Financial Technology Adoption

Job Market Paper

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Abstract

How do the supply and demand sides of the market respond to financial technology adoption? In this paper, I exploit a natural experiment that caused exogenous shocks to the adoption of a financial technology over time and space. Between 2009 and 2012, the Mexican government disbursed about one million debit cards to existing beneficiaries of its conditional cash transfer program. I combine administrative data on the debit card rollout with a rich collection of Mexican microdata on both consumers and retailers. The shock to debit card adoption has spillover effects on financial technology adoption on both sides of the market: small retailers adopt point-of-sale (POS) terminals to accept card payments, which leads other consumers to adopt cards. Specifically, the number of other consumers with debit cards increases by 21 percent. Richer consumers respond to corner stores' adoption of POS terminals by substituting 12 percent of their supermarket consumption to corner stores. Finally, I use microdata on store prices, store geocoordinates, and consumer choices across store types to estimate the consumer gains from the demand-side policy's effect on supply-side POS adoption.

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1 Introduction

New financial technologies are rapidly changing the way that households shop, save, borrow, and make other financial decisions. Payment technologies like debit cards and mobile money—which enable consumers to make retail payments and transfers through a bank account or mobile phone—can benefit both the demand and supply sides of the market. Consumers benefit from financial technologies (FinTech) through lower transaction costs, such as the costs of traveling to a bank branch or ATM to withdraw cash (Bachas, Gertler, Higgins and Seira, 2018*a*), the crime risks of carrying cash (Economides and Jeziorski, 2017), and the large fees to send remittance payments (Jack and Suri, 2014). Retail firms can also benefit from adopting FinTech—such as technology to accept payments by card or mobile money—both by reducing the risk of cash theft (Rogoff, 2014) and by attracting customers who prefer these payment technologies and may not carry cash.

Numerous studies have documented the direct consumer impacts of FinTech—a category that includes card payments (Einav et al., 2017), mobile money (Suri and Jack, 2016; Yermack, 2018), cryptocurrencies (Howell, Niessner and Yermack, 2018), online lending (Buchak, Matvos, Piskorski and Seru, 2018), and smartphone apps that allow users to track their spending (Gelman et al., 2014; Carlin, Olafsson and Pagel, 2017). These technologies have impacted consumer borrowing (Bartlett, Morse, Stanton and Wallace, 2018), saving (Blumenstock, Callen and Ghani, 2018), risk sharing (Jack and Suri, 2014; Riley, 2018), and resilience to shocks (Bharadwaj, Jack and Suri, 2018). Little is known, however, about how the supply side of the market responds to consumers' FinTech adoption, or about spillover effects on other consumers.

In this paper, I exploit a shock to consumers' adoption of a particular financial technology—debit cards—to quantify the supply and demand-side spillovers of consumer FinTech adoption. Specifically, I study how small retailers respond to consumer debit card adoption by adopting point-of-sale (POS) terminals to accept card payments, and how this supply-side response feeds back to the demand side, affecting other consumers' debit card adoption and consumption decisions across stores. The spillovers of FinTech adoption are likely to be substantial because many financial technologies—and payment technologies in particular—have two-sided markets. Two-sided markets generate indirect network externalities, where the benefits a debit card user derives from the technology depend on supply-side adoption of technology to accept card payments, which in turn depends on how many other consumers have adopted the technology.[1] Thus, a sufficiently large shock to consumers' adoption could lead to dynamic increases in FinTech adoption on both sides of the market.

The supply-side response to consumers' FinTech adoption and the spillovers onto other consumers have been difficult to address in the literature for four reasons. First, technology adoption is endogenous. Second, because supply-side adoption of the corresponding technology could require consumer adoption to reach a certain threshold before retailer adoption is optimal, any exogenous shock to consumer adoption would need to be large enough to affect the local market. Randomized control trials of financial technologies, for example, typically involve shocks to consumer adoption that are likely too small to generate supply-side responses. Third, studying spillovers onto other consumers—which can arise due to network externalities in two-sided markets—requires a shock that directly affects only a subset of consumers. This rules out the use of policy shocks that affect all participants on one side of the market such as large-scale subsidies to the cost of technology adoption. Fourth, in most empirical settings there is a lack of high-quality data on firm technology adoption and outcomes for both firms and other consumers.

To overcome the first three barriers, I exploit a natural experiment that created an exogenous shock to FinTech adoption on one side of the market. Between 2009 and 2012, the Mexican government disbursed about one million debit cards as the new payment method for its large-scale conditional cash transfer program, Prospera.[2] This policy shock has a number of notable features that make it ideal for studying supply-side responses to consumers' financial technology adoption, as well as spillovers on other consumers. First, the shock created plausibly exogenous variation over time and space in debit card adoption: it occurred in different localities at different points

[1]Katz and Shapiro (1985) distinguish three ways network externalities can arise, and two of these are due to two-sided markets. The literature has classified these as *indirect* network externalities, which are distinct from *direct* network externalities—e.g., of telephones, where users benefit directly from the number of other users (Katz and Shapiro, 1994).

[2]The program was started as Progresa, was known as Oportunidades during the card rollout, and was rebranded as Prospera in 2014.

in time and was uncorrelated with levels and pre-treatment trends in financial infrastructure and other locality characteristics. Second, the shock was large enough to affect the local market: in the median treated locality, it directly increased the proportion of households with a debit card by 18 percentage points (48%).[3] Third, the shock only reduced the cost of debit card adoption for a subset of consumers (specifically, beneficiaries of Mexico's cash transfer program), which allows me to isolate spillover effects on other consumers whose cost of adoption did not change.

To overcome the fourth barrier—a lack of high-quality data on firm technology adoption and outcomes for both firms and other consumers—I combine administrative data from Prospera on the debit card rollout with a rich collection of Mexican microdata consisting of seven additional data sets on both consumers and retailers. The key data set on supply-side FinTech adoption is a confidential data set on the universe of POS terminal adoptions by retailers over a twelve-year period, accessed on-site at Mexico's Central Bank. I combine this with confidential transaction-level data on the use of POS terminals, which include the universe of debit and credit card transactions at POS terminals in Mexico (over six billion transactions).

For spillovers to other consumers, the two key data sets that I use are quarterly data on the number of debit cards by issuing bank by municipality and consumption data from a nationally representative household survey merged with confidential geographic identifiers. Importantly, the consumption data takes the form of a consumption diary that can be used to identify unique trips to different types of stores, as well as quantities purchased and amount spent. I complement these with three additional confidential data sets: transaction-level data from the bank accounts of Prospera beneficiaries, a panel on store-level sales, costs, and profits for the universe of urban retailers, and high-frequency price data at the store by barcode level from a sample of stores. Finally, I use a rich set of geographic information systems (GIS) data including geocoordinates of the universe of retailers and shapefiles for all roads in Mexico.

Firms respond to the policy-induced shock to consumer FinTech adoption by adopting POS terminals to accept card payments. Using an event study design to accommodate the differing

[3] In the median locality, 36% of households had a debit or credit card prior to the shock (based on household survey data), and the shock increased the proportion of households with a card to 54%.

times of treatment across localities and to allow dynamic treatment effects over time, I find that the number of corner stores with POS terminals increases by 3% during the two-month period in which the shock occurs.[4] Adoption continues to increase over time: two years after the shock, 18% more corner stores use POS terminals in treated localities (relative to localities that have yet to be treated). There is no effect among supermarkets, which already had high levels of POS adoption.

The shock to consumer card adoption and subsequent adoption of POS terminals by small retailers has spillover effects on other consumers' card adoption. Using data on the total number of debit cards issued by banks *other than* the government bank that administered cards to cash transfer recipients, I find that other consumers respond to the increase in FinTech adoption by increasing their adoption of cards. Specifically, one quarter after the shock occurs, the number of cards held by other consumers increases by 19%. Two years after the shock, 28% more consumers (excluding Prospera beneficiaries) have adopted cards.[5]

The adoption of POS terminals by small retailers also affects the consumption behavior of consumers who did not directly receive a card from Prospera. The richest quintile of all consumers—who are substantially more likely to have cards before the shock—substitute about 12% of their total supermarket consumption to corner stores. This is at least partly driven by a change in the number of trips to supermarkets and corner stores: households in the richest quintile make, on average, 0.2 fewer trips per week to supermarkets and 0.8 more trips per week to corner stores after the shock (relative to households in the same income quintile in not-yet-treated localities). While these shifts in consumption across store types occur only for richer consumers (not Prospera beneficiaries), a companion paper looks at the effect of the debit cards on beneficiaries' income, consumption, and savings (Bachas, Gertler, Higgins and Seira, 2018*b*).[6]

[4]Administrative data from Bansefi, the government bank that administers cash transfer beneficiaries' accounts, show that cards were usually distributed during the first week of these two-month periods.

[5]Although I cannot directly test whether these cards from other banks are adopted by other consumers or Prospera beneficiaries and their household members (who might decide to obtain an additional card from another bank after receiving their first card from Prospera), survey data rule out this explanation: after the rollout, only 6% of households that received cards also had an account at another bank.

[6]In that paper, we find that the cards do not affect beneficiaries' income, but that beneficiaries do begin saving more in the bank after receiving cards. Furthermore, this increase in formal savings represents an increase in overall savings, financed by a voluntary reduction in current consumption.

Finally, I estimate the gains of the policy to consumers and producers. To estimate consumer gains from the supply-side POS adoption that occurred as a result of the demand-side policy shock, I take a revealed-preference approach based on consumers' choices of where to shop. I combine consumption survey microdata on consumer choices across store types with data on local prices (from the store-by-product level microdata used to construct Mexico's consumer price index), geo-coordinates of all retailers (to calculate each consumer's distance to the closest corner store and supermarket), and data on local POS adoption from Mexico's Central Bank.

My estimating equation is derived from a discrete choice model where consumers decide, for each shopping trip, whether to go to a supermarket or corner store. Supermarkets are further on average, but have cheaper prices and accept card payments. Corner stores are closer on average, have higher (within-barcode) prices, and may or may not accept card payments. Because demand at each type of store is endogenous to local prices and POS adoption, I use the standard Hausman (1996) instrument for prices and use the policy shock as an instrument for POS adoption. Using the coefficients from this demand model, I estimate the price-index-equivalent consumer surplus resulting from the policy-induced change in the proportion of corner stores accepting cards.

To estimate gains for producers, I use a reduced-form approach with data on the revenues and costs of all urban retailers in Mexico. Over the five-year period between survey rounds, corner store profits increase by 15% more in treated localities. Corner stores increase the amount of merchandise they buy and sell but do not change their other inputs such as number of employees, wages, and rent. This effect does not represent an aggregate gain to producers, however, as the increased sales at corner stores are accompanied by decreased sales at supermarkets.

This paper thus makes three main contributions to the literature. First, I investigate how FinTech adoption by consumers filters through markets to affect retail FinTech adoption, other consumers' technology adoption and consumption decisions, and producer and consumer surplus. Most research on financial inclusion and FinTech adoption, on the other hand, has focused on direct effects for consumers who adopt. For example, studies have looked at the effect of debit card adoption on transaction and travel costs to access money at ATMs (Schaner, 2017; Bachas, Gertler, Higgins and Seira, 2018*a*), the use of debit cards to make in-store purchases (Zinman, 2009), the use of FinTech to monitor account balances (Carlin, Olafsson and Pagel, 2017; Bachas, Gertler, Higgins and Seira, 2018*b*), the consumer surplus from online shopping (Einav et al., 2017), and the effects of mobile money on risk sharing and savings (Jack and Suri, 2014; Suri and Jack, 2016).

Second, I provide evidence that network externalities constrain the adoption of potentially profitable technologies. Other studies identifying constraints to profitable technology adoption primarily focus on upfront costs (Basker, 2012; Bryan, Chowdhury and Mobarak, 2014) or on learning externalities through social networks (Conley and Udry, 2010; Banerjee, Chandrasekhar, Duflo and Jackson, 2013)—which differ from technological externalities like those of card payment technologies (Foster and Rosenzweig, 2010). The network externality constraint—which binds due to low levels of consumer card adoption—also connects to the literature on the constraints to growth posed by a lack of financial intermediation in developing countries (e.g., King and Levine, 1993; Beck, Demirgüç-Kunt and Maksimovic, 2005). I identify precise mechanisms through which a specific type of financial development—through technology adoption—reduces constraints and leads to increased profits for small retailers.

Third, I exploit a change in the cost of adoption for a *subset* of consumers, which allows me to isolate spillover effects on other consumers. Other studies on network externalities or the adoption of network goods—which may have large spillovers—rarely exploit changes in the cost of adoption for just a subset of consumers (e.g., Saloner and Shepard, 1995; Rysman, 2007). This is because most policy shocks have similar effects on the cost of adoption for all participants on one side of the market: for example, subsidies to the cost of technology adoption rarely affect only a subset of consumers or retailers.[7] Given the large spillovers that I find, policy interventions to increase FinTech adoption may only need to target a subset of consumers, as those consumers' adoption could spur further adoption on both sides of the market, benefiting both consumers and firms.

[7] Björkegren (forthcoming) also exploits variation that directly affects only a subset of consumers, specifically an expansion of cell phone towers in rural Rwanda. He finds that the majority of consumer surplus from the expansion accrues to users whose coverage was not affected (mostly to users who had already adopted and can now communicate with users in rural areas, but also to users who are incentivized to adopt sooner knowing they will be able communicate with users in rural areas).

The rest of the paper is organized as follows. Section 2 provides context about financial technology adoption in Mexico, and about the debit card rollout I exploit. Section 3 describes the main sources of data I use. Section 4 describes my identification strategy. Section 5 presents the paper's key results. Section 6 tests various alternative explanations of the results. Section 7 concludes.

2 Financial Technology Adoption in Mexico

The proportion of adults who do not have a debit card, credit card, or mobile money account in Mexico is high, at over 70%—compared to 50% worldwide (Demirgüç-Kunt et al., 2018). The proportion of the population without a debit or credit card is also highly correlated with income, as shown in Figure 1. In urban areas, approximately 16% of households in the poorest income quintile had a debit or credit card in mid-2009, compared to 52% of households in the richest income quintile.[8] Because retailers only benefit from POS terminal adoption if consumers have adopted debit or credit cards, small retailers with mostly unbanked customers might not adopt potentially profitable financial technologies. When the cost of adoption falls for some consumers, this can make the technology profitable for retailers and incentivize dynamic adoption on both sides of the market. Appendix A presents a simple model of financial technology adoption by consumers and retailers to formalize these concepts.

Figure 2 shows the municipality-level correlation between adoption on each side of the market: the y-axis shows the proportion of retailers with POS terminals, and the x-axis shows the number of debit cards per person.[9] Each point on the graph is a municipality, and the size of the points represents population. Visually, there is evidence that a critical mass point may exist: below about 0.15 debit cards per person, the proportion of retailers with POS terminals is low and does not appear to increase in the concentration of cards; above this level, however, there is a clear

[8]Urban areas are defined as localities with at least 15,000 inhabitants. These results are from the Mexican Family Life Survey 2009.

[9]This graph is based on publicly available data from Mexico's National Banking Commission, the Comisión Nacional Bancaria y de Valores (CNBV), and their National Statistical Institute, the Instituto Nacional de Estadística y Geografía (INEGI). The y-axis is constructed by dividing the total number of businesses with one or more POS terminals by the total number of retailers in the municipality, and the x-axis is constructed by dividing the total number of debit cards by the population; unfortunately a measure of the number of individuals with debit cards—rather than number of debit cards—is not available (except in household surveys which do not include the universe of households).

positive slope. Such a critical mass point can arise due to network externalities (Economides and Himmelberg, 1995).

The evolution of card and POS terminal adoption over time also appears highly correlated: Figure 3 shows the variation in adoption on each side of the market across space and time. Comparing the change in adoption of debit cards and POS terminals in particular municipalities over time (i.e., comparing the changes between panels a and b), it is clear that adoption of the technologies are correlated: the municipalities that see large increases in debit card adoption also see large increases in POS terminal adoption.

2.1 Shock to Debit Card Adoption

Between 2009 and 2012, the Mexican government rolled out debit cards to urban beneficiaries of its conditional cash transfer program Prospera. Urban beneficiaries are defined as living in localities with a population greater than 15,000. (Mexico had 195,933 total localities in 2010, but the vast majority are rural and semi-urban localities with less than 15,000 inhabitants; 630 of Mexico's localities are urban.)

Prospera—formerly known as Progresa and later Oportunidades—is one of the first and largest conditional cash transfer programs worldwide, with a history of rigorous impact evaluation (Parker and Todd, 2017). The program provides cash transfers to poor families with children ages 0–18 or pregnant women. Transfers are conditional on sending children to school and completing preventive health check-ups. The program began in rural Mexico in 1997, and later expanded to urban areas starting in 2002. Today, nearly one-fourth of Mexican households receive benefits from Prospera. Beneficiary households receive payments every two months, and payments are always made to women except in the case of single fathers. The transfer amount depends on the number of children in the household, and during the time of the card rollout averaged US$150 per two-month payment period.

Prior to the debit card rollout, the beneficiaries who received cards already received cash benefits deposited directly into formal savings accounts without debit cards. This formal savings account was automatically created for the beneficiaries by the National Savings and Financial

Services Bank (Bansefi), a government bank created in 2001 with the mission of "contributing to the economic development of the country through financial inclusion. . . to strengthen savings and loans mainly for low income segments." To access their transfers, beneficiaries traveled to a Bansefi branch (of which there are about 500 in Mexico). The median road distance between an urban beneficiary household and the closest Bansefi branch is 4.3 kilometers (Bachas, Gertler, Higgins and Seira, 2018*a*); possibly as a result of these indirect transaction costs, prior to receiving a debit card nearly all beneficiaries made one trip to the bank per payment period, withdrawing their entire transfer (Bachas, Gertler, Higgins and Seira, 2018*b*).

The debit card rollout provided a Visa debit card to all beneficiaries in each treated urban locality. The debit card could be used to both withdraw funds from any bank's ATM and to make purchases at POS terminals at any merchant accepting Visa. Although beneficiaries could have voluntarily adopted a Bansefi debit card prior to the rollout, this would have required opening a separate account attached to the debit card, and the transfers would have continued being deposited in the initial account not attached to the debit card. As part of the debit card rollout, Bansefi automatically completed the administrative process of opening these debit card-eligible accounts for beneficiaries, and the direct deposit of their transfers was switched to the new accounts.[10]

All beneficiaries in a treated locality receive cards during the same payment period, and although the overall number of beneficiaries in the program increases nationally over time, the rollout was not accompanied by a differential change in the number of beneficiaries or transfer amounts. Furthermore, conditional on being included in the rollout, the timing of when a locality received the card shock is not correlated with pre-rollout levels or trends in financial infrastructure, nor with other locality-level observables (Section 4).

2.2 Costs and Benefits of POS Adoption

Banks rent point-of-sale terminals to retailers. For a retailer to rent a POS terminal from a bank, it needs to have a bank account with that bank. The terminal has a low upfront cost of US$23, but

[10]Prior to the rollout it was not possible to have the transfers automatically deposited in or automatically transfered to a debit card-eligible Bansefi account, or to an account at another bank.

includes a monthly rental fee of US$27 per month if the business does not transact at least US$2000 per month in electronic sales through the terminal. This constraint binds for most retailers, given that it corresponds to the 70th percentile of *total* retail sales, and even retailers that accept cards still process a substantial proportion of their sales in cash.[11] In addition, there is a marginal cost per transaction that varies by sector and bank, which was 1.75% for POS terminals acquired by retailers from Mexico's largest private bank during the period of the card rollout.

In addition to these direct financial costs, there are indirect costs. First, acquiring a POS terminal requires having or opening an account with the bank issuing the terminal, traveling to the bank to request the terminal, and signing a contract with the bank. In addition, in focus groups with retailers, they perceived that their tax costs could increase after adopting a POS terminal since the data could be used by the government to increase tax compliance. Even though firms were not required to be formally registered with the tax authority in order to obtain a POS terminal at the time, this could affect both unregistered firms that pay no taxes by increasing their probability of being caught, as well as increase the taxes paid by registered firms who underreport their revenues to the tax authority (as in Slemrod et al., 2017). During the time of the card rollout, the tax authority would have had to explicitly audit a retailer in order to access the data generated by its electronic sales; nevertheless, retailers' knowledge of the precise laws governing taxes and electronic payments may be limited.[12]

The perceived benefits of POS adoption, reported by retailers in focus groups, include increased security, convenience, and sales. The increased security can arise due to both having less cash on hand that can be robbed, as well as lower risk that employees themselves skim off cash from the business. The increased convenience arises from reducing the number of physical trips that need to be made to the bank to deposit cash revenues. Finally, retailers reported higher sales after adoption. A number of retailers reported that prior to adopting a POS terminal, they would (i) lose potential sales when customers were not carrying cash at the time and (ii) lose customers who previously

[11]In the National Survey of Enterprise Financing, corner stores that accept card payments report that, on average, 23% of their clients pay by card.

[12]In contrast, in the US, third-party electronic payment data for each firm is automatically sent by electronic payment entities (e.g., Visa) to the Internal Revenue Service through Form 1099-K, first implemented in 2011.

frequented the store once those customers adopted cards. Retailers also reported attracting new customers once they began accepting card payments, and one focus group participant estimated that adopting led to a 15–20% increase in sales.

3 Data

I combine administrative data on the debit card rollout with a rich collection of microdata from Mexico. These data sets fall under four broad categories: (i) data on the card rollout and beneficiaries' use of cards; (ii) data on the adoption of POS terminals and subsequent card use at POS terminals; (iii) data on other consumers' response to retailers' adoption of POS terminals; and (iv) data on retailer outcomes and prices. As described in more detail in Section 4, I restrict each data set to the subsample corresponding to urban localities included in Prospera's debit card rollout. I describe each of the main data sets in this section and provide more detail in Appendix B.

3.1 Card Rollout and Beneficiary Card Use

Administrative data from Prospera. Prospera provided confidential data at the locality by two-month payment period level. The data include the number of beneficiaries in the locality and the payment method by which they are paid. Examples of payment methods include cash, bank account without debit card, and bank account with debit card.[13] These data, which span 2007–2016 and include all 630 of Mexico's urban localities (as well as all rural localities with Prospera beneficiaries), allow me to identify the two-month period during which cards are distributed in each locality. In addition, they allow me to test whether the card rollout was accompanied by an expansion of the number of Prospera beneficiaries, which would be a threat to identification as it would confound the effect of more cash flowing into the locality with the effect of the debit card shock.

Transaction-Level Data from Bansefi. Bansefi provided confidential data on the universe of transactions made in 961,617 accounts held by cash transfer beneficiaries. In addition, I observe when each account holder receives a debit card. Across all transaction types (including cash with-

[13]With a few exceptions, all beneficiaries in a locality are paid using the same payment method. In the exceptional cases, the data show how many beneficiaries within the locality are paid through each payment method.

drawals, card payments, deposits, interest payments, and fees), the data set includes 106 million transactions. I use this data set to measure whether the beneficiaries who directly received cards as part of the exogenous shock I use for identification are indeed using the cards to make purchases at POS terminals. Furthermore, the data contain a string variable with the name of the business at which debit card purchase was made, which allows me to manually classify whether the purchase was made at a supermarket, corner store, or other type of business.

3.2 Data on POS Terminals

Universe of POS terminal adoptions Data on POS terminal adoption was accessed on-site at Banco de México, Mexico's Central Bank. The data set includes all changes to POS contracts between retailers and banks from 2006–2017, where contract changes include adoptions of POS terminals, cancellations, and changes to the fee structure or other contract parameters. The data include the store type (more precisely, the merchant category code) and a geographic identifier (postal code).[14] In total, the data set includes over five million contract changes, 1.4 million of which are adoptions. I use both the adoptions and cancellations—combined with another data set that allows me to back out existing POS terminals prior to 2006 that had no contract changes over the period for which I have data—to construct a data set with the stock of POS terminals in each locality by store type by two-month period.

Universe of card transactions at POS terminals. These data were also accessed on-site at Mexico's Central Bank, and include every card transaction made at a POS terminal between 2009 and 2017. The data include an average of two million card transactions per day, for a total of over six billion transactions. For each transaction, I know the amount of pesos spent and again know the store type (merchant category code) of the business and can determine the locality in which the business is located.

[14]Merchant category codes are four-digit numbers used by the electronic payments industry to categorize merchants. Ganong and Noel (2018) also use the merchant category code to define spending categories. Appendix B explains how I map from postal codes, the geographic identifier in this data set, to localities, the relevant geographic area for the card rollout.

3.3 Consumer Response to Retailer POS Adoption

Other debit cards. To measure adoption of debit cards by other consumers in response to the Prospera card shock and subsequent FinTech adoption by retailers, I use quarterly data from Mexico's National Banking and Securities Commission (CNBV). These data are required by law to be reported by each bank to CNBV, and include the number of debit cards, credit cards, ATMs, and various other financial measures by bank by municipality, over the period 2008–2016. Because the data are at the bank level, I can exclude cards issued by Bansefi—the bank that administers Prospera beneficiaries' accounts and debit cards—when testing for spillovers of the card shock on other consumers' card adoption. While the data do not allow me to test whether the cards from other banks are adopted by Prospera beneficiaries after they discover the benefits of debit cards, I rule out this alternative explanation using survey data.

Consumption. To capture the consumption decisions of consumers throughout the income distribution (not restricted to Prospera beneficiaries) and to observe both their card and cash spending, I use Mexico's household income and expenditure survey (ENIGH). This survey is publicly available from Mexico's National Statistical Institute (INEGI), but does not include locality identifiers prior to 2012. I merge the data with confidential geographic identifiers provided by INEGI, which include the locality and "basic geographic area" (AGEB)—analogous to a US census tract. Because the card rollout occurred between 2009 and 2012, I use the 2006–2014 waves of the ENIGH, which include 49,810 households in localities included in the card rollout. The survey includes comprehensive income and consumption data; importantly, the consumption data takes the form of a consumption diary that includes the store type at which each good was purchased, as well as quantities purchased and amount spent on each good in pesos.

3.4 Retailer Outcomes and Prices

Retailer outcomes. Every 5 years, INEGI conducts an Economic Census of the universe of firms in Mexico. This census includes all retailers, regardless of whether they are formally registered (with the exception of street vendors who do not have a fixed business establishment). On-site at INEGI, I accessed data from the 2008 and 2013 census rounds since these years bracket the rollout of cards; I cannot include additional pre-periods because the business identifier that allows businesses to be linked across waves was introduced in 2008. Each wave includes about 5 million total firms; 532,374 of these are corner stores observed in both census waves. The survey includes detailed questions about various components of sales, costs, and profits. Because I do not observe whether a firm has adopted a POS terminal to accept card payments in this data set, the results using the Economic Census are intent-to-treat estimates.

Prices. I use price quotes from the confidential microdata used by INEGI to construct Mexico's consumer price index (CPI). These panel data record the price for over 300,000 goods at over 120,000 unique stores each week (or every two weeks for non-food items). Importantly, the goods are coded at the barcode-equivalent level (such as "600ml bottle of Coca-Cola"), which helps to disentangle price and quality differences between different types of store—for example, larger stores sell larger pack sizes or higher-quality varieties (Atkin, Faber and Gonzalez-Navarro, 2018). After averaging price quotes across two-month periods for consistency with Prospera's payment periods, the data set includes 5.4 million observations from 2002–2014.

4 Identification

Bansefi and Prospera rolled out debit cards to program beneficiaries in selected urban localities between 2009 and 2012. The government determined which urban localities would be included in the rollout based on the presence of banking infrastructure; 259 of Mexico's 630 localities were selected to be included in the rollout. Cards could not be distributed to all of these localities at once due to capacity constraints. In conversations with them, Bansefi and Prospera officials have asserted that in practice, they did not target localities with particular attributes for the early part of the rollout. On the contrary, they wanted the localities that received cards earlier in the rollout to be similar to those that would receive cards later so that they could test their administrative procedures for the rollout with a quasi-representative sample.

I formally test whether, conditional on being included in the rollout, observable locality-level characteristics or levels and trends in financial infrastructure are correlated with the time at which a locality receives the shock. To test this using a framework that accounts for the staggered timing of the card shock in different localities, I use a discrete time hazard (Jenkins, 1995). I include measures of pre-rollout levels and trends in financial infrastructure (POS terminals, bank branches, and ATMs), as well as all of the variables used by Mexico's national statistical institute and National Council for the Evaluation of Social Development (CONEVAL) to measure locality-level development.[15] Of all the 20 variables, only one (the proportion of households with a dirt floor) is correlated with the timing of the card rollout at the 5% level, as can be expected by chance (Table 1).[16]

The rollout across these 259 urban localities had substantial geographic breadth and does not appear to follow a discernible geographic pattern (Figure 4a). By the end of the rollout, over one million beneficiaries had received cards (Figure 4b).

Because localities are treated at different points in time, my main estimating equation for the results in this paper is the following event study design, which accommodates the varying timing of treatment and dynamic treatment effects over time:

$$y_{jt} = \lambda_j + \delta_t + \sum_{k=a}^{b} \phi_k D_{jt}^k + \varepsilon_{jt}. \tag{1}$$

In most cases, the outcome y_{jt} is for locality j, and I aggregate high-frequency variables to the two-month period t since Prospera is paid every two months and the administrative data allowing me to determine the timing of the card rollout across localities is at the two-month level. The estimating equation includes locality fixed effects λ_j to capture arbitrary time-invariant heterogeneity across localities and time fixed effects δ_t to capture overall time trends. D_{jt}^k is a relative event-time dummy

[15] I also include baseline levels and trends in the number of bank accounts. I use number of bank accounts rather than number of debit cards because debit cards were only included in the CNBV data beginning in the last quarter of 2008; because the rollout began in January 2009, the baseline trend in debit cards cannot be measured. Nevertheless, any differential trend in debit cards would likely be observed as a differential trend in bank accounts as well, since having a bank account is a prerequisite to having a debit card.

[16] I model the probability of receiving cards in period t among accounts that have not yet received cards by period $t-1$ as a function of baseline locality and account characteristics using a discrete-time hazard model. As in Galiani, Gertler and Schargrodsky (2005) I include a fifth-order polynomial in time.

that equals 1 if locality j received the shock exactly k months ago (or will receive the shock $|k|$ months in the future when $k < 0$).[17]

Since the timing of the shock is not correlated with levels or trends in locality-level financial infrastructure or other observables (conditional on being included in the rollout), but the initial selection of which localities to include in the rollout *is* correlated with locality characteristics, I restrict all estimates to the set of 259 urban localities included in the rollout.

I include 18 months prior to the shock and 24 months after the shock regardless of the data set being used (i.e., $a = -18, b = 24$); when this involves changes in the sample of localities underlying each coefficient (e.g., if a data set begins at the end of 2008, a locality treated in 2009 does not enter into the estimate for $k = -18$ because that locality has no observations in the data set 18 months before it is treated), I also show results for the balanced sample of localities over the more restricted time span for which I can include all localities in the rollout in the estimate of each coefficient. Furthermore, as in most event study specifications (e.g., McCrary, 2007), I do not drop observations that are further than 18 months prior to or 24 months after the shock, but rather "bin" these by setting $D_{jt}^{-18} = 1$ if $k \leq -18$ and $D_{jt}^{24} = 1$ if $k \geq 24$.[18]

Some of the data sets I use are at the municipality rather than locality level. While municipalities are slightly larger than localities, most municipalities are made up of one main urban locality and some semi-urban or rural localities. Indeed, the 259 urban localities included in the debit card rollout belong to 255 distinct municipalities. Thus, aggregating to the municipality level when required by the data is reasonable. In the few municipalities with more than one urban locality, I consider the municipality as treated once at least one locality in that municipality has been treated.

In the data sets in which the time dimension is already aggregated at a level higher than two-month periods, I use these periods as t. For example, the data on the number of Prospera beneficia-

[17] To facilitate discussion I have denoted k as the number of months even though time periods are aggregated to the two-month level; hence, the term $\sum_{k=a}^{b} \phi_k D_{jt}^k$ in (1) is a slight abuse of notation, as it will actually include every other integer between a and b, rather than every integer. Each of these integers would represent a two-month period.

[18] Because I only include localities that were included in the debit card rollout in all event study results, there is no "pure control" group that has $D_{jt}^k = 0$ for all k, as any control localities would differ from treated localities in ways that could have a time-varying effect on the outcomes of interest. When there is no pure control group, "binning" in this way is required in order to identify the calendar time fixed effects (McCrary, 2007; Borusyak and Jaravel, 2016).

ries by locality—which I use to test whether the card rollout was accompanied by an expansion in the number of beneficiaries—is at the annual level, while the CNBV data described in Section B.5 is at the quarterly level. For the annual data, I set $a = -3$ years and $b = 3$ years since there would be few coefficients if I used the standard limits of 1.5 years before and 2 years after the shock.

To test whether the rollout of debit cards was accompanied by an expansion of the Prospera program to additional beneficiaries—which would confound my results as any effect of the card rollout could then merely be an effect of increased transfer income in the locality—I estimate (1) with y_{jt} as the log number of Prospera beneficiaries in locality j (regardless of the method of transfer payment in locality j) in the last payment period of year t. I use years rather than two-month periods since the administrative data on the Prospera rollout is available only at the annual level in 2007 and 2008.[19] Appendix Figure C.3 shows the results: there is no differential change in the number of beneficiaries that occurs at the same time as the card rollout. None of the point estimates either before or after the shock is statistically significant from zero. While I do not have data on the total benefits disbursed in each locality, because benefits are based on a strictly-followed formula, the absence of a differential trend in the number of beneficiary households suggests that there was no differential trend in total transfer payments correlated with the card rollout.

Finally, I ensure that the debit card rollout did indeed increase use of cards at POS terminals (as opposed to beneficiaries only using the cards at ATMs, or continuing to visit bank branches and withdraw all of their transfers in cash). Although use of the cards at POS terminals already depends on the endogenous reaction by the supply side, the fraction of beneficiaries making transactions at POS terminals provides a lower bound on the fraction who wanted to use their debit cards to make purchases. The desire by beneficiaries to use their cards for purchases is a necessary condition for the rollout to have an effect on FinTech adoption on the other side of the market. Since Prospera card transactions equal 0 for every pre-rollout time period for all beneficiaries, I simply graph the proportion of beneficiaries who used their card to make at least one transaction at a POS terminal for each two-month period relative to the card rollout. This analysis uses transaction-

[19]The data correspond to the last payment period of those years; for 2009–2016 I thus use data only from the last payment period of the year to make it consistent with the earlier data.

level administrative data from Prospera.

Figure 5 shows that immediately after receiving a card, about 35% of beneficiaries used their cards to make POS transactions. The proportion actively using the cards increases steadily over time, reaching 48% of beneficiaries after they have had the card for 3 years. Beneficiaries who do not use the card to make purchases at POS terminals instead withdraw their transfer benefits at ATMs or Bansefi bank branches.

5 Results

5.1 POS Adoption by Retailers

Using the data set I constructed on the number of POS terminals by store type by locality over time, combined with administrative data from Prospera on the rollout of debit cards, I estimate the effect of the card shock on the number of POS terminals adopted by corner stores and by supermarkets.[20] I estimate (1) with the log number of POS terminals at corner stores or supermarkets in locality j during two-month period t as the dependent variable. The estimation is restricted to urban localities included in the card rollout; with the exception of the two binned endpoint coefficients, all coefficients are based on a balanced sample of localities (given that the data span 2006–2017 while the rollout was 2009–2012).

For corner stores, the coefficients prior to the debit card shock are all statistically insignificant from 0. Within the first two-month period after cards were disbursed, there is an increase in adoption after the debit card shock of about 3%. This rises to about 18% two years after the shock; all coefficients after the shock are positive and statistically significant for corner stores (Figure 6).[21] For supermarkets, the pre-treatment coefficients are similarly nearly all statistically insignificant from 0, but there is no effect of the card shock. This finding is not surprising as supermarkets already had high rates of adoption prior to the debit card shock: in the National

[20] These are just two of the possible merchant category codes in the data, but they are the codes with the highest aggregate volume of transactions in the data set on the universe of card transactions. For store types that—like supermarkets—already had high levels of POS terminal penetration (such as department stores), results are similar to those for supermarkets.

[21] For all regressions with coefficients that are changes in logs, if we denote the change in logs coefficient as x, the percent changes I report are $e^x - 1$. Appendix Figure C.4 shows the results from the same specification using levels rather than logs of the number of POS terminals.

Enterprise Financing Survey, 100% of supermarkets reported accepting card payments.

An alternative explanation for the response by retailers—which would still represent a general equilibrium response to the card shock but would operate through a somewhat different channel than market-based network externalities—would be if banks responded to the card shock by increasing their efforts to get retailers to adopt POS terminals. In theory, either Bansefi (which would have very specific knowledge about the card rollout) or private banks (which might be able to indirectly observe the card shock) could respond. In practice, however, Bansefi does not offer POS terminals since it is a consumer-facing government social bank. In conversations, Mexico's largest private bank has told me that they were not aware of the specific details of when the cards would be distributed in different localities, and did not run any advertising campaigns or other promotions to increase POS adoption in areas where the card shock occurred.

In Section 6, I explicitly test for two types of potential bank response that I have data for: (i) changes in the marginal transaction fees charged to retailers, and (ii) increased bank investment in areas that experienced the card shock, which could increase the ease of adopting a POS terminal. I do not find evidence of these types of bank response.

5.2 Spillovers to Other Consumers

I test for two types of spillovers to other consumers. First, do other consumers adopt cards after the card shock? This could occur due to indirect network externalities: other consumers benefit from the increase in the number of consumers with debit cards due to the shock because this increases the probability that retailers adopt technology to accept card payments. In the model presented in Appendix A, consumers who adopt due to this network externality are in the region of Figure C.1b marked "Cash $\rightarrow$ card." Second, do some consumers (who likely already had cards) shift some of their consumption from supermarkets to corner stores? In Figure C.1b, these consumers are marked "S $\rightarrow$ C;" they are consumers who have a relatively high cost of traveling to the supermarket, but who also have a high disutility of cash and hence will travel to the supermarket if the corner store does not accept cards.

Spillovers on card adoption. I use the quartlery CNBV data on the number of debit cards by issuing bank by municipality to test for spillovers on other consumers' adoption of debit cards. I once again use specification (1), where the dependent variable is the log number of debit cards, excluding cards issued by Bansefi. Importantly, I am able to exclude cards issued by Bansefi directly in this data set because the data are at the bank by municipality level. The estimation is again restricted to urban localities included in the card rollout.

Figure 7 shows the results: while there is no statistically significant effect on adoption of other cards in the quarter during which the shock occurs, in the following quarter the number of cards issued by other banks increases by 19%. Treated localities have 28% more debit cards issued by other banks two years after the shock.[22]

One possibility is that new cards issued by banks other than Bansefi are not spillovers to other consumers, but are instead being adopted by Prospera beneficiaries or other members of their household (e.g., after they discover the benefits of having a card and thus decide to open a debit card account at a different bank). To rule this out, I use data from the Payment Methods Survey described in Section B.11, where Prospera beneficiaries are asked in mid-2012 (after the rollout) if they have a bank account at another bank, which is a prerequisite to having a debit card from another bank. Just 6% of beneficiaries who receive their Prospera benefits by debit card report having an additional bank account at another bank.

Even though we have already seen that retailers responded to the rollout of Prospera cards, these spillovers to other consumers could be driven by mechanisms independent of the new possibility of paying by card at adopting corner stores. For example, banks could have responded to the Prospera card shock by increasing their advertising or investing in financial infrastructure. Alternatively, the spillover on other consumers' adoption could be caused solely by word-of-mouth learning, where Prospera recipients told their friends and relatives about debit cards once they had received one. I test both of these possibilities in Section 6; although it is difficult to design a test that could definitively rule them out, the evidence suggests that these alternative channels do not explain the

[22]Appendix Figure C.5 shows that results are robust to using the number of credit and debit cards rather than just debit cards.

spillover onto other consumers' card adoption.

Spillovers on consumption choices. To estimate changes in consumption as a result of the card shock, I use the consumption module of the nationally representative ENIGH survey. Because the survey is only conducted once very two years, I use a difference-in-differences rather than event study specification. Continuing to restrict the sample to urban localities included in the rollout, I estimate

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

where y_{it} is the outcome (such as log spending at corner stores or the number of trips per week to corner stores) for household i in survey wave t, $\lambda_{j(i)}$ is a set of locality fixed effects, δ_t is a set of time (survey wave) fixed effects, and $D_{j(i)t} = 1$ if locality j in which household i lives has received the card shock yet at time t.[23]

Table 2 shows how consumers change their consumption in response to the shock, with results from (2). Overall, we see a 7% increase in consumption at corner stores—which we know from the earlier results are more likely to accept card payments after the shock. The point estimate for spending at supermarkets is –2% (not statistically significant). In column 3, we see that although the point estimate of the increase at corner stores is higher than the point estimate of the decrease at supermarkets (columns 1 and 2), we cannot reject no change in overall spending ($p = 0.33$).

The ENIGH survey unfortunately does not ask about bank account or debit card ownership, but it does ask about credit card ownership because goverment authorities were interested in access to credit when designing the survey. I thus test for heterogeneity in the effect by interacting whether the consumer has a credit card with the difference-in-differences dummy $D_{j(i)t}$ (equal to 1 if locality j in which consumer i lives has been hit with the Prospera debit card shock at time t). Because credit card ownership is highly correlated with income, I flexibly control for income to isolate the effect of cards: I include a full set of time by income quintile by credit card fixed

[23]I include locality rather than household fixed effects since the survey is a repeated cross-section rather than a panel at the household level.

effects. Specifically, I estimate

$$y_{it} = \xi_{j(i)c(i)} + \eta_{q(i)c(i)t} + \gamma D_{j(i)t} + \omega D_{j(i)t} \times \mathbb{I}(\text{has credit card})_{it} + \varepsilon_{it}, \tag{3}$$

where the $c(i)$ subscript denotes interacting fixed effects with whether the household has a credit card, $\xi_{j(i)c(i)}$ are a set of fixed effects for locality by whether the household has a credit card, and $\eta_{q(i)c(i)t}$ are a full set of income quintile by whether the household has a card by time fixed effects.

If the change in consumption at corner stores is indeed driven by an influx of new customers with cards, we would expect the interaction term ω to be positive and significant when the outcome is log spending at corner stores. Nevertheless, the non-interacted term γ could still be positive since the credit card dummy does not include everyone with cards (if a household has a debit card but no credit card). Column 2 of Table 2 shows that the change in log spending at corner stores is about 7 percentage points higher for households with a credit card than households without, and the p-value of a test of $\gamma + \omega = 0$ is less than 0.01. This indicates that households with credit cards increase spending at corner stores as a result of the shock. For households without a credit card (some of whom likely have a debit card), the point estimate corresponds to a 5% increase in corner store spending, but is not statistically significant. The coefficient for the effect of the card shock on supermarket spending by those with credit cards, relative to those without credit cards, has the expected negative sign and corresponds to a 4% decrease, but is not statistically significant. For overall spending, the point estimate is close to 0.

To further investigate changes in consumption patterns resulting from the debit card shock and subsequent adoption of POS terminals by small retailers, I also estimate changes in consumption patterns throughout the income distribution. To do this, I estimate

$$y_{it} = \lambda_{j(i)} + \theta_{q(i)t} + \gamma D_{j(i)t} + \textstyle\sum_{q=2}^{5} \psi_q \mathbb{I}(quintile = q)_{it} \times D_{j(i)t} + \varepsilon_{it}, \tag{4}$$

where $\theta_{q(i)t}$ is a full set of income quintile by time fixed effects and $\mathbb{I}(quintile = q)_{it}$ is a set of dummies that equal 1 if household i from survey wave t belongs to income quintile $q \in \{1,2,3,4,5\}$.[24]

Figure 8 shows how consumers in each quintile of the income distribution change their consumption in response to the shock. The richest quintile of consumers reduce their consumption at supermarkets by about 12% in localities affected by the debit card shock and increase their consumption at corner stores by 15%. The second-richest quintile also appears to increase its consumption at corner stores (by 9%, significant at the 10% level), while the results for the poorest three quintiles are statistically insignificant from zero (Figure 8a).

This increase in spending appears to be driven (at least partially) by a change in the number of trips: the richest quintile increases trips to corner stores by 0.8 trips per week and decreases trips to the supermarket by 0.2 trips per week on average (Figure 8b). There is again no effect of the card shock on the number of trips made to corner stores or supermarkets for consumers in the bottom three quintiles of the income distribution.

In Section 6, I test whether a portion of the increase in spending by richer consumers at corner stores could be due to a number of other factors: (i) an increase in overall consumption by the rich (for example, because people are prone to spend more when paying with a card than when paying with cash); (ii) increased prices in response to the shock; and (iii) minimum purchase amounts to pay by card, which could lead consumers to purchase additional items that they wouldn't have otherwise purchased in order to meet the minimum and be able to pay by card. I do not find evidence for any of these alternative channels.

5.3 Retailer Profits

Given that corner stores adopt POS terminals in response to the shock and that richer consumers shift part of their consumption in response to corner store adoption, I now investigate how retailer outcomes are affected using the 2008 and 2013 Economic Census waves. Because these census waves bracket the rollout of cards, and because I don't directly observe whether retailers in the

[24]Income quintiles are estimated separately within each survey year, then grouped together (i.e., $q = 1$ corresponds to the poorest 20% of households in each survey wave). Since all localities included in (4) are treated at some point over the time period covered by the data, there is no term interacting a treatment dummy (always equal to 1 for treated localities) with quintile.

census have adopted POS terminals, I estimate intent-to-treat effects exploiting variation in how long before the 2013 survey wave the shock occurred in a locality. Due to the gradual increase in POS adoption over time in response to the debit card shock, we might expect a larger change in retailer outcomes in localities that received the shock earlier.

I thus restrict the Economic Census to corner stores (which make up over half of all retailers, and which are the store type where we saw an effect on POS adoption) and estimate

$$y_{it} = \gamma_i + \delta_t + \textstyle\sum_k \gamma_k \mathbb{I}(\text{received cards at } k)_{j(i)} \times D_{j(i)t} + \varepsilon_{it} \tag{5}$$

for a number of firm-level outcomes including the inverse hyperbolic sine of profits (for a log-like transformation that allows for negative profit values), log merchandise sales, log merchandise costs, and other input costs. The omitted value of k corresponds to localities that received the card shock toward the end of the rollout—specifically, in the second half of 2011 or in 2012, i.e. 0–1.5 years before the 2013 census wave. I include two other values of k corresponding to localities that received the card shock 1.5–3 years before the 2013 census and those that received the card shock 3–4.5 years before the 2013 census.

I find that corner stores in localities treated 1.5–3 and 3–4.5 years before the second census wave experience increases in profits (over the five-year period between survey waves) of 18% and 27% more than corner stores in localities treated 0–1.5 years before the second census wave (Table 3, panel A). Both merchandise sales and costs appear to follow a similar pattern, but coefficients are statistically insignificant from zero. Pooling the two earlier treatment waves and comparing them in a difference-in-differences framework to the same control group (localities treated 0–1.5 years before the second census wave), the effect on profits of the shock is a 22% increase over the five-year period.[25]

[25]Because these results rely on a balanced sample of retailers, I test whether treatment differentially effects the probability of survival. If treatment increases the probability that firms that would have otherwise experience lower profit growth would exit the market, these results would be biased. To test this, I take all corner stores in from the 2008 census in localities included in the rollout and estimate $Survived_i = \alpha + \sum_k \gamma_k \mathbb{I}(\text{received cards at } k)_{j(i)} + \varepsilon_{it}$, where $Survived_i = 1$ if retailer i survived to the 2013 census wave and 0 otherwise. Appendix Figure C.7 shows that earlier vs. later treatment had a statistically significant effect on the probability of survival.

For increased statistical power and to determine how profits, sales, and costs react shortly after the shock, I bring in corner stores from non-treated localities as a control group and again estimate (5)—with the caveat that these localities differ from treated localities, and hence these results should be interpreted with caution, especially if point estimates differ significantly from those in panels A and B of Table 3. The point estimate for profits in localities treated less than 1.5 years before the 2013 survey wave is negative and economically meaningful (a 7% decrease in profits) but statistically insignificant (panel C). A negative initial effect on profits—net of the fixed cost of technology adoption—would be expected in the presence of credit constraints; this is because the firms induced to adopt the technology did not find it optimal to adopt prior to the shock, meaning that the increase in sales from richer customers was not enough to offset the fixed cost of technology adoption. As in the specification without the corner stores in non-treated localities, profits increase in localities treated 1.5–3 and 3–4.5 years before the shock, by 12 and 22%, respectively.

In the specification with non-treated localities as controls, the point estimates for the increase in log merchandise sales and log merchandise costs are similar to those in panels A and B, but are now statistically significant for corner stores in localities treated 3–4.5 years prior to the 2013 census: merchandise sales increase by 5% and merchandise costs by 4% (panel C).[26] The magnitudes of the increase in sales and costs (4–5%) and profits (22%) are consistent: corner stores are low-margin businesses where sales are on average fifteen times larger than profits.

To further disaggregate the components of corner stores' profits increase after adoption, I also examine other input costs (wage costs and rent), the number of employees, and a proxy for whether the firm is formal. Columns 4–7 of Table 3 show the results. The story that emerges is that corner stores increase their profits by increasing both their purchases of and revenues from merchandise while keeping other input costs (wages and rent) fixed. Similarly, corner stores do not grow in terms of the number of employees. There is some evidence that the card shock leads firms to increasingly formalize: in panel B, for example, the probability of formalization increases by 2.5% percentage

[26] The estimates when we pool all treated localities and compare them to control localities in a difference-in-difference specification are also statistically significant: sales and costs both increase by 3% (panel D).

points on a low base of 12.5%, or a 20% increase in formality.[27] Although it is not possible to disentangle the cause of this increase in formality between formalizing due to adopting the POS terminal or formalizing due to the resulting increase in profits, this higher formality could be an additional benefit for small retailers of the increased FinTech adoption in their area.[28]

Retailer prices. In addition to the demand shock that corner stores experience by attracting new customers after they adopt POS terminals, the profits effect could be driven by an increase in prices. The model in Appendix A predicts an ambiguous price effect under imperfect competition, since the demand shock could be offset by an increase in the density of marginal consumers who will choose the supermarket over the corner store if the corner store increases prices.

To empirically test for a price effect, I estimate a variant of (1) with the product-by-store level price data used to construct Mexico's CPI. The data are now at the product-by-store level rather than the locality level; hence, I use the same specification as Atkin, Faber and Gonzalez-Navarro (2018) use with Mexico's micro-CPI data:

$$\log Price_{gst} = \eta_{gs} + \delta_t + \sum_{k=a}^{b} \phi_k D^k_{m(s)t} + \varepsilon_{gst}, \tag{6}$$

where $Price_{gst}$ is the price of barcode-level product g at store s at time t, η_{gs} are product-by-store fixed effects, and δ_t are two-month period time fixed effects.

Figure 9 shows that there is no price response at corner stores or supermarkets. All of the ϕ_k coefficients are statistically insignificant from zero, both before and after the card shock. Furthermore, using each estimate's 95% confidence interval, I can rule out price effects outside of the range $[-1.6\%, 1.2\%]$ during the first ten months after the shock and outside of the range $[-1.7\%, 2.6\%]$ during the first two years after the shock.[29]

[27]Because formality is not directly asked about in the survey, I use a proxy that defines a firm as formal if it reported charging value added tax (VAT) to its customers, or if it reported costs from paying social security for its employees. The point estimates in the specifications using the (not as comparable) non-treated localities as a control group see coefficients on formality close to and statistically insignificant from 0.

[28]Higher formality can also lead to higher costs from tax payments, but these costs are already subtracted out of the profits measure I use here. Indeed, firms in treated localities pay 13% more VAT. I cannot disentangle whether the increase in VAT paid by retailers is due to higher rates of formality or higher profits.

[29]For comparison, Atkin, Faber and Gonzalez-Navarro (2018) find that when Walmart enters a municipality, prices

6 Alternative Explanations

6.1 Bank Response

The effect of the debit card shock on POS adoption could be driven by a response from banks rather than a direct response from merchants to the increased demand for card payment technology. I test two ways banks could respond to the debit card shock that would potentially affect POS terminal adoption. First, they could decrease the marginal cost of each transaction on a POS terminal. Second, they could increase their presence in localities that experienced the debit card shock, which may reduce the indirect costs of adoption (e.g., traveling to the bank branch to sign a contract) or may increase knowledge about the technology.[30]

Marginal fees. To test whether banks adjusted their marginal fees charged on POS terminal transactions in response to the shock, I use data from Mexico's Central Bank on the marginal merchant fees charged by each bank over time. These fees are regulated by the Central Bank, and although they vary across banks and types of merchant (e.g., gas stations, fast food, retail), each bank must set a consistent national price for each merchant category. Nevertheless, banks with a larger presence in areas affected by the debit card shock could potentially change their fees to encourage POS adoption. I thus compute the average merchant fee in each locality as an average across the fees charged by each issuing bank with a branch presence in the municipality.

Appendix Figure C.8 shows the results from (1) where the dependent variable is the log average fee charged by banks operating in municipality m during quarter t. There is no differential change in the marginal fees charged by banks for POS transactions before or after the debit card shock: the point estimates are all statistically insignificant from 0, close to 0, and have tight confidence intervals: we can rule out changes in the fee of more than about 1% of the initial fee—i.e., we can rule out changes larger than about 2 basis points.

at traditional retailers fall by 3–4%.

[30] Banks could also decrease the fixed cost and monthly fee for a POS terminal, or they could increase their advertising, targeted at either retailers or consumers. While I do not have data to test these two possibilities, in conversations with executives at Mexico's largest private bank, they reported not following these strategies. The monetary costs of adopting a POS terminal—like the marginal transaction fee—are set nationally by each bank.

Bank presence. Banks could increase their presence in localities that received the card shock, by establishing additional branches in those localities. Bansefi, the government bank issuing cards to Prospera beneficiaries, only has about 500 branches in Mexico and rarely closes or opens a branch—indeed, the number of branches stayed nearly constant at around 500 during the entire rollout. In this section, I test whether *commercial* banks increased their presence in localities that received the card shock, perhaps anitcipating an increase in demand for financial services. I use CNBV data on the number of bank branches in each municipality over time.

Appendix Figure C.9 shows the results from (1) where the dependent variable is the log number of commercial bank branches in municipality m during quarter t. The point estimates are statistically insignificant from 0 in all periods, and the pooled difference-in-differences coefficient is close to zero, at 0.005 (also statistically insignificant).

6.2 Word-of-mouth Learning

It is possible that the spillover effect onto other consumers' adoption of cards occurs purely through social word-of-mouth learning, independently of whether corner stores start adopting point-of-sale terminals in response to the shock. While this would still constitute spillover effects of the government policy to provide debit cards to cash transfer beneficiaries, the channel would be different from network externalities through the two-sided market.

Directly testing this alternative is difficult, since many of the pathways through which word-of-mouth learning would occur—for example, among people with close geographic proximity and of similar social status—are also the channels through which the network externality would occur (since other customers adopting cards would need to shop at the same corner stores as beneficiaries to experience the network externality). Nevertheless, I present a number of tests that do not definitively rule out word-of-mouth learning but that, taken together, are suggestive that this is not the channel through which the spillovers occur.

Before turning to those tests, it is worth noting that debit cards are not a new technology. In urban Mexico in 2009, knowledge of the existence of debit cards and the ability to make card payments at POS terminals was likely high even among poorer households. Hence, any word-of-mouth learning effect would likely need to be learning from friends or contacts about the *benefits* of using cards, not their existence as a technology.

Heterogeneity in where beneficiaries shop. In some localities, the majority of beneficiaries live close to supermarkets and thus have a low relative cost of traveling to the supermarket. Because supermarket adoption of POS terminals is near-universal (indeed, 100% of supermarkets in the National Enterprise Financing Survey report accepting card payments), the network externality channel would not occur in places where beneficiaries shop at supermarkets. Thus, if network externalities explain the effect on other consumers' card adoption, we would not expect to see other consumers adopting cards in localities where beneficiaries shop at supermarkets. The effect would instead be concentrated only in localities where beneficiaries shop at corner stores.

On the other hand, if the effect were driven by social learning, we would expect other consumers to adopt cards regardless of whether the locality is one in which beneficiaries shop at supermarkets. I use the revealed preference shopping patterns of beneficiaries within the first 6 months they have the card to split the municipalities into two equal-sized groups: those in which the proportion of debit card transactions made by beneficiaries at super markets is high, and those in which it is low (where "high" and "low" are defined as above or below the median). I use the *proportion* of their debit card transactions made at supermarkets to abstract away from differences across localities in the overall propensity to use a card. To divide the municipalities in this way, I use the Bansefi transactions data to measure the total number of transactions made by beneficiaries during their first six months with the card at each type of store. I then estimate (1) with log debit cards from other banks as the dependent variable, separately in municipalities above and below median preference for supermarkets.

Appendix Figure C.11a shows the results: in municipalities where beneficiaries prefer shopping at supermarkets (which already accepted cards), there is no statistically significant effect on other consumers' card adoption. Furthermore, the (statistically insignificant) point estimates never exceed 0.13, which would indicate a 14% increase in cards.[31] This is inconsistent with word-of-

[31] Note that some beneficiaries still shop at corner stores in these municipalities, so we would not expect precise 0 point estimates.

mouth learning, unless learning had already occurred in these localities due to people's preference for supermarkets—a possibility I return to below. In contrast, in municipalities where beneficiaries have a low preference for supermarkets (and hence shop at corner stores, where the network externality can occur), we see a large effect on other consumers' card adoption (Appendix Figure C.11b). The effect in these municipalities is statistically significant in all quarters after the initial quarter in which the shock occurs, and the point estimate reaches 0.47 two years after the shock.

These results could still be consistent with learning if (i) beneficiaries' social contacts also shop more at supermarkets in municipalities in which beneficiaries shop more at supermarkets, which is likely; and (ii) these contacts had thus already learned about the existence or utility of using cards, in which case the ones who would adopt cards with perfect information would have already adopted cards. Consistent with this possibility, baseline adoption of other banks' cards is *slightly* higher in municipalities where supermarkets are preferred by beneficiaries (at 0.50 debit cards per person compared to 0.43). However, we can directly test this possibility by further splitting the municipalities in which beneficiaries prefer supermarkets into those with above-median and below-median baseline cards per person.[32] In the municipalities where beneficiaries prefer supermarkets but the concentration of debit cards is below-median, we would still expect room for learning—since under this alternative explanation there was room for learning in municipalities where beneficiaries preferred corner stores but their contacts' baseline adoption was similar or higher. Appendix Figure C.12 shows that even in these municipalities with low baseline card adoption but where beneficiaries prefer supermarkets, there is no spillover on other consumers' card adoption. The point estimates are again statistically insignificant and do not exceed 0.14.

Timing of effect. In this test for word-of-mouth learning, I exploit heterogeneity in how long after the card shock corner stores began adopting point of sale terminals. Because the data on other consumers' card adoption is quarterly while the results on POS adoption were aggregated to two-month periods (since Prospera payments are made every two months), for this test I aggregate to

[32]The median is defined within the subset of localities where beneficiaries prefer to shop at supermarkets.

the six-month level to have a common time period across data sets. In addition, I aggregate data on the card rollout and POS adoption to the municipality rather than locality level since the data on other card adoption is at the municipality level. If the spillover on other consumers' card adoption were driven by word-of-mouth social learning independent of a network externality through POS terminals, we would expect to see the effect on learning occur shortly after the card shock regardless of whether there is a delay before corner stores respond. If, on the other hand, the spillovers are caused by network externalities, we should only see a response in card adoption during the first period after the card shock in places where we also see no delay in retailers' response to the card shock.

Specifically, I split the sample into a set of municipalities in which corner stores respond to the card shock within the first six months after the shock, and municipalities in which there is a delay of between six months and one year before corner stores respond. A response is defined as an increase in the number of corner stores with POS terminals; this occurs during the first six months after the card shock in 146 of the 255 treated municipalities; in another 21 municipalities the response by corner stores occurs during the following six months, while in the remaining 88 treated municipalities there is not an increase in the number of corner stores with POS over the first year after the shock.

Due to the smaller samples in this test, I create dummy variables for just three broad periods: the 6-month period prior to the card shock, the first 6-month period when cards are received (since I am exploiting heterogeneity in whether retailers responded during this period), and all following periods. I then estimate (1) with the outcome variable as the log stock of debit cards issued by banks other than Bansefi—as in Section 5.2—separately for the two sets of municipalities described above. The omitted period in the regression is the 6-month period before the shock, and periods further before the shock are not included. This test thus has the flavor of a Granger causality test, where I retain the event study framework rather than perform the usual Granger test since the network externality channel still implies a feedback loop between card adoption and POS adoption.

Table 4 shows the results: in municipalities in which retailers responded during the first six months after the card shock, we also see an 11% increase in cards issued by other banks during those first 6 months after the card shock (significant at the 5% level). In the following periods, even more cards are adopted, and the stock of other bank debit cards is 26% higher (significant at the 10% level). On the other hand, in municipalities in which retailers did not respond during the first six months, we see no spillover over the same time period to other consumers' card adoption: the point estimate is statistically insignificant from 0 (and slightly negative, at –2%). Despite the small sample, we can rule out an effect larger than a 5% increase in the number of cards during that initial six-month period. For the initial six-month period, we can also reject (at the 5% significance level) equality in the effect sizes in municipalities in which retailers responded within the first six months and municipalities in which they responded between six months and one year after the shock (column 3). As expected, the point estimate for other card adoption in periods at least six months after the card shock in this second set of municipalities—i.e., the estimate for time periods once retailers have responded in these municipalities—is positive and similar in magnitude to the first period in municipalities where retailers responded quickly, at 13%.[33]

6.3 Alternative Explanations of Increase in Corner Store Consumption

Increase in overall consumption. An alternative possible explanation for the increase in the consumption of the richest quintile at corner stores following corner stores' POS adoption is that these consumers already shopped at corner stores, but now switch to using cards, which affects their total spending. This would be consistent with lab experiments finding that consumers spend more when paying by card than when paying by cash (Raghubir and Srivastava, 2008). However, the increase in corner store consumption by the rich cannot be entirely driven by the rich spending more overall, given two results from Section 5.2. First, the increase in consumption at corner stores by the richest quintile was accompanied by a decrease in consumption at supermarkets by those same consumers; furthermore, it was accompanied by an decrease in the number of trips to corner stores. Second, there was no statistically significant increase in overall consumption in

[33]This estimate is statistically insignificant from zero, which is not surprising given the small number of municipalities (21) in this regression.

Table 2. If we zoom into the richest quintile's change in overall consumption by estimating (4) with log total spending as the outcome variable, the point estimate for the richest quintile is 0.03 with a p-value of 0.46. Thus, the richest quintile—which is the income group we see most clearly substituting consumption from supermarkets to corner stores—does not appear to increase its total consumption as a result of the card shock.

Prices. In Section 5.3, I tested for a price effect using high-frequency product by store by week price data, and found no evidence of a change in prices at either corner stores or supermarkets in response to the shock. Nevertheless, because not all corner stores adopt POS terminals in response to the shock, those results were intent-to-treat, and there is a possibility that we would not detect a price effect even if it occurred. In this section, I use two additional tests to see if the increase in consumption at corner stores can be explained by an increase in prices at those corner stores.

For food items purchased in the ENIGH, the quantity purchased is also recorded, and follow-up questions are included so that this quantity can be converted into kilograms or liters. Thus, I construct a measure of the total quantity of food purchased, where quantity is measured as the sum of kilograms and liters (depending on which unit a particular food item is measured in). Appendix Figure C.10 shows that it is not just the amount spent (price $\times$ quantity) at corner stores that increases for the richest quintile, but also the quantity purchased. Specifically, the richest quintile increases quantity purchased from corner stores by 16% and decreases quantity purchased from supermarkets by 15%.

Minimum card payment amounts. In the US, it is common for small stores to impose a minimum payment by credit or debit card (and legal for retailers to impose these minimums up to $10 under the Dodd-Frank Wall Street Reform and Consumer Protection Act). However, this is a result of the transaction fee structure that retailers face in the US, which includes a fixed cost of $0.10 per transaction plus a marginal fee (currently 1.51%). Thus, the proportional cost of the transaction—combining these two fees—is decreasing in the transaction amount, which motivates retailers to impose a minimum payment by card. In Mexico, on the other hand, the fee structure does not include a fixed cost; instead, there is only a marginal fee of 1.75% for retail (for POS terminals issued by Mexico's largest bank), which means that the fees are proportional to the transaction amount regardless of transaction size. Thus, retailers in Mexico do not have the same incentive to impose minimums.

It is nevertheless an empirical question whether many Mexican retailers impose minimums in practice. Appendix Figure C.13 shows a histogram of debit card transaction amounts for transactions made at POS terminals by Prospera beneficiaries. There is no evidence of retailers imposing a minimum payment: about 10% of all transactions are between 0 and 20 pesos, which is less than $2.

7 Conclusion

Due to the network externalities of financial technologies—which arise from the interactions between consumers' and retailers' financial technology adoption decisions—the spillovers of consumers' FinTech adoption could be large. As a result, assessing the overall effects of increased financial inclusion of the poor requires us to quantify not only how the supply side of the market responds, and how this supply side response feeds back to the demand side.

I exploit a natural experiment that causes shocks to the adoption of a particular financial technology—debit cards—over time and space. When the Mexican government provided debit cards to cash transfer recipients in urban areas, small retailers responded to the increased demand for card payments (and the threat that customers would shop elsewhere if they didn't adopt) by adopting point-of-sale terminals to accept card payments. Two years after the shock, the number of POS terminals in treated localities had increased by 18% relative to not-yet-treated localities. Other consumers responded to the increase in FinTech adoption by retailers in two ways. Some—who likely already shopped at the corner stores that were now adopting POS terminals—adopted debit cards. Richer consumers—who likely already had cards—shift 13% of their supermarket consumption to corner stores. Corner stores, in turn, benefit from the demand shock: their profits increase due to an ability to turn over more inventory, increasing both sales and merchandise costs by about 3% while keeping other input costs fixed.

Governments and non-governmental organizations (NGOs) around the world are increasingly fostering financial technology adoption by their poorest citizens, often by paying government welfare payments into bank accounts tied to debit cards (Muralidharan, Niehaus and Sukhtankar, 2016), or into mobile money accounts that can be accessed through a mobile phone and network of deposit/withdrawal agents (Haushofer and Shapiro, 2016). However, because many financial technologies have indirect network externalities arising from two-sided markets, recipients only benefit from these technologies if the other side of the market has adopted the corresponding technology. While the motives of governments and NGOs for using these technologies to pay cash transfer recipients is often to reduce administrative costs and leakages to corrupt officials, by lowering the costs of adopting financial technology and coordinating the simultaneous adoption of many consumers, they might inadvertently also overcome market failures arising from network externalities in two-sided markets. This, in turn, could incentivize technology adoption on the other side of the market without any further government intervention. In other words, government policy that spurs adoption on one side of the market can lead to dynamic, market-driven FinTech adoption on both sides of the market that benefits both consumers and retailers.

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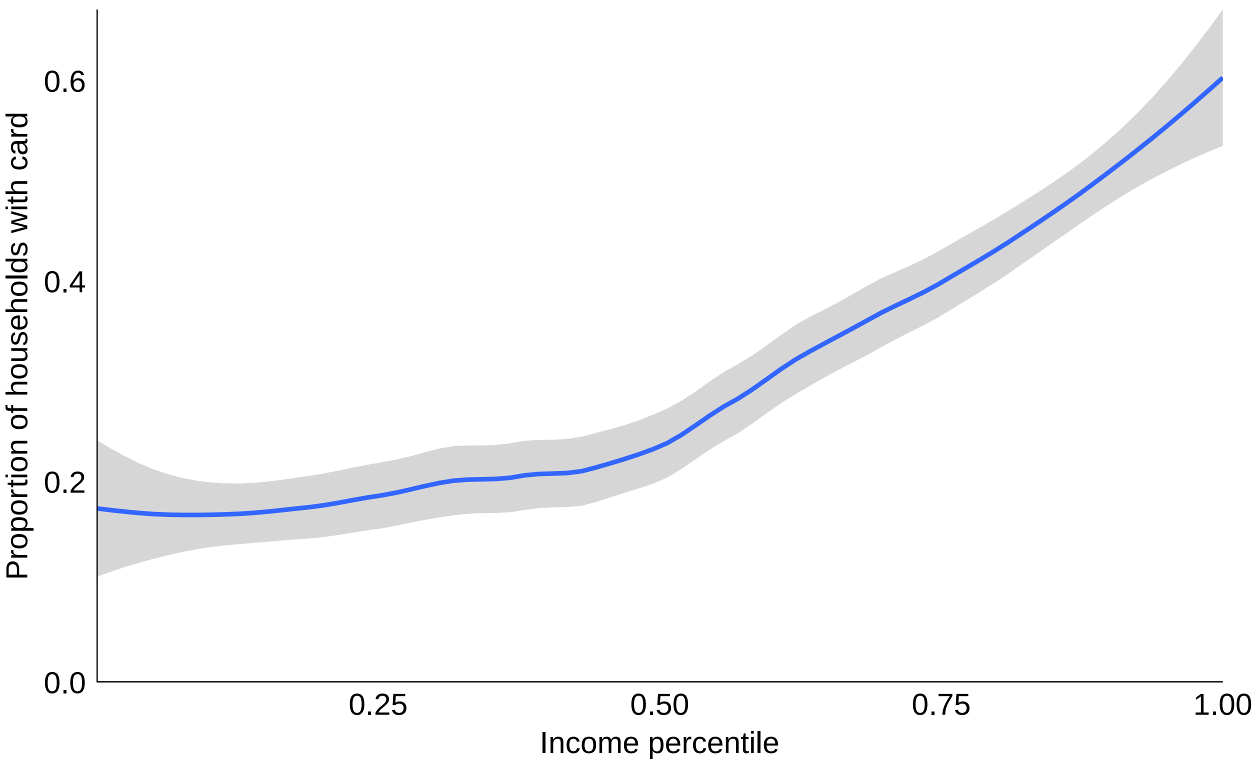
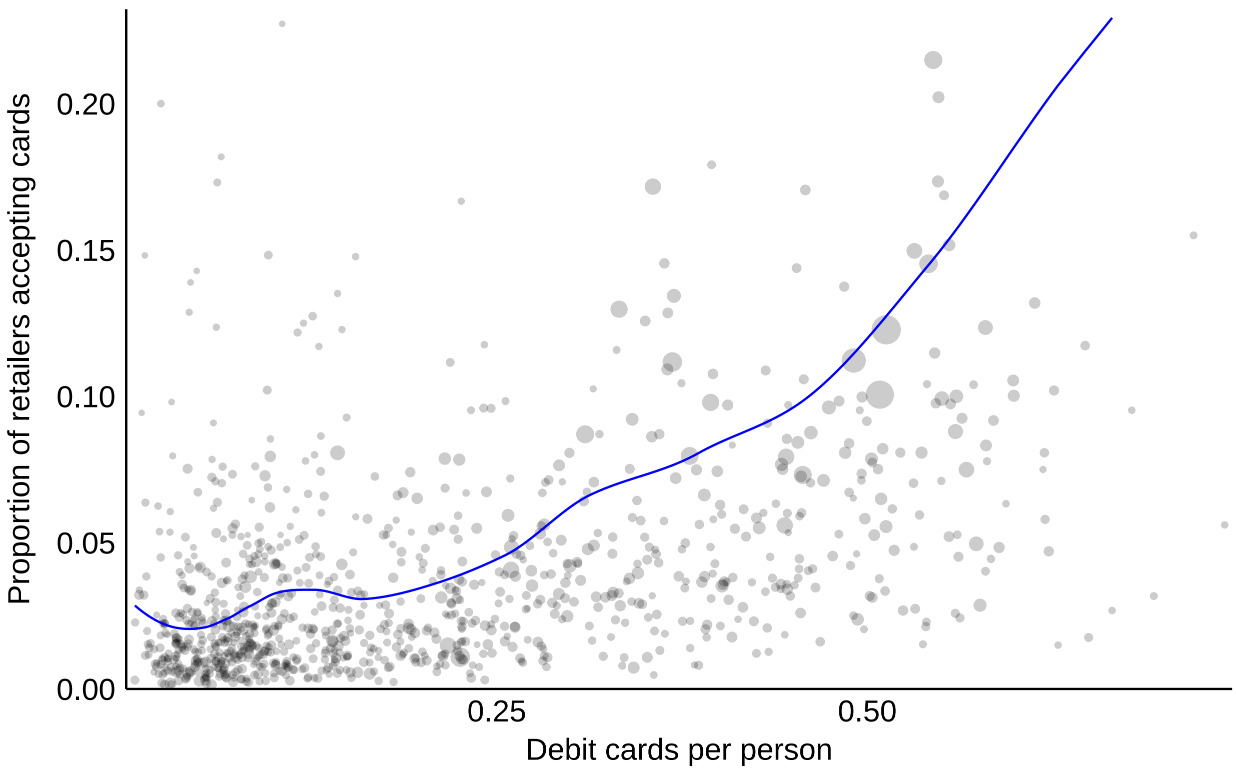
Figures and Tables

Figure 1: Proportion of urban households with debit or credit cards



This figure shows the proportion of urban households with a debit or credit card across the income distribution. The data are restricted to households in urban localities (i.e., localities with at least 15,000 inhabitants), and income percentiles are defined within the set of urban households. The data source is the 2009 Mexican Family Life Survey.

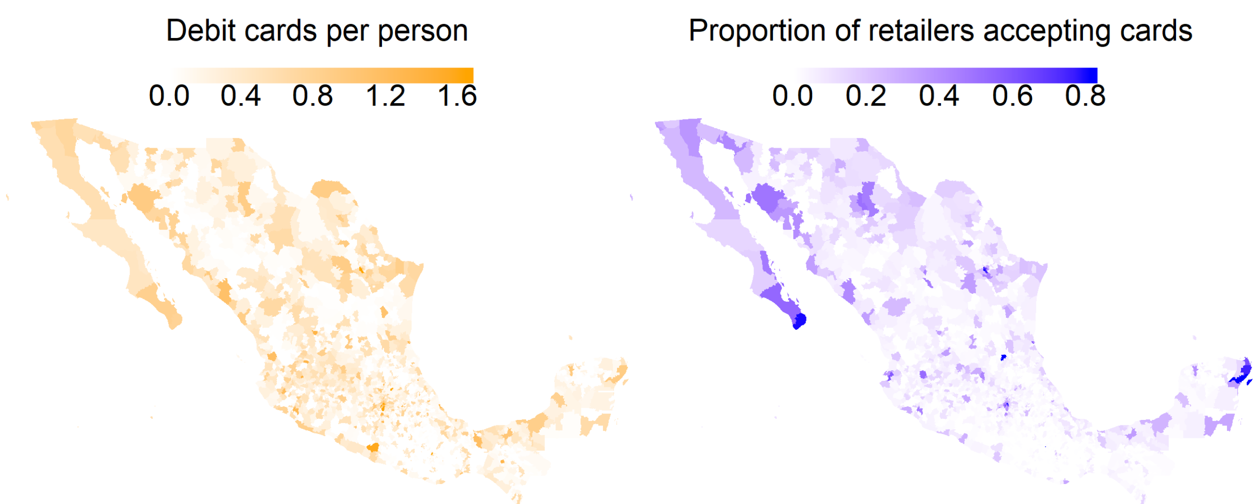
Figure 2: Cross-sectional correlation between adoption of cards and POS terminals



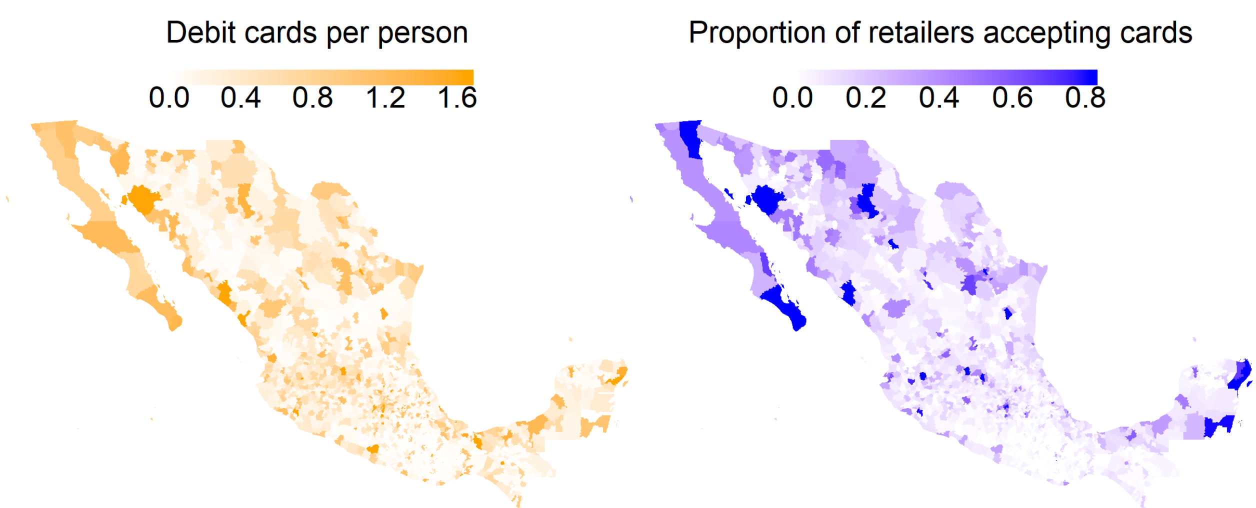
This figure shows the proportion of retailers accepting cards (constucted as the number of businesses with POS terminals using CNBV data divided by the number of retailers using INEGI data) and the number of debit cards per person (constructed as the number of debit cards using CNBV data divided by the population using INEGI data) in April 2011, the first month for which both figures are available from the CNBV data. Each is measured at the municipality level. Each dot is a municipality and the size of the dots is proportional to municipality population. The fitted curve is a local polynomial regression of degree 2, weighted by municipality population. For legibility, the top 5% of observations on each axis are excluded.

Figure 3: Concentration of cards and POS terminals over space and time

(a) April 2011



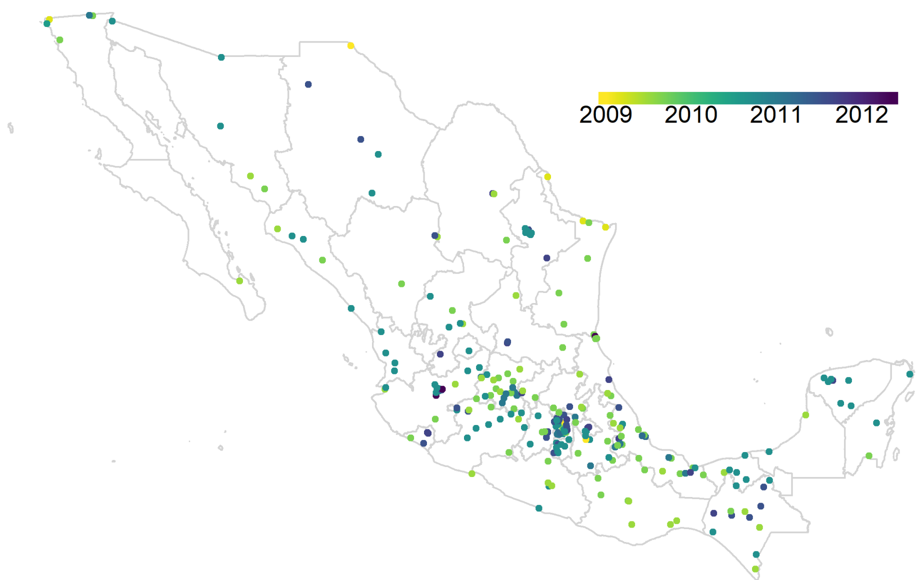
(b) December 2016



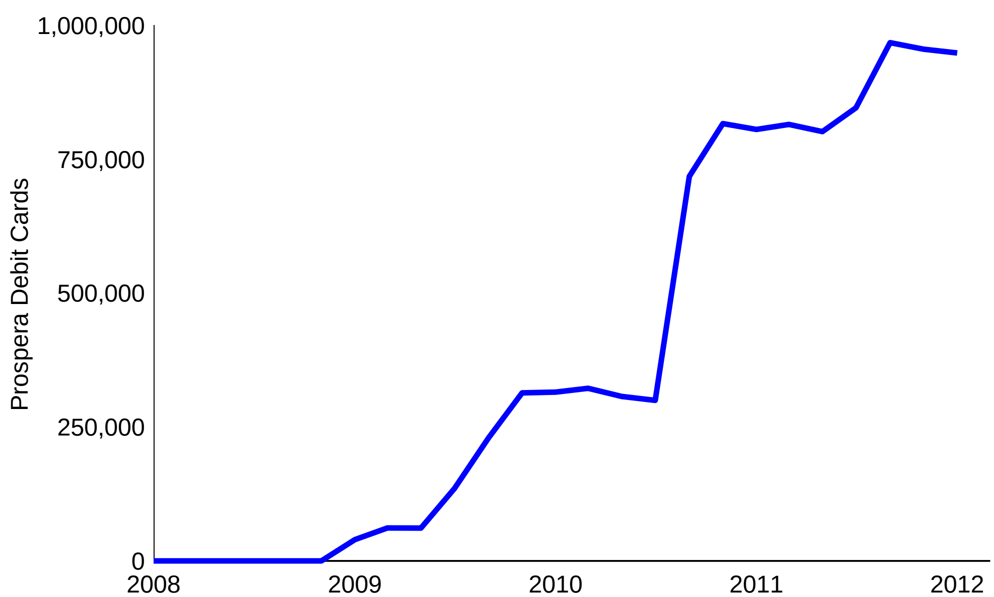
This figure shows the municipality-level number of debit cards per person (constructed as the number of debit cards using CNBV data divided by the population using INEGI data) and proportion of retailers accepting cards (constucted as the number of businesses with POS terminals using CNBV data divided by the number of retailers using INEGI data).

Figure 4: Debit card rollout over space and time

(a) Rollout over space and time

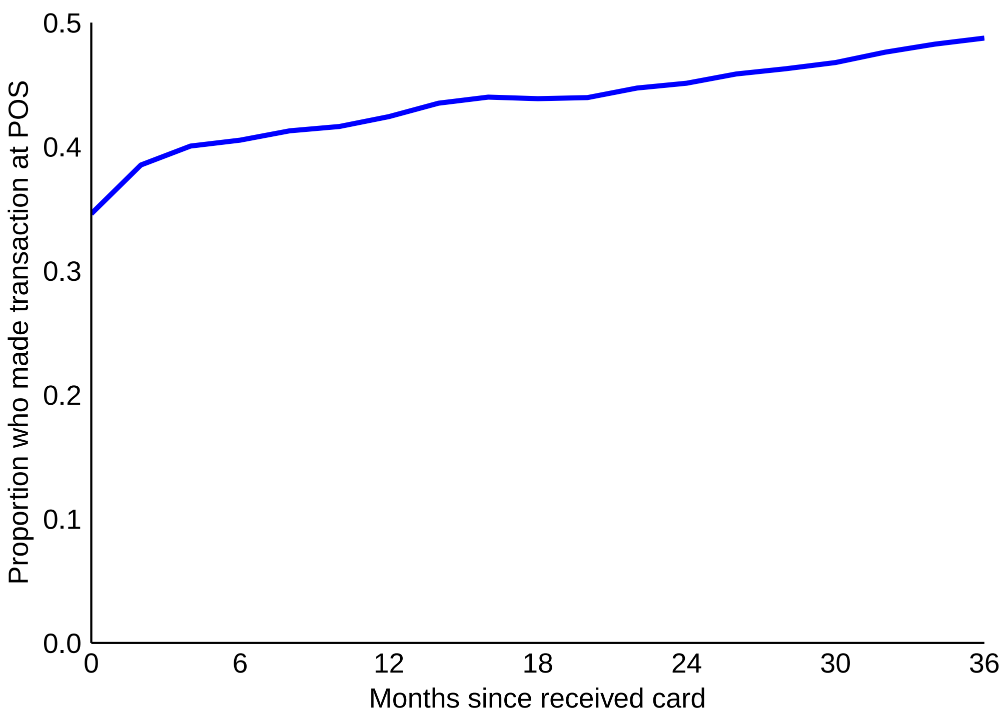


(b) Number of households treated over time



This figure shows when beneficiaries in each urban locality received debit cards from Prospera. It uses administrative data from Prospera on the number of beneficiaries and payment method in each locality during each payment period, which I used to impute when the debit card shock occurred in each locality; it also uses locality and state shapefiles. $N = 259$ urban localities in card rollout.

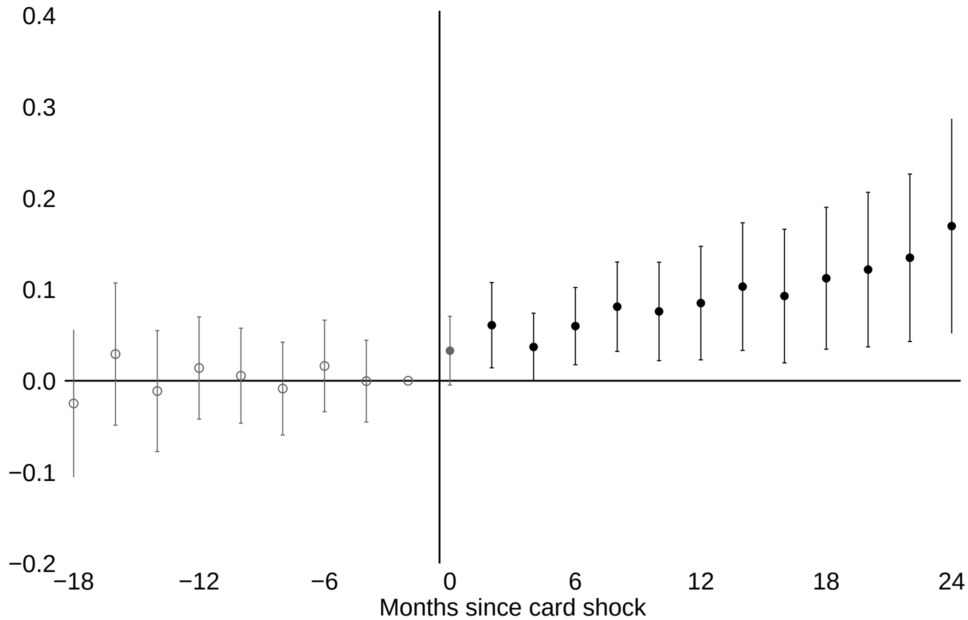
Figure 5: Proportion of Prospera cardholders who make transactions at POS terminals



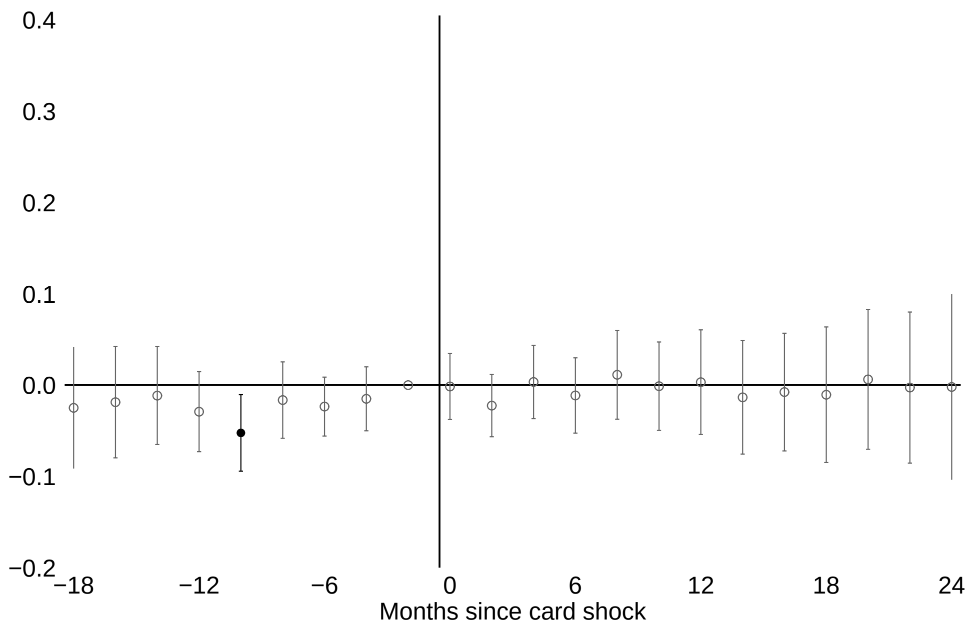
This figure shows the proportion of Prospera cardholders who make at least one transaction at a POS terminal using their card during each two-month period. Periods are binned in two-month intervals because the cash transfer is paid every two months. Source: data on 106,449,749 transactions from 961,617 Prospera accounts.

Figure 6: Effect of card shock on POS adoption (event study estimates)

(a) Corner stores

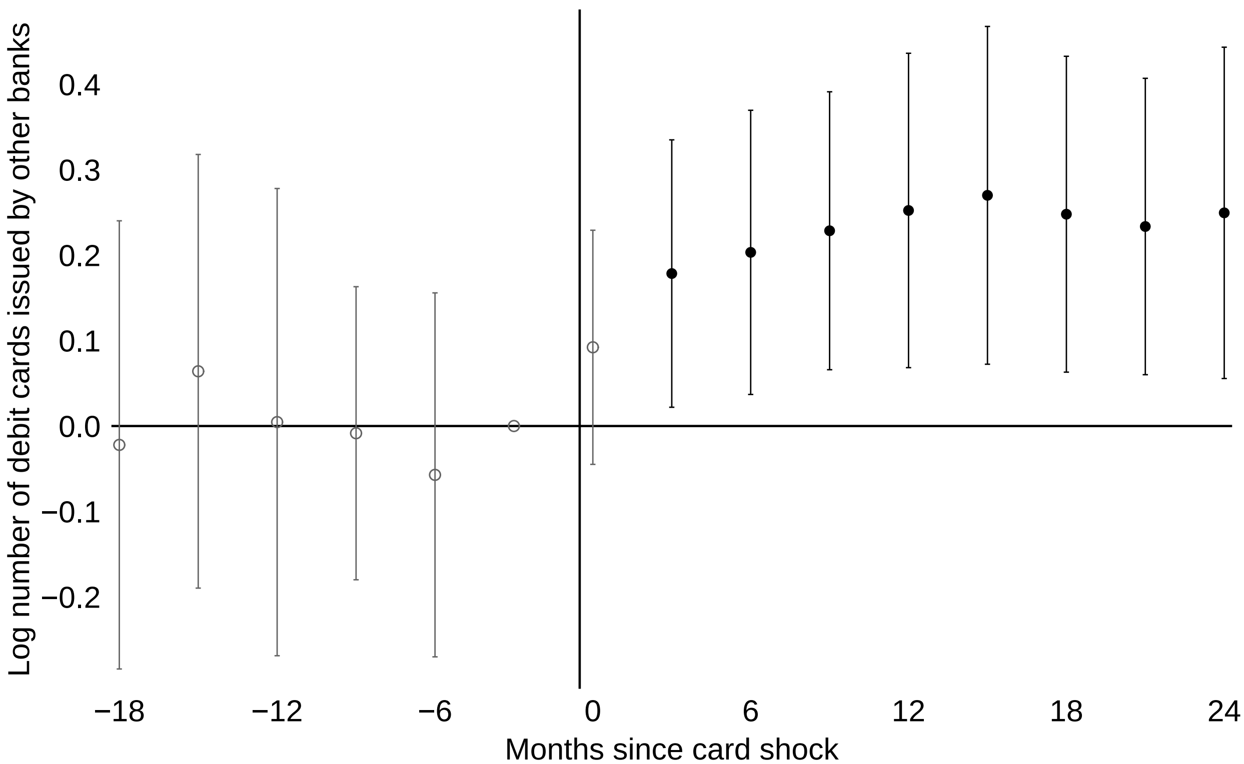


(b) Supermarkets



This figure shows the effect of the debit card shock on the stock of point-of-sale (POS) terminals at corner stores (panel a) and supermarkets (panel b). Corner stores respond to the debit card shock by adopting POS terminals. The dependent variable is the log number of point of sale terminals by type of merchant (corner store or supermarket). Observations are at the locality by two-month period level. Data is the universe of POS terminal adoptions. $N = 8806$ locality by time observations from 259 localities. The same results can be found in table form in Table C.1.

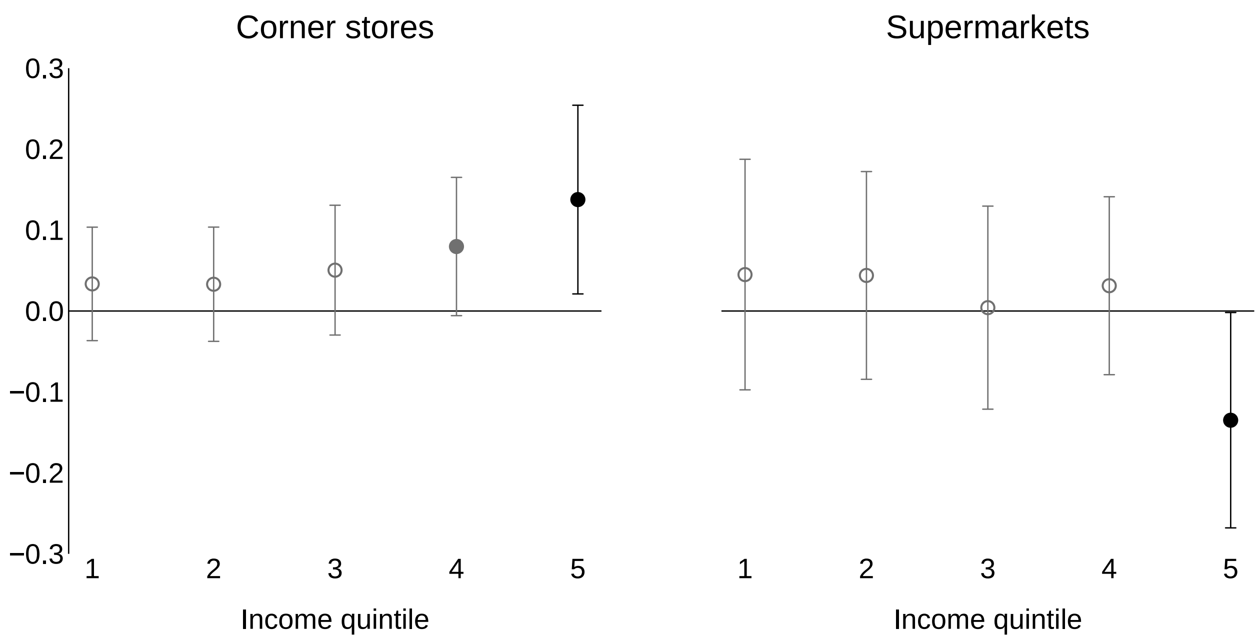
Figure 7: Spillover effect to other consumers' card adoption (event study estimates)



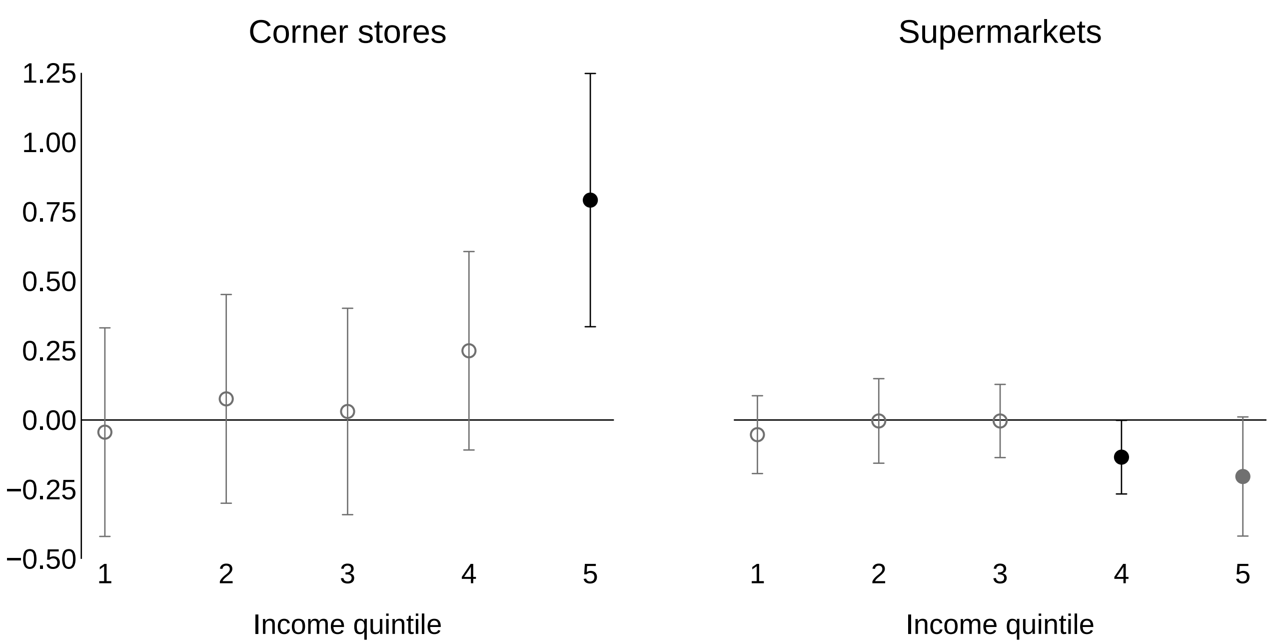
This graph shows that adoption of debit cards at other banks increases after the debit card shock. It graphs the coefficients from (1), where the outcome variable is the log stock of debit cards issued by banks other than Bansefi in municipality m in quarter t; this variable comes from the CNBV data. $N = 8243$ municipality by quarter observations from 255 municipalities. Pooled difference-in-differences coefficient = 0.189^{***} (0.067).

Figure 8: Effect of corner store POS adoption on consumer choice

(a) Log spending



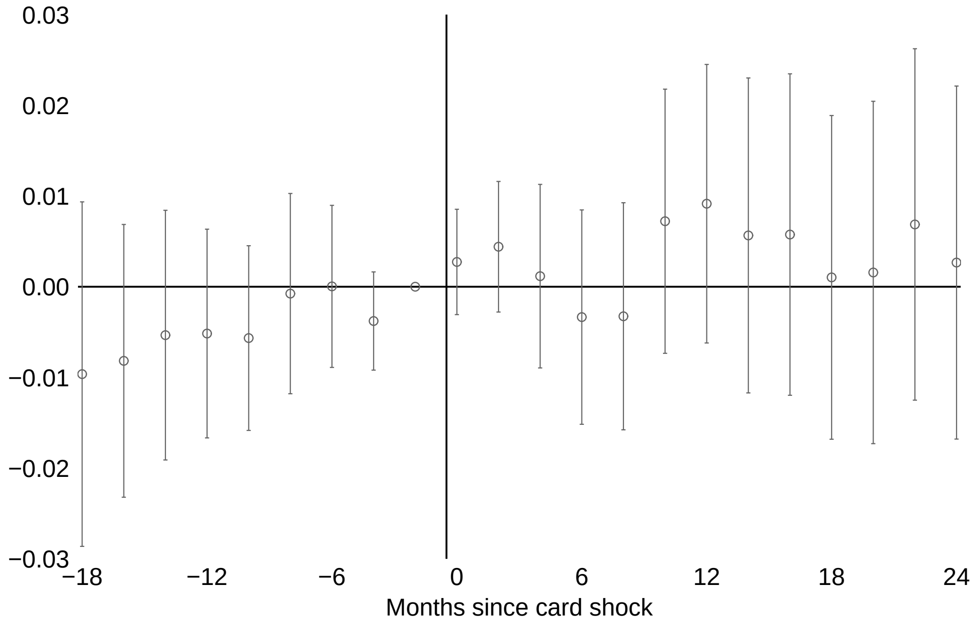
(b) Number of trips



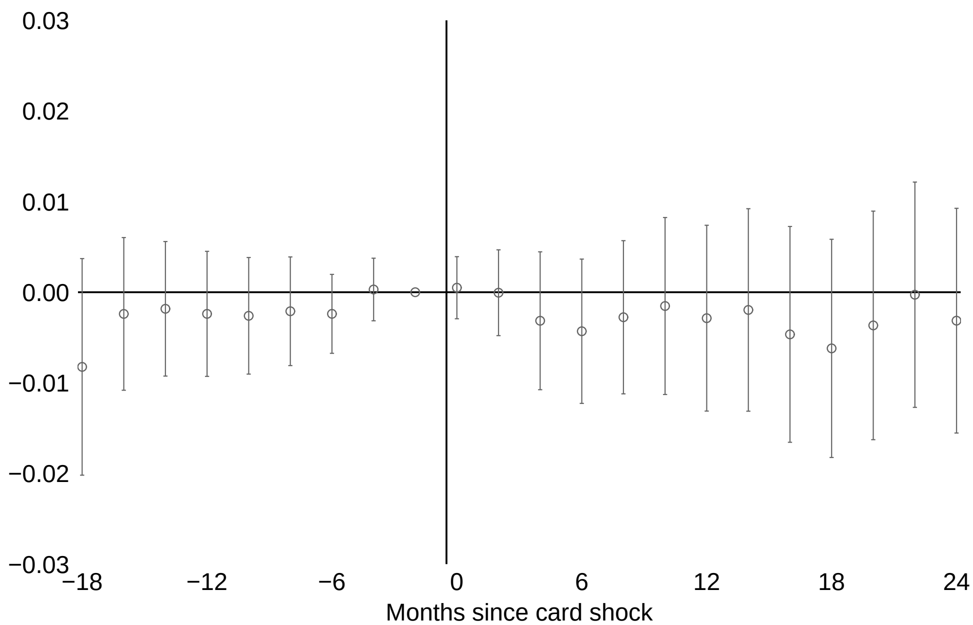
This figure shows that richer consumers substitute spending from supermarkets to corner stores (panel a), and that this is driven at least in part by a change on the extensive margin in the number of trips they make to each type of store (panel b). The figure graphs coefficients from (4) where the outcome variable is log spending in pesos at the particular store type (supermarkets or corner stores) in panel a, and number of trips over the course of one week in panel b. $N =$ 49,810 households from 220 localities.

Figure 9: Price effects

(a) Corner stores



(b) Supermarkets



This figure shows that neither corner stores nor supermarkets change prices in response to the debit card shock. It shows the results from (6), where the outcome variable is the log price of barcode-level product g at store s at time t. The regression is run separately for corner stores and supermarkets. The data are microdata used to construct Mexico's Consumer Price Index; the data were collected by Mexico's Central Bank from 2002–2010 and by INEGI from 2011–2014. (a) $N = 531{,}762$ product by store by month observations from 72 municipalities; (b) $N = 979{,}108$ product by store by month observations from 64 municipalities.

Table 1: Correlation between pre-rollout locality characteristics and timing of card shock

Variable	(1) Mean	(2) Standard Deviation	(3) Discrete Time Hazard: Linear Probability	(4) Discrete Time Hazard: Proportional Hazard
Log point-of-sale terminals	4.47	2.11	−0.0003	0.0047
			(0.0093)	(0.0837)
Δ Log point-of-sale terminals	0.81	0.38	−0.0266	−0.2534
			(0.0179)	(0.1549)
Log bank accounts	9.27	3.27	0.0062	0.0535
			(0.0054)	(0.0451)
Δ Log bank accounts	1.78	3.61	0.0055	0.0620
			(0.0067)	(0.0562)
Log commercial bank branches	2.58	1.42	−0.0215	−0.2047
			(0.0185)	(0.1495)
Δ Log commercial bank branches	0.61	0.95	−0.0224	−0.2581
			(0.0248)	(0.2192)
Log Bansefi bank branches	0.58	0.41	0.0072	0.0812
			(0.0246)	(0.2039)
Log commercial bank ATMs	3.15	1.74	0.0108	0.0980
			(0.0095)	(0.0940)
Log population	11.26	1.24	0.0101	0.0919
			(0.0160)	(0.1340)
% illiterate (age 15+)	6.14	3.69	0.0005	0.0053
			(0.0049)	(0.0422)
% not attending school (age 6-14)	4.15	1.65	0.0006	−0.0013
			(0.0092)	(0.0808)
% without primary education (age 15+)	40.98	9.59	0.0015	0.0139
			(0.0019)	(0.0169)
% without health insurance	45.68	16.15	−0.0011	−0.0104
			(0.0008)	(0.0065)
% with dirt floor	5.28	4.83	0.0051**	0.0497**
			(0.0024)	(0.0197)
% without toilet	5.89	3.60	−0.0061	−0.0500
			(0.0041)	(0.0344)
% without water	6.45	9.12	−0.0007	−0.0060
			(0.0010)	(0.0094)
% without plumbing	3.94	6.39	0.0020	0.0164
			(0.0015)	(0.0129)
% without electricity	4.29	2.24	0.0049	0.0426
			(0.0047)	(0.0403)
% without washing machine	33.64	14.33	−0.0003	−0.0038
			(0.0010)	(0.0093)
% without refrigerator	16.80	9.73	0.0006	0.0034
			(0.0016)	(0.0147)

$N = 240$ localities in the debit card rollout (where 19 treated localities are missing data for some of the financial infrastructure variables and are hence excluded), and 1851 locality by two-month-period observations in columns 3 and 4. 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 linear probability discrete time hazard and a discrete proportional hazard using complementary log-log regression (respectively), both with a 5th-order polynomial in time. 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 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 POS terminals is from the POS adoption data 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. I do not include trends in Bansefi bank branches or commercial bank ATMs because these variables are first available in the last quarter of 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. Standard errors are clustered at the locality level.

Table 2: Spillovers on consumption decisions

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: log spending at...					
	Corner stores		Supermarkets		Total	
Diff-in-diff	0.067**	0.049	–0.018	0.011	0.029	0.031
	(0.032)	(0.033)	(0.043)	(0.047)	(0.030)	(0.030)
Diff-in-diff × has credit card		0.071*		–0.043		0.009
		(0.040)		(0.059)		(0.034)
P-value diff-in-diff + (diff-in-diff × has credit card)		[0.006]***		[0.457]		[0.140]
Number of households	49,810	49,810	49,810	49,810	49,810	49,810
Number of localities	220	220	220	220	220	220
Locality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Locality by credit card fixed effects		Yes		Yes		Yes
Income quintile by credit card by time fixed effects		Yes		Yes		Yes

This table shows the effect of the debit card shock on consumption at corner stores and supermarkets. The outcome variable is log spending from the consumption module of ENIGH (at corner stores in column 1, at supermarkets in column 2, and total—including corner stores, supermarkets, and other venues such as open-air markets—in column 3). Standard errors are clustered at the locality level.

Table 3: Components of corner store profit increase

	(1) asinh Profits	(2) Log Merchandise Sales	(3) Log Merchandise Costs	(4) Log Wage Costs	(5) Log Rent	(6) Number Employees	(7) Charged VAT or Paid Social Security
Panel A: Last localities to receive cards as control							
Shock 3–4.5 years ago	0.238***	0.049	0.027	–0.010	–0.015	–0.017	0.017*
	(0.082)	(0.033)	(0.029)	(0.019)	(0.041)	(0.038)	(0.009)
Shock 1.5–3 years ago	0.169**	0.026	0.005	–0.012	0.019	–0.010	0.032***
	(0.085)	(0.033)	(0.030)	(0.019)	(0.039)	(0.034)	(0.012)
Shock 0–1.5 years ago (omitted)	0	0	0	0	0	0	0
Number of firms	270,673	270,673	270,673	270,673	270,673	270,673	270,673
Panel B: Pooled diff-in-diff, last localities to receive cards as control							
Shock 1.5–4.5 years ago	0.201***	0.036	0.016	–0.011	0.003	–0.014	0.025***
	(0.076)	(0.029)	(0.026)	(0.016)	(0.038)	(0.034)	(0.009)
Number of firms	270,673	270,673	270,673	270,673	270,673	270,673	270,673
Panel C: Non-treated localities as control							
Shock 3–4.5 years ago	0.198***	0.051**	0.041**	–0.010	–0.032	0.001	–0.002
	(0.059)	(0.023)	(0.021)	(0.014)	(0.020)	(0.022)	(0.007)
Shock 1.5–3 years ago	0.112*	0.026	0.018	–0.012	0.003	0.009	0.015
	(0.062)	(0.023)	(0.022)	(0.014)	(0.016)	(0.014)	(0.010)
Shock 0–1.5 years ago	–0.074	–0.011	–0.002	–0.012	–0.002	0.015	0.000
	(0.069)	(0.024)	(0.022)	(0.016)	(0.029)	(0.027)	(0.011)
Number of firms	532,374	532,374	532,374	532,374	532,374	532,374	532,374
Panel D: Pooled diff-in-diff, non-treated localities as control							
Shock 0–4.5 years ago	0.139***	0.034**	0.027*	–0.011	–0.013	0.006	0.007
	(0.050)	(0.017)	(0.016)	(0.010)	(0.013)	(0.013)	(0.047)
Number of firms	532,374	532,374	532,374	532,374	532,374	532,374	532,374
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table shows that the debit card shock led to an increase in corner store profits, by enabling corner stores to increase their turnover of inventory through higher merchandise sales and costs, while keeping other input costs such as rent and wages fixed. It shows intent-to-treat estimates of the effect of the card shock on various outcomes listed in the column headings. It shows results from (5) using localities treated less than 1.5 years before the second census wave as control (panel A), as well as the pooled difference-in-difference version of this specification (panel B), specification (5) using untreated localities as control (panel C), and the pooled difference-in-difference version (panel D). The "number of employees" column is in levels rather than logs given that the vast majority of corner stores have just 1–3 employees. The "charged VAT or paid social security" column is a dummy variable equal to 1 if the firm reports charging any VAT to customers, or any costs from paying social security for employees. VAT = value added tax. Standard errors are clustered at the locality level.

Table 4: Spillovers on other card adoption: heterogeneity by delay in POS response

	(1) Municipalities with $<$ 6 month delay in POS response	(2) Municipalities with 6 month–1 year delay in POS response	(3) p-value of difference
First 6 months after card shock	0.102**	–0.020	0.043**
	(0.049)	(0.034)	
Subsequent periods	0.234*	0.124	0.583
	(0.136)	(0.147)	
N (municipality $\times$ 6-month periods)	2,127	288	
Number of municipalities	146	21	
Municipality fixed effects	Yes	Yes	
Time fixed effects	Yes	Yes	

This table shows results from a regression of the log number of debit cards issued by other banks in municipality m during 6-month period t on a set of dummies for broad categories of time: the 6 months preceding the Prospera card shock (omitted dummy), the 6 months after the card shock, and all subsequent periods. Periods prior to 6 months before the card shock are excluded. Time periods are aggregated to the 6-month level since the table relies on quarterly data on number of debit cards issued by other banks and the results on POS adoption which were aggregated to the 2-month level. Column 1 is restricted to municipalities in which the number of corner stores with POS terminals increased during the first 6 months after the card shock. Column 2 is restricted to municipalities in which the number of corner stores with POS terminals did *not* increase during the first 6 months but did increase between 6 months and 1 year after the card shock. Municipalities in which there was no increase in the number of corner stores with POS terminals for at least one year after the card shock are not included in the regressions or in this table. Standard errors are clustered at the municipality level.

Appendix A Simple model of financial technology adoption

To provide a conceptual framework and fix ideas, I present a very simple model of financial technology adoption by consumers and firms. In the model, there are two types of firms: small retailers (in practice, these are corner stores) and large retailers (supermarkets). Corner stores and supermarkets each sell a single good x; consumers each consume one unit of x, choosing whether to consume that good at a supermarket or corner store. For simplicity, I assume that each consumer only chooses between one corner store and supermarket (e.g., the closest of each).

The model is set up as a two-stage game. In the first stage of the game, each corner store decides whether to adopt the financial technology ($T \in \{0,1\}$) and what price p to charge for good x. Adopting the technology to accept card payments has a fixed cost a, and the corner stores seeks to maximize profits. The supermarket is an exogenous agent with a fixed price p^S for good x, and the supermarket has already adopted the financial technology (a POS terminal to accept debit and credit card payments), consistent with the stylized facts of a near-100% penetration of debit card technology among large supermarket chains.[34] For simplicity, both stores have a zero-cost production technology.

In the second stage, consumers decide whether to adopt a debit card and whether to purchase good x at their local corner store or supermarket. The continuum of consumers indexed by i has individual-specific disutilities of cash η_i, costs of traveling to the supermarket ψ_i^S, and costs of traveling to the corner store ψ_i^C. The disutility of cash could depend, for example, on the time spent going to the bank branch to withdraw cash or the security risks of carrying cash. The costs of traveling to the supermarket and corner store could depend on both the distances individual lives from the corner store and supermarket, as well as whether the individual has a form of convenient transportation such as a car. There is a fixed cost A, initially A_0, of adopting a card. I denote $D_i = 1$ if consumer i adopts a card, $C_i = 1$ if she goes to a corner store, and $T = 1$ if the corner store accepts card payments. Because each consumer consumes 1 unit of x, consumer i's utility

[34] Indeed, in the National Survey of Enterprise Financing, 100% of supermarkets reported accepting card payments.

maximization problem can be expressed as the following expenditure minimization problem:

$$\min_{\substack{D_i \in \{0,1\},\\ C_i \in \{0,1\}}} (p+\psi_i^C)C_i + (p^S+\psi_i^S)(1-C_i) + A_0 D_i + \eta_i(1-D_i+D_iC_i(1-T)).$$

Since each consumer is making two binary choices, she merely compares costs under the four possible scenarios (taking T as given) and chooses the (C_i, D_i) combination with the lowest cost. Costs under each scenario are shown in the following 2×2 matrix.

$$\begin{array}{cc|c|c|}
 & & \multicolumn{2}{c}{C_i} \\
 & & \multicolumn{1}{c}{0} & \multicolumn{1}{c}{1} \\
\cline{3-4}
\multirow{2}{*}{D_i} & 0 & p^S+\psi_i^S \quad\quad +\eta_i & p+\psi_i^C \quad\quad +\eta_i \\
\cline{3-4}
 & 1 & p^S+\psi_i^S+A_0 & p+\psi_i^C+A_0+\eta_i(1-T) \\
\cline{3-4}
\end{array}$$

The inequalities used to compare costs and the resulting choice are illustrated graphically in Figure C.1a if $T=0$ and Figure C.1b if $T=1$. Consumers are defined by their disutility of cash η and relative cost of traveling to the supermarket, $\psi \equiv \psi^S - \psi^C$. If $T=0$, there are three regions of consumers: those who shop at the supermarket and pay with cash, those who travel to the supermarket and pay with card (adopting the technology to do so), and those who shop at the corner store and pay with cash. If $T=1$, on the other hand, there is a fourth region who shops at the corner store and pays with a card. We can see in Figure C.1b that this region is made up of some consumers who would have gone to the supermarket to pay with card and hence switch from supermarket to corner store, and others who would have gone to the corner store anyway but now switch to paying by card. The customers who switch from paying with a card at the supermarket to paying with a card at the corner store after the corner store adopts technology to accept card payments are the customers satisfying

$$p-p^S < \psi_i^S - \psi_i^C < p - p^S + \eta_i - A_0. \tag{7}$$

The first inequality in (7) means that the relative cost of traveling to the supermarket is high enough that these consumers would prefer to shop at the corner store if they could use their card there, while the second inequality means that their disutility of cash is sufficiently high to overcome their preference for going to the corner store if they cannot use their card there.

Each corner store's profit maximization problem is

$$\max_{\substack{p\in\mathbb{R}^+,\\ T\in\{0,1\}}} p \iint C(p,T;A,\eta,\psi,a,p^S)\,d\eta\,d\psi - aT$$

where $C(p,T;A,\eta,\psi,p^S,a)$ denotes that each individual's decision of whether to shop at the corner store is a function of the corner store's choices (prices and whether to accept cards). It is also a function of the cost of debit card adoption A and consumer characteristics—the disutility of cash η and relative cost of traveling to the supermarket ψ. Finally, it is a function of two fixed parameters: the exogenous supermarket price p^S and the cost of adopting a POS terminal a. Denoting the equilibrium price as $p^*(T)$, the first order condition for T tells us that a corner store will adopt a POS terminal if

$$\underbrace{p^*(1)\iint \frac{\Delta C}{\Delta T}\,d\eta\,d\psi}_{\text{Revenue from new customers}} + \underbrace{\frac{\Delta p}{\Delta T}\iint C(p^*(1),1;\eta,\psi,a,p^S)\,d\eta\,d\psi}_{\text{Revenue change from change in equilibrium price}} > a.^{35}$$

In Figure C.1b, the corner store's new customers after adopting the technology—$\iint \frac{\Delta C}{\Delta T}\,d\eta\,d\psi$—are represented by the (density of the) region marked "$S \to C$ (new customers)."

In this set-up, we can model the shock explored in this paper—where the government distributed debit cards to cash transfer beneficiaries—as a reduction in the cost of debit card adoption from A_0 to A_1 for some consumers. Figure C.1c shows how this reduction affects consumer choices and corner store profits if the corner store did not previously adopt the technology, and does not adopt as a result of the shock: an additional region of consumers marked "lost customers" adopts cards and switches to the supermarket. If the corner store adopts, on the other hand, it regains these customers as well as the set of customers it would have gained from adopting in the status quo.

[35] $C(p^*(1),1)$ is independent of A, as the cost of card adoption no longer plays a role in consumers' choice between the supermarket and corner store if they can choose between a card and cash at both.

The set of "regained customers" (Figure C.1d) are customers for whom

$$p - p^S + \eta_i - \min(A_0, \eta_i) \ < \ \psi_i^S - \psi_i^C \ < \ p - p^S + \eta_i - A_1.$$

As a result of these forces, the set of corner stores that only adopt after the debit card shock are those for which

$$p^*(1) \iint \frac{\Delta C(A_0)}{\Delta T} d\eta \, d\psi \ < \ a - \frac{\Delta p}{\Delta T} \iint C(p^*(1), 1) \, d\eta \, d\psi \ < \ p^*(1) \iint \frac{\Delta C(A_1)}{\Delta T} d\eta \, d\psi. \quad (8)$$

Note that $\iint \frac{\Delta C(A_1)}{\Delta T} d\eta \, d\psi$ includes new customers whom the corner store could have already attracted by adopting a POS terminal in the status quo, i.e. $\iint \frac{\Delta C(A_0)}{\Delta T} d\eta \, d\psi$, as well as customers who would choose to pay cash at the corner store when the cost of adoption was A_0 and the corner store does not accept cards, but choose the supermarket so that they can use a debit card when the cost of debit card adoption is lower at A_1 and the corner store does not accept cards. Since the former group has a high disutility of cash and would adopt a card and use it in the status quo, in practice it is likely that these are richer consumers who already had cards.

To observe how *other* consumers (non-beneficiaries of Prospera who thus did not receive a shock to the cost of card adoption) respond to the shock, we can return to the analysis from Figures C.1a and C.1b; if their corner store does not adopt, they will not change their behavior, whereas if their corner store adopts in response to the card shock (i.e., if (8) holds), we will see two effects. First, some of these consumers who were not directly affected by the shock will adopt cards, and second, some customers who already had cards will shift consumption from supermarkets to corner stores. Thus, without explicitly specifying network externalities in consumers' utility, this model microfounds indirect network externalities: consumers benefit from other consumers' card adoption because whether their corner store accepts card payments is a function of how many consumers have cards. The model also microfounds a critical mass point, since the fixed cost of POS terminal adoption implies that the density of customers who would switch to shopping at a corner store if the corner store accepts cards has to exceed a certain treshold before the corner store will adopt.

Will corner stores that adopt the technology raise prices? If we denote equilibrium aggregate demand at a corner store as $Q(p^*(T),T) \equiv \int\int C(p^*(T),T;\cdot)\, d\eta\, d\psi$, then from the first order conditions of the firm's profit maximization we can derive

$$\frac{\Delta p^*}{\Delta T} = \frac{-\frac{\Delta Q}{\Delta T} - p^*(0)\frac{\Delta(\partial Q/\partial p^*)}{\Delta T}}{\left.\frac{\partial Q}{\partial p^*}\right|_{T=1}}.$$

Thus, the effect of POS terminal adoption on corner store prices are ambiguous: the corner store will increase prices after adopting a POS terminal if $\frac{\Delta Q}{\Delta T}/p^*(0) > -\frac{\Delta(\partial Q/\partial p^*)}{\Delta T}$, where the term on the left is positive and the term on the right is ambiguous. Intuitively, after adopting the corner store has an increased customer base which can lead to higher revenues if it increases prices, but a different group of consumers is now marginal between the corner store and supermarket: if the density of marginal consumers has increased after adopting, the price effect is ambiguous.

We thus have the following main predictions from the model:

1. The card shock will cause an increase in the adoption of POS terminals by corner stores. There is a set of corner stores for which the fixed cost of technology adoption is only worthwhile when they would both gain new customers *and* when there is a threat that current customers will leave if they don't adopt.
2. The increased adoption of POS terminals will, in turn, cause other consumers to adopt cards.
3. If the corner store starts accepting cards, some consumers (who already had cards in the status quo) switch from shopping with a card at the supermarket to shopping with a card at the corner store.

Appendix B Data Appendix

The data I use include (i) transactions-level data from the bank accounts of the cash transfer recipients who received debit cards, (ii) the universe of point-of-sale (POS) terminal adoptions in Mexico, (iii) the universe of debit and credit card transactions at POS terminals (by all cardholders, not just Prospera beneficiaries), (iv) the number of debit cards by issuing bank by municipality by quarter, (v) household-by-product level consumption and price data from a representative household survey, (vi) high-frequency product-by-store level price data from a sample of retailers, and (vii) a panel on profits (including those from cash sales) of the universe of retailers. This appendix describes each of these data sets in turn.

B.1 Administrative data on card rollout

My source of information on the timing of the card rollout is a locality by 2-month period level administrative data set from Prospera that includes the total number of families receiving government transfers in each locality at each point in time, as well as the payment method by which they receive their transfers. The data span 2009–2016 and include 5.1 million locality by 2-month period observations because all 133,927 localities included in the Prospera program are included in the data set; I restrict it to the 630 urban localities eligible to be included in the rollout, and after using these data to determine which urban localities were included in the rollout I further restrict these data (and all other data sets I use in the analysis) to those 259 localities.[36] In addition, I have data at the locality by year level for the years 2007 and 2008, which I combine with the data for 2009–2016 when testing whether the rollout was accompanied by an overall expansion of the program to new beneficiaries.

B.2 Transactions of new cardholders (cash transfer beneficiaries)

See the description in Section 3.1.

B.3 Universe of POS terminal adoptions

Data on POS terminal adoption comes from Banco de México (Mexico's Central Bank). This data is reported to the Central Bank by the Asociación de Bancos de México (Mexican Bank Association), which is made up of representatives from each bank in Mexico and which collects the data from the individual banks. I use two underlying data sets to construct a data set with the number of businesses with POS terminals during each two-month period since 2006 (aggregated

[36] In addition, I validate the rollout information provided by Prospera using data from the government bank Bansefi that administers the accounts. In these data, described in Section B.2, I observe when the beneficiary is switched to a debit card account.

to the two-month period for consistency with the administrative rollout data): (1) a data set of all changes to a POS terminal contract since 2006, which contains 5 million contract changes including 1.4 million POS adoptions, as well as cancellations and changes to contract terms; (2) a data set with all currently active POS terminals, which I use to back out the number of existing POS terminals at the beginning of 2006 that did not have any contract changes from 2006 to 2017.

These data sets include the store type (e.g., corner store, supermarket)—which is determined by the merchant category code (MCC).[37] In addition, they include the postal code and an anonymized business ID. Because the card rollout occurred at the locality level and neither an official mapping between localities and postal codes nor complete shapefiles for postal codes exist, I create a crosswalk between postal codes and localities using a census of firm geocoordinates in Mexico which includes both the postal code and locality of each firm.[38] This data set on the geocoordinates of the universe of firms is described in more detail in Section B.10.

B.4 Universe of card transactions at POS terminals

I compiled this data set from a number of separate data sets corresponding to each of Mexico's account clearing houses. In this data set, I use a string variable with the locality name to determine the locality of the retailer, and I match the locality strings to INEGI locality codes using a crosswalk created by the Central Bank that accounts for the many typos in the locality strings. The data do not include identifiers that can be used to link transactions made on the same card nor at a particular business.

B.5 Quarterly data on number of debit cards by issuing bank by municipality

Mexico's National Banking and Securities Commission (CNBV) publishes quarterly—and, since April 2011, monthly—data on a number of measures related to banks' operating activities. These

[37]Merchant category codes are four-digit numbers used by the electronic payments industry to categorize merchants. Ganong and Noel (2018) also use the merchant category code to define spending categories.

[38]Shapefiles for a partial set of postal codes are available at `https://datos.gob.mx/busca/dataset/poligonos-de-codigo-postal`, but around one-third of postal codes are not included in the data set, including the postal codes of Mexico's third-largest city Monterrey. I contacted Mexico's Postal Service, which produced the data set, and they reported that they have not yet completed the project of constructing shapefiles for all postal codes in Mexico. The census of firm geocoordinates is publicly available at `http://www.beta.inegi.org.mx/app/descarga/`.

numbers are reported at the bank by municipality level, and include the number of ATMs, number of bank branches, number of employees, number of checking and savings accounts, number of debit cards, and number of credit cards. I use these data to (i) test whether the card rollout is correlated with pre-treatment levels and trends of financial infrastructure and (ii) test for spillovers of retailer POS adoption on other consumers' card adoption. Because the data are at the bank level, I can exclude cards issued by Bansefi—the bank that administers Prospera beneficiaries' accounts and debit cards—for the spillover test.

These data are at the municipality level, which is larger than a locality (the level of the card rollout). Nevertheless, most urban municipalities only include one urban locality; because my analysis focuses only on urban localities, using municipality rather than locality for these results should merely create noise that attenuates any observed effect. I restrict to municipalities with at least one urban locality, and consider a municipality as treated at a particular time if it contains an urban locality that has been treated by that time. Of Mexico's 2456 municipalities in 2010, 521 contain at least one urban locality, and 255 of these are included in the debit card rollout.

The number of debit and credit cards are first included in the data in the last quarter of 2008, as are the number of ATMs; in total, the data include 139,436 municipality by quarter (or month starting in April 2011) observations from the last quarter of 2008 to the last month of 2016. For consistency over time, I use the last month of each quarter from 2011–2016 so that the data is at the municipality by quarter level throughout. Other variables, such as the number of bank accounts and number of bank branches, extend back to 1995. For the test of whether pre-treatment trends in financial infrastructure are correlated with the debit card rollout (for variables available prior to the last quarter of 2008), I use the change between the last quarter of 2006 and the last quarter of 2008 for consistency with the data on the number of POS terminals which begin in 2006.

B.6 Consumption data by store type from household survey

I use the Encuesta Nacional de Ingresos y Gastos de los Hogares (ENIGH), Mexico's household income and expenditure survey. The survey is a repeated cross-section conducted every two years by INEGI. Because the card rollout occurred between 2009 and 2012, I use the 2006–2014 waves of the ENIGH. In the survey's consumption module, each household is asked to record all purchases over the course of a one-week period in a consumption diary format. For each item purchased, they record the product, total expenditure, quantity purchased (for food items only), and type of store such as open market, corner store, supermarket, etc. I use these data to construct a measure of total spending at each of the different types of store.

Across all survey years, there were 106,351 households included in the survey. Of these, I restrict the analysis to the 49,810 households living in localities included in the rollout (220 of the 259 treated urban localities are included in ENIGH). The ENIGH is used extensively both by the government—for example, to construct its official poverty statistics—and by researchers (e.g., Atkin, Faber and Gonzalez-Navarro, 2018). The data are publicly available, with exception of the locality identifier which is confidential in the survey waves prior to 2012. To determine which households live in treated localities, I obtained the locality identifier corresponding to each household from INEGI. I also obtained a finer-grained geographic variable, the "basic geographic area" (AGEB) which I use to approximate each household's location and calculate its road distance to the nearest corner store and nearest supermarket.[39]

B.7 High-frequency product-by-store price panel

Mexico collects weekly price estimates for food products and biweekly price estimates for other products to construct its consumer price index (CPI). I use the store by product by week price microdata to test whether the debit card shock had a general equilibrium price effect. Until 2010 the data were collected by Mexico's Central Bank, and from 2011 on it was collected by INEGI. I have data from 2002–2014, with monthly price averages for each store by product observation through 2010 and weekly price quotes from 2011–2014; as with the other data sets, I average across two-month periods for consistency with Prospera's payment periods. After making this aggregation, the data set includes 5.4 million product by store by two-month period observations; over the twelve-year period, price quotes are collected from 122,789 unique stores for 313,915

[39]Within Mexico's 630 urban localities, there are 61,424 AGEBs, making them roughly analogous to census tracts in the US.

barcode-equivalent goods (such as "600ml bottle of Coca-Cola").

I again restrict the data to municipalities included in the card rollout.[40] Because the Mexican government focuses on the largest urban areas when collecting price data for its CPI, most stores are in urban municipalities included in the card rollout: after removing stores in other municipalities, there are still 4.9 million product by store by two-month period observations. I further restrict the analysis to the category of goods encompassing food, beverages, alcohol, and tobacco, as this is likely the main type of product for which consumers are deciding between purchasing at the supermarket and corner store.[41] This leaves 1.8 million observations, 1.1 million of which are from supermarkets and 659,441 from corner stores.

B.8 Economic Census on the universe of retailers

Every 5 years, Mexico's national statistical institute, the Instituto Nacional de Estadística y Geografía (INEGI), conducts an Economic Census of the universe of firms in Mexico. This census includes all retailers, regardless of whether they are formally registered (with the exception of street vendors who do not have a fixed business establishment). I use the 2008 and 2013 census since these years bracket the rollout of cards; I cannot include additional pre-periods because the business identifier that allows businesses to be linked across waves was introduced in 2008.

The 2008 census includes about 5 million firms, 2 million of which are retailers. Of the 2 million retailers, about 1 million are also observed in the 2013 census, indicating that they survived over the five year period between census waves. This rate of firm survival is consistent with estimates of firm survival in developing countries (McKenzie and Paffhausen, forthcoming). the majority are corner stores and other small specialty stores (butcher shops, dairy shops, bakeries, etc.). Specifically, 532,374 of the retailers from the balanced sample are corner stores and other small specialty stores, while 20,426 are supermarkets. In the census data, store type is determined by the North American Industry Classification System (NAICS), which is available at the six-digit

[40]For each store, I have a string variable identifying the municipality, but do not have a locality identifier. As a result, I follow the same approach as with the CNBV data described in section B.5.

[41]The other product categories are clothing, shoes and accessories; housing; furniture, appliances, and domestic products; health and personal hygiene; transport; education and recreation; and other.

level in the data; corner stores (together with other small specialty stores) and supermarkets are distinguished at three-digit level.[42]

I use the Economic Census to measure the effect of the debit card shock on sales, costs, and profits. Because I do not observe whether a firm has adopted a POS terminal to accept card payments, the results using the Economic Census are intent-to-treat estimates.

B.9 Locality-level measures from population census

INEGI conducts a comprehensive population census every ten years and an intermediate population census—which still includes a number of sociodemographic variables from all households in the country—every five years between full census rounds. I use locality-level summary statistics constructed from the 2005 intermediate population census (since this is the most recent census prior to the beginning of the debit card rollout) to test whether the card rollout is correlated with locality characteristics. I use the same characteristics that are used to measure locality-level development by INEGI and Mexico's National Council for the Evaluation of Social Development (CONEVAL).

B.10 Geographic information systems (GIS) data

Geocoordinates of the universe of retail firms in Mexico. These data are a directory of all firms in Mexico, including the name and economic activity code of the firm (which allow me to identify the type of store), its locality, postal code, and exact geocoordinates. I use these data, merged with road shapefiles and the approximate locations (based on AGEB) of households from ENIGH to calculate the shortest road distance between each household and a corner store or supermarket. I also use the data to create a mapping between localities and postal codes since some of the Central Bank data includes postal code but not locality. Finally, I use these data to determine the number of each type of store by locality and municipality, which I use to construct the measure of the proportion of all retailers and proportion of each type of retailer that accepts cards, after merging

[42] Corner stores are distinguished from other small specialty stores at the four-digit level; results are robust to excluding other specialty stores and only using corner stores based on the four-digit code. Other studies using NAICS codes to distinguish firm types include Mian and Sufi (2014), Giroud and Mueller (2017), and Giroud and Rauh (forthcoming).

the data with the number of retailers with POS terminals from the Central Bank data.

Geocoordinates of Bansefi branches. I use this data set, which Prospera prepared in 2008 to assess beneficiaries' access to their benefits through Bansefi branches, to construct a measure of the number of Bansefi branches by locality, which I use to test whether the rollout is correlated with the number of Bansefi branches.

Shapefiles. I use two main sets of shapefiles: the polygons corresponding to the border of each municipality, locality, and AGEB (analogous to a US census tract) from INEGI, and the shapefiles of all roads in Mexico from Open Street Maps. I use the AGEB shapefiles to calculate the centroid of the AGEB and use this as the approximate geolocation of each household living in the AGEB, and I use the road shapefiles to calculate each household's road distance to the closest corner store and supermarket.

B.11 Auxiliary survey data

Mexican Family Life Survey. This survey has more detailed information about debit and credit card ownership than other household surveys in Mexico. The most recent wave of the Mexican Family Life Survey was conducted in 2009, prior to the debit card rollout in nearly all localities included in the rollout. I thus use it for summary statistics prior to the card rollout, such as the proportion of households with a debit or credit card across the income distribution. The 2009 wave includes 9,436 households; because the survey oversampled rural areas, just 4,232 of these households live in urban areas, which is the sample I use for the summary statistics presented in the paper.

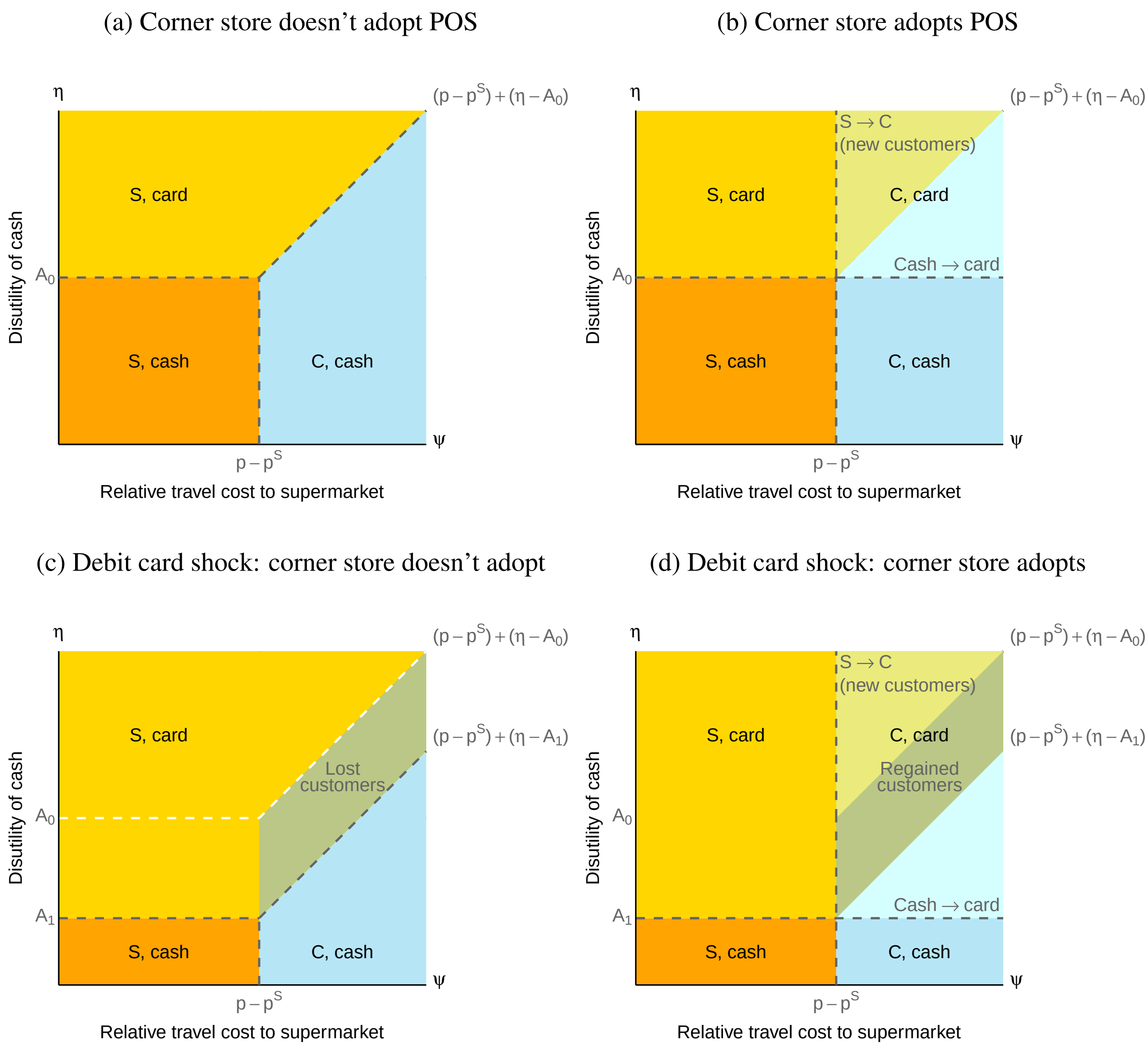
Payment Methods Survey of Prospera beneficiaries. This survey was conducted by Prospera after the card rollout was completed. Because it was conducted in mid-2012, most beneficiaries had already accumulated at least one year with the card at the time they were surveyed. Appendix Figure C.2 shows their self-reported amount of time with the card at the time of the survey.[43] The

[43]I use self reports of number of months with the card at the time of the survey for this figure because the exact month in which different households are surveyed is not available, which makes it impossible to accurately back out the exact number of months using administrative data.

data set includes 5,381 Prospera beneficiaries, 1,641 of whom live in localities included in the rollout and hence received cards. Restricting the analysis to these 1,641 who received cards, I use this data set to investigate whether Prospera beneficiaries open other bank accounts after receiving a debit card, which could explain the increase in cards adopted at other banks.

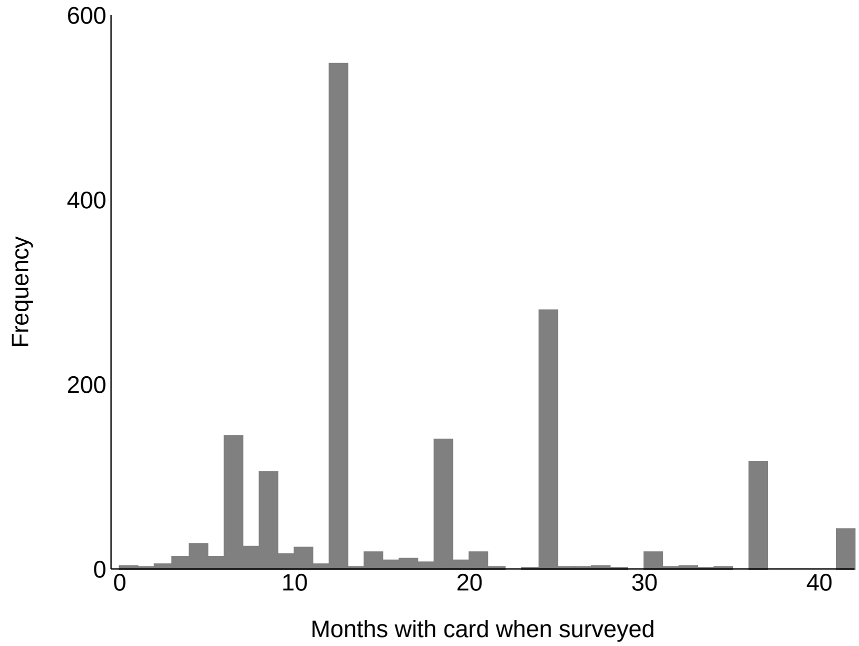
Appendix C Appendix Figures and Tables

Figure C.1: Simple model of financial technology adoption



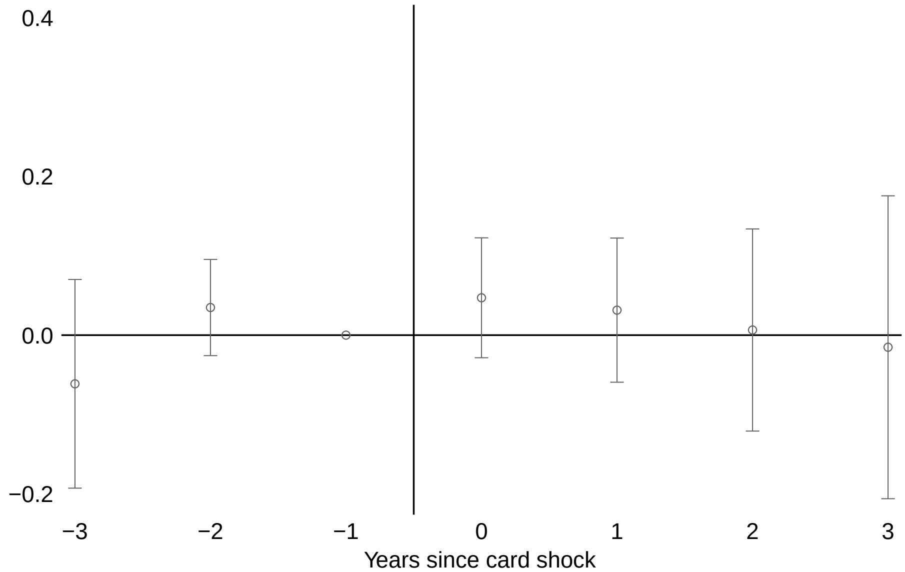
This figure shows the comparative statics from the effect of a debit card shock in a simple model of financial technology adoption.

Figure C.2: Months with debit card at time of Payment Methods Survey



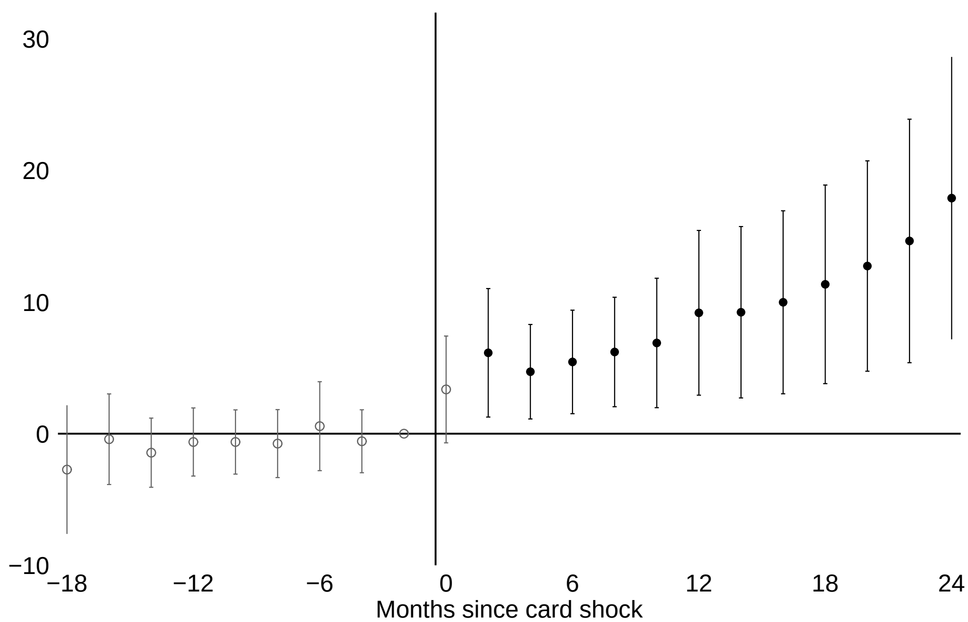
This figure shows how long ago households had received Bansefi debit cards before being surveyed in the Payment Methods Survey. I use self-reported months with the card from the survey. $N = 1{,}617$ beneficiaries.

Figure C.3: Number of beneficiaries not correlated with rollout



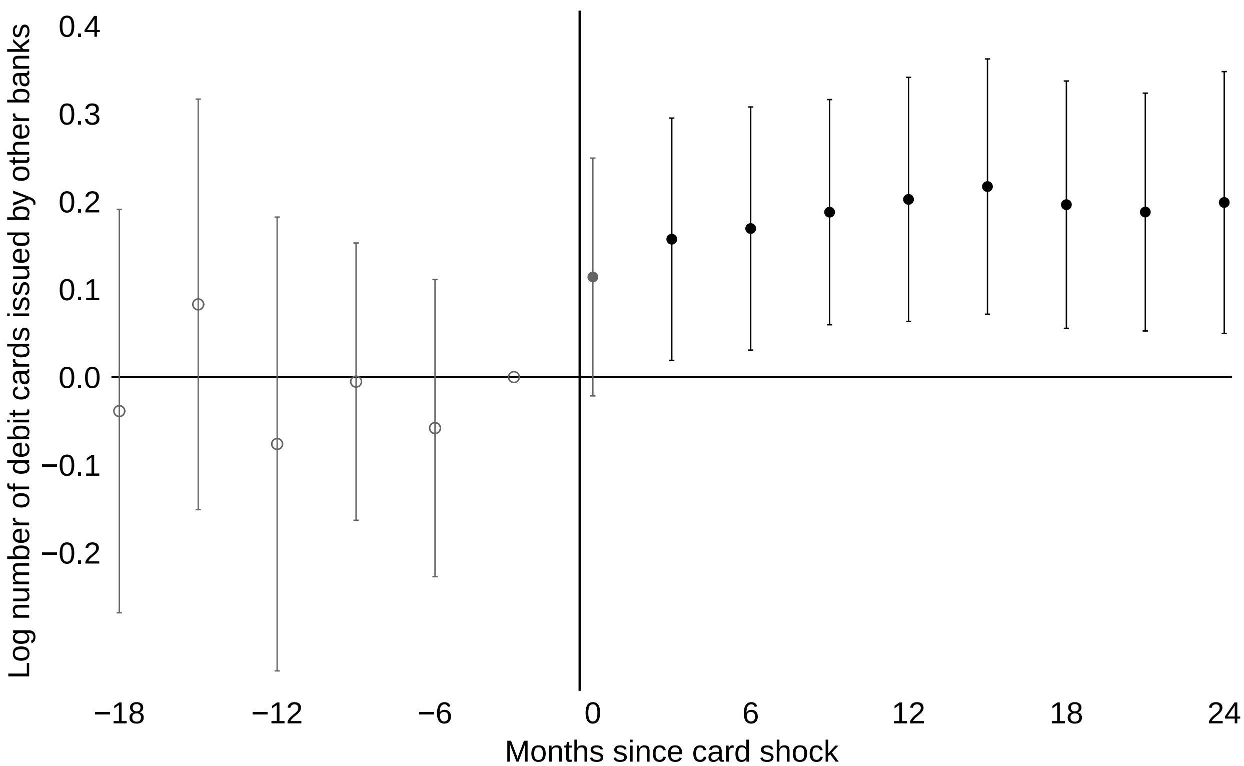
This figure shows that the rollout of debit cards is not correlated with changes in the number of beneficiaries. It shows the coefficients from (1), where the outcome is the log number of Prospera beneficiaries in locality j during the last two-month period of year t. The estimation uses administrative 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 treated localities.

Figure C.4: Effect of card shock on corner store POS adoption **in levels** (event study estimates)



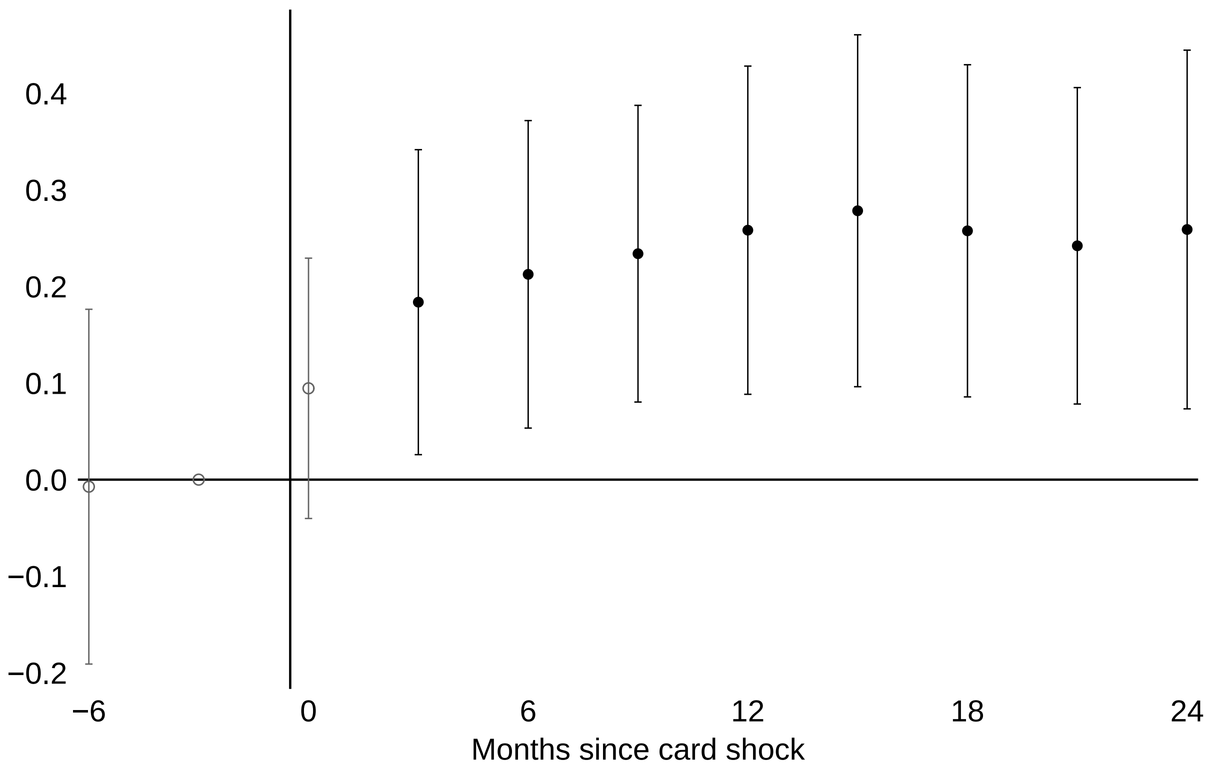
This figure shows the effect of the debit card shock on the stock of point-of-sale (POS) terminals at corner stores, measured in levels. See the notes of the corresponding figure in logs (Figure 6) for more detail.

Figure C.5: Spillover effect to other consumers' **credit and debit** card adoption (event study estimates)



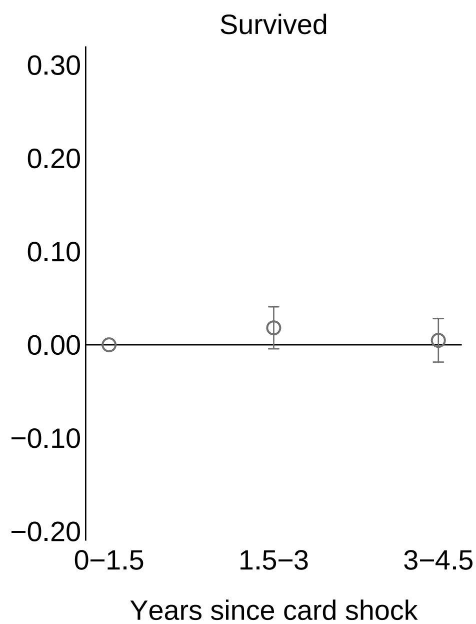
This figure shows the adoption of debit *and credit* cards at other banks relative to the debit card shock. See the notes of the corresponding figure for only debit cards (Figure 7) for more detail.

Figure C.6: Spillover effect to other consumers' card adoption (**balanced sample of municipalities**)



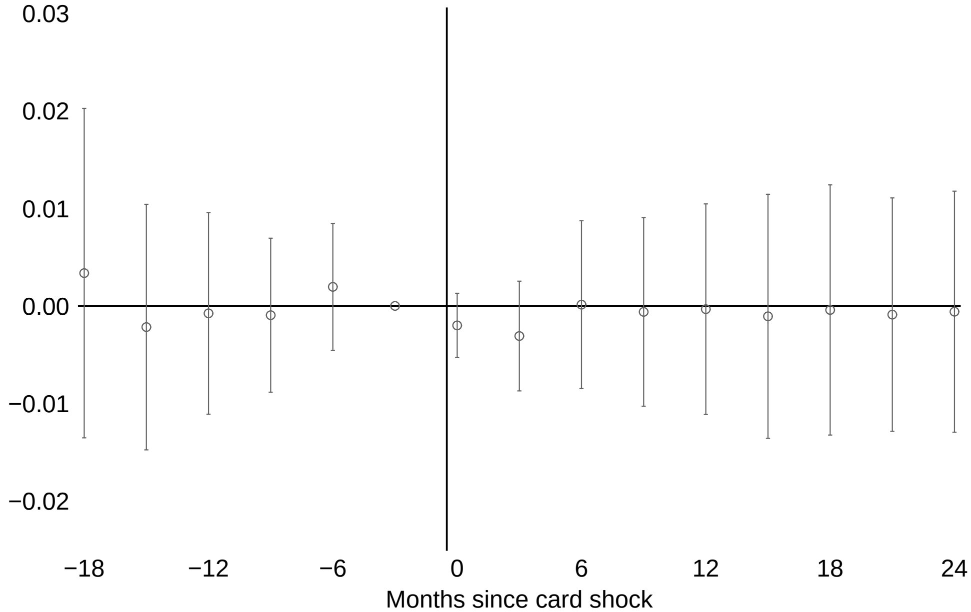
This figure shows the adoption of debit cards at other banks relative to the debit card shock, restricting the sample to the sample of localities that can be observed for all the periods in the graph. Because the data begin in the last quarter of 2008 and the card rollout began in 2009, this requires extending back only two periods before the shock (extending back further would drastically reduce the sample). This graph includes 212 of the 255 urban municipalities. See the notes of the corresponding figure for only debit cards (Figure 7 for more detail.

Figure C.7: Effect of treatment on probability of survival



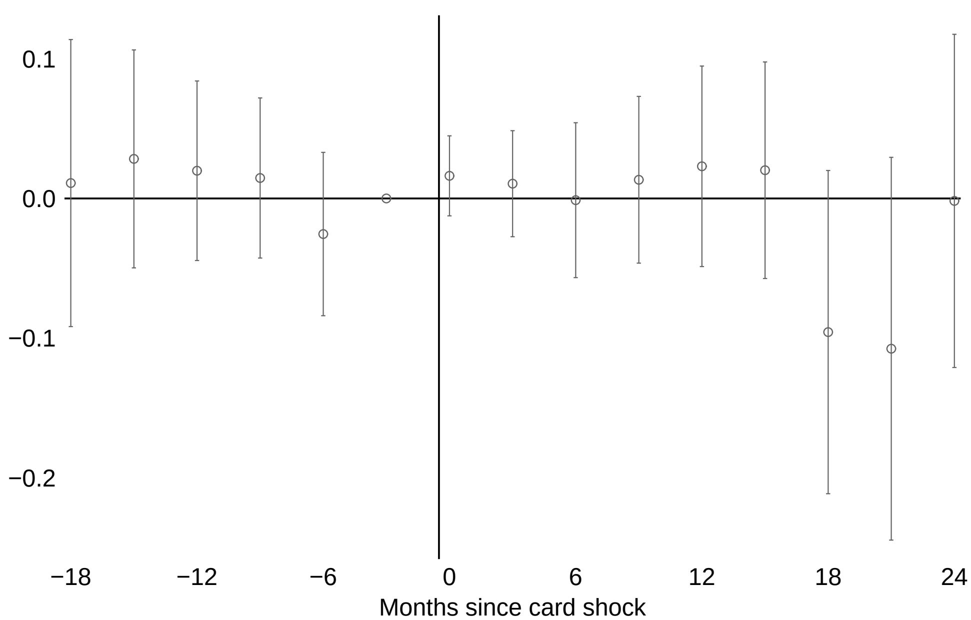
This figure shows γ_k coefficients from $Survived_i = \alpha + \sum_k \gamma_k \mathbb{I}(\text{received cards at } k)_{j(i)} + \varepsilon_{it}$, where the omitted k corresponds to localities treated 0–1.5 years before the 2013 survey way (in other words, the latest-treated localities are controls). The data are from the Economic Census. $N = 460{,}922$ corner stores included from the 2008 survey wave in localities included in the debit card rollout.

Figure C.8: Effect of card shock on per-transaction merchant fee at POS terminals



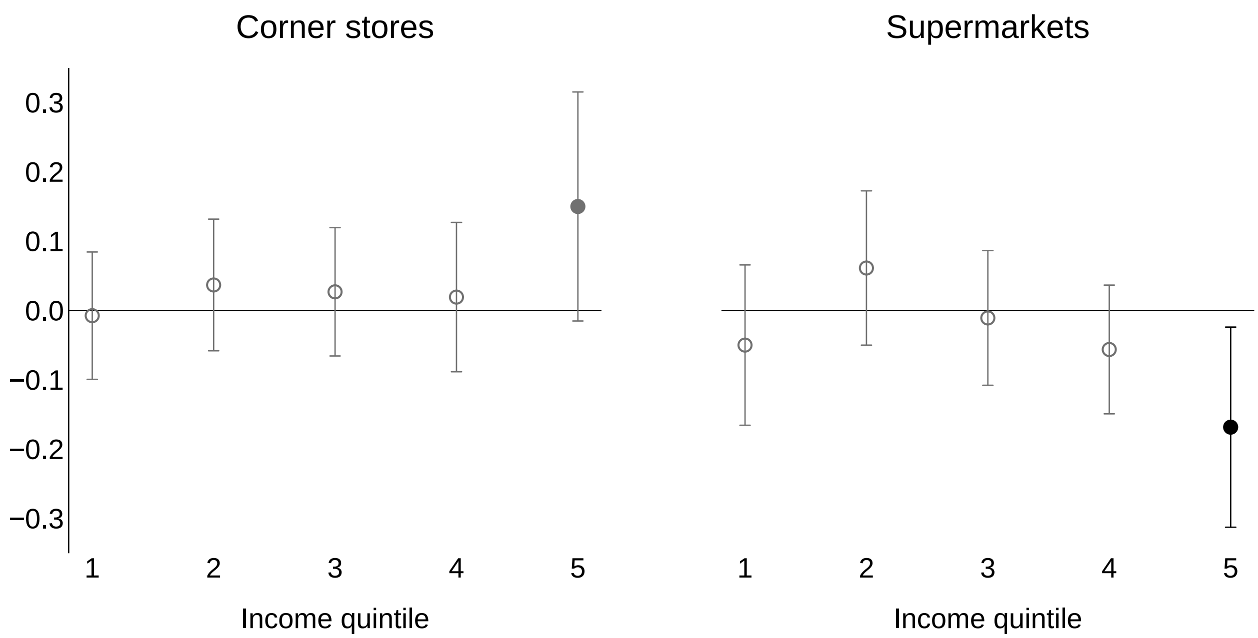
This figure shows that the POS terminal merchant fee charged by banks does not change in response to the card shock. The data include merchant fee rates by bank and were obtained from Mexico's Central Bank. $N = 4574$ municipality by quarter observations from 255 municipalities.

Figure C.9: Effect of card shock on number of commercial bank branches



This figure shows that the number of commercial bank branches does not change in response to the card shock, i.e., banks are not investing in banking infrastructure in localities that received the shock. It shows coefficients from (1) where the outcome variable is the log number of commercial bank branches in the locality, using quarterly data from CNBV. $N = 9261$ municipality by quarter observations from 255 municipalities.

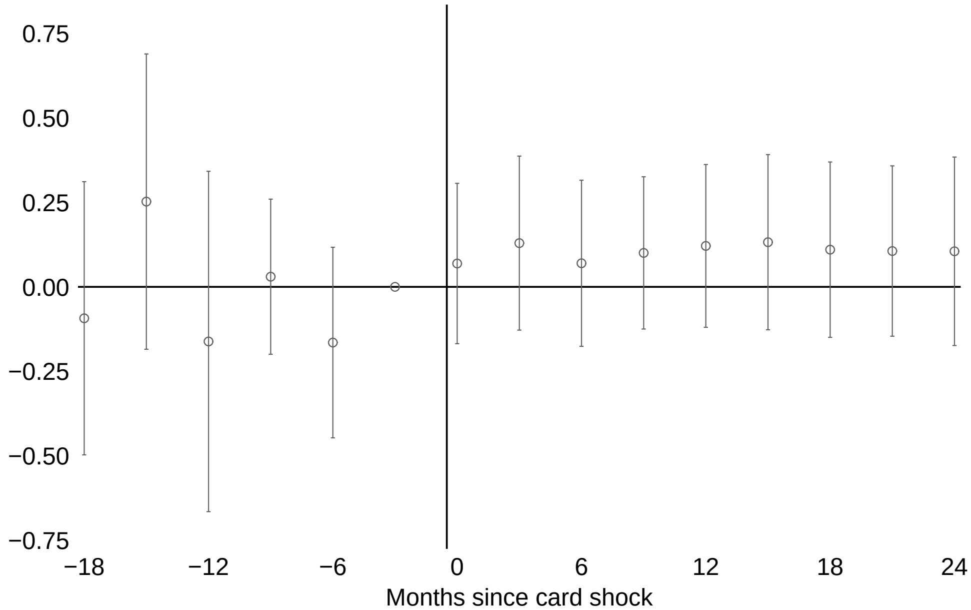
Figure C.10: Effect of corner store POS adoption on quantity consumed



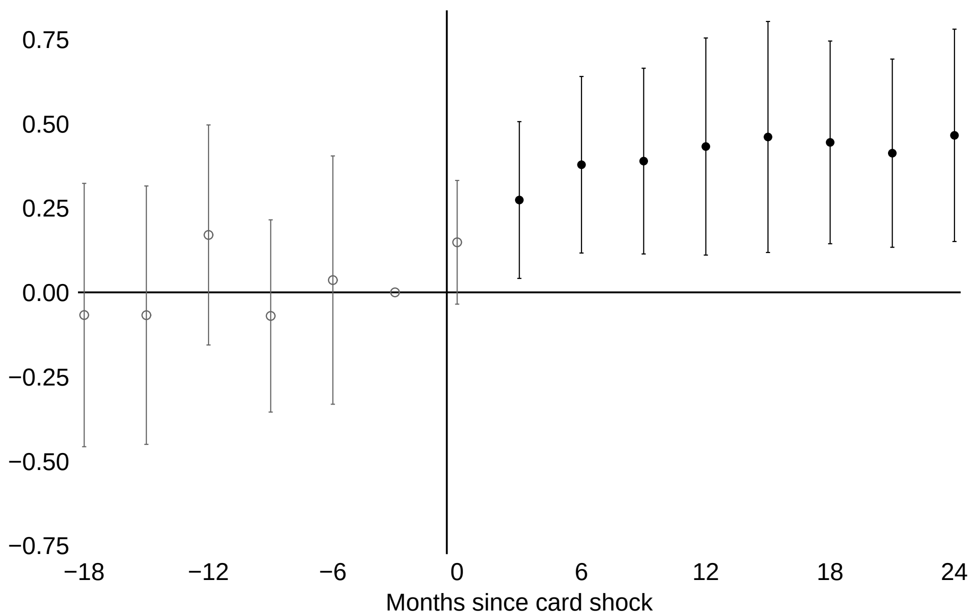
This figure shows that richer consumers substitute some of the quantity (measured in kilograms and liters) that they purchase from supermarkets to corner stores. This suggests that the results are not explained by prices. The figure graphs coefficients from (4) where the outcome variable is log(kilograms + liters purchased) at the particular store type (supermarkets or corner stores). $N = 48{,}810$ households from 220 localities.

Figure C.11: Other consumers' card adoption

(a) Municipalities where beneficiaries prefer supermarkets

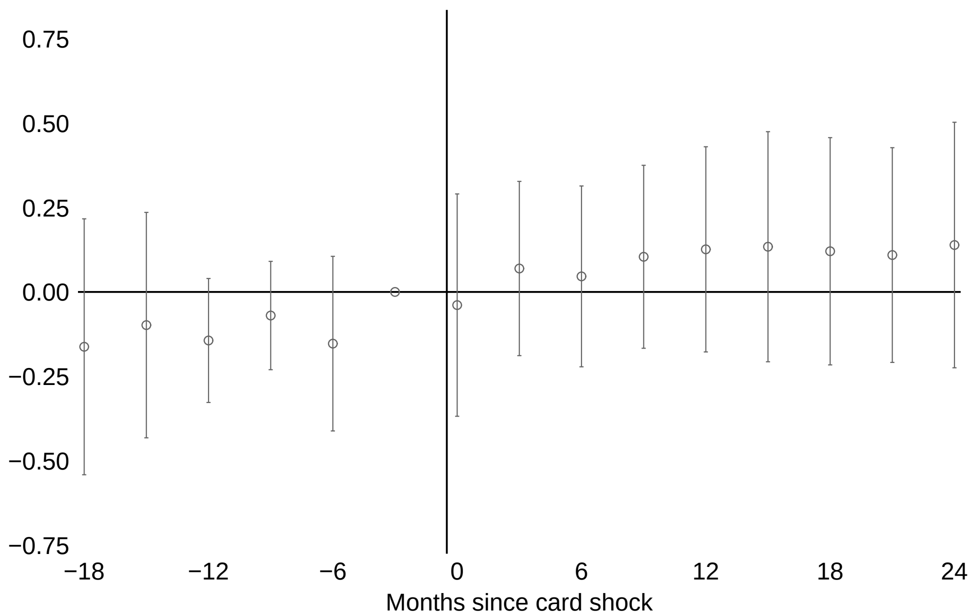


(b) Municipalities where beneficiaries prefer corner stores



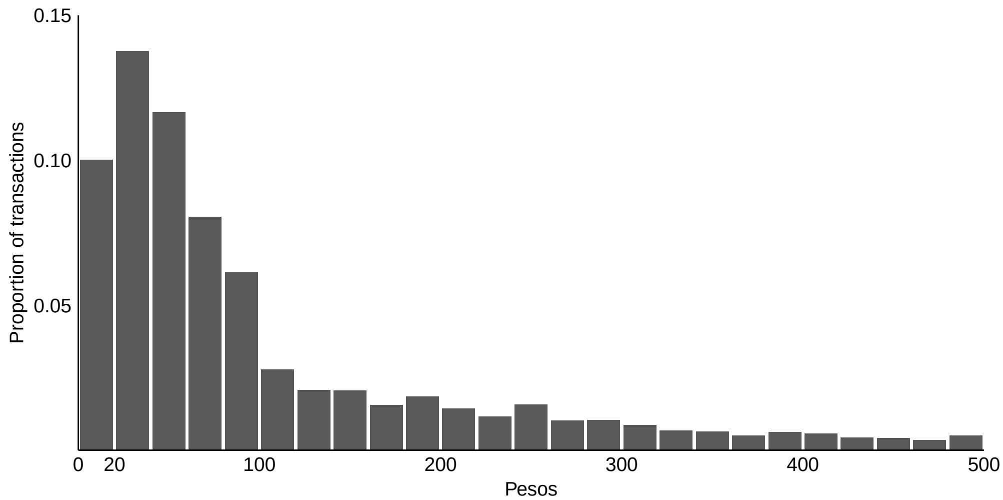
This figure shows that the spillovers on other consumers' card adoption are concentrated in localities where beneficiaries use their cards relatively more at corner stores, suggesting that the effect on other consumers' card adoption is driven by network externalities rather than word-of-mouth learning. It graphs coefficients from (1), where panel (a) shows results for municipalities with relatively more beneficiary spending at supermarkets and panel (b) for municipalities with relatively more spending at corner stores. Relatively more/less spending is based on above/below median proportion of transactions made by beneficiaries at supermarkets during their first 6 months with the card. This measure is based on Bansefi transaction data, while the outcome variable is from CNBV data.

Figure C.12: Other consumers' card adoption: municipalities where beneficiaries prefer supermarkets and baseline card adoption is below median



This figure shows that even in municipalities where beneficiaries prefer supermarkets *and* baseline card adoption by other consumers is below median, we do not see evidence (or see a small and statistically insignificant effect) of the card shock on other consumers' card adoption. It graphs coefficients from (1) restricted to this subset of municipalities. This further suggests it is not word-of-mouth learning since there was presumably still room for learning to take place in municipalities with below-median baseline card adoption. See notes to Figure C.11 on data sources.

Figure C.13: Histogram of transaction amounts (transactions at POS terminals)



This figure shows that retailers likely do not impose minimum transaction amounts because a substantial proportion of transactions are made for very small amounts (20 pesos is less than $2). It graphs the histogram of transaction amount sizes using the Bansefi transactions data.

Table C.1: Effect of card shock on POS adoption (event study estimates)

	In logs			In levels
Months since card shock	Corner stores	Supermarkets	Department stores	Corner stores
–18 to –17	–0.025	–0.025	–0.034	–2.73
	(0.041)	(0.034)	(0.031)	(2.48)
–16 to –15	0.029	–0.019	–0.031	–0.42
	(0.040)	(0.031)	(0.029)	(1.75)
–14 to –13	–0.011	–0.012	–0.031	–1.44
	(0.034)	(0.027)	(0.029)	(1.33)
–12 to –11	0.014	–0.029	–0.031	–0.63
	(0.028)	(0.022)	(0.021)	(1.31)
–10 to –9	0.005	–0.052**	–0.012	–0.63
	(0.026)	(0.021)	(0.023)	(1.24)
–8 to –7	–0.009	–0.016	–0.002	–0.75
	(0.026)	(0.021)	(0.018)	(1.31)
–6 to –5	0.016	–0.024	–0.011	0.57
	(0.026)	(0.016)	(0.015)	(1.72)
–4 to –3	0.000	–0.015	–0.008	–0.57
	(0.023)	(0.018)	(0.016)	(1.21)
–2 to –1 (omitted)	0	0	0	0
0 to 1	0.033*	–0.001	–0.010	3.36
	(0.019)	(0.018)	(0.013)	(2.06)
2 to 3	0.061**	–0.023	–0.011	6.15**
	(0.024)	(0.017)	(0.016)	(2.48)
4 to 5	0.037**	0.003	0.009	4.71***
	(0.019)	(0.020)	(0.021)	(1.82)
6 to 7	0.060***	–0.011	0.020	5.45***
	(0.022)	(0.021)	(0.021)	(2.00)
8 to 9	0.081***	0.011	0.009	6.21***
	(0.025)	(0.025)	(0.021)	(2.11)
10 to 11	0.076***	–0.001	0.012	6.90***
	(0.027)	(0.025)	(0.023)	(2.50)
12 to 13	0.085***	0.003	0.011	9.19***
	(0.032)	(0.029)	(0.027)	(3.18)
14 to 15	0.103***	–0.013	0.021	9.23***
	(0.036)	(0.032)	(0.032)	(3.31)
16 to 17	0.093**	–0.008	0.037	9.99***
	(0.037)	(0.033)	(0.035)	(3.53)
18 to 19	0.112***	–0.011	0.019	11.40***
	(0.040)	(0.038)	(0.036)	(3.83)
20 to 21	0.122***	0.006	0.036	12.70***
	(0.043)	(0.039)	(0.038)	(4.06)
22 to 23	0.135***	–0.003	0.036	14.60***
	(0.047)	(0.042)	(0.040)	(4.70)
24 to 25	0.169***	–0.002	0.055	17.90***
	(0.060)	(0.052)	(0.051)	(5.45)
N (locality $\times$ 2-month period)	8806	8806	8806	8806
Number of localities	259	259	259	259
Locality fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes

This table shows the point estimates from Figures 6 and C.4. See the notes to those figures for more detail.