How Debit Cards Enable the Poor to Save More

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Abstract

We study an at-scale natural experiment in which debit cards are given to cash transfer recipients who already have a bank account. Using administrative account data and household surveys, we find that beneficiaries accumulate a savings stock equal to 2 percent of annual income after two years with the card. The increase in formal savings represents an increase in overall savings, financed by a reduction in current consumption. There are two mechanisms: first, debit cards reduce transaction costs of accessing money; second, they reduce monitoring costs, leading beneficiaries to check their account balances frequently and build trust in the bank. (*JEL*: D14, D83, G21, O16)

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1 Introduction

A remarkably large number of households do not have sufficient savings to cope with relatively small shocks. 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 savings devices at formal financial institutions (Karlan, Ratan and Zinman, 2014). When households do have access to financial institutions, there are a number of well-documented causal impacts including increased entrepreneurial investment, wealth accumulation, and ability to cope with shocks (Bruhn and Love, 2014; Célérier and Matray, 2019; Stein and Yannelis, forthcoming).

Nevertheless, take-up and active use of bank accounts "remain puzzlingly low" (Karlan et al., 2016, p. 2), even when accounts are offered without fees (Dupas et al., 2018). In fact, 40% of adults worldwide do not have a formal bank or mobile money account (Demirgüç-Kunt et al., 2018). 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., 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. 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 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.[1]

Using high-frequency administrative data from nearly 350,000 beneficiary bank accounts in 357 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 beneficiaries 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 built up a stock of savings equal to 2% of annual income. This increase in savings—caused by an at-scale intervention that could be feasibly replicated with cash transfer beneficiaries in other countries—contrasts with the smaller and sometimes null effects on savings found by other interventions in the literature (Figure 1).

Using a rich household panel survey covering a subsample of the beneficiaries, we then test whether the increase we observe in formal savings is an increase in *overall* savings or a substitution from other forms of saving, both formal and informal. We focus on beneficiaries who have had the card for about a year at the time they are surveyed, and find that after one year with the card, there is no change in income and a significant reduction in consumption equal to about 4.9% of income. Because consumption and income are flows, and because the administrative bank account data show that the savings stock does not evolve linearly over time, we carefully compare this reduction in consumption of 4.9% of income to the change in the savings *rate* for beneficiaries from the same localities after they have had the card for the same amount of time as in the survey. This change in the savings rate from the comparable administrative data is 4.6% of income.

This suggests that the total savings rate likely rose by a similar amount to what we observe in the administrative bank account data (assuming that total savings is income minus consumption). More precisely, the administrative data suggest an increase in the savings rate of 4.6% of income and the survey estimates show no change in income and a consumption reduction of 4.9% of income, both for beneficiaries from the same localities who have had the card for approximately one year. Furthermore, the point estimates from the two sources of data are nearly identical (within 0.2% of income, or less than 50 cents per month) and each lies within the 95% confidence interval of the other. As with most household surveys, however, our survey estimates are noisy: the lower bound of the 95% confidence interval in the survey is 1.0% of income. Thus, while we can reject

[1] 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, and South Africa, in which cash transfers are also paid through bank accounts and recipients generally withdraw the entire transfer amount in one lump sum withdrawal each pay period (CGAP, 2012; Muralidharan, Niehaus and Sukhtankar, 2016).

that the increase in formal savings was *purely* substitution from other forms of savings, we cannot rule out that *part* of the formal savings increase was.

Why would debit cards lead to increased *total* savings? This would require it to be both difficult to save informally and for debit cards to make saving formally more attractive. Indeed, we find evidence consistent with it being difficult to save informally due to household members' easier access to the savings (consistent with lab-in-the-field experiments in Ashraf, 2009; Jakiela and Ozier, 2016). We find suggestive evidence of a larger proportional drop in spending on temptation goods compared to other goods, but no change to investment in education and health or assets. We also find suggestive evidence that beneficiaries with low intra-household bargaining power at baseline increase savings by more after receiving a debit card.

How do debit cards make saving formally more attractive? An obvious candidate is that debit cards decrease the transaction costs of accessing money, which makes saving in the account more attractive since savings can be easily accessed when needed. Indeed, debit cards reduce the indirect transaction costs of accessing the account: before receiving a card, account holders had to go to one of only 500 Bansefi branches nationwide to withdraw money, traveling a median road distance of 4.8 kilometers.[2] After receiving the card, each beneficiary could withdraw their balance from *any* bank's ATM, i.e. at any of the more than 27,000 ATMs in Mexico; they could also use the debit card to make purchases at point-of-sale (POS) terminals. The median road distance between a beneficiary's house and the closest ATM is 1.3 kilometers (Bachas et al., 2018). We find that the number of withdrawals made per month jumps by 36% immediately after receiving the card and stays relatively flat afterwards. Many beneficiaries start making two or three withdrawals per transfer period, while almost all beneficiaries used to make a single withdrawal of the entire transfer. Furthermore, 16% of beneficiaries begin accumulating savings immediately, likely due to the immediate reduction in transaction costs to access their money.

However, 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. Although a beneficiary could check her balance at Bansefi branches by asking a bank teller prior to receiving the card, the debit card makes balance checks much more convenient since it can be done at any bank's ATM. Thus, a reduction in transaction costs enables trust

[2]This may explain their low initial use of the accounts to save. If clients were already saving in their accounts and the transaction costs provided a form of commitment device, as was the case for one of the households profiled by Morduch and Schneider (2017), it is possible that a reduction in transaction costs would reduce savings.

building. Once trust is established, beneficiaries take advantage of the reduced transaction costs of accessing money associated with debit cards and increase the amount of savings held in their bank accounts.[3]

Two 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 leave small amounts of money in the account and use the card to check their account balances frequently, but reduce balance check frequency over time. We show that the reduction in balance checks over time is not driven by checking whether the transfer has arrived or checking whether there is enough money in the account before using the debit card to make a transaction at a POS terminal (furthermore, the Bansefi accounts do not charge overdraft fees). 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 trust in the bank than beneficiaries who have had their debit cards longer. We also rule out a number of competing mechanisms including falling transaction costs over time and learning the banking technology, among others.

Our main contribution to the literature is to show that a nationwide, at-scale rollout of a low-cost financial technology caused a large and significant increase in the number of active account users in terms of both number of withdrawals and savings. The stock of savings accumulated after two years corresponds to 2% of annual income. This is larger than estimates from most other savings interventions—including offering commitment devices, no-fee accounts, higher interest rates, and financial education (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. (2019), 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 (although these studies do not directly document the importance of these two channels).[4] Unlike debit cards, however, these technologies involved large implementation costs: given the existing ATM infrastructure in most countries, debit cards are very low-cost, while

[3]In addition, the reduced indirect transaction costs of accessing money in the account and the ability to use the debit card for purchases increase the potential benefit of saving formally. These factors could increase both the beneficiary's desire to learn whether the bank is trustworthy and the amount the beneficiary decides to save once they trust the bank.

[4]Mobile money clients can easily check account balances from their phones, and Callen et al.'s (2019) 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.

mobile money requires setting up an infrastructure of mobile money agents throughout the country and sending deposit collectors is labor-intensive.[5] Our paper also goes beyond these studies in two ways that we detail below: by showing that savings in the account are new savings financed by a reduction in consumption, and by providing evidence that both lower transaction costs and account monitoring are at play in explaining the savings increase.

Our second contribution is to show that the savings effect comes—at least partially—from an increase in total savings achieved by reducing current consumption, rather than a substitution from other forms of saving. Other studies testing whether an increase in formal savings represents an increase in total savings or a substitution from informal savings do not typically have sufficient power to rule out full substitution, even when they find large point estimates on total savings (e.g., Ashraf et al., 2015; Kast, Meier and Pomeranz, 2018).[6] While account holders appear to reduce consumption as they increase savings over time in Somville and Vandewalle (2018, Figure 2), their consumption results are not statistically significant; Breza and Chandrasekhar (2019) similarly find noisy consumption results suggestive of a reduction in consumption to finance savings. In this paper, we definitively show that a portion (and based on the point estimates, possibly all) of the increase in formal savings is financed by reducing current consumption.

Our third contribution is to directly investigate two barriers to saving: indirect transaction costs and distrust. We show evidence that some beneficiaries begin saving immediately after receiving a debit card—likely due to the decreased transaction costs of accessing the account—while others begin saving only after a delay. This delay is partly explained by beneficiaries first monitoring the bank by checking account balances and increasing their trust in the bank over time. Studies have explored the role of trust in stock market participation, use of checks instead of cash, and mortgage refinancing in developed countries (Guiso, Sapienza and Zingales, 2004, 2008; Johnson, Meier and Toubia, 2019) and the role of trust in borrowing and take-up of insurance products in developing countries (Karlan et al., 2009; Cole et al., 2013). There are few studies, however, that

[5] Schaner (2017) is, to our knowledge, the only other paper which evaluates the impact of debit cards on savings. Her setting is very different from ours. First, recipients of the card (and the control group that received an account but no card) were a selected group: they had already expressed interest in opening an account at the partner bank. Second, in the rural Kenyan town in which her experiment was conducted, there was only one ATM located just outside one of the bank's branches. Thus, providing debit cards did not reduce travel costs to access money in the account or monitor account balances. Instead, withdrawing money at the ATM had a lower fee than interacting with a bank teller. She finds an increase in the number of transactions and the value deposited and withdrawn, but no change in savings.

[6] An exception is Callen et al. (2019), who find a statistically significant impact on total savings that is similar in magnitude to the impact on formal savings. Unlike our paper, they find no impact on consumption but rather find that an increase in labor supply in response to the savings intervention enables the increase in savings.

rigorously explore the role of distrust as a constraint to saving or the role of financial technology in increasing trust (Karlan, Ratan and Zinman, 2014).[7]

In summary, debit cards combined with ATMs or POS terminals 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, prevalent, and potentially scalable to hundreds of millions of households worldwide; they could be especially useful to enable the poor to save when combined with direct deposits (Blumenstock, Callen and Ghani, 2018). In particular, government cash transfer programs could be a promising channel to increase financial inclusion, not only because of the sheer number of people 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., Haushofer and Shapiro, 2016; Muralidharan, Niehaus and Sukhtankar, 2016). When these enabling conditions—direct deposits of sizable transfers and an expansive ATM network—are not present, the effects of debit cards on savings could be lower and our effect size is likely to be an upper bound for such contexts.

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 every two months ("bimester") to poor families, conditional on sending their children to school and having preventive health check-ups; due to program rules, the payments are made directly to women in nearly all beneficiary households. It began in rural Mexico in 1997 under the name Progresa, and later expanded to urban areas as Oportunidades starting in 2002. By 2011, nearly one-fourth of Mexican households received benefits from Oportunidades, and in 2014 it was rebranded as Prospera.[8]

[7]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 (forthcoming), who promote interactions with bankers and find that account savings is strongly associated with trust in one's own banker.

[8]The program has led to increases in school attendance and grade completion, improvements in childrens' health outcomes, modest increases in household consumption and caloric intake, and no change in labor market participation

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 at 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.[9] 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. The median urban household receiving its transfers in a Bansefi account was 4.8 kilometers from the nearest Bansefi branch (Bachas et al., 2018). Overall, the savings accounts were barely used prior to the introduction of debit cards: 89.9% of clients made one withdrawal each bimester, withdrawing 99.5% of the transfer on average (Table B.1).

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. Overdrafting is not permitted and there are no fees for attempting to overdraft: if the account has insufficient funds when attempting to make an ATM withdrawal or POS transaction, the transaction does not go through and an "insufficient funds" message is displayed. 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 2a, beginning in January 2009 debit cards tied to these existing bank accounts were rolled out to beneficiaries by locality. When Bansefi distributed cards in a particular locality, all beneficiaries in that locality received cards during the same payment period. By the end of 2009, about 75,000 beneficiaries had received debit cards

or labor market income (see Parker and Todd, 2017, for a review). Note that these effects do not confound our study since everyone in our analysis is a beneficiary.

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

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 at the locality level over time. The map in Figure 2b shows that the card expansion had substantial national geographic breadth throughout the rollout.

While the timing of the rollout of cards was not explicitly randomized across localities, we show in Section 4 that the timing was uncorrelated with observed locality-level characteristics. This is consistent with conversations we conducted with Oportunidades officials, who asserted that they did not target localities with particular attributes because they wanted to test their administrative procedures for the rollout—such as how easy it would be to distribute cards—on a quasi-representative sample. In addition, we show that the variables we use from the transactions-level data, as well as other variables such as wages, prices, and financial infrastructure, exhibit parallel pre-trends. Importantly, when a locality is treated, all beneficiaries in the locality receive cards that period. Furthermore, although the total number of beneficiaries increases slightly over time at the national level, we show that the rollout was not accompanied by a differential change in the number of beneficiaries in a locality (Section 4).

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 given 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 data from Bansefi on the average monthly balance and all transactions 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 357 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 shows summary statistics from this dataset. Using data from the first bimester of 2008 (before any debit cards were disbursed to beneficiaries), the accounts in our sample make 0.01 client deposits and 1.1 withdrawals per bimester on average, and the average amount withdrawn is 99.5% of the Oportunidades transfer, indicating very low use of the account for saving prior to receiving the card. End-of-period balances are 124 pesos or about US$11 on average; the distribution of end-of-period balances is skewed: the 25th percentile is just 2 pesos (US$0.20) and the median is 42 pesos (US$4). The average amount transfered by Oportunidades in the first bimester of 2008 is 1,540 pesos, or about US$144, 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 3.5 years by January 2008, so beneficiariaries in our study had substantial experience with a savings account prior to receiving the debit card.

3.2 Survey Data

Since its inception in 1997, Oportunidades has a long history of collecting high-quality surveys 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 B.1 shows when survey respondents received cards in each of these surveys, relative to the timing of the survey. In all surveys, the sample we use for estimation consists of households that received cards at some point during the rollout; these households correspond to a subset of the accounts in the administrative data described in Section 3.1. Note that we cannot merge the survey data to the administrative account data.

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 in urban areas. 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, most of 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 exclude the small group of beneficiary households in this survey that received cards in late 2009, shortly before the post-treatment survey wave, for cleaner comparisons with the administrative data results.[10] We merge the survey with administrative data from Oportunidades on the debit card expansion (at the locality level) 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 a 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.

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."[11]

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

[10]Because only 74 of the 2942 households in this survey living in urban localities included in the rollout are in localities treated in late 2009, our results hardly change if we do not drop these households.

[11]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").

know their card's PIN by heart. We restrict the analysis to the 1,617 surveyed beneficiaries in the sampled urban localities that had received cards prior to the survey; median exposure time to the card is 12 months.

3.3 Auxiliary Data

We use auxiliary data for four purposes: to identify (in survey data) when beneficiaries in each locality received cards as part of the rollout, to test for balanced pre-trends across a broad range of observables, to test whether the timing of the rollout is correlated with locality-level characteristics, and to test for a supply-side response by banks to the debit card rollout.

3.3.1 Auxiliary Administrative Data

We use administrative data from Oportunidades on the number of beneficiaries and payment method by locality by bimester beginning in 2007. These data allow us to identify the timing of the rollout of debit cards (which is not necessary for results using the Bansefi administrative data—where we directly observe when each account receives a debit card—but which we use for results using survey data). We also use data from Mexico's National Banking and Securities Commission (CNBV) which include a number of financial indicators at the bank by municipality by quarter level beginning in the fourth quarter of 2008. From these data, we use the number of bank branches, ATMs, debit cards, and credit cards to test for balanced pre-trends and to test for a supply-side response by banks. We use administrative data from Mexico's Central Bank on the universe of point-of-sale terminal adoptions and cancelations since 2006 (Higgins, 2019) to test for balanced pre-trends in financial technology adoption on the supply side of the market. Finally, we use local elections data that we digitized to test whether the timing of the rollout was determined by political considerations (specifically, the party in power at the local level).

3.3.2 Auxiliary Survey Data

We use data from three surveys to test for balanced levels and pre-trends in the economic performance of the localities. First, we use locality-level indicators derived from Mexico's 2005 Population Census. The indicators we use are the ones used by Mexico's National Council of Social Development and Policy Evaluation (CONEVAL) to measure locality-level development gaps. Second, we use microdata on wages from Mexico's quarterly labor force survey, the Encuesta Nacional de Ocupación y Empleo (ENOE). This data set includes wages for 20 million individual by quarter observations over 2005–2016. Third, we use price quotes from the microdata used to construct Mexico's consumer price index. These data include over 4 million price quotes over 2002–2014 at the product by store by month level for food, beverages, alcohol, and tobacco.

4 Empirical Strategy and Identification

We exploit variation generated by the staggered rollout of debit cards to different localities by Oportunidades. In this section, we show that conditional on being included in the rollout, the timing of when a locality receives treatment is not correlated with levels or trends in observables from a number of datasets. These datasets include microdata on wages and food prices, locality-level data on financial infrastructure and poverty, local elections data, and transaction-level data from beneficiaries' bank accounts.

Our empirical strategy depends on the data being used, but the underlying variation we use always stems from the plausibly exogenous rollout of debit cards over time. When the data have 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 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 period. In other words, we use an event study specification with a pure control group throughout the study period—the pure control group are those who were treated after October 2011, the last period for which we have data. 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 (as opposed to event-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.[12] 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.[13] We estimate cluster-robust standard errors, clustering ε_{it} by locality.

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 card recipients would have had the same changes in account use and savings behavior. While this is untestable, we test for parallel pre-intervention trends by showing that $\phi_k = 0$ for all $k < 0$. We perform these tests not only for the outcomes that we use with specification (1), which come from the Bansefi savings and transactions data, but also for a number of other outcomes from numerous data sources.

Figure 3 panels a and b show that the timing of when different localities receive cards as part of the rollout is not correlated with pre-trends in wages, food prices, or financial technology (point-of-sale terminals, bank branches, ATMs, or debit and credit cards). Furthermore, panel c shows that it is not correlated with beneficiary savings or the number of withdrawals they make from their accounts. Using less granular annual data, we also test and rule out that the rollout was correlated with pre-trends in the number of program beneficiaries or whether the party in power at the municipal level corresponds with the party in power at the national level (Figure B.2). In addition to demonstrating parallel pre-trends, Figure B.2 shows that there was no differential change in the number of program beneficiaries or local politics *as a result of* the debit card rollout.

As an additional test of whether the timing of the rollout is correlated with levels or trends in locality-level observables, we follow Galiani, Gertler and Schargrodsky (2005) and use a discrete time hazard model. This is equivalent to testing whether in a given period t, the probability of being treated at t conditional on not being treated yet at $t-1$ is correlated with observables. We combine several data sets to include measures of the pre-treatment levels and trends of financial infrastructure, politics, and the locality-level variables used by Mexico's National Council of So-

[12] 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 $\overline{k}$.).

[13] 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 months.

cial Development Policy Evaluation—the independent government agency that produces Mexico's official poverty estimates—to determine locality-level development gaps.[14] We reject that the timing of the rollout is correlated with observables among localities included in the rollout: of the 22 variables included in the model, the coefficient on one variable is statistically significant at the 5% level (as expected by chance) and the remaining coefficients are statistically insignificant (Table 2).

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

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

where y_{it} is consumption, income, or the 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.[15]

The identifying assumption is again parallel trends. In addition to the evidence that the rollout of cards was not correlated with levels or trends of variables from several data sets in Section 4.1, we verify parallel pre-treatment trends in the household survey panel data by estimating

$$y_{it} = \lambda_i + \delta_t + \sum_k \omega_k T_{j(i)} \times \mathbb{I}(k = t) + \eta_{it}, \tag{3}$$

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-

[14] We include trends for the variables for which it is possible, i.e. those from data sets with at least annual frequency (ruling out the 2005 Census) with data beginning prior to 2008. The data on Bansefi branches and ATMs begin in the last quarter of 2008; hence we only include levels of those variables.

[15] In our data, very few households migrate. In theory this could be a result of migrating households attriting from the survey. Nevertheless, we confirm using other data that migration in these localities is low. Using data from a panel of 12 million voter registrations (a 15% random sample from the universe of 80 million voter registrations in Mexico), we check the proportion of residents from the *same* localities as those in the Household Panel Survey who migrate over a three-year period and find that only 4.5% of residents migrate to another locality.

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 3b, 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. Note that the variation in time with the card is still determined by the plausibly exogenous rollout of cards by the government.

We regress the outcome variable—such as self-reported reasons for not saving—on a dummy of whether beneficiary i's time with the card is above the median:

$$y_i = \alpha + \gamma \mathbb{I}(\text{Card} \geq \text{median time})_i + u_i, \tag{4}$$

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.[16] We thus conduct balance tests using (4) with characteristics that should not be affected by debit card receipt as the dependent variable, such as number of household members, age, gender, status, education level, assets, and income. Table 3a shows that in our survey samples, those with the card for less and more than the median time are balanced, consistent with our finding that the timing of treatment of localities included in the rollout was not correlated with observables.[17] As a robustness check, we also add controls to (4) for the household-level characteristics from Table 3a and the baseline locality-level characteristics from Figure 3.

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

[16] 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.

[17] The Payment Methods Survey includes fewer measures of household and sociodemographic characteristics since the survey was focused on experience with the debit cards and ATMs. We find no statistically significant differences in the 10 variables on household and sociodemographic characteristics included in the Trust Survey nor the 5 variables included in the Payment Methods Survey.

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 transactions (withdrawals and deposits) and savings in the accounts. To interpret the results, we first note that beneficiaries begin using their debit cards to make withdrawals at ATMs almost immediately (rather than continue to make withdrawals at bank branches). In the four-month period in which they receive cards, 83% of beneficiaries withdraw money from an ATM, and this increases to around 90% in subsequent periods (Figure 4). A subset of beneficiaries (45% on average across periods) also use the cards to make purchases at POS terminals. Conditional on making a POS transaction, they average 2.2 transactions per period and the average amount spent per POS transaction is 92 pesos (US$7). Throughout the remainder of the paper, "withdrawal" includes bank withdrawals, ATM withdrawals, and POS transactions.

5.1 Number of 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 5a 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. Meanwhile, the number of withdrawals in the control group does not change over time (Figure B.3). 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.

On the other hand, there is no effect on client deposits: Figure 5b shows that 99% of accounts have zero client deposits per bimester before and after receiving the card. Account holders thus do not add savings from other sources of income to their Bansefi accounts. This is unsurprising for two reasons. First, deposits can not be made at ATMs (since these belong to banks other than Bansefi): beneficiaries still need to travel to a Bansefi bank branch to make deposits. Hence the transaction cost of depositing money into the account remains unchanged. Second, 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 6a plots the ϕ_k coefficients of average withdrawals per bimester for each four-month period, compared to the period just before receiving cards (also shown in Table 4 column 1). 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, both treatment and control accounts average just under one withdrawal per period on average. 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 0.36 withdrawals per bimester on average. Table B.2 shows that the results are not sensitive to winsorization, to including baseline account characteristics interacted with time dummies, or to using the inverse hyperbolic sine transformation for the outcome.

5.2 The Stock of Savings (Account Balances)

Next, we explore whether debit cards cause an increase in savings from period to period. The results on withdrawals tell us nothing about period-to-period savings, as beneficiaries could continue withdrawing once per period but reduce the total amount they withdraw during the period and thereby increase their savings. On the other hand, they could increase the number of withdrawals and leave some money in the account after the first withdrawal in the pay period, but withdraw the remaining money later in the same period thereby leaving the account balance close to zero by the end of that period. The latter possibility means that we have to construct our measure of savings carefully, as it would lead to a mechanically higher average balance *within* each period that does not correspond to accumulating saving in the account over time, i.e., *across* periods.

Instead, we are interested in measuring savings across periods. Ideally, we could measure the stock of savings as the end-of-period balance, calculated as the beginning-of-period balance plus all deposits minus all withdrawals. Unfortunately, while we have transactions data beginning in

January 2007, we do not have the initial balances for the first period of our data (January 2007), and cannot assume that these equal zero because we know from the average balance data that a non-trivial share of beneficiaries save in their accounts prior to 2007. Thus, to construct a reliable estimate of end-of-period balance we combine data on the average balance for each period with transactions-level data on the timing and amount of each transaction (see Appendix D for details).

We estimate (1) using account i's end-of-period balance in period t as the dependent variable. Following other papers measuring savings (e.g., Kast, Meier and Pomeranz, 2018), we winsorize savings balances at the 95th percentile to avoid results driven by outliers. The ϕ_k terms thus measure the causal effect of debit cards on the stock of savings k periods after receiving a card. Figure 6b plots the ϕ_k coefficients and their 95% confidence intervals (also shown in Table 4 column 2). We note the empirical support for parallel trends, shown by the zero coefficients for pre-event periods, $k < 0$.[18] In the first few periods after receiving a card, we observe a small savings effect of 100 to 200 pesos (about US$8 to 15). The initial effect is small because only a minority of beneficiaries begin saving shortly after receiving a card—we explore this further below. Savings increase substantially after about one year with the card: three four-month periods after card receipt, the savings effect is 447 pesos, while it is 768 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). Table B.3 shows that the results are not sensitive to winsorization, to including baseline locality characteristics interacted with time dummies, or to using the inverse hyperbolic sine transformation for the outcome.

The effect of debit cards on the average stock of savings shown in Figure 6b combines two effects: the impact of debit cards on the probability of saving and the saving amount conditional on saving. Figure 7 decomposes these two components. Figure 7a shows the proportion of treated beneficiaries who have at least a small positive balance at the end of each period: while only 13% of beneficiaries saved in their account in the period before receiving cards, an additional 16% start saving immediately after receiving a card.[19] 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,

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

[19] We set the threshold for a "small positive balance" at 150 pesos, given that balances below 50 or 100 pesos could be due to ATMs not disbursing exact change rather than voluntary savings.

and after two years nearly all beneficiaries (87%) save in their Bansefi account.

To estimate the second component, i.e. the amount of savings conditional on having started to save, we define a new event as the period in which a beneficiary begins saving (rather than when the beneficiary receives a card). This event is not causal—it occurs at different points in time for different beneficiaries, due to both the quasi-exogenous timing of receiving cards and the endogenous timing of when they choose to start saving once they receive the card. Our goal with this estimation is merely descriptive: to estimate the amount of savings each period after having started to save. We estimate (1) using this new event and show the results in Figure 7b.[20] In the first period when beneficiaries save in the account, they deposit 618 pesos on average, or 4.9% of their total income that period. They deposit significantly less in the following periods, consistent with models of precautionary saving in which an individual's savings rate is decreasing in her stock of savings as it approaches her buffer stock target (Carroll, 1997).

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 represents an increase in overall savings or 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, 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. We estimate (2) in columns 1–3 and (2) with the additional interaction of time fixed effects and baseline household characteristics in column 4, separately for three dependent variables: consumption, income, and an asset index.[21] The additional interaction of time fixed effects and baseline household characteristics follows de Janvry et al. (2015), and absorbs variation in how the dependent variable evolves

[20]Because the majority do not begin saving until they have had the card for a year, we only graph the savings stock for three post-saving periods (as further-period estimates would be based solely on the small sample of earlier savers).

[21]Standard errors shown in parentheses are cluster-robust asymptotic standard errors, clustered at the locality level. There are 46 localities in our estimation sample from the survey. We also show wild cluster bootstrap percentile-*t* 95% confidence intervals in square brackets, as well as clustered randomization inference p-values in square brackets.

over time for different types of households.

6.1 Total Consumption and Income

Table 5, column 4 shows that consumption decreased by about 155 pesos per month among treated households relative to control (statistically significant at the 5% level). We do not find any effect on income. 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 5 or 10% level in all specifications (the p-value of the F-test of equality of the coefficients on consumption and income is 0.057 in column 4). Table 5, columns 1–3 show that our results are robust to the extent of winsorizing and to removing the controls for flexible time trends as a function of household characteristics.

The point estimates of the effect of debit cards on consumption and lack of effect on income suggest that the increase in formal savings shown in Section 5 represents an increase in total savings (since total savings is income minus consumption). To compare estimates from the survey and administrative data, we first note that the survey estimate on reduced consumption—which equals 4.9% of income—is measured as a flow at a specific point in time relative to receiving a card. Specifically, this survey estimate corresponds to the effect of a card on the flow of consumption after approximately one year.[22] The timing of this effect matters for our comparison given our finding from the administrative data that the savings stock does not evolve linearly over time.

We therefore compare our estimate of the reduction in consumption from the survey (4.9% of income) to the change in the *flow* of savings after one year with the card, for a comparable set of beneficiaries in the administrative data. We achieve this by restricting the administrative data for this comparison to the same set of treatment localities included in our survey estimates. Recall that we cannot restrict to the exact same *accounts* because we cannot merge the two data sources at the account/beneficiary level. We then compute the change in the average $\Delta Savings_{it}$ for this subset of the administrative data, where we restrict t to the period after exactly one year with the card. For ease of interpretation, we divide this change in the flow of savings after one year with the card, measured in pesos, by average income (taken from the survey).[23] This gives us an estimate

[22] This is because the treated localities included in the post-treatment survey wave were deliberately selected to be primarily the localities that received cards at the beginning of the rollout; see Figure B.1. We exclude beneficiaries in the survey who live in localities that received cards shortly before the survey wave.

[23] As usual, Δ is computed relative to the preceding four-month period. The change in $\Delta Savings_{it}$ is relative to

of the effect of debit cards on the flow of formal savings of 4.6% of income, which is within 0.2% of income—or less than 50 cents per month—of our survey estimate of reduced consumption. Furthermore, each of the two estimates is within the 95% confidence interval of the other.

As in most household surveys, however, our estimates of the change in consumption are noisy: while we can reject that the increase in formal savings was purely substitution from other forms of saving, we cannot rule out that some but not all of the increase in formal savings was substitution. To maximize power, we focus on the specification from Table 5 column 4, which absorbs additional variation in how the dependent variable evolves over time for different types of households by including time fixed effects interacted with baseline household characteristics. The lower bound of the 95% confidence interval estimated using a percentile-t wild cluster bootstrap is a reduction in consumption equal to 33 pesos per month or 1.0% of income; the lower bound of the 90% confidence interval is 52 pesos per month or 1.6% of income.

6.2 Exploring the Fall in Consumption

The finding that consumption decreases by the same amount as formal savings increase, while income does not appear to change, provides evidence that the increase in formal savings represents new savings rather than purely substitution from other forms of saving. Why would receiving debit cards increase overall savings, financed by a reduction in consumption? One possibility is that cash is "hot" in hand (or when being saved at home) and that it is easier for other household members to access the money when saved at home rather than in a bank account (Ashraf, 2009).

Under this hypothesis, receiving a card should cause consumption to fall relatively more in categories where temptation is the greatest. We test and find suggestive evidence for this in Table 6, which shows the effect of debit cards on the proportion of income spent on various consumption categories (estimated using (2) with the proportion of income spent on a category as the dependent variable). To assess the relative change in consumption in each category, column 3 shows the point estimates divided by the control group's proportion of income spent on that category. Indeed, we find a more negative point estimate (–14%) for the change in consumption of temptation goods than for other consumption.[24] However, households also reduce consumption in other categories: consumption of other food and drink and of other non-durable goods (clothing, personal care,

the period before receiving cards (when, in any event, beneficiaries were not accumulating savings in their accounts). Since the change in the flow of savings in pesos is calculated for a four-month period, we convert monthly income from the survey to four-month income before dividing.

[24] Temptation goods are defined based on Banerjee and Mullainathan (2010), and include alcohol, tobacco, sugar, soda, sweets, junk food, and fats.

household cleaning items, and fuel) change by –10% each. Although we cannot reject that the coefficient on temptation goods equals those of other food and drinks or other non-durable goods, we *can reject* that it equals the positive but not statistically significant coefficient on education and health spending.

We also use the survey to test whether the increase in formal savings observed in the administrative bank account data crowds out a particular form of informal saving: investment in durable assets. We test whether beneficiary households are disinvesting or investing less in assets by constructing an asset index. We find that the difference-in-differences coefficients on this measure are small, positive, and statistically insignificant (Table 5). Together with the results on education and health spending, this suggests that beneficiaries are not increasing savings by substituting from investments in human or physical capital, but rather by decreasing their consumption of non-durable goods. We also note that our time horizon might be too short to see potentially positive effects of saving on future investments.

Despite the prevalence of ATMs and POS terminals—which make it easy to spend money saved in a bank account—the debit card likely increases savings for two reasons. First, saving in the bank account prior to having a debit card involved high transaction costs and beneficiaries had low trust in the bank, both of which prevented formal savings. Second, intra-household bargaining issues could make saving informally at home difficult when household members have different preferences (as in Anderson and Baland, 2002; Schaner, 2015). In other words, it may be difficult for the women receiving transfers to save at home due to a lack of control over their partners' access to the savings. To test this hypothesis, we construct an index of bargaining power at baseline for households that have at least one male adult present in addition to the female cash transfer beneficiary.[25] Table B.6 shows suggestive evidence that the effect of debit cards on consumption (and hence on savings) is concentrated among households where the woman has below-median baseline bargaining power. While the coefficients on the interaction term are quantitatively large, they are only marginally significant in some specifications and insignificant in others; hence, these results are speculative.

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 are at work in causing the increased

[25]The bargaining power index is based on five questions about whether important household decisions are made by the woman, the man, or jointly. Details are in the notes to Table B.6.

active use of the accounts and the large increase in savings. We also explore other potential mechanisms such as learning the ATM technology.

7.1 Transaction Costs to Access Account

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 6a). 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 83% of beneficiaries—and then is fairly stable in subsequent periods (Figure 4). We also observe that among beneficiaries who were not saving prior to receiving a debit card, 16% begin saving immediately after receiving the card, likely due to the change in transaction costs (Figure 7a).

While we observe an immediate increase and then flat time profile of the share of beneficiaries who withdraw their benefits at ATMs (Figure 4) and the number of withdrawals per period (Figure 6a), the share of beneficiaries who start saving in their Bansefi accounts increases only gradually over time after receiving cards (Figure 7a). This gradual increase could be partially explained by the transaction costs of accessing the account or by the gradual increase in retailer adoption of POS terminals documented in Higgins (2019). Because these likely only explain part of the gradual increase in the proportion of beneficiaries who save—for example, for the POS terminal expansion, after two years with the card 87% of beneficiaries save in the account but only 44% use the card to make POS transactions—we investigate other mechanisms beyond transaction costs to access money in the account. In particular, in the remainder of this subsection we test if other types of transaction costs are changing over time; in the next subsection, we investigate beneficiaries' use of the debit card to monitor their account and build trust in the bank over time.

First, we test and rule out that banks disproportionately expanded complementary infrastructure (e.g., the 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 quarterly data on the number of ATMs and bank branches by municipality 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,

$$y_{mt} = \lambda_m + \delta_t + \sum_{k=-6}^{6} \beta_k D_{m,t+k} + \varepsilon_{mt}, \tag{5}$$

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 by banks 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.7).

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.4 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 rule out 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 (Table B.8).

7.2 Monitoring Costs and Trust

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. Although a beneficiary could check her balance at Bansefi branches, by asking a bank teller, 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 (Bachas et al., 2018).

Under this hypothesis, each additional balance 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. Below, we show that balance checks fall over time in both administrative and survey data and we use survey data to test whether self-reported trust in the bank increases over time with the card.

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. On average (pooling data across periods after beneficiaries receive cards), beneficiaries check their balances 1.7 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:

$$Balance\ Checks_{it} = \lambda_i + \sum_{k=0}^{\bar{k}_i - 1} \pi_k D_{it}^k + \varepsilon_{it}. \tag{6}$$

The π_k coefficients estimate 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.[26]

[26] $\bar{k}_i$ denotes the last period with the card for account i in our data, which varies depending on when i received a card. We do not include time fixed effects since we can only include treated beneficiaries after treatment in the regression, and the within-account trend in balance checks over time (among this group) is precisely the variation we are exploiting. Standard errors are clustered at the locality level.

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 (also shown in Table 4 column 3). For example, in the period after receiving the card, beneficiaries make an average of 0.9 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 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.[27]

Although beneficiaries were given calendars with exact transfer dates and should know the dates on which transfers are deposited (see Figure C.3), we additionally use two more restrictive definitions of a balance check, to ensure that a balance check constitutes bank monitoring and not just checking that the Oportunidades deposit arrived. The first alternative definition excludes all balance checks that occurred *prior* to the transfer being deposited that bimester, since these checks might be to see if the transfer has arrived. It 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 made a subsequent withdrawal*. Because the next transfer would not arrive until the following bimester and the beneficiary has already made a withdrawal after the transfer arrived 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.[28] Figures 8b and 8c plot the results with these two alternative definitions and show a very similar decrease in balance checks over time (also shown in Table 4 columns 4–5).

A separate possibility is that beneficiaries are using balance checks to ensure that they have money in their account before making a transaction at a POS terminal. Bansefi does not charge overdraft fees; if a beneficiary attempts to make a purchase at a POS terminal but does not have

[27]For these statistics, because we do not have initial January 2007 balance (and hence do not know the precise balance at any point in time), 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.

[28]Figure B.5 illustrates the four definitions of balance checks that we use.

enough money in the account, the transaction is denied and an "insufficient funds" message is sent to the POS terminal. While the beneficiary would therefore not face a monetary penalty for attempting to make a debit card purchase with insufficient funds, there might be a social penalty: the beneficiary would prefer to avoid having their transaction denied when attempting to make a purchase. Indeed, some balance checks do appear to be made to ensure money is in the account prior to making a transaction, as the number of balance checks increases in the 7 days preceding or day of a POS transaction (Figure B.6). However, excluding checks made on the same day as an ATM withdrawal, only 20% of balance checks are in the week preceding a POS transaction. Furthermore, Figure 8d shows that the magnitude of the decrease in balance checks over time is similar when using an alternative definition of balance checks that excludes checks made in the week preceding a POS transaction (also shown in Table 4 column 6).

We validate the above results using survey data from the Payment Methods Survey. Specifically, we estimate (4) using the self-reported number of balance checks without withdrawing cash over the past four-month period as the dependent variable. Table B.8 shows that those who have had the card for more than the median time (12 months) report making 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 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 (4) 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 (4) 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 for the Trust Survey sample support this assumption (Table 3a), as does the finding that conditional on being included in the debit card rollout, the timing of when cards were distributed to the locality is uncorrelated with observables (Table 2 and Figure 3). Table 7 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.[29] For comparison, Table 7 also shows results for two alternative forms of learning discussed in Sections 7.3.1 and 7.3.2: 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.

Finally, although we cannot causally relate decreased balance checks with higher account balances, we show in Figure B.7 that, within account, the number of balance checks and savings balances are strongly negatively correlated: in a period when beneficiaries make one balance check they save 300 pesos less than in a period where they make no balance checks, and when making 3 or more balance checks they save 400–500 pesos less.[30] This is consistent with the mechanism that monitoring balances leads to increased trust which over time leads to increased savings.

7.3 Learning

The time delay for many beneficiaries between getting the card and saving suggests some type of learning. Monitoring the bank and building trust is one type of learning; in this section we explore whether other types of learning occur and do not find evidence of these other types of learning.

7.3.1 Learning the Technology

Learning how to use the technology 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

[29] 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.

[30] This trend is robust to the definition of balance check; the numbers we cite here use the two most restrictive definitions of balance checks from panels c and d. Interestingly, using data from an online financial platform in Iceland, Olafsson and Pagel (2017) find the opposite correlation: when balances are high people log into their account more often. The paper then presents a model of anticipatory utility which can explain these findings.

in (4). Table B.8 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.[31] Moreover, there is no difference between those who have had the card for less vs. more than the median time (Table 7).

7.3.2 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 (4) 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 (Table 7).

7.3.3 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. 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.

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

[31] 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]."

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 increase their stock of savings by 2% 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.

Both low transaction costs to access savings and trust in banks appear to be necessary but not (individually) sufficient conditions to save in formal financial institutions. Thus, high indirect transaction costs and low trust could potentially explain why a number of studies offering savings accounts with no fees or minimum balance requirements have found low take-up and, even among adopters, low use of the accounts. Today, over 100 million poor households receive government cash transfers worldwide, and a growing share are getting their transfers through automatic deposits. In urban areas, these deposits can be withdrawn at ATMs from an expansive ATM infrastructure. Our study suggests that for these populations, the reduction in transaction costs of accessing money and monitoring the bank achieved through debit cards promises to increase financial inclusion and enable the poor to save. We acknowledge that relative to contexts without direct deposits of income into a bank account, with less ATM infrastructure, or with less willingness of retailers to adopt POS terminals, our measured effect of debit cards on savings may be an upper bound.

While our limited time frame prevents us from directly assessing the welfare implications of this policy, a growing literature suggests that enabling the poor to save in formal financial institutions leads to increased welfare through greater investment, wealth accumulation, 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 accumulate 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. In terms of mechanism, our results suggest that cash saved at home was easily spent, potentially due to intra-household bargaining issues. Indeed, after receiving the card beneficiaries strongly reduce their spending on temptation goods, and the reduction in overall consumption is largest in households with low baseline bargaining power for women. Finally, 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: of beneficiaries with the card for at least 14 months (the median time), 75% indicate that receiving payment by debit card it is better than before, and 13% that is the same as before.

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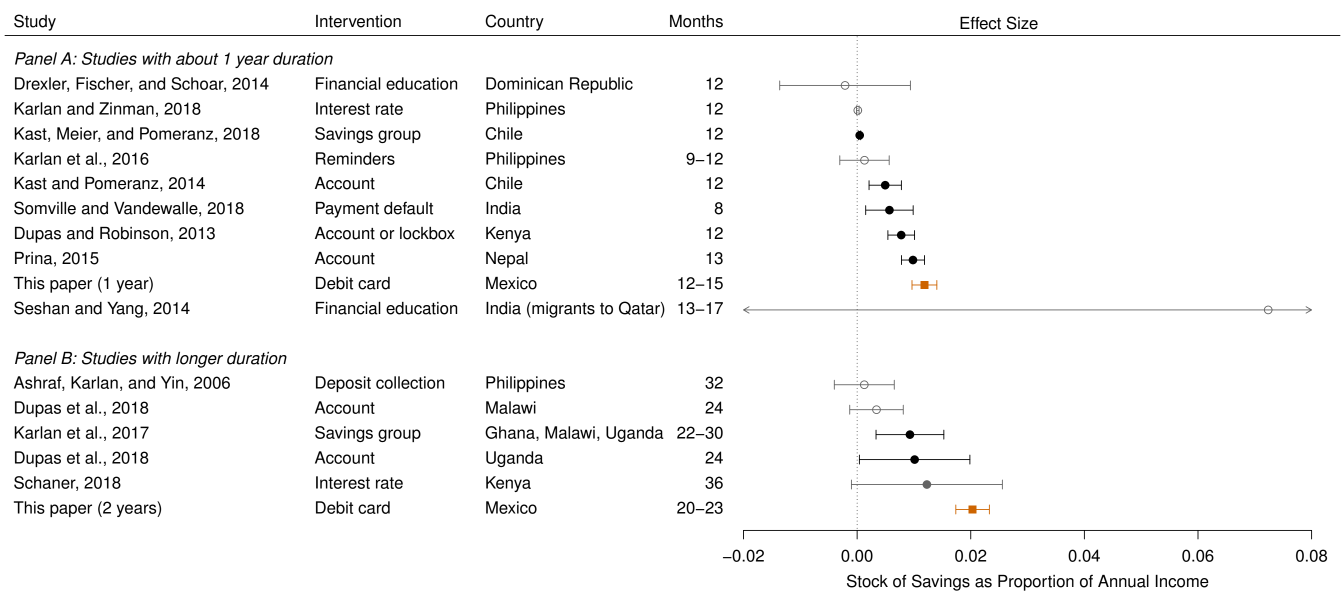
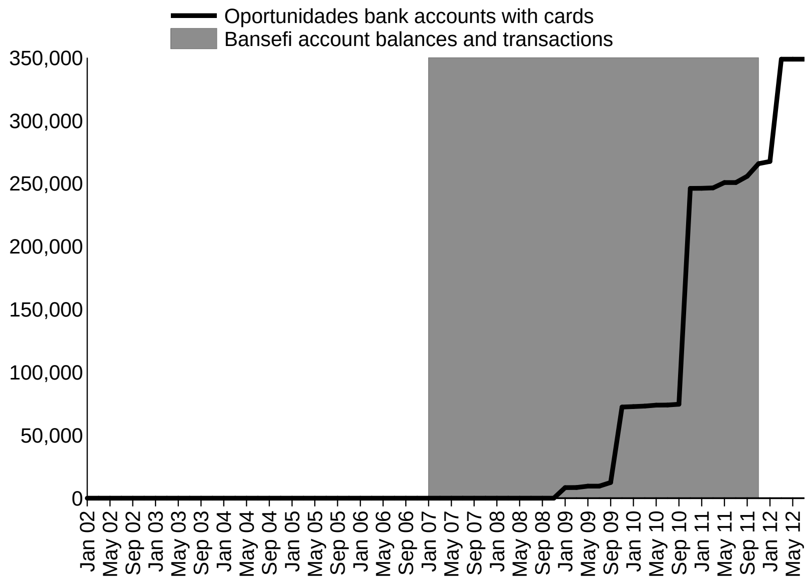
Figure 1: Comparison with Other Studies



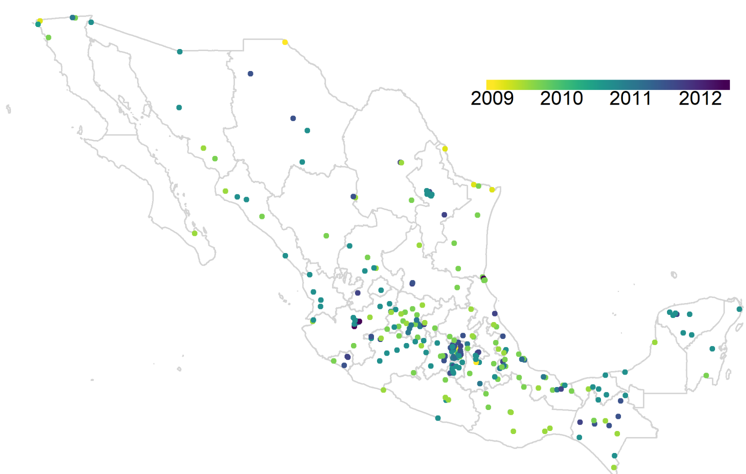
Notes: This figure compares the results from our study after 1 and 2 years with a debit card (orange squares) to other studies of savings interventions, and shows that we find larger effects than most studies with a comparable duration. Panel (a) shows studies with about a 1 year duration and panel (b) 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: Debit Card Rollout over Time and Space

(a) Timing of Rollout and Administrative Data



(b) Geographic Coverage

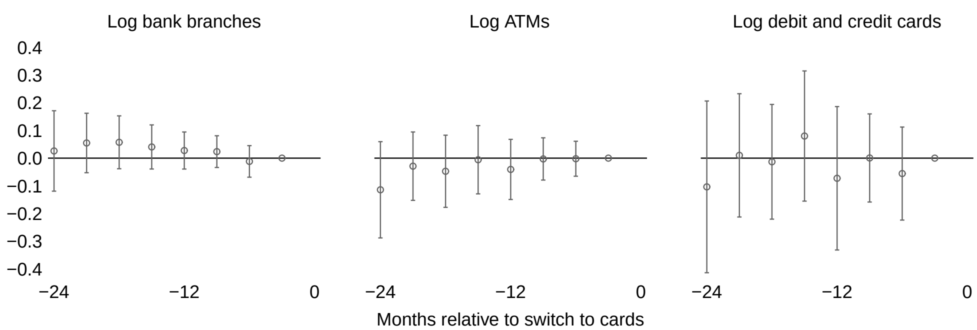


Notes: This figure shows the number of Oportunidades bank accounts with debit cards over time (using administrative data from Bansefi) and across space (using administrative data from Oportunidades). This was determined by the staggered rollout of debit cards, which generated variation across space and 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) shows the rollout across space.

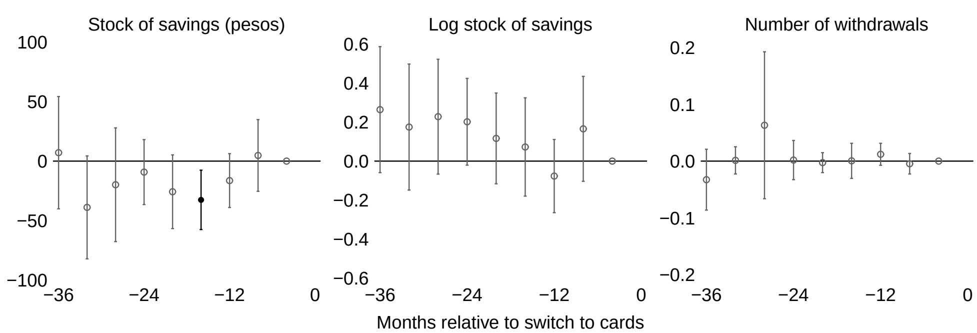
Figure 3: Parallel Pre-trends

(a) Microdata from INEGI and Central Bank

(b) Municipality-level data from CNBV

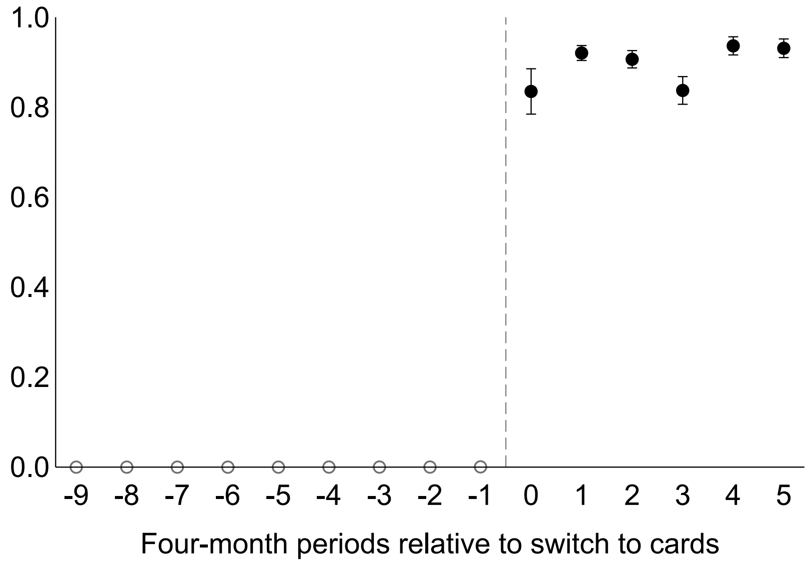


(c) Microdata from Bansefi



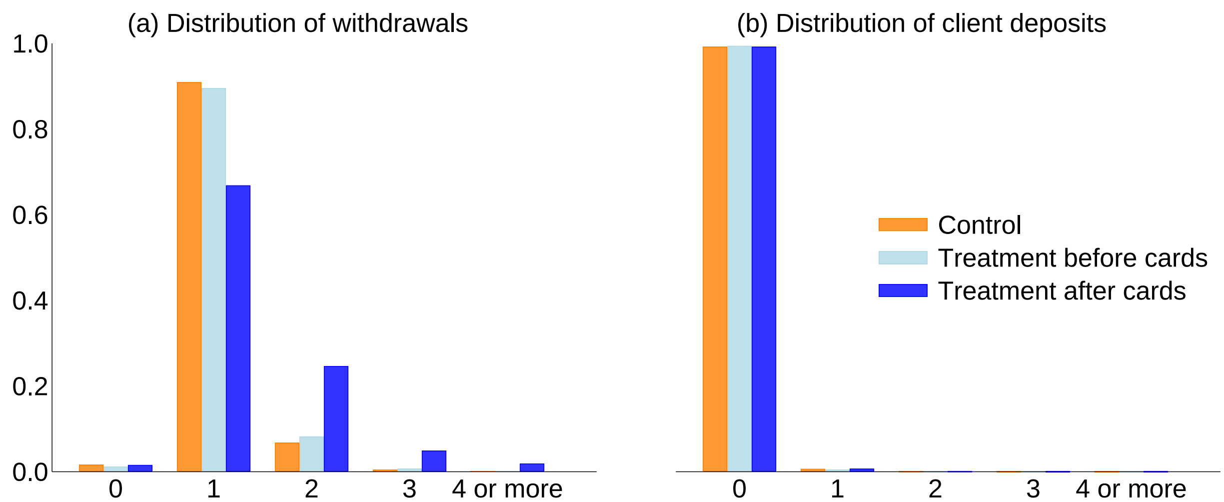
Notes: This figure shows parallel pre-trends in variables from (a) microdata on wages from INEGI's ENOE labor force survey, food prices from 4 million product by store by month price quotes from INEGI, and point-of-sale terminal adoptions from Mexico's Central Bank; (b) municipality-level data on financial variables from CNBV; and (c) microdata from beneficiaries' bank accounts. Point estimates are ϕ_k for $k < 0$ from (1), where $k = -1$ is the omitted period. In the wage regression, i in (1) is a worker; in the food price regression, i is a product by store; in the POS terminals regressions the data are aggregated to the postal code level and i is a postal code; in panel (b) the data are aggregated to the municipality level and i is a municipality; in panel (c) i is a Bansefi account. The frequency of ϕ_k coefficients depends on the frequency of each data set. Panel (b) only includes 24 rather than 36 months of pre-trends because the CNBV data begin in the last quarter of 2008, so the sample of localities with more than 24 months pre-treatment is very small. Standard errors are clustered at the locality level in panels (a) and (c), and at the municipality level—since the data are only available by municipality, which is slightly larger than locality—in panel (b). Black circles indicate results that are significant at the 5% level, and hollow circles statistically insignificant from 0.

Figure 4: Share of Clients Using Debit Cards to Withdraw at ATMs



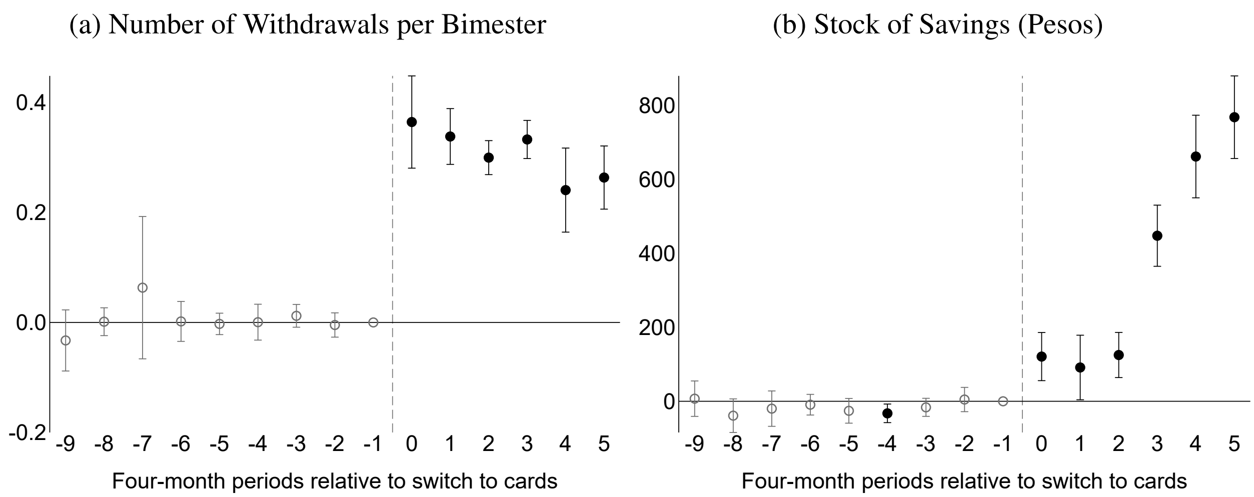
Notes: This figure shows the share of clients using their debit card for at least one withdrawal during a four month period. It shows that beneficiaries immediately adopt the new technology and use their cards to withdraw their transfers, instead of going to the Bansefi bank branch. Note that in periods before the card the share of clients using debit cards to withdraw at ATMs is necessarily zero. $N = 2{,}799{,}372$ account-period observations from 255,781 *treated* beneficiaries. Asymptotic standard errors clustered at the locality level are included in parentheses. Whiskers denote 95% confidence intervals. Dashed vertical line indicates timing of debit card receipt.

Figure 5: Distribution of Withdrawals and Client Deposits per Bimester



Notes: This figure 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. It shows the distribution of withdrawals per bimester in panel (a) and of client deposits (i.e., excluding Oportunidades deposits) per bimester 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. Based on $N = 35{,}236{,}129$ transactions from 348,802 beneficiaries over 5 years.

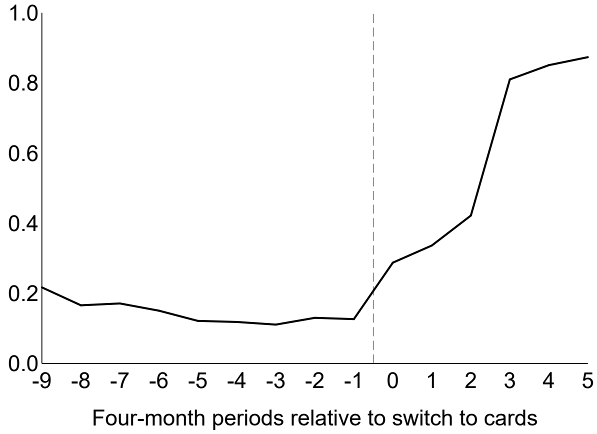
Figure 6: Effect of Debit Card on Withdrawals and Savings



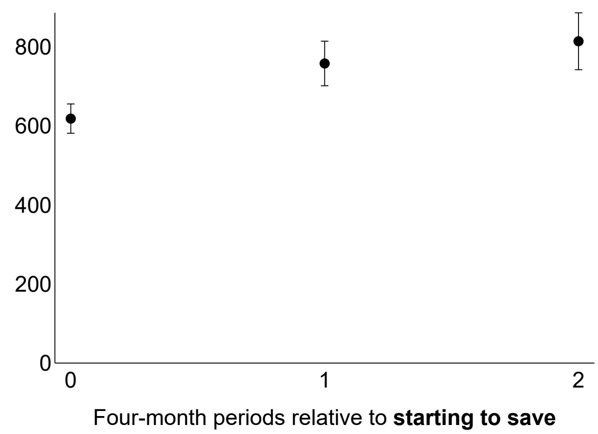
Notes: Panel (a) shows the effect of the debit card on the number of withdrawals per bimester. The figure plots the ϕ_k coefficients from equation (1), where the dependent variable is number of withdrawals. $N = 4{,}740{,}331$ account-period observations from 348,802 beneficiaries. Panel (b) shows the effect of debit cards on the stock of savings and the proportion who save. Dashed vertical lines indicate timing of debit card receipt. The figure 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. Asymptotic standard errors clustered at the locality level are included in parentheses. Whiskers denote 95% confidence intervals. Black circles indicate results that are significant at the 5% level, and hollow circles statistically insignificant from 0.

Figure 7: Decomposition of Savings Effect

(a) Proportion Who Save

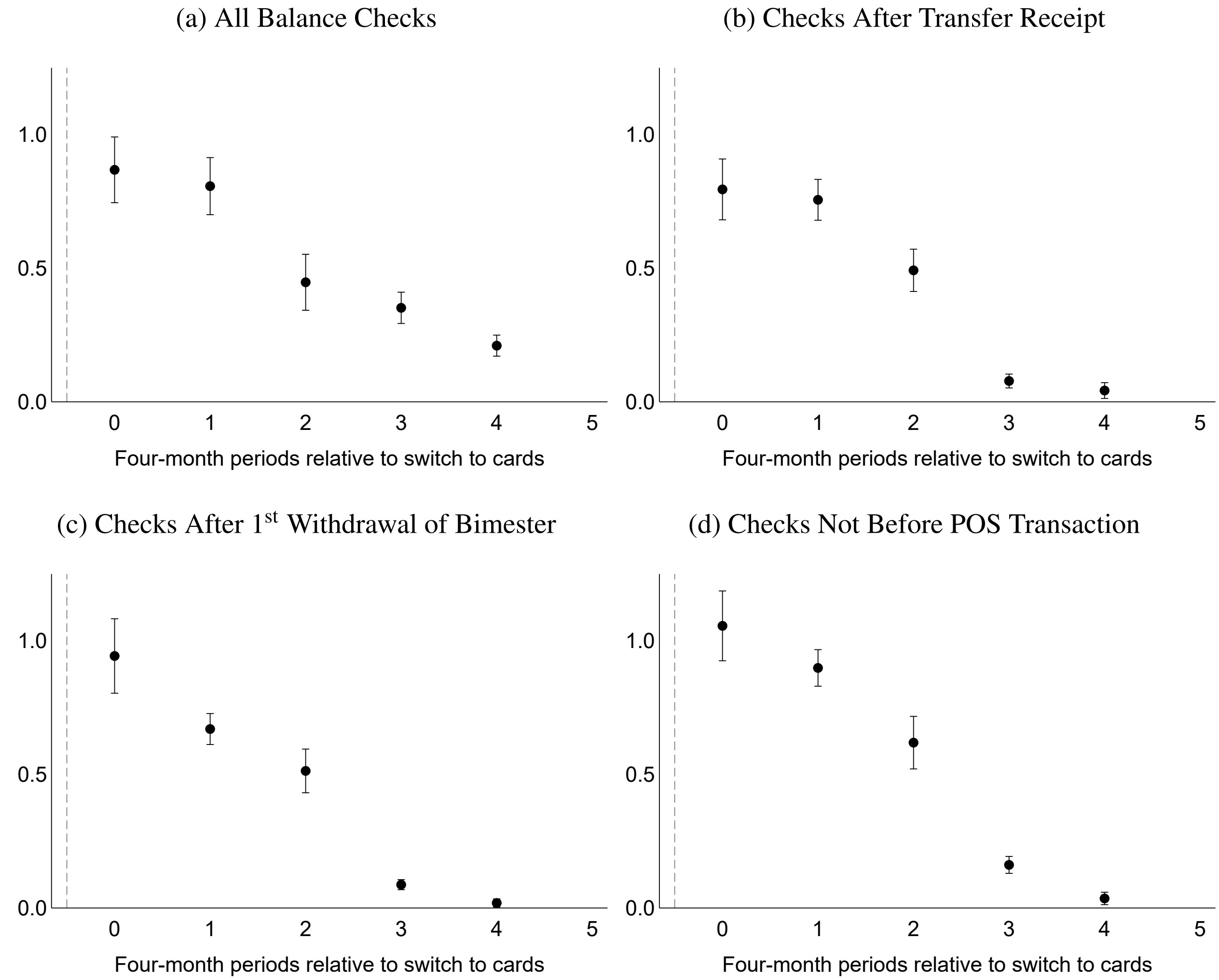


(b) Stock of Savings Conditional on Saving (Pesos)



Notes: This figure decomposes the effect of the debit card on the stock of savings into the extensive margin effect on the proportion who save over time, and the intensive margin effect on the stock of savings conditional on saving. Panel (a) shows the proportion of treated beneficiaries who save in each period relative to when they receive a debit card. $N = 2{,}968{,}628$ account-period observations for 255,784 *treated* beneficiaries. Panel (b) plots ϕ_k from (1) with the event time dummies redefined relative to when an individual starts saving in the account, and we impose a zero pre-treatment trend by setting $a = 0$ (for reasons explained in Section 4.1). $N = 4{,}668{,}575$ account-period observations from 348,802 beneficiaries. Asymptotic standard errors clustered at the locality level are included in parentheses. Whiskers denote 95% confidence intervals. Black circles indicate results that are significant at the 5% level.

Figure 8: Number of Balance Checks Over Time



Notes: This figure shows the number of balance checks over time after receiving the card. Panels (a)-(d) use the administrative transactions data and show the number of balance checks relative to the last period in the data for each observation by plotting the π_k coefficients from (6). 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. Panel (a) includes all balance checks, while panels (b)-(d) correspond 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 (b) shows balance checks after the transfer was received and on a different day than a withdrawal, panel (c) balance checks after the first withdrawal occurred in the bimester and on a different day than a withdrawal, and panel (d) balance checks not within the 7 days before or day of a POS transaction (see Section 7.2.1). $N = 873{,}848$ account-period observations from 233,080 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. Black circles indicate results that are significant at the 5% level, and hollow circles statistically insignificant from 0. In all panels, standard errors are clustered at the locality level and whiskers denote 95% confidence intervals.

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 2007–Oct 2011	Balances, transactions, balance checks	Generalized difference-in-differences (event study with control) using phased geographic rollout
(2) Household Panel Survey from Oportunidades (ENCELURB)	2,868	Panel (four waves): 2002, 2003, 2004, and Nov 2009–Feb 2010	Consumption, income, assets	Difference-in-differences: received card in 2009 versus received card later
(3) Trust Survey from Oportunidades (ENCASDU)	1,694	Cross-section: Oct–Nov 2010	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 2012	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: Summary Statistics and Discrete Time Hazard of Locality Characteristics

	(1)	(2)	(3)	(4)
			Discrete Time Hazard	
Variable	Mean	Standard Deviation	Linear Probability	Proportional Hazard
Log point-of-sale terminals	4.47	2.11	0.0002	0.0043
			(0.0095)	(0.0842)
Δ Log point-of-sale terminals	0.81	0.38	−0.0260	−0.2360
			(0.0185)	(0.1601)
Log bank accounts	9.27	3.27	0.0061	0.0537
			(0.0052)	(0.0435)
Δ Log bank accounts	1.78	3.61	0.0049	0.0495
			(0.0065)	(0.0558)
Log commercial bank branches	2.58	1.42	−0.0225	−0.2160
			(0.0187)	(0.1508)
Δ Log commercial bank branches	0.61	0.95	−0.0215	−0.2267
			(0.0240)	(0.2178)
Log Bansefi bank branches	0.58	0.41	0.0033	0.0420
			(0.0241)	(0.2001)
Log commercial bank ATMs	3.15	1.74	0.0130	0.1203
			(0.0103)	(0.0997)
Log population	11.26	1.24	0.0117	0.1072
			(0.0159)	(0.1317)
Mayor = PAN	19.58	39.77	−0.0003	−0.0027
			(0.0003)	(0.0023)
Δ Mayor = PAN	−12.08	57.67	0.0002	0.0021
			(0.0002)	(0.0016)
% illiterate (age 15+)	6.14	3.69	0.0004	0.0049
			(0.0048)	(0.0417)
% not attending school (age 6-14)	4.15	1.65	0.0003	0.0063
			(0.0094)	(0.0848)
% without primary education (age 15+)	40.98	9.59	0.0018	0.0145
			(0.0019)	(0.0169)
% without health insurance	45.68	16.15	−0.0011	−0.0099
			(0.0008)	(0.0066)
% with dirt floor	5.28	4.83	0.0051**	0.0513**
			(0.0024)	(0.0209)
% without toilet	5.89	3.60	−0.0063	−0.0526
			(0.0040)	(0.0335)
% without water	6.45	9.12	−0.0007	−0.0058
			(0.0010)	(0.0094)
% without plumbing	3.94	6.39	0.0021	0.0180
			(0.0015)	(0.0122)
% without electricity	4.29	2.24	0.0052	0.0430
			(0.0048)	(0.0394)
% without washing machine	33.64	14.33	−0.0006	−0.0071
			(0.0010)	(0.0098)
% without refrigerator	16.80	9.73	0.0010	0.0068
			(0.0017)	(0.0153)

Notes: Columns 1 and 2 show summary statistics of locality-level financial infrastructure, trends in financial infrastructure, and other locality characteristics. Columns 3 and 4 test whether these characteristics predict the timing of when localities receive debit cards as part of the debit card rollout, using a discrete time hazard model. Column 3 shows results from a linear probability discrete time hazard model. Column 4 shows results from a discrete proportional hazard using a complementary log-log regression. Both models also include a 5th-order polynomial in time as in Galiani, Gertler and Schargrodsky (2005); time is measured in two-month periods. The dependent variable in the discrete time hazard model is a dummy variable indicating if locality j has been treated at time t. A locality treated in period t drops out of the sample in period $t+1$ since it is a hazard model. All variables are measured prior to the debit card rollout. The financial variables come from various sources, and are each measured in the last day or quarter of 2008 (just prior to the debit card rollout); pre-rollout trends (variables with a Δ) compare the last day or quarter of 2006 to the last day or quarter of 2008. The number of point-of-sale (POS) terminals is from Mexico's Central Bank and includes POS terminals from all merchant categories; checking accounts, commercial bank branches, and commercial bank ATMs are from CNBV; Bansefi bank branches are from a data set of Bansefi branch geocoordinates. We do not include trends in Bansefi bank branches or commercial bank ATMs because these variables are first available in 2008. The non-financial locality characteristics include all characteristics that are used to measure locality-level development by Mexico's national statistical institute (INEGI) and its National Council for the Evaluation of Social Development (CONEVAL), and come from publicly available locality-level totals from the 2005 Population Census published by INEGI. $N = 240$ localities in the debit card rollout, and 1851 locality by two-month-period observations in columns 3 and 4.

Table 3: Balance and Parallel Trends in Survey Data

Panel (a): Cross-Sectional Data	Trust Survey (1)	(2)	(3)	Payment Methods Survey (4)	(5)	(6)
	α (Mean for card < median time)	γ (Difference card ≥ median time)	P-value of difference	α (Mean for card < median time)	γ (Difference card ≥ median time)	P-value of difference
# Household members	5.44	–0.26	0.11	4.74	0.04	0.77
	(0.12)	(0.15)	[0.16]	(0.13)	(0.13)	[0.78]
Age	45.53	−1.04	0.20	39.03	1.18	0.15
	(0.83)	(0.78)	[0.30]	(0.86)	(0.80)	[0.14]
Male	0.01	0.00	0.95	0.02	0.00	0.87
	(0.00)	(0.01)	[0.81]	(0.01)	(0.01)	[0.88]
Married	0.72	–0.03	0.33	0.72	0.01	0.87
	(0.02)	(0.03)	[0.42]	(0.03)	(0.03)	[0.87]
Education level	5.84	0.33	0.19	6.04	–0.03	0.91
	(0.16)	(0.24)	[0.27]	(0.28)	(0.29)	[0.91]
# Children	2.22	–0.03	0.74			
	(0.09)	(0.10)	[0.76]			
Occupants per room	3.48	0.03	0.80			
	(0.08)	(0.11)	[0.82]			
Health insurance	0.63	–0.05	0.16			
	(0.03)	(0.03)	[0.26]			
Asset index	0.01	0.05	0.53			
	(0.08)	(0.08)	[0.58]			
Income	3443.60	–218.20	0.16			
	(136.42)	(149.03)	[0.23]			

Panel (b): Panel Data	Household Panel Survey (1)	(2)	(3)	(4)
	Control Mean	ω_k (Placebo DD) 2003	2004	Parallel p-value
Consumption	2731.20	2.04	9.37	0.95
	(82.83)	(81.01)	(85.17)	[0.97]
Income	3148.28	265.96	275.18	0.47
	(89.06)	(219.09)	(224.06)	[0.54]
Asset index	0.47	–0.03	–0.02	0.67
	(0.10)	(0.05)	(0.05)	[0.71]

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 (4): 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 p-values for a test of $\gamma = 0$ (asymptotic cluster-robust without brackets and randomization inference randomization-t p-values based on 2000 draws in brackets). 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 (3); we additionally include household baseline characteristic by time fixed effects to increase power (which works against finding parallel trends), as in our preferred specification in Table 5. 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 $\omega_{2003} = \omega_{2004} = 0$. $N =$ 7,754 household-period observations from 2,200 households in the Household Panel Survey as in column 4 of Table 5. Asymptotic standard errors clustered at the locality level are included in parentheses. P-values based on asymptotic cluster-robust standard errors are included without brackets and randomization inference p-values based on 2000 draws are in square brackets.

Table 4: Effect of Debit Cards from Administrative Data

	Number of withdrawals	Stock of savings	Number of balance checks			
Period	(1)	(2)	(3)	(4)	(5)	(6)
–9	–0.03	7.08				
	(0.03)	(24.32)				
	[0.28]	[0.79]				
–8	0.00	–38.87*				
	(0.01)	(23.06)				
	[0.93]	[0.13]				
–7	0.06	–19.86				
	(0.07)	(24.31)				
	[0.52]	[0.46]				
–6	0.00	–9.24				
	(0.02)	(14.18)				
	[0.93]	[0.56]				
–5	0.00	–25.74				
	(0.01)	(16.97)				
	[0.83]	[0.14]				
–4	0.00	–32.58**				
	(0.02)	(12.76)				
	[0.99]	[0.01]				
–3	0.01	–16.37				
	(0.01)	(12.40)				
	[0.33]	[0.21]				
–2	0.00	4.74				
	(0.01)	(16.67)				
	[0.73]	[0.78]				
–1						
0	0.36***	120.85***	0.87***	0.79***	0.94***	1.06***
	(0.04)	(33.05)	(0.06)	(0.06)	(0.07)	(0.07)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
1	0.34***	91.37**	0.81***	0.76***	0.67***	0.90***
	(0.03)	(44.29)	(0.05)	(0.04)	(0.03)	(0.03)
	[0.00]	[0.03]	[0.00]	[0.00]	[0.00]	[0.00]
2	0.30***	125.09***	0.45***	0.49***	0.51***	0.62***
	(0.02)	(30.99)	(0.05)	(0.04)	(0.04)	(0.05)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
3	0.33***	447.48***	0.35***	0.08***	0.09***	0.16***
	(0.02)	(41.98)	(0.03)	(0.01)	(0.01)	(0.02)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
4	0.24***	661.54***	0.21***	0.04***	0.02**	0.04***
	(0.04)	(56.68)	(0.02)	(0.01)	(0.01)	(0.01)
	[0.00]	[0.00]	[0.00]	[0.02]	[0.03]	[0.01]
5	0.26***	767.87***				
	(0.03)	(56.72)				
	[0.00]	[0.00]				
N observations	4,740,331	4,668,575	873,848	873,848	873,848	873,848
N accounts	348,802	348,802	233,080	233,080	233,080	233,080

This table shows the effect of debit cards on the key outcomes of interest. Columns 1–2 show the effect of the card on the number of withdrawals per bimester and the stock of savings for each four month period relative to the card shock, estimated using (1). These results are equivalent to those shown in Figure 6. Columns 3–6 show the effect of the card on the number of balance checks by card recipients relative the final period in the sample, estimated using (6); they correspond to the four definitions of balance checks used in the paper: all balance checks (column 3), balance checks after receiving the cash transfer (column 4), balance checks after the first withdrawal of the bimester (column 5) and balance checks not in the week prior to a POS transaction (column 6). These results are equivalent to those shown in Figure 8. Asymptotic standard errors clustered at the locality level are included in parentheses. Randomization inference p-values based on 2000 draws are included in square brackets. Stars are based on p-values from asymptotic cluster-robust standard errors; * indicates statistical significance at $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table 5: Effect of Debit Cards from Household Panel Survey

	(1)	(2)	(3)	(4)
Consumption	–175.36**	–150.51**	–136.52**	–155.11**
	(81.31)	(70.43)	(61.75)	(62.07)
	[–353.11 , –1.52]	[–306.24 , –2.30]	[–276.37 , –4.75]	[–288.02 , –33.10]
	[0.04]	[0.04]	[0.04]	[0.02]
Income	98.16	106.01	75.50	38.11
	(170.03)	(150.31)	(127.77)	(106.12)
	[–290.77 , 486.11]	[–230.64 , 468.97]	[–219.75 , 376.72]	[–175.00 , 251.64]
	[0.63]	[0.56]	[0.61]	[0.74]
Asset index	0.06	0.06	0.07	0.03
	(0.08)	(0.08)	(0.07)	(0.08)
	[–0.12 , 0.24]	[–0.12 , 0.24]	[–0.08 , 0.23]	[–0.20 , 0.24]
	[0.54]	[0.54]	[0.41]	[0.82]
P-value consumption vs. income	[0.047]	[0.041]	[0.056]	[0.057]
Number of observations	9,246	9,246	9,246	7,754
Number of households	2,868	2,868	2,868	2,200
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, and assets using the Household Panel Survey combined with administrative data from Oportunidades on the debit card rollout, estimated using (2). Each row label is the dependent variable from a separate regression; each column is a different specification. Means for each dependent variable can be found in Table 3b. 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), dwelling characteristics (dirt floors, no bathroom, no piped water, no sewage, and number of occupants per room), and pre-trends in the dependent variables (consumption, income, and asset index). The number of households in column (4) is lower because households have missing values for one of the household characteristics included, or are not included in enough pre-treatment waves to construct household-level pre-trends of the outcome variables, which are interacted with time fixed effects in that specification. Asymptotic cluster-robust standard errors (clustered at the locality level, using pre-treatment locality) are included in parentheses; wild cluster bootstrap percentile-t 95% confidence intervals based on 1000 draws are included in square brackets. Randomization inference p-values based on 2000 draws are included in square brackets. Stars are based on p-values from asymptotic cluster-robust standard errors; * indicates statistical significance at $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table 6: Effect of Debit Cards on Proportion of Income Spent by Category

	(1) Control Baseline Mean	(2) Effect on Proportion of Income	(3) Relative Change	(4) N	(5) Number of households
Temptation goods	0.082	–0.017***	–0.138***	7,077	2,066
	(0.003)	(0.006)	(0.047)		
		[0.011]	[0.011]		
Other food & drinks	0.557	–0.086**	–0.104**	7,077	2,066
	(0.017)	(0.034)	(0.042)		
		[0.065]	[0.065]		
Other non-durable goods	0.151	–0.023**	–0.096**	7,077	2,066
	(0.008)	(0.009)	(0.037)		
		[0.031]	[0.031]		
Education and health	0.070	0.008	0.095	7,077	2,066
	(0.003)	(0.006)	(0.069)		
		[0.285]	[0.285]		
Other services	0.001	–0.000	–0.073	7,077	2,066
	(0.000)	(0.000)	(0.139)		
		[0.656]	[0.656]		

Notes: This table shows the effect of the debit cards on consumption by category, estimated using (2) where the outcome variable is the proportion of income spent on each category. Column 1 shows the control group's mean proportion of income spent on each category at baseline to show the relative importance of the categories in total consumption. Column 2 shows the coefficients from (2). Column 3 divides those coefficients by the control group's mean proportion of income spent on that category of consumption to show the relative change in the proportion of income spent on each category. We include household baseline characteristic by time fixed effects to increase power, as in our preferred specification in Table 5 column 4. The number of households and observations is lower than in Table 5 column 4 due to missing values in particular consumption categories. Asymptotic standard errors clustered at the locality level are included in parentheses. Randomization inference p-values based on 2000 draws are included in square brackets. Stars are based on p-values from asymptotic cluster-robust standard errors; * indicates statistical significance at $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table 7: Self-Reported Reasons for Not Saving in Bansefi Account

	Mean card $<$ median time	Difference card $\geq$ median time			N
	(1)	(2)	(3)	(4)	(5)
Lack of trust	0.238	–0.078**	–0.073**	–0.073**	1,694
	(0.033)	(0.033)	(0.030)	(0.031)	
		[0.064]	[0.057]	[0.058]	
Lack of knowledge	0.014	0.008	0.011*	0.011	1,694
	(0.006)	(0.007)	(0.006)	(0.007)	
		[0.340]	[0.178]	[0.245]	
Fear of program ineligibility	0.030	–0.013	–0.013	–0.013	1,694
	(0.009)	(0.009)	(0.011)	(0.011)	
		[0.204]	[0.264]	[0.268]	
Household-level controls		No	Yes	Yes	
Locality/municipality-level controls		No	No	Yes	

Notes: This table 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, estimated using equation 4. 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. Column 1 shows the mean for those who have had the card for less than the median time (α) and columns 2–4 show the difference (γ). Column 2 does not include any additional controls. Column 3 controls for the household-level controls that would not be affected by treatment from Table 3 (number of household members; age, gender, marital status, and education level of the Prospera beneficiary). Column 4 controls for both household-level controls and locality- or municipality-level controls for the variables from Figure 3 (log wage, log food prices, log POS terminals, log bank branches, log ATMs, log debit and credit cards, average stock of savings, average log stock of savings, and average number of withdrawals). Asymptotic standard errors clustered at the locality level are included in parentheses. Randomization inference p-values based on 2000 draws are included in square brackets. Stars are based on p-values from asymptotic cluster-robust standard errors; * indicates statistical significance at $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Internet Appendix

Appendix A Comparison with Other Studies (Internet Appendix)

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. 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 after both 12 and 32 months. 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 (2018). 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.

Breza and Chandrasekhar (2019). This study looks at the effect of using savings monitors from individuals' social networks as a commitment device to increase savings in India. We exclude this study from the comparison because it does not include 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. (2019). 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.)

Cole, Sampson, and Zia (2011). This study looks at the effect of subsidies and financial literacy training on the opening and use of bank accounts in Indonesia. We exclude this study from the comparison because it uses opening of an account and a dummy variable for positive savings in the account as outcome variables, but does not look at the stock of savings.

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). 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. (2018). 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 (2018). 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 (2018). 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, 1871 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 (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 (2018). 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.

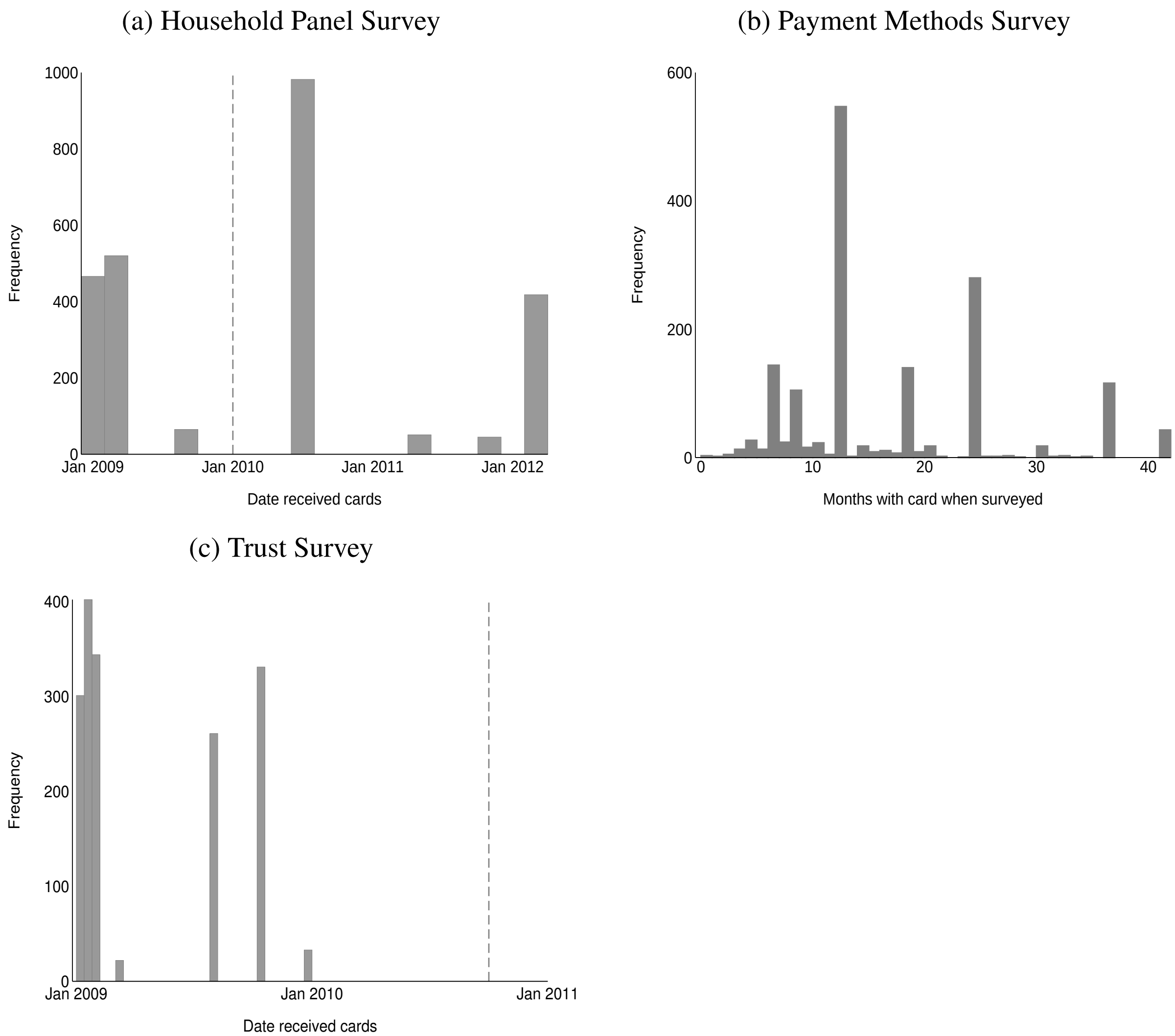
Seshan and Yang (2014). This study looks at the effects of inviting migrants from India working in Qatar to a motivational workshop that sought to promote better financial habits and joint decision-making with their spouses in India. The intent-to-treat effect on the level of savings comes from Table 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 (2018). 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 (Internet Appendix)

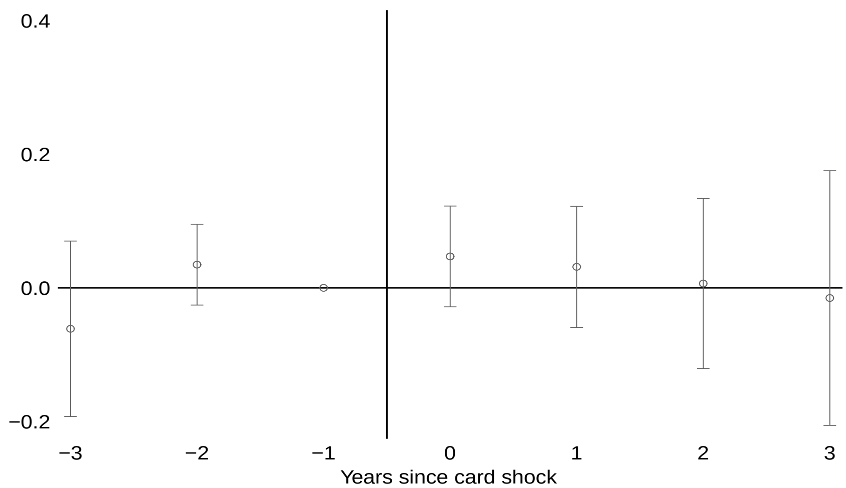
Figure B.1: Distribution of Timing of Card Receipt in Survey Data



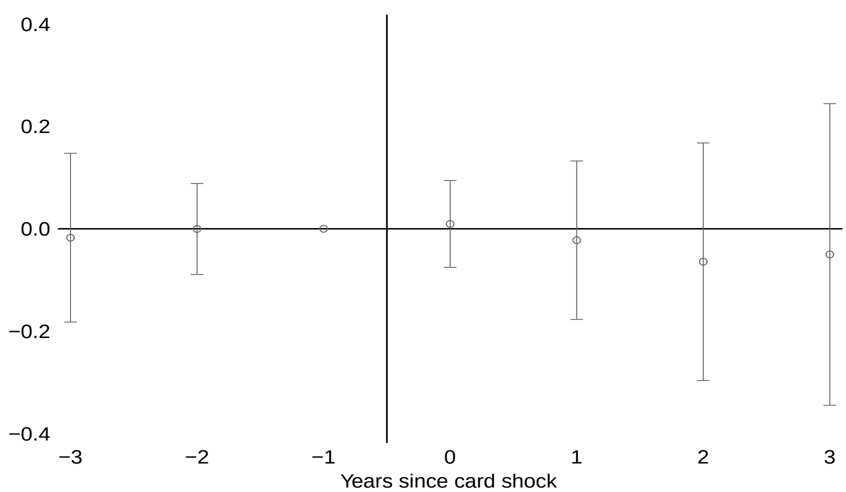
Notes: This figure shows the timing of reception of debit cards in the three surveys used in the paper compared to the time of each survey. Panel (a) shows when households in the Household Panel Survey received debit cards relative to the time of the survey, using survey data merged with administrative 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. Panel (b) 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. Panel (c) 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.2: Rollout Correlation with Number of Beneficiaries & Political Party

(a) Number of Beneficiaries

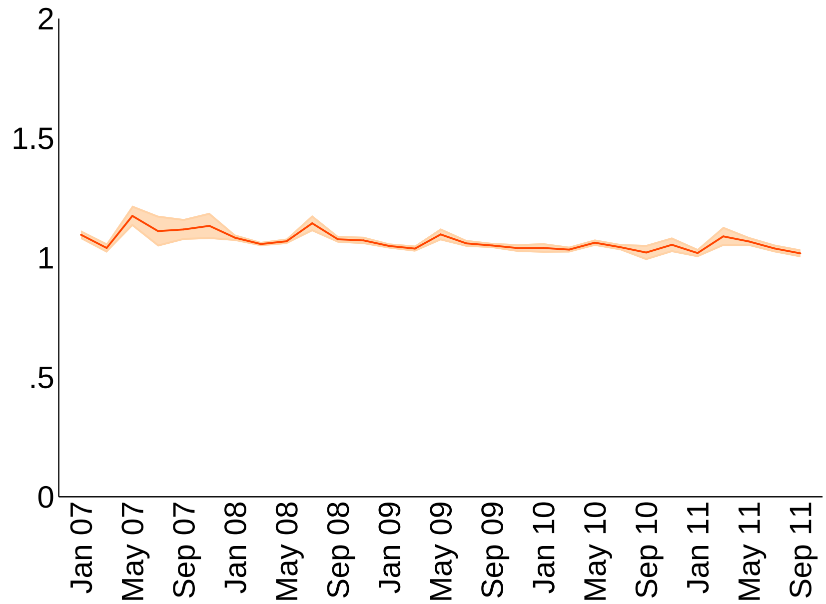


(b) Political Party in Power



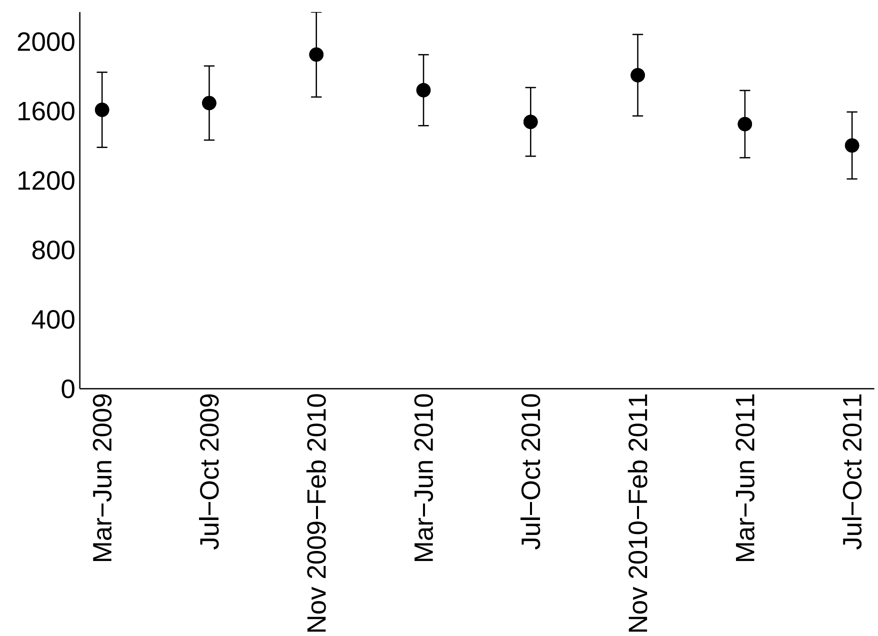
Notes: This figure shows that the rollout of debit cards is not correlated with changes in the number of beneficiaries nor with whether the party in power at the local level is the same party as the one in power nationally. Panel (a) shows the coefficients from (1), where the outcome is the log number of Prospera beneficiaries in locality i at the end of year t. The estimation uses administrative data from Prospera on the number of beneficiaries in each locality and the method by which they are paid. $N = 2590$ locality by year observations in 259 localities. Panel (b) shows the coefficients from (1), where the outcome is a dummy variable equal to one if municipal president is from the PAN, the party of the country's president during the card rollout, in municipality i during year t. The estimation uses data from municipal elections that we digitized. $N = 2805$ locality by year observations in 255 municipalities. Asymptotic standard errors clustered at the locality level are included in parentheses. Whiskers denote 95% confidence intervals. Hollow circles indicate results that are statistically insignificant from 0.

Figure B.3: Number of Withdrawals Over Calendar Time in the Control Group



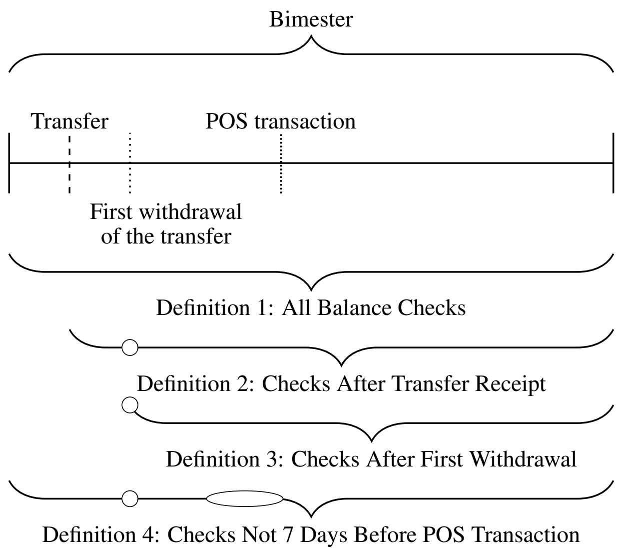
Notes: This figure shows the number of withdrawals in the control group per bimester using the administrative transaction data. Since the control did not receive cards during our study period, the x-axis is in calendar time rather than in time relative to switch to cards. The shaded area denotes the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

Figure B.4: Savings among Non-Oportunidades Debit Card Account Holders (Pesos)



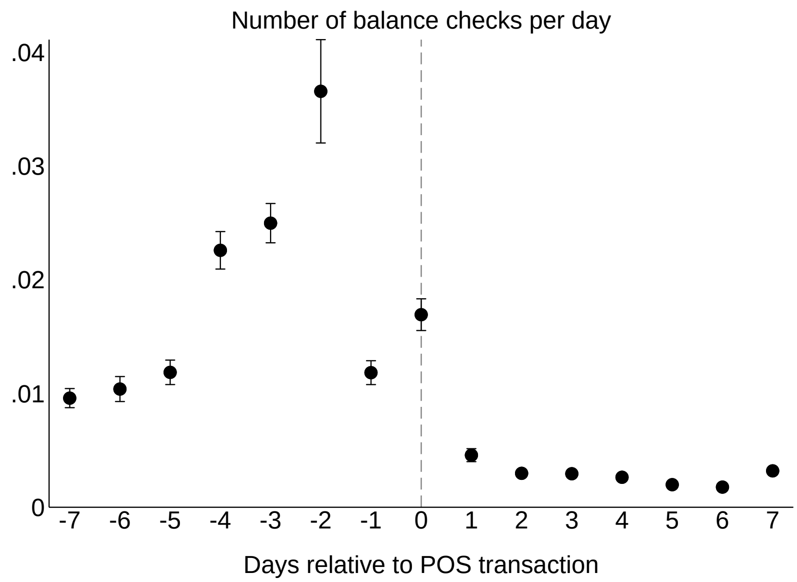
Notes: This figure shows mean savings per four-month period among non-Oportunidades beneficiaries with a debit card who opened accounts in 2007 (in pesos). Savings among non-Oportunidades debit card holders were not increasing over time during the period of our study, which suggests that our results are not driven by a decrease in transaction costs over time. Asymptotic standard errors clustered at the locality level are included in parentheses. Whiskers denote 95% confidence intervals. $N = 2721$ non-Oportunidades accounts opened at a sample of 117 Bansefi branches in the year 2007.

Figure B.5: Stylistic Illustration of Balance Check Definitions



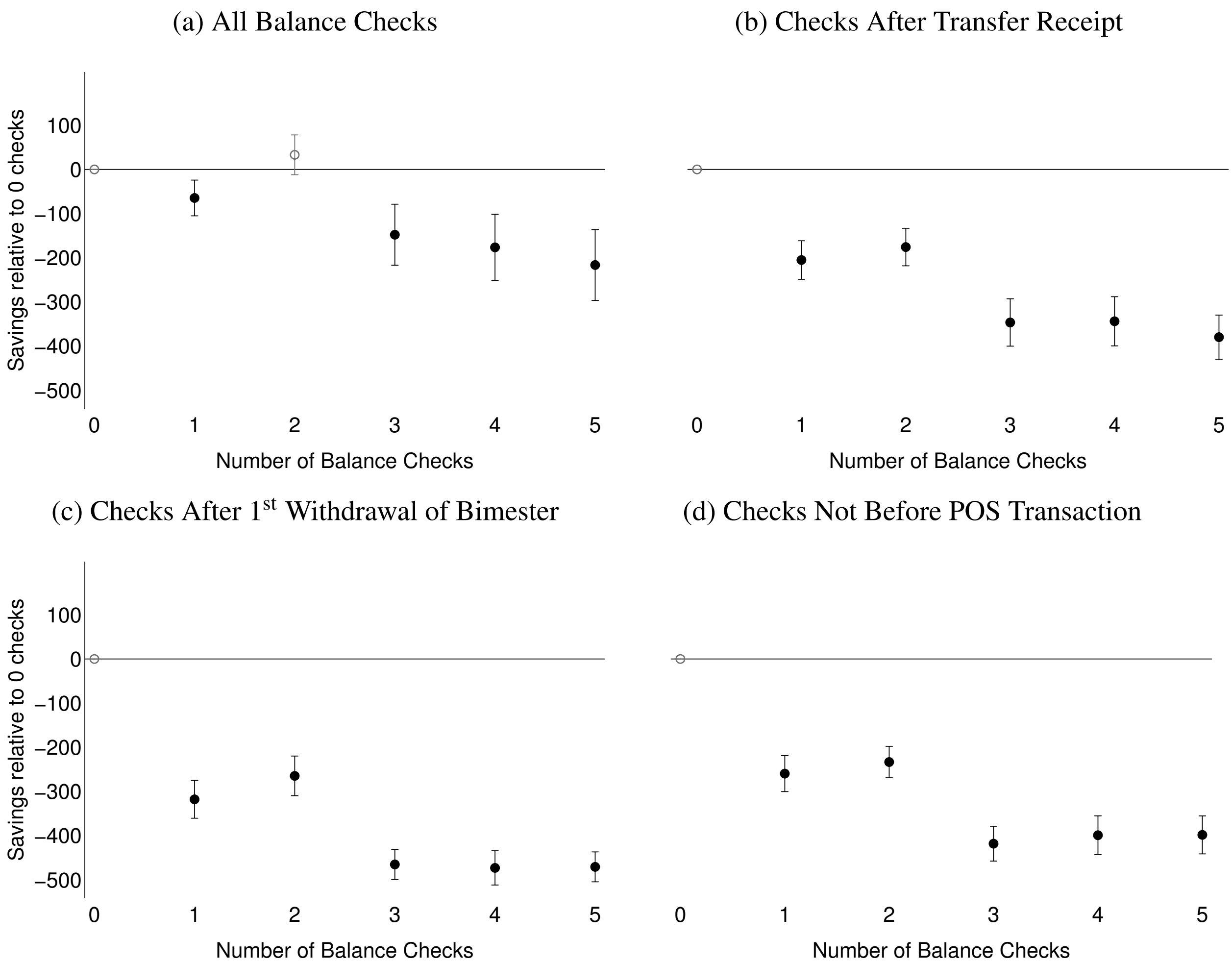
Notes: This figure illustrates the three definitions of balance checks that we use. For illustration we use the scenario where one ATM withdrawal is made during the bimester, and potentially one POS transaction later in the bimester. The first definition includes all balance checks in the bimester. The 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. The fourth definition excludes balance checks that occurred in the 7 days prior to or on the day of a POS transaction, represented by the hollow ellipse, as well as balance checks on the day of an ATM withdrawal, represented by the hollow circle.

Figure B.6: Number of Balance Checks Within 7 Days of POS Transaction



Notes: This figure shows the average number of balance checks made per day during each of the 7 days before, on the same day as, or 7 days after a transaction at a POS terminal. Transactions made on the same day as an ATM withdrawal are excluded. Based on N = 119,949,919 account-day observations from 251,985 accounts.

Figure B.7: Within-Account Relation Between Balance Checks and Savings



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 $Savings_{it} = \lambda_i + \sum_{c \neq 0} \eta_c \mathbb{I}(Checks_{it} = c) + \varepsilon_{it}$. 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. $N = 595{,}655$ account-bimester observations from 142,075 *treated* beneficiaries who began saving at some point after receiving a debit card. Asymptotic standard errors clustered at the locality level are included in parentheses. 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.

Table B.1: Baseline Summary Statistics from Administrative Account Data

	(1) Mean	(2) Standard Deviation	(3) 25th Percentile	(4) Median	(5) 75th Percentile
Number of client deposits	0.01	0.11	0	0	0
Number of withdrawals	1.10	0.29	1	1	1
Made exactly 1 withdrawal	0.90	0.30	1	1	1
Made exactly 2 withdrawals	0.09	0.28	0	0	0
Made 3 or more withdrawals	0.01	0.10	0	0	0
% of transfer withdrawn	99.51	3.24	100	100	100
Size of Oportunidades transfer (pesos)	1539.96	1029.94	470.00	1315.00	2180.00
End-of-period balance (pesos)	123.85	181.49	1.86	41.68	161.62
Years with account	3.49	1.50	2.63	3.43	5.19

Notes: This table shows means and standard deviations for account use summary variables constructed from the transactions-level data, measured at baseline (i.e., before the debit card rollout started). Specifically, data from the first bimester of 2008 is used, so each measure can be interpreted as the average across accounts in a single bimester before the rollout began. Based on data from $N = 268{,}222$ beneficiary accounts, which is the subset of accounts that existed at the beginning of 2008.

Table B.2: Effect of Debit Card on Withdrawals

	Number of withdrawals				asinh(# of withdrawals)
Period	(1)	(2)	(3)	(4)	(5)
–9	–0.03	–0.03	–0.03	0.00	–0.02
	(0.03)	(0.03)	(0.03)	(0.01)	(0.02)
–8	0.00	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
–7	0.06	0.06	0.04	0.04	0.03
	(0.07)	(0.06)	(0.05)	(0.05)	(0.03)
–6	0.00	0.00	0.00	0.01	0.00
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
–5	0.00	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
–4	0.00	0.00	0.00	0.00	0.00
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
–3	0.01	0.01	0.00	0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
–2	0.00	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
–1					
0	0.36***	0.32***	0.24***	0.24***	0.18***
	(0.04)	(0.04)	(0.02)	(0.02)	(0.02)
1	0.34***	0.30***	0.23***	0.22***	0.16***
	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)
2	0.30***	0.27***	0.22***	0.22***	0.15***
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
3	0.33***	0.31***	0.27***	0.27***	0.18***
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
4	0.24***	0.21***	0.16***	0.17***	0.12***
	(0.04)	(0.04)	(0.03)	(0.03)	(0.02)
5	0.26***	0.24***	0.20***	0.20***	0.13***
	(0.03)	(0.03)	(0.02)	(0.02)	(0.01)
N observations	4,740,331	4,740,331	4,740,331	4,740,331	4,740,331
N accounts	348,802	348,802	348,802	348,802	348,802
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Account fixed effects	Yes	Yes	Yes	Yes	Yes
Baseline characteristics × time	No	No	No	Yes	No
Winsorized	No	1%	5%	5%	No

This table shows the effect of the debit card on the number of withdrawals per period relative to receiving cards, estimated using (1). In columns 1–4, the number of withdrawals is the dependent variable. Column 5 shows robustness to using the inverse hyperbolic sine of the number of withdrawals as the dependent variable. Asymptotic cluster-robust standard errors are included in parentheses. Randomization inference p-values based on 2000 draws are included in square brackets. Stars are based on p-values from asymptotic cluster-robust standard errors; * indicates statistical significance at $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table B.3: Effect of Debit Card on Stock of Savings

	Stock of savings				asinh(stock of savings)
Period	(1)	(2)	(3)	(4)	(5)
–9	–11.74	–18.27	7.08	3.51	–0.03
	(36.67)	(28.24)	(24.32)	(24.68)	(0.22)
–8	–44.22	–47.05*	–38.87*	–40.77*	0.06
	(31.04)	(25.99)	(23.06)	(23.41)	(0.21)
–7	–13.77	–23.19	–19.86	–20.63	0.30
	(35.20)	(28.19)	(24.31)	(24.10)	(0.19)
–6	–2.41	–11.58	–9.24	–12.02	0.26
	(21.00)	(16.30)	(14.18)	(14.41)	(0.18)
–5	–15.04	–24.14	–25.74	–26.38	0.01
	(19.49)	(18.71)	(16.97)	(16.92)	(0.18)
–4	–25.46	–32.48**	–32.58**	–32.98**	0.05
	(16.70)	(14.59)	(12.76)	(13.36)	(0.17)
–3	–9.21	–14.08	–16.37	–15.23	–0.14
	(14.81)	(13.71)	(12.40)	(12.23)	(0.16)
–2	7.30	2.55	4.74	1.56	–0.14
	(21.27)	(19.06)	(16.67)	(15.91)	(0.20)
–1					
0	134.43***	140.20***	120.85***	121.18***	0.37
	(37.11)	(36.46)	(33.05)	(33.52)	(0.24)
1	69.08	87.79*	91.37**	95.84**	0.66**
	(52.26)	(49.95)	(44.29)	(45.10)	(0.31)
2	140.18***	150.10***	125.09***	131.15***	1.18***
	(43.78)	(37.53)	(30.99)	(30.02)	(0.20)
3	465.62***	476.55***	447.48***	458.24***	2.24***
	(49.37)	(44.80)	(41.98)	(41.65)	(0.26)
4	710.30***	727.42***	661.54***	676.61***	3.01***
	(70.27)	(65.32)	(56.68)	(57.31)	(0.32)
5	835.02***	846.24***	767.87***	768.70***	3.62***
	(73.13)	(64.19)	(56.72)	(56.05)	(0.26)
N observations	4,668,575	4,668,575	4,668,575	4,668,575	4,668,574
N accounts	348,802	348,802	348,802	348,802	348,801
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Account fixed effects	Yes	Yes	Yes	Yes	Yes
Baseline characteristics × time	No	No	No	Yes	No
Winsorized	No	1%	5%	5%	No

This table shows the effect of the debit card on the stock of savings per period relative to receiving cards, estimated using (1). In columns 1–4, the stock of savings is the dependent variable. Column 5 shows robustness to using the inverse hyperbolic sine of the stock of savings as the dependent variable. Asymptotic cluster-robust standard errors are included in parentheses. Randomization inference p-values based on 2000 draws are included in square brackets. Stars are based on p-values from asymptotic cluster-robust standard errors; * indicates statistical significance at $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table B.4: Proportion Withdrawing at ATMs and Saving

Period	Proportion Withdrawing at ATMs (1)	Proportion Saving (2)
–9		0.22
		(0.02)
–8		0.17
		(0.01)
–7		0.17
		(0.01)
–6		0.15
		(0.01)
–5		0.12
		(0.01)
–4		0.12
		(0.01)
–3		0.11
		(0.01)
–2		0.13
		(0.01)
–1		0.13
		(0.01)
0	0.83	0.29
	(0.03)	(0.04)
1	0.92	0.34
	(0.01)	(0.04)
2	0.91	0.42
	(0.01)	(0.04)
3	0.84	0.81
	(0.02)	(0.05)
4	0.94	0.85
	(0.01)	(0.05)
5	0.93	0.87
	(0.01)	(0.04)
N observations	2,799,372	2,968,628
N accounts	255,781	255,784

This table shows the proportion of account holders who withdraw at ATMs each period after receiving the debit card (column 1, which corresponds to Figure 4) and the proportion who save in their account each period relative to receiving a card (column 2, which corresponds to Figure 7a). Asymptotic cluster-robust standard errors are included in parentheses.

Table B.5: Stock of Savings Conditional on Saving (Pesos)

	Stock of savings				asinh(stock of savings)
Period since saving	(1)	(2)	(3)	(4)	(5)
0	677.84***	670.78***	618.20***	618.15***	3.38***
	(21.65)	(20.85)	(18.82)	(18.54)	(0.17)
1	845.21***	829.44***	757.68***	758.07***	4.23***
	(35.13)	(33.54)	(28.63)	(28.01)	(0.19)
2	934.69***	908.80***	813.91***	813.17***	4.40***
	(51.72)	(47.68)	(36.57)	(36.31)	(0.17)
N observations	4,668,575	4,668,575	4,668,575	4,668,575	4,668,575
N accounts	348,802	348,802	348,802	348,802	348,802
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Account fixed effects	Yes	Yes	Yes	Yes	Yes
Baseline characteristics × time	No	No	No	Yes	No
Winsorized	No	1%	5%	5%	No

This table shows the stock of savings for each period relative to starting to save and corresponds to Figure 7b, estimated using (1) with the event time dummies redefined relative to when an individual starts saving in the account, and we impose a zero pre-treatment trend by setting $a = 0$ (for reasons explained in Section 4.1). In columns 1–4, the stock of savings is the dependent variable. Column 5 shows robustness to using the inverse hyperbolic sine of the stock of savings as the dependent variable. Asymptotic cluster-robust standard errors are included in parentheses. Stars are based on p-values from asymptotic cluster-robust standard errors; * indicates statistical significance at $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table B.6: Heterogeneous Effect of Debit Card on Consumption by Intra-Household Bargaining Power

	(1)	(2)	(3)	(4)
Diff-in-diff	–99.76	–8.01	–4.42	–74.22
	(187.27)	(143.74)	(123.39)	(128.20)
Diff-in-diff $\times \mathbb{I}$(Baseline barganing power $<$ median)	–231.13	–322.46*	–306.39*	–165.07
	(206.45)	(174.08)	(160.42)	(151.74)
Number of households	995	995	995	801
Number of observations	3324	3324	3324	2856
Time fixed effects	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes
$\mathbb{I}$(Baseline bargaining power $<$ median)$\times$ time	Yes	Yes	Yes	Yes
Household characteristics $\times$ time	No	No	No	Yes
Winsorized	No	1%	5%	5%

Notes: This table shows heterogeneous effects of the debit cards on consumption by baseline bargaining power using the Household Panel Survey combined with administrative data from Oportunidades on the debit card rollout. Since the cash transfer beneficiaries are women in 99% of households, the sample is restricted to households in which there is at least one male adult in the household in addition to the female beneficiary (since intra-household bargaining power would not be relevant in single-headed households). Sample sizes are thus smaller than in Table 5 for two reasons: first, the sample is restricted to households with at least one male adult in addition to the female beneficiary; second, approximately 30% of these households have a missing value (corresponding to answering "not applicable") for at least one of the bargaining power questions. The 2002 survey includes five questions about bargaining power (but these questions are not included in the 2003 or 2004 survey waves): (1) If a child is sick, who decides when it is necessary to take them to the doctor?; (2) If a child doesn't want to go to school one day, who decides if he/she has to go?; (3) When it is necessary to buy clothes and shoes for the children, who decides whether to spend money on this?; (4) Who makes important decisions that affect the household members (transport, moving homes, changing jobs)?; (5) When you have some extra income, do you decide what to use it on, do you give it to your partner, or do you both decide how to use it? For each question, we code a bargaining power variable equal to 1 if either the woman makes the decision or the decision is made jointly, and 0 if the man makes the decision. We then create a Kling, Liebman and Katz (2007) normalized summary measure that averages across these five questions within each household in the 2002 survey that has a male adult in the household. Our interaction variable in the difference-in-difference specification is a dummy equal to 1 if the household is below-median based on this normalized summary measure of baseline bargaining power. Consumption is measured in pesos. For column 4, the household characteristics interacted with time fixed effects are defined in the notes to Table 5. The number of households in column (4) is lower because households have missing values for one of the household characteristics included, or are not included in enough pre-treatment waves to construct household-level pre-trends of the outcome variables, which are interacted with time fixed effects in that specification. Asymptotic cluster-robust standard errors (clustered at the locality level, using pre-treatment locality) are included in parentheses. Stars are based on p-values from asymptotic cluster-robust standard errors; * indicates statistical significance at $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table B.7: Supply-Side Response by Banks

	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 (3). The F-test of lags tests $\beta_{-6} = \cdots = \beta_{-1} = 0$; the F-test of leads tests $\beta_1 = \cdots = \beta_6 = 0$. Standard errors are clustered at the municipality level. $N = 2,491$ municipality-quarter observations from 199 municipalities.

Table B.8: Self-Reported Knowledge of Fees, Balance Checks, Knowledge of Technology

	Mean card $<$ median time	Difference card $\geq$ median time			N
	(1)	(2)	(3)	(4)	(5)
Fees to check balance (pesos)	13.08	0.44	0.43	0.02	1,142
	(0.75)	(0.85)	(0.85)	(0.84)	
		[0.62]	[0.63]	[0.98]	
Fees to withdraw (pesos)	23.30	0.06	0.10	0.08	1,364
	(0.93)	(0.98)	(0.98)	(0.90)	
		[0.96]	[0.92]	[0.94]	
Balance checks without withdrawing	0.94	–0.29**	–0.29**	–0.30**	1,490
	(0.13)	(0.12)	(0.12)	(0.13)	
		[0.03]	[0.03]	[0.03]	
Hard to use ATM	0.11	0.01	0.01	0.01	1,617
	(0.02)	(0.02)	(0.02)	(0.02)	
		[0.68]	[0.73]	[0.47]	
Gets help using ATM	0.49	0.02	0.00	–0.01	1,612
	(0.04)	(0.04)	(0.03)	(0.03)	
		[0.73]	[0.99]	[0.74]	
Knows PIN	0.56	0.01	0.02	0.02	1,609
	(0.03)	(0.03)	(0.03)	(0.03)	
		[0.76]	[0.55]	[0.59]	
Household-level controls		No	Yes	Yes	
Locality/municipality-level controls		No	No	Yes	

Notes: This table shows differences between those who have had the card for less vs. more than the median time, estimated using equation 4. N varies by row due to missing values for the outcome variables (total N in the Payment Methods Survey is 1,617). Column 1 shows the mean for those who have had the card for less than the median time (α) and columns 2–4 show the difference (γ). Column 2 does not include any additional controls. Column 3 controls for the household-level controls that would not be affected by treatment from Table 3 (number of household members; age, gender, marital status, and education level of the Prospera beneficiary). Column 4 controls for both household-level controls and locality- or municipality-level controls for the variables from Figure 3 (log wage, log food prices, log POS terminals, log bank branches, log ATMs, log debit and credit cards, average stock of savings, average log stock of savings, and average number of withdrawals). To maintain the same sample in column 4, if a locality is not included in one of the data sets used as a control, we replace the missing value with 0 and include a set of dummy variables that equal one if the locality was missing from each control data set. Asymptotic cluster-robust standard errors are included in parentheses. Randomization inference p-values based on 2000 draws are included in square brackets. Stars are based on p-values from asymptotic cluster-robust standard errors; * indicates statistical significance at $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Appendix C Sample of Materials Received by Beneficiaries (Internet Appendix)

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


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

0100, PRUEBA 010001000200
0100, ALPINA
Río Magdalena 115, 01090
Alvaro Obregon, DF
0100, PRUEBA

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

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

IMPORTANTE

1.- DESPRENDE tu Tarjeta de Débito.

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

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

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

¡¡¡CUIDADO!!!

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

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

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

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

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

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

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

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 políticos, electorales, de lucro y de otros distintos a los establecidos. Quien haga uso indebido de los recursos de este programa deberá ser denunciado y sancionado de acuerdo a la ley aplicable y ante la autoridad competente"

Consecutivo: **1005**

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

Appendix D Mechanical Effect (Internet Appendix)

This appendix defines the "mechanical effect," which we use to compute end-of-period balances. Section 5.2 explains why we cannot instead compute end-of-period balances by simply taking beginning-of-period balance, adding all deposits, and subtracting all withdrawals. 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. Our measure of end-of-period balance is equal to the account's average balance over the period (provided by Bansefi) minus the mechanical effect (computed from the Bansefi transactions data).

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 end-of-period balance. 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 also began the period with 0 balance then 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 end-of-period 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.

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.

D.3 Steps

More generally we follow the steps below:

1. We separate the sample based on the number of transfers received by Opportunidades' beneficiaries: 85% of beneficiary-bimester pairs receive a single transfer in the bimester and 15% received two transfers in the same bimester. See Appendix E for a description of the reasons

some benefeciary-bimester pairs include more than one transfer.

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

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

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

Appendix E Reasons for Variance in Transfers (Internet Appendix)

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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