} = \lambda_i + \delta_t + \sum_{k=a}^{b} \alpha_k D_{it}^k + \theta Savings_{i,t-1} + \sum_{k=a}^{b} \xi_k D_{it}^k \times Savings_{i,t-1} \\ + \gamma Transfers_{it} + \sum_{k=a}^{b} \psi_k D_{it}^k \times Transfers_{it} + \varepsilon_{it},\end{aligned} \tag{4}$$

where $Savings_{it}$ now refers to the stock of savings *in the account* and $\Delta Savings_{it} \equiv Savings_{it} - Savings_{i,t-1}$ refers to its flow.

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 time.[22]

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

[22] Results are robust to excluding the $Transfer_{it}$ interaction terms; see Figure B.6. Because transfer

We estimate the dynamic effect of the debit card on the savings rate from (4) as

$$\hat{\Phi}_k \equiv (\hat{\alpha}_k + \hat{\xi}_k \omega_{k-1} + \hat{\psi}_k \mu_k)/\overline{Y}, \tag{5}$$

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 (5) 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.[23] We use the delta method to estimate standard errors and thereby construct confidence intervals.

Despite lacking data on total savings and total income, under a set of testable assumptions we can interpret the $\hat{\Phi}_k$ coefficients as causal effects of the debit card on the flow of savings. Specifically, we need to assume that (i) there are no deposits into the account other than the transfer, (ii) having a debit card does not affect other sources of income, and (iii) having a 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, enabling us to use transfer income rather than total income in (4). The third assumption implies that any increase in savings in the bank account does not substitute for other forms of saving, so that an increase in bank savings constitutes an increase in total savings. This assumption allows us to use bank account savings rather than total savings in (4).

Empirically we find that all three assumptions hold. First, almost no beneficiaries deposit any funds in addition to the transfers into their savings accounts in any period (Figure 3). Second, using the Household Panel Survey in Section 6, we find that the debit cards do not affect income. Third, using the same data, we find that debit cards reduce consumption by a very similar magnitude as the increase in savings from the administrative bank account data, suggesting that the increase in bank savings is an increase in total savings and that the debit card does not affect other savings.

The right-hand side of (4) includes both individual fixed effects and lagged net balance; since the dependent variable is a function of net balance, the assumption that the individual fixed effects are uncorrelated with the error does not hold, and the bias that this introduces could be significant if the number of time periods is small (Nickell, 1981). To avoid this

amounts vary for a number of reasons (described in Appendix E), we control for them in the preferred specification.

[23] Average income is obtained from the 2009–10 wave of the Household Panel Survey (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.

bias, in practice we do not include the individual fixed effects λ_i and instead include a simple treatment dummy in their place.[24]

The results in Figure 6a 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 $k < 0$.[25] After receiving the card, some beneficiaries start saving immediately, and in the first year after receiving a card (relative periods 0 to 2) we thus see an average effect on the savings rate of between 0 and 1.5% of income. In the second year after receiving the card, most individuals save (Figure 5b) and we see that the debit card causes an increase in the savings rate of 3 to 4% of income.

Models of precautionary saving 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 indeed find $\theta = -0.69 < 0$ (with a cluster-robust standard error of 0.01) and $\theta + \xi_k < 0$ for all k. We also find a decreasing pattern in the savings rate after about one year with the card (after most beneficiaries have started saving), from 4% to about 3% of income.

5.4 Heterogeneity in the Savings Rate

The effect of debit cards on the savings rate is the average of the savings rate of savers and the savings rate of non-savers (i.e., zero). Most of our sample is composed of eventual savers, as 87% begin saving within two years of receiving the debit card. However, because different proportions of beneficiaries are saving in each period, the average effect of debit cards underestimates the effect of the debit card on the marginal savings rate. In this section we attempt to estimate the marginal savings rate trajectory once an individual decides to begin saving.

We define a new event as the period in which a beneficiary begins saving (rather than when the beneficiary receives a card), and estimate (4) with D_{it}^k redefined as periods relative to this event.[26] This method allows us to directly test the prediction from precautionary savings models that the savings rate is decreasing as savers approach their savings targets.

[24] To assess the robustness of our results to including the individual fixed effects without biasing our estimates, we also use a system GMM estimator with individual fixed effects proposed by Blundell and Bond (1998) that is consistent for fixed T, large N and performs well in Monte Carlo simulations.

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

[26] We set $a = 0$ for this estimation since $Savings_{i,t-1}$ would be zero for all periods prior to saving, and hence the ξ_k would be unidentified for $k < 0$. In other words, we force the pre-trend to equal 0, which is consistent with our previous estimates. Because the majority do not begin saving until they have had the card for a year, we only graph the savings rates for three post-saving periods (as further-period estimates would be based solely on the small sample of earlier savers).

Figure 6b shows that the period after beginning to save, the average beneficiary saves 5.3% of income, and this falls over time (to a savings rate of 3.8% of income after one year of saving) as her stock of savings approaches her target.

We can also use these results to estimate the equilibrium buffer stock for beneficiaries who save in their accounts. Since many beneficiaries are still accumulating savings after two years with the card, we do not have sufficient time periods to *directly* measure their equilibrium buffer stock. Instead, to predict the buffer stock they will accumulate, we note that once a beneficiary has reached her equilibrium buffer stock, $Savings_{it} = Savings_{i,t-1}$ (where "savings" refers to the stock of savings); we plug this into (4) to solve for the equilibrium savings stock for those with a card and obtain $Savings = (\alpha + \psi \cdot Transfers)/(-\xi)$. Using averages for these coefficients from periods after beneficiaries begin saving, we predict that the average equilibrium buffer stock is 1945 pesos (US$150); to put this quantity in context, it equals 5.1% of beneficiaries' annual income. After one year of saving in the account (and up to two years with the card), the 87% of beneficiaries who save have accumulated 47% of their desired buffer stock. After two years of saving in the account, the small subset of beneficiaries who received the card and began saving early have accumulated 65% of the equilibrium buffer stock.

6 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 savings. This section investigates whether the observed increase in Bansefi account savings crowds out other savings. We take advantage of Oportunidades' Household Panel Survey, conducted in four waves during the years 2002, 2003, 2004 and November 2009 to February 2010.

We use a simple difference-in-differences identification strategy where we examine changes in beneficiaries' consumption, income, purchases of durables, and stock of assets, again exploiting the differential timing of debit card receipt. We compare trends of those with cards at the time of the fourth survey wave to those who had not yet received cards. Section 4 formally tested for parallel pre-treatment trends for each dependent variable and failed to reject the null hypothesis of parallel trends. Having established that the identification assumption is plausible, we estimate (2) separately for four dependent variables: consumption, income, purchase of durables, and an asset index.

Our findings indicate that the increase in formal savings shown in Section 5 represents an increase in total savings. Figure 7a shows that consumption decreased by about 138 pesos per month among treated households relative to control (statistically significant at the 5% level). We do not find any effect on income.[27] Purchases of durables and the stock of assets do not change, ruling out a crowding out of these forms of saving.

Comparing the decrease in consumption from the household survey data to the increase of savings in administrative data suggests that increased formal savings in bank accounts does not crowd out other forms of saving, consistent with Dupas and Robinson (2013*a*), Ashraf, Karlan and Yin (2015), and Kast, Meier and Pomeranz (2016). In Section 5.3 we estimate that after 1 year with the card, beneficiaries save 4.0% more of their income than the control group. In our survey data, we find a decrease in consumption of 138 pesos per month; dividing by average household income in the post-treatment survey wave, 3,151 pesos per month, this equates to 4.4% of income. We cannot reject that the increase in savings in the administrative data and the decrease in consumption in the survey data are equal. Therefore total savings—not just account savings—increase, and this increase is fueled by lower current consumption.

6.1 Why Does the Debit Card Increase Total Savings?

The literature suggests that saving informally is difficult and that keeping money in a formal financial institution may solve many of the problems associated with informal savings. In particular, it may be tempting to spend money that had been intended to be saved if it is easily accessible, especially at times when the beneficiary is more financially constrained (Carvalho, Meier and Wang, 2016). Intra-household bargaining issues may prevent women from saving at home (Ashraf, 2009; Schaner, 2015). Cash saved at home could be in demand from friends and relatives (Dupas and Robinson, 2013*b*), and informal savings can be more easily stolen (Banerjee and Duflo, 2007; Schechter, 2007).

While we do not have the data to test all of these possibilities, we do 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. Specifically, we test whether card receipt causes consumption to fall more in categories where temptation is greatest.

We estimate the difference-in-differences specification (2) separately for each con-

[27] 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 consumption and income are noisily measured, the difference in the coefficients is significant at the 10% level ($p = 0.092$). Table B.2 shows that these results are robust to the extent of winsorizing and to allowing flexible time trends as a function of household characteristics. Standard errors are clustered at the locality level.

sumption category, where the dependent variable is the proportion of income spent on consumption category g. Figure 7b 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.[28] Nevertheless, this evidence is merely suggestive, as spending on temptation goods and entertainment make up a small proportion of total income (shown by the blue bars in Figure 7b): as a result, the decrease in consumption in these categories only explains 18% of the total decrease in consumption. We refrain from speculating about the remaining 82% of the decrease in consumption since we have limited power and results for other consumption categories are statistically insignificant from zero.

7 Mechanisms

The card decreases indirect transaction costs to both access savings and monitor account balances. In this section we provide evidence that both mechanisms were at work in causing the increased active use of the accounts and the large increase in savings. We also explore several other mechanisms such as learning the ATM technology.

7.1 Transaction Costs

The debit card causes an immediate decrease in the indirect transaction costs—such as time and travel costs—of accessing money in a bank account. The median household lives 4.8 kilometers (using the shortest road distance) from the nearest Bansefi branch, compared to 1.3 kilometers from an ATM.[29] Consistent with economic theory on the effect of an immediate decrease in transaction costs (Baumol, 1952; Tobin, 1956), we observe an immediate increase in the number of withdrawals per period (Figure 4). 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 also increases immediately after receiving the card—to about 85% of beneficiaries—and then is fairly stable in subsequent periods (Figure B.7). We also observe that 16% of beneficiaries were not saving prior to receiving a debit card and begin

[28]We group the three most frequently listed temptation goods in Banerjee and Duflo (2007): alcohol, tobacco, and sugar. Since this grouping of temptation goods could be viewed as arbitrary (and, indeed, we do not find a decrease in the grouping of fats and sweets—junk food, fats, and soda—which could also be classified as temptation goods), we look separately at each item in the temptation good category, and find a statistically significant decrease in consumption of alcohol and sugar, but not of tobacco.

[29]This calculation is based on the following subsample: for 70% of beneficiary households who received cards by October 2011, we were provided their census block identifier and were able to merge this with census block shapefiles to calculate the centroid of the block. (For the other 30%, Oportunidades did not have their census block identifier, or had an identifier that was not present in the shapefiles.) This leaves us with a sample of 180,204 *treated* beneficiaries. We then calculated road distances between the 74,710 unique blocks on which these beneficiaries live and the 505 Bansefi branches and over 27,000 ATMs in Mexico. See Bachas et al. (forthcoming) for more details.

saving immediately after receiving the card, likely due to the change in transaction costs (Figure 5b). In Bachas et al. (forthcoming), we also show that the increase in withdrawals and savings immediately after receiving a card is positively correlated with the reduction in travel distance associated with receiving a debit card.

The immediate decrease in transaction costs provided by debit cards cannot, however, explain the gradual increase over time in the proportion of beneficiaries who save in their Bansefi accounts after receiving cards (Figure 5b). The only way transaction costs could *solely* explain the increase in savings caused by debit cards—and in particular the gradual increase over time with the card in the proportion of beneficiaries who save—would be if transaction costs were also gradually changing over time. This, however, would be inconsistent with the immediate increase and then relatively flat time profile of both the number of withdrawals per period (Figure 4) and the proportion of beneficiaries who withdraw their benefits at ATMs (Figure B.7).

In addition, there is substantial direct evidence that changing transaction costs over time cannot explain the gradual increase in the proportion who save. First, we test and reject that banks disproportionately expanded complementary infrastructure (e.g. number of ATMs) in treated localities, which would further decrease the transaction cost of accessing funds in a way that is geographically correlated with the debit card expansion. We use data on the number of ATMs and bank branches by municipality by quarter from the Comisión Nacional Bancaria y de Valores (CNBV), from the last quarter of 2008—the first quarter with available data—through the last quarter of 2011. We estimate a difference-in-differences specification with six leads and lags, $y_{mt} = \lambda_m + \delta_t + \sum_{k=-6}^{6} \beta_k D_{m,t+k} + \varepsilon_{mt}$, 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. We conduct an F-test of whether lags of debit card receipt predict banking infrastructure (i.e., whether there is a supply-side response to the rollout of debit cards: $\beta_{-6} = \cdots = \beta_{-1} = 0$), and an F-test of 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: $\beta_1 = \cdots = \beta_6 = 0$). We find evidence of neither relationship (Table B.3).

Second, we test whether the increase in the proportion of savers over time with the card could be explained by a concurrent increase in the number of ATMs across all localities. Only beneficiaries in treatment localities can access money at ATMs and hence take advantage of an expansion of ATMs. If the gradual increase in the proportion saving over time is due to a gradual decrease in transaction costs that is uncorrelated with the geographical expansion of debit cards, we would also expect savings to increase among Bansefi debit card holders who are not Oportunidades beneficiaries. We look at mean savings among non-Oportunidades debit card account holders who opened their accounts in 2007 and hence have had the account for about two years when our study period begins. Figure B.8 shows that savings among non-Oportunidades debit card holders do not increase over the study time period, and instead stay relatively flat. This suggests that the increase over time in the proportion who save cannot be explained by a gradual decrease in transaction costs over time.

Third, beneficiaries' *perceptions* of transaction costs might change even if transaction costs remain constant over time with the card. For example, perhaps they are checking balances to learn about direct transaction costs (i.e., fees), in which case they would check balances less frequently once transaction costs are learned. We directly test and reject this hypothesis using the Payment Methods Survey, which asks beneficiaries how much the bank charges them for (i) a balance check and (ii) a withdrawal after the initial free withdrawals. We find that beneficiaries get the level of these fees about right and, more important, that there is no difference across beneficiaries who have had the card for less vs. more than the median time (Figure B.9a).

In sum, the debit cards lead to an immediate change in transaction costs to access savings, which causes an immediate increase in the number of withdrawals per period and an immediate increase in the proportion who save. However, the proportion who save continues to increase over a two-year period, and this effect cannot be explained solely by transaction costs.

7.2 Trust

Trust in financial institutions is low worldwide (Figure B.10) and is positively associated with saving in formal bank accounts (Figure B.11). Furthermore, a lack of trust in banks is frequently cited by the poor as a primary reason for not saving (Dupas et al., 2016; FDIC, 2016). The time delay between receiving the debit card and starting to save (for most beneficiaries) is consistent with the hypothesis that the debit card reduces the indirect cost of checking account balances, leading to an increase in balance checks to monitor that the bank is not regularly reducing beneficiaries' account balances.[30] Each additional bal-

[30] 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. The median household lives 4.8 kilometers (using the shortest road distance) from the nearest Bansefi branch, compared to 1.3 kilometers from an ATM.

ance check provides additional information about the bank's trustworthiness. With simple Bayesian learning, balance checks have a decreasing marginal benefit as a beneficiary updates her beliefs about the bank's trustworthiness, which would lead to a decrease in the number of balance checks over time. Hence, over time with the card, we expect the number of balance checks to fall and trust to rise.

We provide support for this in four steps. We first show that balance checks fall over time in both administrative and survey data. Second, we examine whether higher savings balances are negatively correlated with the number of balance checks *within* accounts in the administrative data, as they should be if beneficiaries begin saving once they've used the card to monitor the bank and build trust through balance checks. Third, we use survey data to test whether self-reported trust in the bank increases over time with the card. Finally, we merge survey data on trust in the bank with administrative data and document a direct relationship between self-reported trust and saving in the account, using an instrumental variables strategy.

7.2.1 Balance Checks Fall Over Time with the Debit Card

We first use the Bansefi transactions data to test whether balance checks fall over time with the card. 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.[31] On average over these periods beneficiaries check their balances 1.9 times per four-month period. To test the hypothesis of a decreasing time trend in balance checking, we regress the number of balance checks on account fixed effects and event-time dummies (omitting the last period with the card): $Balance\ Checks_{it} = \lambda_i + \sum_{k=0}^{4} \pi_k D_{it}^k + \varepsilon_{it}$. The π_k coefficients graph the number of balance checks k periods after receiving the card relative to the last period in the sample (July–October 2011), which depending on the beneficiary corresponds to one to two years after receipt of the card.

Figure 8a plots the π_k coefficients using any balance check to construct the dependent variable, and shows that the number of balance checks in the periods following receipt of the debit card is higher than in later periods. For example, in the period after receiving the card, beneficiaries make 1.03 more balance checks compared to two years after receiving the card. After having the card for about one year, this falls to about 0.4 more checks.

For learning to occur, beneficiaries need a positive balance in their account at the time

[31] We do not observe balance checks at Bansefi branches in the transaction data since these are not charged a fee. However, it is unlikely that many beneficiaries used this mechanism to monitor the bank prior to receiving a card due to the high costs of traveling to the nearest Bansefi branch.

of checking. We find that in the four months after getting the card, 89% of accounts have a positive (small) balance at the time of a balance check after receipt of the transfer: 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.[32]

To ensure that a balance check constitutes bank monitoring and not just checking that the Oportunidades deposit arrived, we additionally use two alternative, more restrictive definitions of a balance check.[33] The first alternative definition excludes all balance checks that occurred *prior* to the transfer being deposited that bimester, and also excludes balance checks that occur on the same day as a withdrawal. The idea is that if a beneficiary is checking whether the transfer has arrived, and she finds that it has, she would likely withdraw it that same day. An even more conservative definition only includes balance checks that occur after that bimester's transfer has arrived *and the client has already withdrawn part of the transfer*. Because the next transfer would not arrive until the following bimester and the beneficiary has already made a withdrawal in the current bimester, the beneficiary knows that the current bimester's transfer has arrived. Hence, these checks cannot be an attempt to see if the transfer has arrived. Figures 8b and 8c plot the results with these two alternative definitions and show a very similar decrease in balance checks over time.

We validate the above results using survey data from the Payment Methods Survey. Specifically, we estimate (3) using the self-reported number of balance checks over the past bimester as the dependent variable. Figure 8d shows that 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. The self-reported survey responses thus confirm the findings from the administrative data, and also show that balance checking behavior is salient for beneficiaries.

7.2.2 Negative Correlation between Balance Checks and Savings Balances

Our hypothesis—that monitoring balances leads to increased trust which leads to increased savings—predicts that there will be a negative correlation between balance checks and savings *within* accounts. To test this, we estimate $Savings_{it} = \lambda_i + \sum_{c \neq 0} \eta_c \mathbb{I}(Checks_{it} = c) + \varepsilon_{it}$, where $Savings_{it}$ is the net balance in account i at time t, the λ_i are account-level

[32] For these statistics, 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).

[33] Note that beneficiaries were given calendars with exact transfer dates and hence should know the dates on which transfers are deposited; see Figure C.3. Figure B.12 illustrates the three definitions of balance checks that we use.

(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.[34] The η_c coefficients thus measure the within-account correlation between the stock of savings and number of balance checks, relative to the omitted zero balance checks ($c = 0$) category. Our hypothesis suggests that $\eta_c < 0$, and that η_c is decreasing (i.e., becoming more negative) in c.

Figure 9 shows the results. Account balances are indeed negatively correlated with the number of balance checks within accounts. Using any of the three definitions of balance checks described earlier, η_c is less than 0 and decreasing in c. Furthermore, the negative correlation between savings and balance checks is stronger when we restrict the definition of balance checks to those that we argued earlier are more likely to be the type of checks used to monitor the bank. Using balance checks that occur only after the beneficiary has already made a withdrawal in the same bimester (panel c), we find that beneficiaries who make one balance check save 300 pesos less than those who make no balance checks, while beneficiaries who make 3 or more balance checks save nearly 500 pesos less.

7.2.3 Trust Increases over Time with the Debit Card

We now test the hypothesis that longer tenure with the debit card induces higher trust in the bank. As described in Section 3.2, the Trust Survey first asks the beneficiary if she saves in her Bansefi bank account, and if she answers no, it asks why not. If she does not save in the account and indicates that she does not trust the bank, we code *lack of trust* as 1; otherwise (including if the beneficiary saves in the account) we code lack of trust as 0.

We 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. As explained in Section 4, to interpret γ in (3) as a causal effect we need to assume that time with the card is orthogonal to our potential outcomes of interest. The balance tests conducted in Table 2a for the Trust Survey sample support this assumption. Figure 10 shows that trust increases over time: beneficiaries with more than the median time with the card are 33% less likely to report not saving due to low trust.[35] For comparison, Figure 10 also shows results for

[34] We do not include time fixed effects since the within-account changes in the stock of savings over time is precisely the variation we are exploiting. ε_{it} are clustered at the bank branch level.

[35] Note that because of the timing of the Trust Survey, those with the card for less than the median time have still 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. If anything, this may bias our results downward relative to what we would find if it were possible to compare those who have a sufficient tenure with the card to those who have not yet received the card.

two alternative forms of learning discussed in Sections 7.3 and 7.4: 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, and the proportion does not change over time with the card.

7.2.4 The Direct Relationship between Trust and Saving

We now link our survey measure of trust in the bank with administrative data on saving by *the same beneficiaries*. Using administrative identifiers provided by Oportunidades, we are able to merge 1330 of the 1694 beneficiaries in the Trust Survey with their corresponding administrative data on saving. We restrict the sample period in the administrative data to the cross-section that coincides with the survey. Everyone in this sample has had the card for between 9 and 18 months; we exploit cross-sectional variation in time with the card for identification. To estimate the effect of trust on saving, we regress the flow of savings on a trust dummy (the complement of the *lack of trust* dummy used in Section 7.2.3): $\Delta Savings_{it} = \zeta Trust_{it} + \varepsilon_{it}$.

Trust is likely endogenous in this specification—for instance, richer people may trust the account more but have a better outside option for saving and thus save less in this account, or those with initially high trust prior to the card may have already reached their savings targets and thus not be adding additional savings. To overcome this, we instrument trust with a set of dummy variables for the timing of debit card receipt, which is exogenous. We already know from Section 7.2.3 that this instrument has a strong first stage. Three pieces of evidence suggest that the instrument should satisfy the exclusion restriction. First, time with the card is uncorrelated with sociodemographic characteristics in this sample (Table 2a). Second, time with the card does not affect other types of learning (Figures 10 and B.9). Third, *time* with the card (as opposed to the card itself) does not affect transaction costs, as shown in Section 7.1.

Table 3 estimates the direct effect of trust on savings. Coefficients are expressed as a proportion of average income from the survey, and standard errors are clustered at the locality level. Column 2 shows our main result, instrumenting trust with timing of card receipt. The first stage is strong (with an F-statistic of 40) and large in magnitude: an average of six additional months with the card leads to a 10.3 percentage point increase in the probability of trusting the bank.[36] The IV coefficient shows that beneficiaries who are induced to trust the bank as a result of having the card for a longer period of time save an

[36] We take a weighted average to report the first stage coefficient, since our instrument is a set of dummies.

additional 2.8% of their income, statistically significant at the 5% level.[37]

7.3 Learning the Technology

The time delay for many beneficiaries between getting the card and saving suggests some type of learning. Building trust is one form of learning. Here we explore an alternative type of learning: learning how to use the technology. This type of learning would have to occur gradually over time to explain our results. However, in addition to the survey evidence against this form of learning that we present below, learning the technology is inconsistent with the result from the administrative data that the number of withdrawals and use of ATMs increase *immediately* after receiving the card and remain fairly stable over time afterwards.

Beneficiaries could be learning how to use their debit cards over time. The Payment Methods Survey asks each respondent whether (i) it is hard to use the ATM, (ii) she gets help using the ATM, and (iii) she knows her PIN by heart. We use these three questions as dependent variables in (3). Figure B.9b shows that there is no statistically significant difference between the group who have had the card for less vs. more than the median time. Beneficiaries could instead be learning how to save in the account (rather than how to use the card). This is unlikely as these beneficiaries have already had the account for years prior to receiving a debit card. Consistent with this, less than 2% respondents to the Trust Survey cite not saving due to lack of knowledge.[38] Moreover, there is no difference between those who have had the card for less vs. more than the median time (Figure 10).

7.4 Learning the Program Rules

Beneficiaries may have initially thought that saving in the account would make them ineligible for the program, but learned over time that this was not the case. In the Trust Survey, there are some responses along these lines such as "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. Less than 4% of beneficiaries do not save due to fear of being dropped from the program, and the proportion does not change when comparing those who have had the card for less vs. more than the median time (Figure 10).

[37] The result is robust to using a specification more analogous to (4) that includes lagged balances and transfers on the right-hand side (column 3). Column 1 shows the OLS relationship between trust and the flow of savings; the finding of no effect in the OLS is not surprising due to the endogeneity issues discussed above.

[38] Examples of responses coded as lack of knowledge are "I don't know how to use the card so I withdraw everything at once" and "I don't know how [to save in the account]."

7.5 Time with the Bank Account

Experience with the savings account rather than time with the debit card itself cannot explain the delayed savings effect. First, savings accounts were rolled out between 2002 and 2005, and therefore beneficiaries had several years of experience with the account when debit cards were first introduced in 2009. Second, both treatment and control accounts are accumulating time with their savings accounts simultaneously, and have had accounts for the same amount of time on average (Table B.1). 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, when we split the sample based on whether the account was *opened* before or after the median opening date, we find similar results across the two subsamples (Figure B.13).

8 Conclusion

Debit cards tied to savings accounts could be a promising avenue to facilitate formal savings, as debit cards reduce transaction costs and provide a mechanism to check balances and build trust in financial institutions. We find large effects of debit cards on savings. The debit cards were rolled out over time to beneficiaries of Mexico's cash transfer program Oportunidades, who were already receiving their benefits in a bank account, but who—for the most part—were not saving in their accounts. After two years with a debit card, beneficiaries accumulate a stock of savings equivalent to 2% of annual income. Extrapolating our estimates from a precautionary savings model to future periods, we predict that beneficiaries are saving towards an equilibrium buffer stock of about 5% of annual income. The effect we find 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. Furthermore, this effect arises in an at-scale policy change affecting hundreds of thousands of cash transfer beneficiaries across the country.

Both trust in banks and low transaction costs to access savings appear to be necessary but not (individually) sufficient conditions to save in formal financial institutions. While cross-country and qualitative evidence had shown that transaction costs and low trust in banks might be barriers to saving, we provide evidence that a causal relationship exists: we combine high-frequency administrative bank transactions and survey data with an empirical design that exploits a staggered, plausibly exogenous rollout of debit cards. High indirect transaction costs and low trust 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.

While we are not able to directly assess the welfare implications of this policy, a growing literature suggests that enabling the poor to save in formal accounts leads to increased welfare through greater investment and ability to cope with shocks, leading to higher long-term consumption. It is worth noting that beneficiaries with the debit card voluntarily use the technology and build savings in their accounts (whereas they could continue withdrawing all of their benefits from the bank branch, as they did prior to receiving the card); this indicates a revealed preference for saving in formal financial institutions once transaction costs are lowered and trust is built. Furthermore, beneficiary survey responses in the Trust Survey indicate that satisfaction with the payment method is higher after receiving the debit card, particularly for those who have had the card longer: 75% of beneficiaries who have had the card for at least 14 months (the median time) indicate that receiving payment by debit card is better than before.[39]

Taken together, these results suggest that combining debit cards or mobile banking with government cash transfer programs could be a promising channel to increase financial inclusion and enable the poor to save.

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[39] Another 13% report being paid by debit card as the same as before and 5% worse than before.

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Table 1: Summary of Data Sources and Identification

Data Source	# Benef.	Period	Main Variables	Variation Used
(1) Administrative bank account data from Bansefi	348,802	Continuous panel: Jan 07–Oct 11	Balances, transactions, balance checks	Generalized difference-in-differences (event study with control) using phased geographic rollout
(2) Household Panel Survey from Oportunidades (ENCELURB)	2,942	Panel (four waves): 02, 03, 04, and Nov 09–Feb 10	Consumption, income, purchase of durables, assets	Difference-in-differences: received card in 2009 versus received card later
(3) Trust Survey from Oportunidades (ENCASDU)	1,694	Cross-section: Oct–Nov 10	Self-reported reasons for not saving: e.g. lack of trust, lack of knowledge	Tenure with card below/above median time in survey (median = 14 months)
(4) Payment Methods Survey from Oportunidades	1,617	Cross-section: Jun 12	Self-reported number of balance checks, knowledge of technology	Tenure with card below/above median time in survey (median = 12 months)

Notes: This table presents details for the four main data sources included in our paper.

Table 2: Balance and Parallel Trends in Survey Data

Panel (a): Cross-Sectional Data	Trust Survey			Payment Methods Survey		
	(1)	(2)	(3)	(4)	(5)	(6)
	α (Mean for card $>$ median time)	γ (Difference card $\leq$ median time)	P-value of difference	α (Mean for card $>$ median time)	γ (Difference card $\leq$ median time)	P-value of difference
# Household members	5.18	0.26	0.114	4.78	−0.04	0.767
	(0.08)	(0.15)		(0.07)	(0.13)	
Age	44.73	0.96	0.246	40.21	−1.18	0.146
	(0.08)	(0.80)		(0.49)	(0.80)	
Male	0.67	0.02	0.603	0.03	0.00	0.874
	(0.03)	(0.03)		(0.01)	(0.01)	
Married	0.70	0.02	0.459	0.73	−0.01	0.867
	(0.04)	(0.03)		(0.02)	(0.03)	
Education level	9.30	−0.33	0.092	6.01	0.03	0.910
	(0.16)	(0.18)		(0.20)	(0.29)	
# Children	2.19	0.03	0.743			
	(0.08)	(0.10)				
Occupants per room	3.50	−0.03	0.801			
	(0.07)	(0.11)				
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	3193.93	222.55	0.151			
	(47.53)	(147.11)				

Panel (b): Panel Data	Household Panel Survey			
	(1)	(2)	(3)	(4)
	Control Mean	ω_k (Placebo DD) 2003	2004	Parallel p-value
Consumption	2725.87	−69.73	−55.22	0.313
	(78.54)	(45.48)	(62.06)	
Income	3150.82	202.75	200.23	0.138
	(88.37)	(100.22)	(133.20)	
Purchase of Durables	33.97	−1.08	−9.19	0.130
	(3.28)	(4.62)	(4.47)	
Asset Index	0.50	−0.01	−0.07	0.415
	(0.09)	(0.03)	(0.05)	

Notes: This table tests for balance between those who have had a debit card for more vs. less than the median time in the two cross-sectional surveys, and for parallel trends in the panel survey. Panel (a) shows results from $y_i = \alpha + \gamma\mathbb{I}(\text{Card} \leq \text{median time})_i + u_i$: column 1 shows the mean for those with a card for more than the median time (α), column 2 the difference in means for those with the card less than the median time (relative to those with the card more than the median time; γ), and column 3 reports the p-values for a test of $\gamma = 0$. In the Trust Survey, individual sociodemographic characteristics refer to those of the household head (but the program beneficiary responded to the trust questions). The Payment Methods Survey was a more focused survey that included fewer sociodemographic questions, which is why some rows are blank in the columns corresponding to that survey; individual sociodemographic characteristics are those of the program beneficiary. $N =$ 1,694 beneficiary households for the Trust Survey and 1,617 for the Payment Methods Survey. Panel (b) shows the control mean and a parallel trend test for each of the outcome variables used in the household panel survey. The parallel trends test is from $y_{it} = \lambda_i + \delta_t + \sum_k \omega_k T_{j(i)} \times \mathbb{I}(k = t) + \eta_{it}$, where k indexes survey waves. The "Placebo DD" columns (where DD = difference-in-differences) show ω_{2003} and ω_{2004} ($k = 2002$ is the omitted reference period), while the "Parallel p-value" column is from an F-test of $\omega_{2003} = \omega_{2004} = 0$. $N =$ 9,496 household-period observations from 2,942 households in the Household Panel Survey.

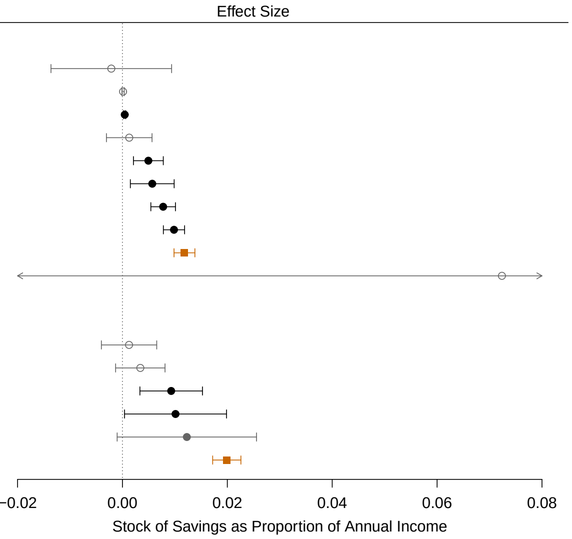
Table 3: Relationship between Trust and Savings Rates

	(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 $Trust_{it} \times Savings_{i,t-1}$			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

Notes: This table shows the direct relationship between trust and savings rates for the Trust Survey merged with a cross-section from the corresponding time period in the administrative bank account data. Columns 1 and 2 show ζ coefficients from $\Delta Savings_{it} = \zeta Trust_{it} + \varepsilon_{it}$, where in column 2, $Trust_{it}$ is instrumented by a set of dummies for each possible four-month period of card receipt. Coefficients are expressed as a proportion of income (measured over the same period as the flow of savings) for ease of interpretation. Column 3 uses a specification more similar to (4) with lagged savings and transfers on the right hand side: $\Delta Savings_{it} = \zeta Trust_{it} + \theta Savings_{i,t-1} + \xi Trust_{it} \times Savings_{i,t-1} + \gamma Transfers_{it} + \psi Trust_{it} \times Transfers_{it} + \varepsilon_{it}$, instrumented by dummies for the timing of card receipt as well as these dummies interacted with lagged savings and with transfers. Column 3 shows $(\hat{\zeta} + \hat{\xi}\omega_{-1} + \hat{\psi}\mu)/\overline{Y}$ where ω_{-1} is average lagged savings, μ is average transfers, and $\overline{Y}$ is average income (each for the corresponding period). $N = 1,330$ beneficiary households, the sample that we were able to successfully merge with administrative account data.

Figure 1: Comparison with Other Studies

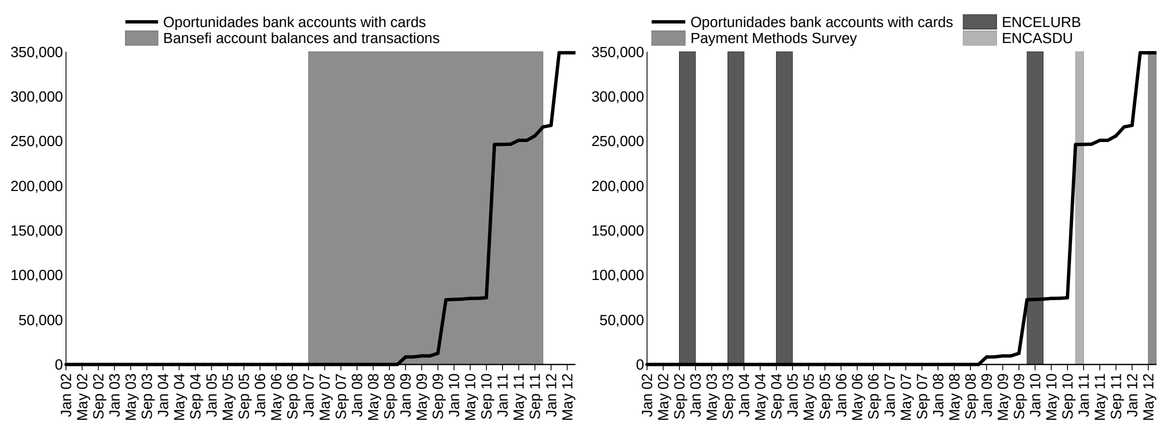
Study	Intervention	Country	Months
Panel A: Studies with about 1 year duration			
Drexler, Fischer, and Schoar, 2014	Financial education	Dominican Republic	12
Karlan and Zinman, 2016	Interest rate	Philippines	12
Kast, Meier, and Pomeranz, 2016	Savings group	Chile	12
Karlan et al., 2016	Reminders	Philippines	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



Notes: This figure compares the results from our study after 1 and 2 years with a debit card (orange squares) to other studies of savings interventions, and shows that we find larger effects than most studies with a comparable duration. Panel (a) shows studies with about a 1 year duration and panel (b) studies with a longer duration. The effect sizes are intent-to-treat effects of the intervention on the stock of savings, measured as a proportion of annual income. Appendix A details the selection criteria to determine which studies could be included and how we obtained their effects on the stock of savings as a proportion of annual income. Whiskers denote 95% confidence intervals. Black circles indicate results that are significant at the 5% level, gray circles at the 10% level, and hollow circles statistically insignificant from 0.

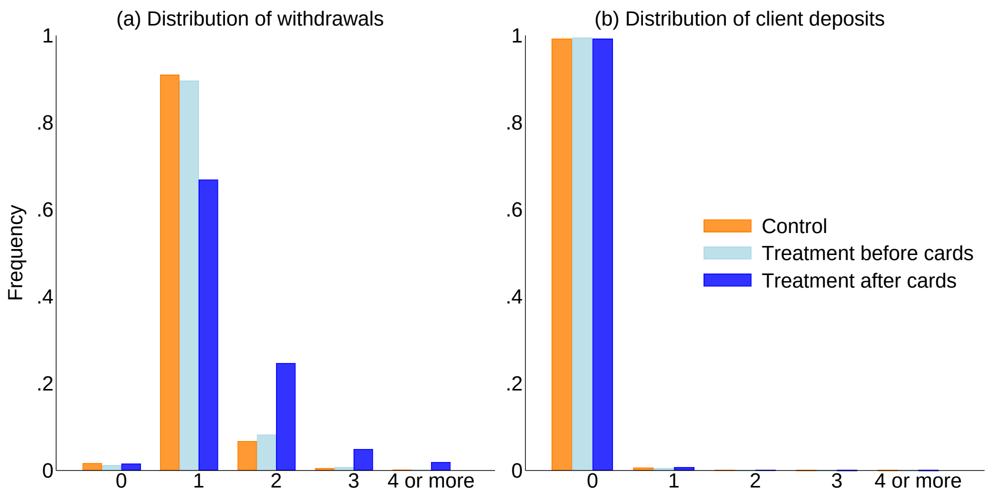
Figure 2: Timing of Rollout and Data

(a) Administrative Bank Account Data (b) Household Survey Data



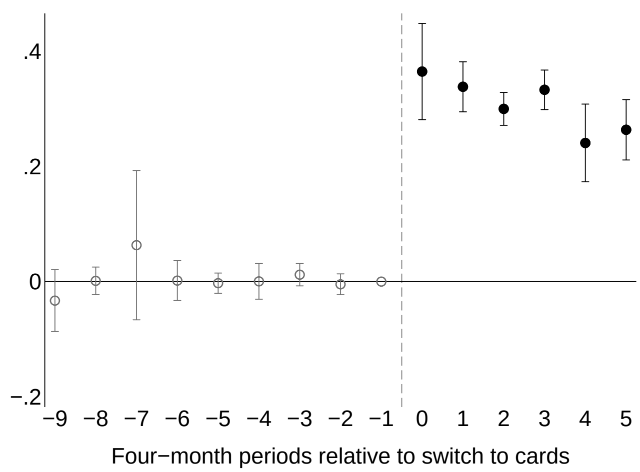
Notes: This figure shows the number of Oportunidades bank accounts with debit cards over time (administrative data from Bansefi). This was determined by the staggered rollout of debit cards, which generated variation across space and time in having a debit card tied to the bank account in which beneficiaries receive their benefits. Panel (a) compares the timing of the rollout to the timing of the administrative bank account data and panel (b) compares it to the timing of the household survey data.

Figure 3: Distribution of Withdrawals and Client Deposits per Bimester



Notes: This figure shows the distribution of withdrawals per bimester in panel (a) and of client deposits per bimester (i.e., excluding Oportunidades deposits) in 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. Within each group, all account-bimester observations are included. It shows that after receiving a card, a substantial portion of beneficiaries began making 2, 3, or 4 or more withdrawals per bimester rather than one. Based on all transactions from 348,802 beneficiaries over 5 years.

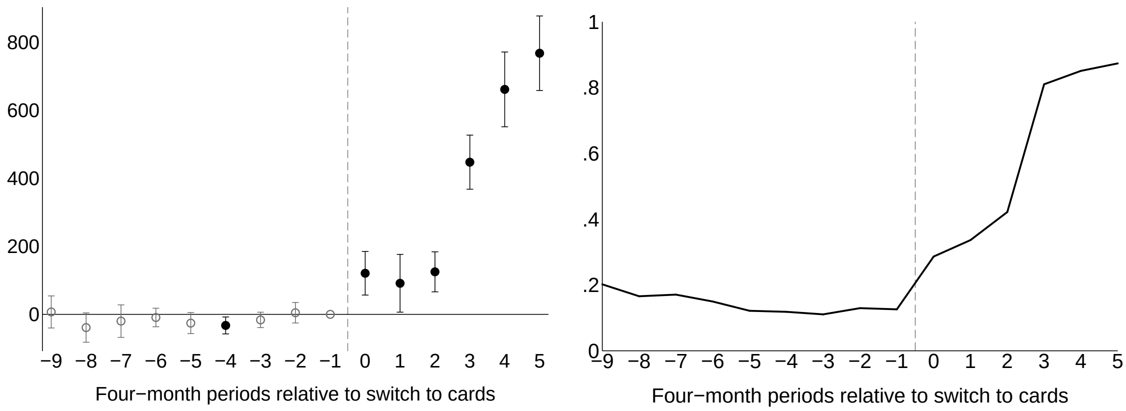
Figure 4: Effect of Debit Cards on Number of Withdrawals per Bimester



Notes: This figure shows the effect of the debit card on the number of withdrawals per bimester. It plots the ϕ_k coefficients from equation (1), where the dependent variable is number of withdrawals. $N = 4{,}740{,}331$ account-period observations from 348,802 beneficiaries. Whiskers denote 95% confidence intervals. Black circles indicate results that are significant at the 5% level, and hollow circles statistically insignificant from 0.

Figure 5: Effect of Debit Cards on the Stock of Savings

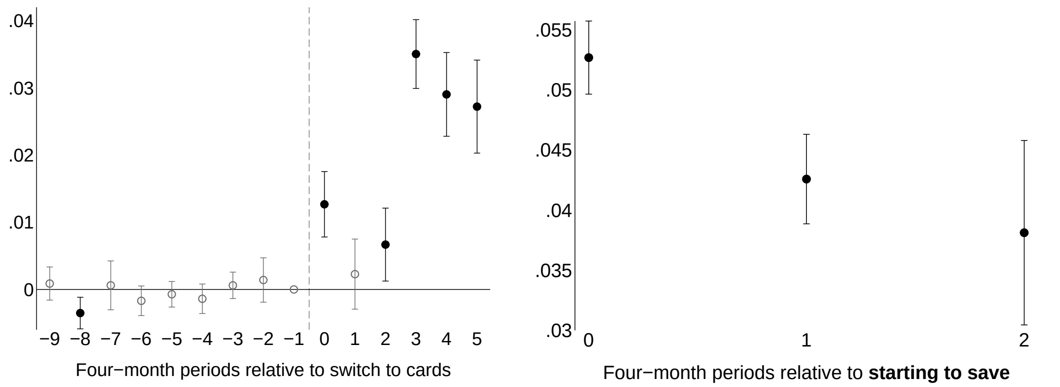
(a) Savings Balances (Pesos) (b) Proportion Who Save



Notes: This figure shows the effect of debit cards on the stock of savings and the proportion who save. Dashed vertical lines indicate timing of debit card receipt. Panel (a) plots the ϕ_k coefficients from equation (1), where the dependent variable is net savings balance. $N = 4{,}664{,}772$ account-period observations from 348,802 beneficiaries. Panel (b) shows the proportion of treated beneficiaries who save in each period relative to when they receive a debit card. $N = 3{,}183{,}050$ account-period observations for 255,784 *treated* beneficiaries. Whiskers denote 95% confidence intervals. Black circles indicate results that are significant at the 5% level, and hollow circles statistically insignificant from 0.

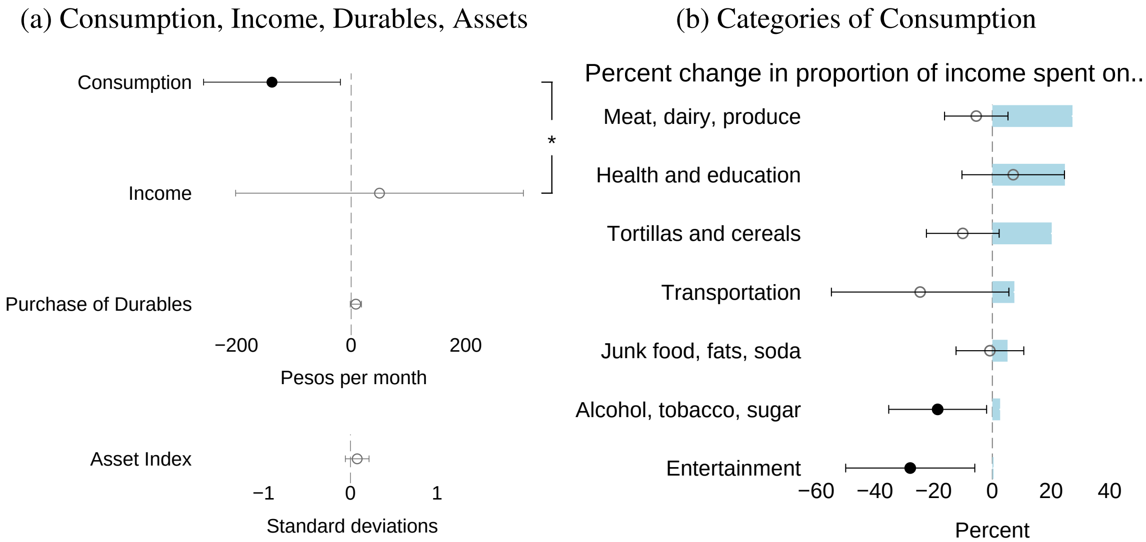
Figure 6: Effect of Debit Cards on the Savings Rate

(a) Savings Rate (% of Income) (b) Savings Rate Conditional on Saving



Notes: This figure shows the effect of debit cards on the savings rate and on the savings rate conditional on saving. Panel (a) plots Φ_k, defined in equation (5), where the components of Φ_k are estimated using (4). Dashed vertical line indicates timing of debit card receipt. $N = 4{,}315{,}970$ account-period observations from 348,802 beneficiaries. The lower number of account-period observations compared to Figure 5 is due to omitting a period to include lagged net balance. Panel (b) plots Φ_k, defined in equation (5), where the components of Φ_k are estimated using a modified version of equation (4) with two changes: we redefine D_{ij}^k as relative to when the beneficiary *started to save* rather than when she received a card, and we impose a zero pre-treatment trend by setting $a = 0$ (for reasons explained in Section 5.4). $N = 1{,}636{,}135$ account-period observations for the 140,193 treatment accounts who began saving at some point after receiving a debit card. Whiskers denote 95% confidence intervals. Black circles indicate results that are significant at the 5% level.

Figure 7: Effect of Debit Cards from Household Panel Survey

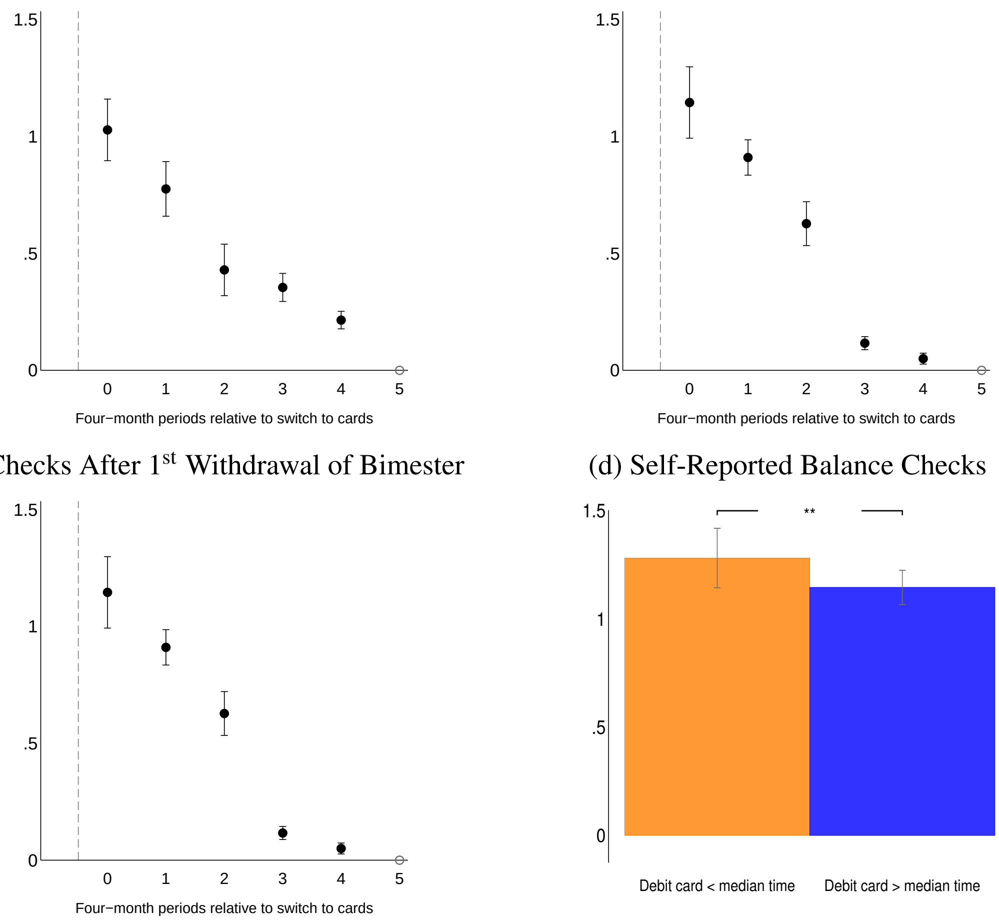


Notes: This figure shows the effect of the debit cards on consumption (and consumption by category), income, purchase of durables, and assets using the Household Panel Survey combined with administrative data from Oportunidades on the debit card rollout. Panel (a) shows estimates from four separate regressions using equation (2). 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. 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. For full results and robustness see Table B.2. Panel (b) shows estimates from separate regressions using equation (2), where the dependent variable is the percent of income spent on that consumption category. Consumption categories are sorted in descending order of the percent of income spent on each consumption category at baseline, which is shown by the thick horizontal bars. $N = 9{,}496$ household-period observations from 2,942 households.

Figure 8: Balance Checks Over Time

(a) All Balance Checks (b) Checks After Transfer Receipt

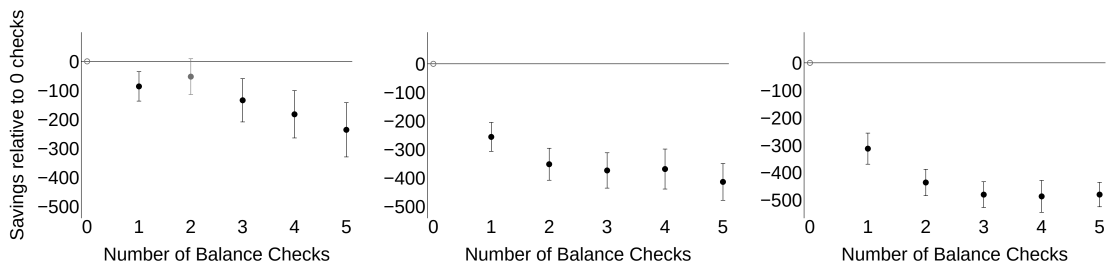
(c) Checks After 1st Withdrawal of Bimester (d) Self-Reported Balance Checks



Notes: This figure shows the number of balance checks over time after receiving the card. Panels (a), (b), and (c) use the administrative transactions data and express the number of balance checks relative to the last period in the data for each observation. They plot the π_k coefficients from $\textit{Balance Checks}_{it} = \lambda_i + \sum_{k=0}^{4} \pi_k D_{it}^k + \varepsilon_{it}$, where $k = 5$ is omitted. Dashed vertical lines indicate timing of debit card receipt. Periods before receiving the card are not included since it was only possible to check balances at Bansefi branches, and these balance checks are not recorded in our data. Each panel corresponds to a narrower definition of balance checks, where the narrower definitions attempt to rule out balance checks for purposes other than monitoring the bank. Panel (a) includes 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. These definitions are explained in more detail in Section 7.2.1. $N =$ 848,664 account-period observations from 223,788 unique *treated* beneficiaries with cards. Accounts in which cards are received in the last period of our data must be excluded in order to omit a D_{it}^k dummy; we also exclude those who receive the card in the second-to-last period in our data since they only have one additional post-card period. Panel (d) shows how self-reported balance checks (from the Payment Methods Survey) differ based on time with the debit card. It plots the number of balance checks per *bimester* among those who have had a card for less vs. more than the median time, and shows the statistical significance of the difference in means, estimated with equation (3), where * indicates $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. $N =$ 1,617 households in the survey. Whiskers denote 95% confidence intervals.

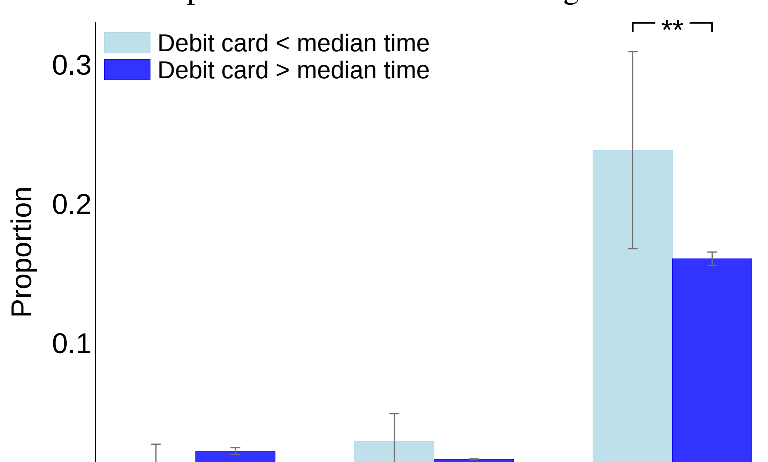
Figure 9: Within-Account Relation Between Balance Checks and Savings

(a) All Balance Checks (b) After Transfer (c) After 1st Withdrawal



Notes: This figure shows the negative within-account correlation between the number of balance checks and savings in the account, using the administrative savings and transactions data. It plots the n_c coefficients from $Savings_{it} = \lambda_i + \sum_{c\neq 0} \eta_c \mathbb{I}(Checks_{it} = c) + \varepsilon_{it}$, where savings are expressed in pesos, balance checks are top-coded at 5, and $c = 0$ is the omitted number of balance checks. Each panel corresponds to a narrower definition of balance checks, where the narrower definitions attempt to rule out balance checks for purposes other than monitoring the bank. Panel (a) includes 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. Whiskers denote 95% confidence intervals. Black circles indicate results that are significant at the 5% level, gray circles at the 10% level, and hollow circles statistically insignificant from 0. N = 577,295 account-bimester observations from 139,205 *treated* beneficiaries who began saving at some point after receiving a debit card.

Figure 10: Self-Reported Reasons for Not Saving in Bansefi Account



Notes: This figure compares reasons for not saving in the Bansefi bank account among Oportunidades beneficiaries who have had a debit card for less than vs. more than the median time. It compares the proportion of respondents in each group who have provide the corresponding reason for not saving in response to the questions "Do you leave part of the monetary support from Oportunidades in your bank account?" and if not, "Why don't you keep part of the monetary support from Oportunidades in your Bansefi savings account?" Beneficiaries who report saving are coded as 0 for each reason for not saving and still included in the mean proportion measures and regressions. The statistical significance of the difference in means is estimated with (3) and displayed at the top of the figure, where * indicates $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Whiskers denote 95% confidence intervals. $N = 1,694$ beneficiaries.

Appendix A Comparison with Other Studies (For Online Publication)

The savings rates in Figure 1 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 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 the stock of savings (i.e., savings balances), which we divide by annual household income. 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 effects in Figure 1 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 (2006). 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 (2013*b*). 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 result s 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.

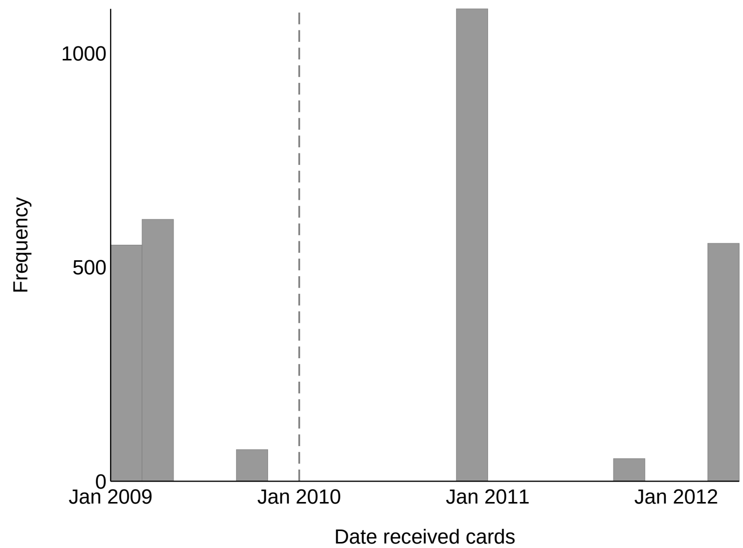
Suri and Jack (2016). This study looks at the effects of mobile money access in Kenya. The authors find that an increase in the penetration of mobile money agents within 1 kilometer of a household increases their log savings by 0.021 per agent for male-headed households and 0.032 per agent for female-headed households (Table 1). We exclude this study from the comparison because it does not include a measure of total household income.

Appendix B Additional Figures and Tables (For Online Publication)

Figure B.1: Geographic Coverage and Expansion of Debit Cards across Time and Space

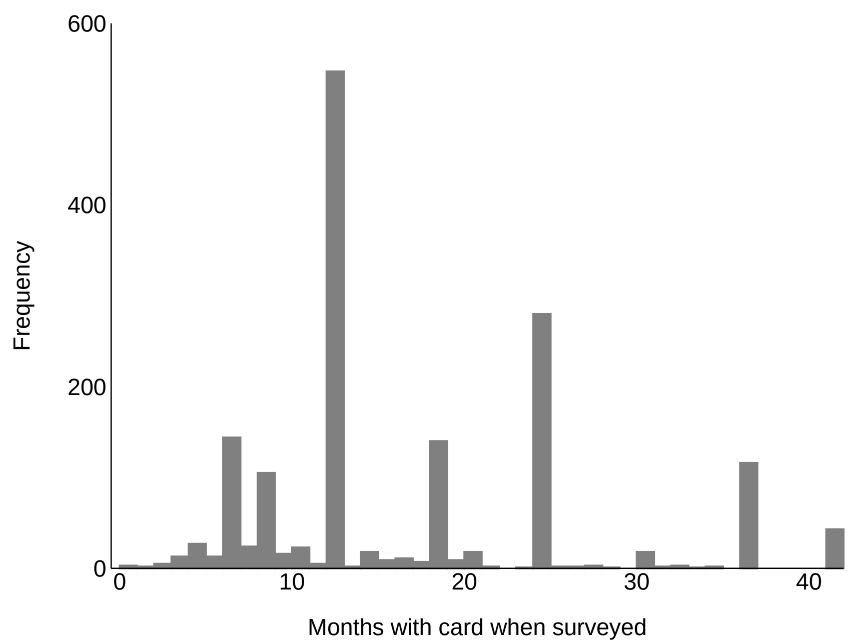
Notes: This figure shows the timing of the rollout of debit cards across urban localities, using administrative data from Oportunidades on the payment method in each locality for each bimester The area of each urban locality included in the study is shaded according to its wave of treatment. Urban localities 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 Timing of Card Receipt in Household Panel Survey



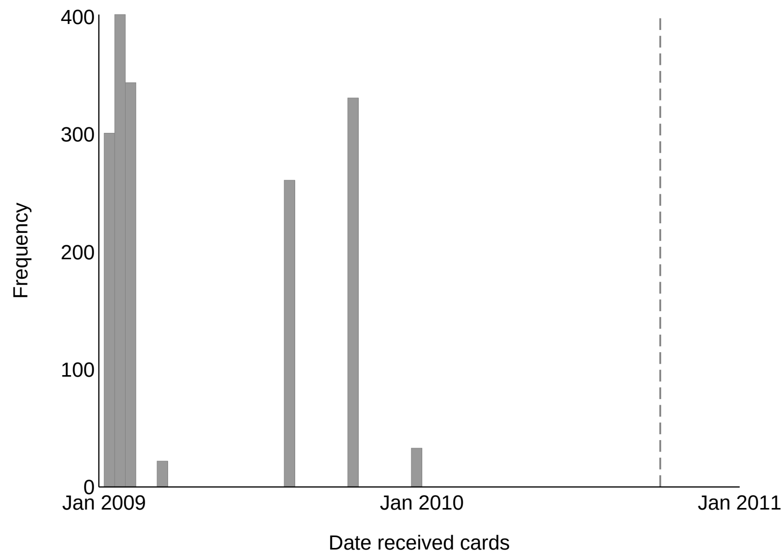
Notes: This figure shows when households in the Household Panel Survey received debit cards relative to the time of the survey, using survey data merged with administrative data on time of switch to debit cards. For the results using the Household Panel Survey, those who received cards prior to the survey are the "treatment" group and those who received cards after the survey are the "control." Dashed vertical line indicates timing of survey. $N = 2{,}942$ households.

Figure B.3: Distribution of Months with the Card at Time of Payment Methods Survey



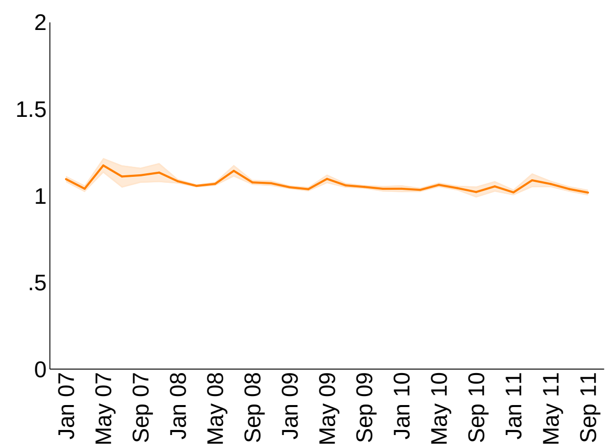
Notes: This figure shows how long ago households had received Bansefi debit cards before being surveyed in the Payment Methods Survey. We use self-reported months with the card from the survey. $N = 1{,}617$ beneficiaries.

Figure B.4: Distribution of Timing of Card Receipt in Trust Survey



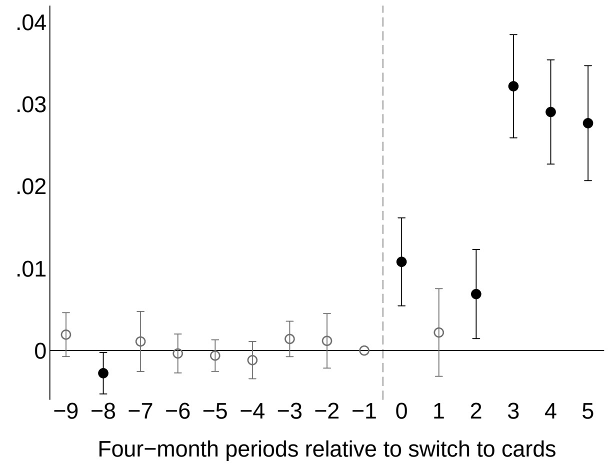
Notes: This figure shows when households in the Trust Survey received debit cards relative to the time of the survey, using survey data merged with administrative data on time of switch to debit cards. Dashed vertical line indicates timing of survey. $N = 1{,}694$ beneficiaries.

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



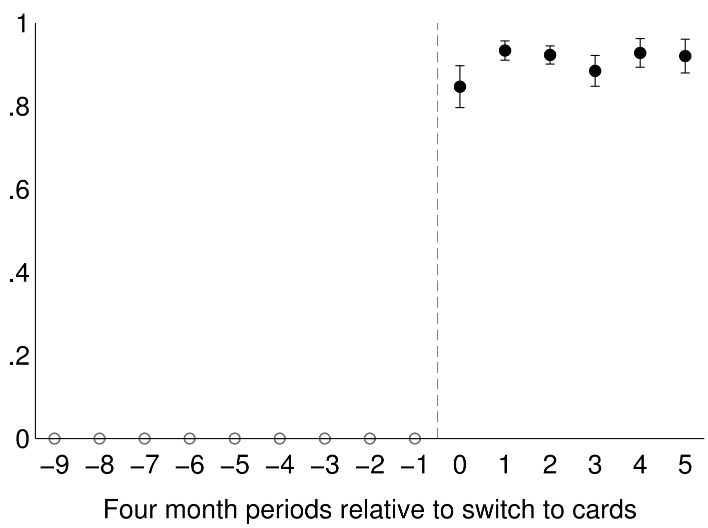
Notes: This figure shows the number of withdrawals in the control group per bimester over time using the administrative transactions data. Since the control did not receive cards during our study period, the x-axis is in calendar time rather than in time relative to the switch to cards. The shaded area denotes the 95% confidence interval. Standard errors are clustered at the bank branch level. $N = 2{,}584{,}375$ account-bimester observations from 93,018 unique *control* beneficiaries.

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



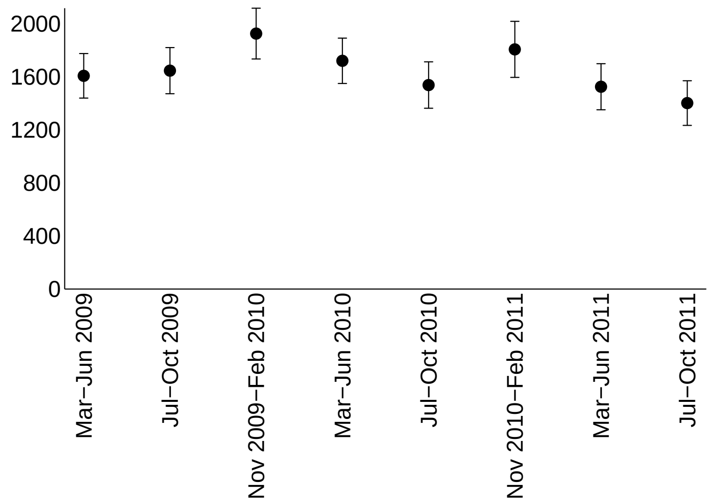
Notes: This figure shows the effect of debit cards on the savings rate when $Transfers_{it}$ are not included on the right hand side of (4). Specifically, it plots $(\hat{\alpha}_k + \hat{\xi}_k \omega_{k-1})/\overline{Y}$ from $\Delta Savings_{it} = \lambda_i + \delta_t + \sum_{k=a}^{b} \alpha_k D_{it}^k + \theta Savings_{i,t-1} + \sum_{k=a}^{b} \xi_k D_{it}^k \times Savings_{i,t-1} + \varepsilon_{it}$, where ω_{k-1} is average lagged transfers and $\overline{Y}$ is average income. Whiskers denote 95% confidence intervals, estimated using the delta method. Black circles indicate results that are significant at the 5% level, and hollow circles statistically insignificant from 0. $N = 4{,}315{,}970$ account-period observations from 348,802 beneficiaries.

Figure B.7: Share of Clients Using Debit Cards to Withdraw at ATMs



Notes: This figure shows the share of clients using their debit card for at least one withdrawal during a four month period. It shows that beneficiaries immediately adopt the new technology and use their cards to withdraw their transfers, instead of going to the Bansefi bank branch. Note that in periods before the card the share of clients using debit cards to withdraw at ATMs or convenience stores is necessarily zero. $N =$3,362,690 account-period observations from 250,792 *treated* beneficiaries. Standard errors are clustered at the bank branch level. Whiskers denote 95% confidence intervals. Dashed vertical line indicates timing of debit card receipt.

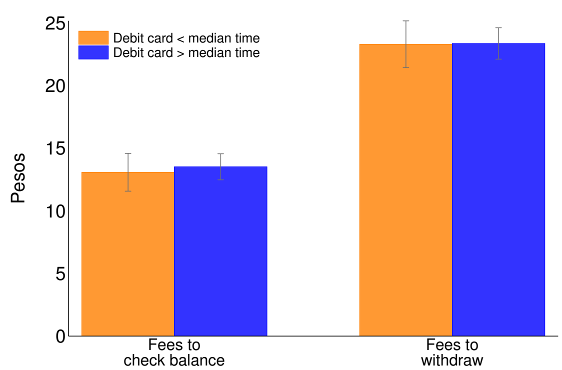
Figure B.8: Savings among Non-Oportunidades Debit Card Account Holders (Pesos)



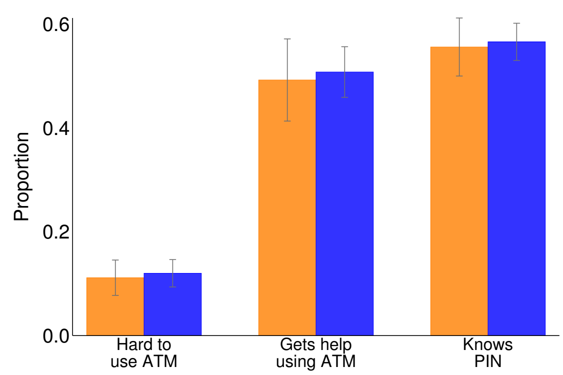
Notes: This figure shows mean savings per four-month period among non-Oportunidades beneficiaries with a debit card who opened accounts in 2007 (in pesos). Savings among non-Oportunidades debit card holders were not increasing over time during the period of our study, which suggests that our results are not driven by a decrease in transaction costs over time. $N = 2721$ non-Oportunidades accounts opened at a sample of 117 Bansefi branches in the year 2007.

Figure B.9: Self-Reported Knowledge of Technology and Fees

(a) Fees

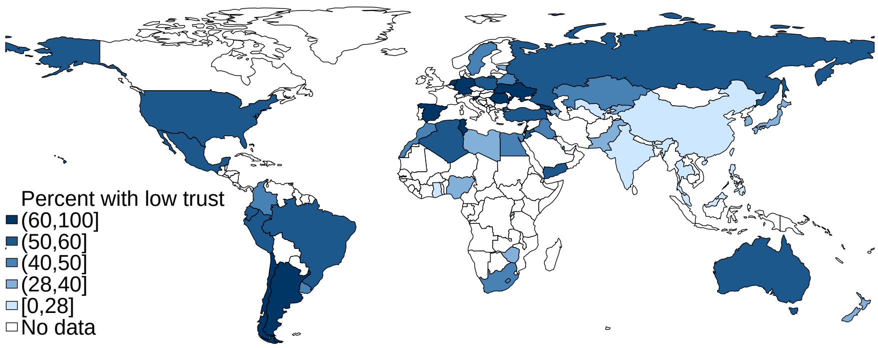


(b) Debit Card Technology



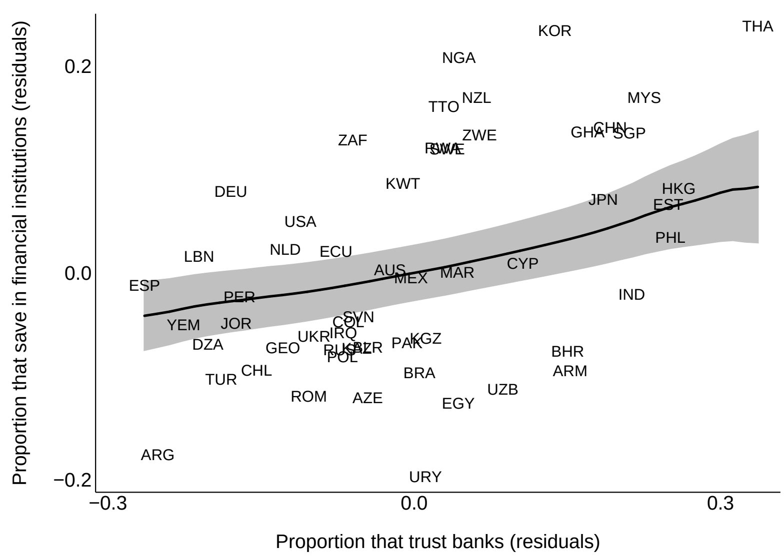
Notes: $N = 1,617$ from the Payment Methods Survey. In some regressions if there were respondents who reported "don't know" or refused to respond N can be smaller. It plots the outcome variable among those who have had a card for less vs. more than the median time, and shows the statistical significance of the difference in means, estimated with equation (3). In Panel (a) outcomes are self-reported transaction fees and in Panel (b) outcomes are self-reported knowledge of how to use the debit card. Standard errors are clustered at the locality level, using pre-treatment (2004) locality. Whiskers denote 95% confidence intervals. None of the differences in means is statistically significant from 0.

Figure B.10: Low Trust in Banks Around the World



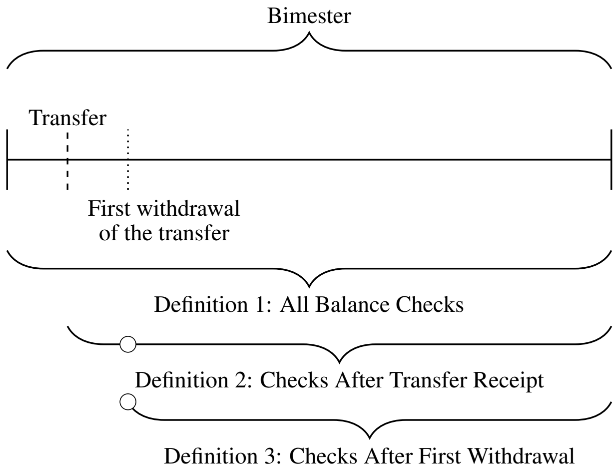
Notes: This figure shows that trust in banks is low across the world. Low trust in banks is defined as “not very much confidence” or “none at all” in response to the following question from the World Values Survey, Wave 6 (2010–2014): “Could you tell me how much confidence you have in banks: a great deal, quite a lot, not very much or none at all?” Darker shades indicate countries with a higher share of the population reporting low trust in banks. $N = 82{,}587$ individuals in 60 countries.

Figure B.11: Cross-Country Trust in Banks and Saving in Financial Institutions



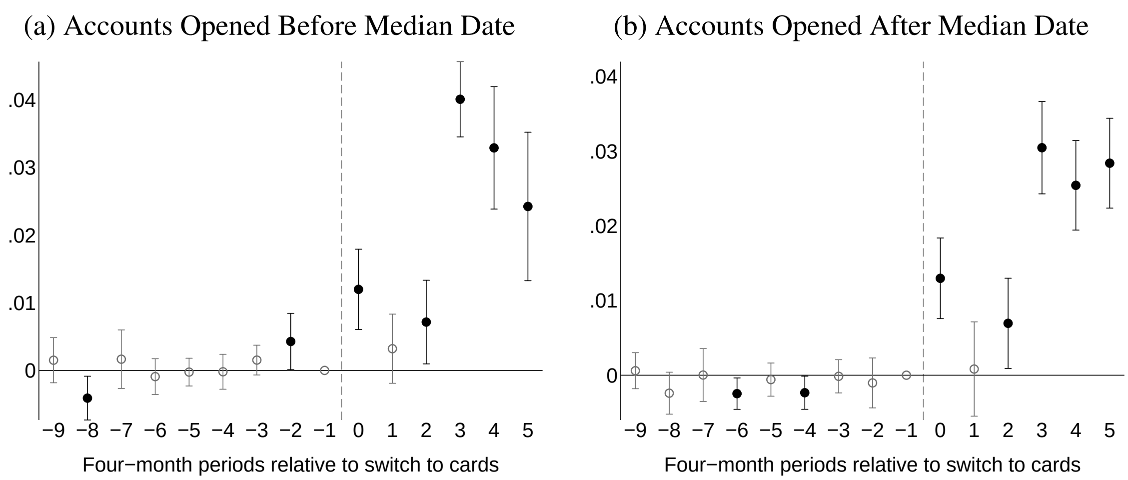
Notes: This figure shows that internationally, the proportion of adults who save in financial institutions is associated with the proportion that trust banks. The y-axis plots residuals from a regression of the proportion saving financial institutions (from Global Findex) on controls (average age, education, and perceived income decile from the World Values Survey Wave 6, GDP per capita levels and growth from World Development Indicators). The x-axis plots residuals from a regression against the same controls of the proportion that respond “a great deal of confidence” or “quite a lot of confidence” in response to the WVS question “could you tell me how much confidence you have in banks?” The solid line shows a kernel-weighted local polynomial regression, while the gray area is its 95% confidence interval. $N = 56$ countries.

Figure B.12: Stylistic Illustration of Balance Check Definitions



Notes: This figure illustrates the three definitions of balance checks that we use. For illustration we use the scenario where one withdrawal is made during the bimester. The first definition includes all balance checks in the bimester. The second definition includes balance checks that occur after the transfer, not including checks on the same day as a withdrawal (hence the hollow circle in the bracket for definition 2). The third definition includes only balance checks that occur after the first withdrawal of the bimester, when it is not conceivable that the beneficiary could be checking if the transfer has arrived.

Figure B.13: Effect of Debit Cards on Savings Rate for Older and Younger Accounts



Notes: This figure shows the effect of debit cards on the savings rate separately for older accounts opened before the median account opening date in panel (a), and younger accounts opened after the median date in panel (b). In both cases, the pattern is similar to the pattern from Figure 6a. The figure plots Φ_k, defined in equation (5), where the components of Φ_k are estimated using (4) separately for older and younger accounts. $N = 2{,}293{,}940$ from 154,022 beneficiaries in panel (a) and 2,176,052 account-period observations from 194,779 beneficiaries in panel (b).

Table B.1: Summary Statistics of Treatment and Control Localities

Variable	(1) Control	(2) Treatment	(3) Difference T–C	(4) 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)

Notes: This table shows the means of locality-level variables (from CONEVAL data based on the 2005 Census) and account-level variables (based on administrative accountd ata from Bansefi). Because the rollout was not randomized, variables are not perfectly balanced across treatment and control. Treatment refers to localities and accounts that received cards by October 2011, and control refers to localities and accounts that received cards between November 2011 and April 2012 (after our study period). 260 localities are in treatment and 30 in control. Column 3 shows the difference in means, where T = treatment and C = control. Column 4 shows results from a discrete time hazard model that uses the timing of debit card receipt throughout the rollout (January 2009 to April 2012) rather than a binary distinction between treatment and control. It includes a 5th-order polynomial in time, where time is measured by bimester; the coefficients are from a single discrete time hazard regression including all variables from both panels.

Table B.2: Effect of Debit Cards from Household Panel Survey

	(1)	(2)	(3)	(4)
Consumption	–178.11	–153.96	–138.09	–143.63
	(80.15)	(69.49)	(60.86)	(62.11)
Income	78.98	85.09	49.44	46.28
	(168.11)	(149.46)	(128.00)	(130.40)
P-value Consumption vs. Income	[0.058]	[0.055]	[0.092]	[0.103]
Purchase of durables	9.77	8.64	8.20	7.54
	(12.41)	(8.61)	(4.99)	(4.98)
Asset index	0.06	0.06	0.08	0.07
	(0.08)	(0.08)	(0.07)	(0.07)
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%

Notes: This table shows the effect of the debit cards on consumption, income, purchase of durables, and assets using the Household Panel Survey combined with administrative data from Oportunidades on the debit card rollout. Each row label is the dependent variable from a separate regression; each column is a different specification. The results from Figure 7 are in column 3 (winsorized at 5%). Means for each dependent variable can be found in Table 2b. 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. For column 4, 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

Notes: This table shows that there was no supply-side response of banking infrastructure to the debit card expansion, using data on ATMs and bank branches by municipality by quarter from CNBV. It also shows that the debit card rollout did not follow a recent expansion of banking infrastructure. Each column is a separate regression with a different dependent variable; the table shows β_k from $y_{mt} = \lambda_m + \delta_t + \sum_{k=-6}^{6} \beta_k D_{m,t+k} + \varepsilon_{mt}$. The F-test of lags tests $\beta_{-6} = \cdots = \beta_{-1} = 0$; the F-test of leads tests $\beta_1 = \cdots = \beta_6 = 0$. $N = 2,491$ municipality-quarter observations from 199 municipalities.

Appendix C Sample of Materials Received by Beneficiaries (For Online Publication)

Figure C.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 C.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 C.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: [redacted] Código Postal: **55450**

Domicilio: [redacted]

Folio Titular: [redacted]

Nombre Titular: [redacted]

Identificador de Familia: [redacted]

Fase de Incorporación: **35**

Esquema de Apoyos: **Urbano 1**

Colonia: [redacted]

Estimada Titular:

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

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

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

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

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

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

"Este programa es de carácter público, no es patrocinado ni promovido por partido político alguno y sus recursos provienen de los impuestos que pagan todos los contribuyentes. Está prohibido el uso de este programa con fines politícos, 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 D Mechanical Effect (For Online Publication)

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 D.1.

D.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.

D.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 D.1 shows the most common of the cases we considered as well as their prevalence in the data.

D.3 Steps

More generally we follow the steps below:

1. We separate the sample based on the number of transfers received by Oportunidades' beneficiaries: 85% of beneficiary-bimester pairs receive a single transfer in the bimester and 15% received two transfers in the same bimester. See footnote 22 for a description of the reasons some benefeciary-bimester pairs include more than one transfer.

Table D.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.

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.

Appendix E Reasons for Variance in Transfers (For Online Publication)

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.

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