

Financial Technology Adoption

Job Market Paper

Sean Higgins
UC Berkeley*

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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; Fuster, Plosser, Schnabl and Vickery, forthcoming), 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

¹Katz 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).

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

over time and space in debit card adoption: it occurred in different localities at different points 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 eight 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

³In 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%.

terminals to accept card payments. Using an event study design to accommodate the differing 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, 2018b).⁶

⁴Administrative 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.

⁵Although 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.

⁶In 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

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 and prices with data on local POS adoption from Mexico's Central Bank and the geocoordinates of all retailers.

My estimating equation is derived from a discrete choice model where consumers decide, for each shopping trip, whether to go to a supermarket, corner store, or open-air market. Supermarkets are further than corner stores 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 sales increase by 3% more in treated localities. Corner stores increase the amount of merchandise they buy and sell but do not change other inputs such as number of employees, wages, and rent, which leads to an increase in profits. 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, and how this supply-side response spills over onto other consumers' technology adoption and consumer surplus. Most research on financial inclusion and FinTech, on the other hand, has focused either on direct effects for consumers who adopt or on supply-side FinTech providers. For example, studies

savings, financed by a voluntary reduction in current consumption.

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). Other studies focus on the supply side of FinTech markets, such as online lenders (Bartlett, Morse, Stanton and Wallace, 2018; Buchak, Matvos, Piskorski and Seru, 2018; Fuster, Plosser, Schnabl and Vickery, forthcoming), other financial service providers (Philippon, 2016), and initial coin offerings (Howell, Niessner and Yermack, 2018).

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. Although network goods may have large spillovers, other studies on network externalities (e.g., Saloner and Shepard, 1995; Rysman, 2007) rarely exploit changes in the cost of adoption for just a subset of consumers. 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.

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 on the debit card shock's effect on supply-side POS adoption and spillovers to other consumers. Section 6 imposes additional assumptions to quantify the consumer and producer gains from the demand-side policy shock. Section 7 tests various alternative explanations of the results. Section 8 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.

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

⁷Björkegren (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).

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

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 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 Progresas 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

⁹This 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).

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, 2018a); 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, 2018b).

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

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

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 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 would bind for about one-third of supermarkets and 95% of corner stores.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

¹¹The proportion of retailers for which the constraint would bind is not conditional on accepting card payments. It is based on a back-of-the-envelope calculation combining data on the sales of the universe of retailers from Mexico's Economic Census with the average proportion of transaction value made on cards at each type of retailer (23% at supermarkets and 46% at corner stores, based on data from Mexico's 2015 National Survey of Enterprise Financing).

¹²In contrast, in the US, third-party electronic payment data for each firm is automatically sent by electronic

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 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

payment entities (e.g., Visa) to the Internal Revenue Service through Form 1099-K, first implemented in 2011.

¹³With 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.

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 withdrawals, 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

¹⁴Merchant 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.

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.

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.

Retail wages and employment. To test whether the demand shock leads to changes in wages (using data with more power than the infrequently-collected Economic Census), I use a quarterly labor force survey with approximately 400,000 adults in each wave. The data span 2005–2016, and have a total of over 20 million individual by quarter observations; individuals are surveyed over five quarters in a rotating panel. I use these data to see whether retail wages and employment across store types respond to the debit card shock, given that sales are affected.

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

¹⁵I 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.

¹⁶I 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.

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:

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

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 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

¹⁷To 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.

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 beneficiaries 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 5.2 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.2 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

¹⁸Because 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).

¹⁹The 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.

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-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

²⁰These 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

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 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 7, 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

those for supermarkets.

²¹For 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.3 shows the results from the same specification using levels rather than logs of the number of POS terminals.

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. Alternatively, it could occur due to social learning—an alternative explanation that I test in Section 7. Second, do some consumers shift some of their consumption from supermarkets to corner stores now that more corner stores accept card payments?

Spillovers on card adoption. I use the quarterly 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.12, 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

²²Appendix Figure C.4 shows that results are robust to using the number of credit and debit cards rather than just debit cards.

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 7; 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 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 every 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

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

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

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

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 government 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 effects. Specifically, I estimate

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

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 of ω is close to 0 indicating that those with credit cards do not respond differentially to the shock; for overall spending neither ω nor $\omega + \gamma$ are statistically significant.

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

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

where $\theta_{q(i)t}$ is a full set of income quintile by time fixed effects and $\mathbb{I}(\text{quintile} = q)_{it}$ 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 7, 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

²⁴Income 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.

(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.

6 Consumer and Producer Gains

6.1 Consumer Surplus

To quantify the consumer gains from the demand-side policy shock, I impose structural assumptions on consumer utility and combine data on consumption and local product prices across store types, point-of-sale terminal adoptions, and store geocoordinates to estimate a discrete-continuous choice model. Because estimating such a model requires a number of assumptions, the results in this section are more speculative; nevertheless, it is valuable to quantify the gains from the debit card shock on various types of consumers. My empirical strategy is related to the discrete-continuous choice literature that began with Hanemann (1984); it combines features of the demand models in Atkin, Faber and Gonzalez-Navarro (2018), Björnerstedt and Verboven (2016), and Einav et al. (2017).

Model. First, I assume that for each trip that an individual makes, the individual has a set budget and decides where to make the shopping trip. Because I do not observe whether the particular store at which an individual shops accepts card payments, I assume that if the consumer chooses to shop at a store of type s , the particular store of type s they go to for a particular trip is randomly drawn from the set of stores of type s in their census tract. I assume that the store has adopted a POS terminal with probability equal to the fraction of stores of that type in the postal code that have adopted a POS terminal. Furthermore, I assume the consumer observes whether the store accepts cards after arriving at the store, which is reasonable for early trips after the card shock (since consumers may shop at multiple corner stores), but less reasonable once the consumer has determined which stores accept card payments.

Thus, the problem facing consumer i who wants to make a trip to the store and spend a fixed budget y_{it} is to choose between different store types (e.g., supermarket, corner store, open-air market), then determine how much to spend on each good at the store. Consumer i 's utility from trip t , U_{it} , is additively separable in utility from consumption at each type of store, consistent with numerous studies in the discrete choice literature since Domencich and McFadden (1975):

$$U_{it}(\mathbf{x}) = f \left(\sum_s u_{ist}(x_{i1st}, \dots, x_{iGst}) \right), \quad (5)$$

where $f' > 0$, i indexes consumers, s indexes store types, t indexes shopping trips, and $g = 1, \dots, G$ indexes goods. The utility function u_{ist} thus gives the utility for consumer i of a trip t to store type s . I assume that consumers who have cards have Cobb-Douglas preferences over the goods they consume and also get some utility from store-specific characteristics, including whether the store accepts card payments.²⁵ Specifically,

$$u_{ist} = \left(\prod_g x_{igst}^{\phi_{a(i)gst}} \right)^{\alpha_{k(i)}} \cdot \exp(\theta_{k(i)} POS_{ist} + \xi_{a(i)k(i)st} + \varepsilon_{ist}), \quad (6)$$

where $a(i)$ denotes the census tract in which individual i lives, $k(i)$ denotes consumer types over which the parameters α and θ are allowed to vary, x_{igst} is the quantity of product g consumed by individual i during trip t to store type s , $\sum_g \phi_{a(i)gst} = 1 \forall a, s, t$, $POS_{ist} = 1$ if the store of type s at which individual i makes trip t has adopted a POS terminal and consumer i has adopted a card, $\xi_{a(i)k(i)st}$ denote unobserved taste shifters that are common across census tract by consumer group by store type by time, and ε_{ist} are unobserved individual by store type by time shocks.

The parameters governing the price elasticity of substitution across store types, $\alpha_{k(i)}$, and the value of being able to use a card, $\theta_{k(i)}$, are allowed to vary by consumer type for two reasons. First, identification of $\theta_{k(i)}$ relies on consumers who had cards both before and after the shock, so the identified parameter is $\theta_{k(i)}$ for this consumer group (which I will refer to as “always-takers”).

²⁵Atkin, Faber and Gonzalez-Navarro (2018) assume Cobb-Douglas preferences over product categories, while Björnerstedt and Verboven (2016) show how assuming “constant expenditures demand” (or, equivalently, Cobb-Douglas preferences) affects the estimating equation relative to the unit demand assumption in Berry (1994).

Second, both the price elasticity $\alpha_{k(i)}$ and the value of using a card $\theta_{k(i)}$ might vary by income, which I will explicitly test.

From the first order condition for good g from utility maximization with a linear budget constraint, $x_{igst} = \phi_{a(i)gst} y_{it} / p_{a(i)gst}$; plugging this into (6) and taking logs:

$$\log u_{ist} = \underbrace{\alpha_{k(i)} \log y_{it} - \alpha_{k(i)} \sum_g \phi_{a(i)gst} \log p_{a(i)gst} + \theta_{k(i)} POS_{ist} + \tilde{\xi}_{a(i)k(i)st} + \varepsilon_{ist}}_{\equiv v_{ist}}, \quad (7)$$

where $\tilde{\xi}_{a(i)k(i)st} \equiv \xi_{a(i)k(i)st} + \sum_g \phi_{a(i)gst} \log \phi_{a(i)gst}$.

From (5), for a particular trip the consumer will choose the store type that gives the most utility. Thus the probability of choosing store type s over all other store types $r \neq s$ is $\pi_{ist} = Prob(u_{ist} > u_{irt} \forall r \neq s) = Prob(\varepsilon_{irt} < \varepsilon_{ist} + v_{irt} - v_{ist} \forall r \neq s)$. Appendix A shows that after integrating over the probability distribution that a particular store has adopted POS and over the distribution of the stochastic error term (assuming it is distributed extreme value 1), the share of expenditures at store type s by consumer group k in census tract a and survey wave t , denoted ϕ_{akst} , is given by

$$\log \phi_{akst} = -\alpha_k \log P_{ast} + \theta_k \overline{POS}_{z(a)st} + \tilde{\xi}_{akst} - \log \sum_r \exp \gamma_{akrt}, \quad (8)$$

where P_{ast} is a Stone price index implicitly defined by $\log P_{ast} = \sum_g \phi_{a(i)gst} \log p_{a(i)gst}$ (i.e. a consumption share-weighted average of log prices across goods), $\overline{POS}_{z(a)kst}$ is the fraction of stores of type s in postal code $z(a)$ that have POS terminals at time t that can be used by consumer group k , and $\gamma_{akst} \equiv -\alpha_k \log P_{ast} + \theta_k \overline{POS}_{z(a)kst} + \tilde{\xi}_{akst}$.²⁶ Finally, to remove the $\log \sum_r \exp \gamma_{akrt}$ term, I subtract the log share of spending on the outside option of open-air markets, denoted ϕ_{ak0t} , which I assume do not accept card payments (i.e., $\overline{POS}_{z(a)k0t} = 0 \forall z(a), k, t$).²⁷

²⁶The phrase “that can be used by consumer group k ” is meant to capture that for consumer groups without cards, POS terminals cannot be used and hence $\overline{POS}_{z(a)krt} = 0$.

²⁷There is no merchant category code for merchants at open air markets. Over the time period studied (up to 2014), it is reasonable to assume that no merchants at open air markets had adopted POS terminals to accept card payments. Today, now that non-bank e-payment companies (analogous to Square and Clover in the US) have entered the market, some open-air merchants have adopted technology to accept card payments.

Thus the estimating equation is

$$\log \phi_{akst} - \log \phi_{ak0t} = -\alpha_k(\log P_{ast} - \log P_{a0t}) + \theta_k \overline{POS}_{z(a)kst} + \eta_{j(a)ks} + \delta_{kst} + v_{akst}. \quad (9)$$

In this estimating equation I have rewritten $\tilde{\xi}_{akst} - \tilde{\xi}_{ak0t} = \eta_{j(a)ks} + \delta_{kst} + v_{akst}$ so that the estimation will include locality by consumer group by store type and consumer group by store type by survey wave fixed effects, where $j(a)$ denotes the locality of census tract a .²⁸

Endogeneity and identification. There are two endogenous variables on the right-hand side of (9): $(\log P_{ast} - \log P_{a0t})$ and $\overline{POS}_{z(a)kst}$, which both likely respond to stochastic time and store type-varying demand shocks and are thus correlated with v_{akst} . I instrument for prices using a Hausman (1996) price index, which is based on prices in different areas. Specifically, following Atkin, Faber and Gonzalez-Navarro (2018), the instrument is the leave-one-out average price difference in *other* census tracts within the same region, $\frac{1}{|R(a)|-1} \sum_{b \neq a \in R(a)} (\log P_{bst} - \log P_{b0t})$, where $|R(a)|$ is the number of census tracts in region $R(a)$ in which census tract a is located.²⁹ If demand shocks consist of independent local (census tract level) and regional components, this instrument will be uncorrelated with v_{ast} . Identification of α_k thus depends on shocks of this type that lead to price changes that induce consumers to shift consumption across store types.

I instrument for adoption of POS terminals, $\overline{POS}_{z(a)kst}$ —which may also be endogenously related to local demand shocks—with the exogenous shock to debit card adoption $D_{j(a)t} = 1$ if locality j has received the card shock yet at time t . We have already seen that this instrument is plausibly exogenous and has a strong first stage on POS terminal adoption.

Identification of θ_k then depends on the debit card shock leading to a change in corner stores' POS terminal adoption, which leads some consumers to shift some of their shopping trips from super markets to corner stores. We have already seen that the shock indeed has both of these effects in Section 5. If the value to a consumer of a store having adopted POS is the same across store types

²⁸Because the household survey is not a panel of individuals, it is also not a panel at the census tract level. Including census tract by store type fixed effects would entail the loss of many observations.

²⁹There are five official regions in Mexico, defined by the Instituto Federal Electoral.

(as assumed in this model) and nearly all supermarkets have a POS terminal even before the shock (as observed in the data), consumer trips made to the supermarket prior to a consumer adopting a card would still be made to the supermarket after the consumer adopts a card. (Intuitively, the consumer’s revealed preference was to shop at the supermarket when the consumer paid by cash and could hence shop at either store type. Once the consumer adopts a card and the corner store adopts POS, she will still be able to shop at either store type but now pay by card which she values equally across store types. Hence she will still prefer the supermarket.) Hence, θ_k is only identified for those who already had cards prior to the shock. Thus I will restrict the estimation to consumers with a credit card. Because the shock affects debit but not credit card adoption, credit card holders are a subset of the group for which θ_k is identified.

Data. Log spending shares among cardholders are estimated using the ENIGH consumption module described in Section 3.3. Because store type is only reported for the consumption of food and beverages, other consumption is excluded. While the ENIGH is publicly available, the 2006–2010 waves only include geographic identifiers at the municipality level; I obtained confidential census tract-level geographic identifiers for each household in the data set from INEGI, accessed on-site at INEGI’s Microdata Lab. Because the survey only asks about credit card ownership, θ_k is estimated for credit card holders, and thus might be upward-biased relative to θ_k for debit or credit card always-takers (since debit card always-takers may be poorer and value card use less). Nevertheless, the restriction to credit card holders has the advantage that the debit card shock only has a spillover effect on other consumers’ debit card but not credit card adoption. Hence, worries about the composition of the sample responding to the shock, which would lead to a violation of the exclusion restriction for the instrument $D_{j(a)t}$, do not occur in practice. Thus we can think of credit card holders as a subset of “always-takers” (Angrist, Imbens and Rubin, 1996), i.e. non-beneficiaries who would have cards regardless of whether the policy shock occurs.

Prices are unit values from ENIGH, where the price of good g in store type s at time t is averaged across the price reported by each household that consumed that good in each census tract a . The alternative of using prices from the micro-CPI data adds additional noise since the geographic

identifier in those data is the municipality and only 96 urban municipalities are included, so over half of the sample would be lost. The weights $\phi_{a(i)gst}$ used to construct the price indices are expenditure shares calculated within each census tract by good by store type by survey wave in ENIGH. Each good is one of the 242 food and beverage product categories included in the survey’s consumption module. Goods that are not available in a particular area or store type are accounted for in the estimation since these will have zero expenditures and thus zero weight in the price index; the welfare impacts of differences in available variety across store types are captured in the locality by consumer group by store type and consumer group by store type by survey wave fixed effects $\eta_{j(a)ks}$ and δ_{kst} (as long as these differences are not time-varying within a locality by store type).

The share of stores of type s that have adopted POS terminals in postal code $z(a)$ at time t is constructed by combining two data sets. The number of stores with POS terminals comes from the data from Mexico’s Central Bank described in Section 3.2, where store type is identified using the merchant category code. The total number of stores in each postal code is constructed from a data set on the geocoordinates of the universe of firms in Mexico, provided by INEGI. In this data set, store type is identified using the four-digit North American Industry Classification System (NAICS) code.³⁰

Estimation results. I first estimate (9) for just one consumer group—cardholders. As discussed above, the survey only asks about credit (but not debit) card ownership, and the shock does not have a direct or spillover effect on credit card adoption. Hence, we can think of this group as a subset of existing cardholders or “always-takers.” Column 1 of Table 3 estimates (9) for this one consumer group: I find $\hat{\alpha}_k$ of 3.23 (significant at the 5% level) and $\hat{\theta}_k$ of 0.93 (significant at the 1% level). Noting that $\alpha_k + 1$ gives the elasticity of substitution across store types if utility exhibits constant elasticity of substitution (CES) across store types, this estimate of $\hat{\alpha}_k$ is at the upper end of the range of estimates from Atkin, Faber and Gonzalez-Navarro (2018) and Einav et al. (2017),

³⁰Other studies using merchant category codes to identify store types include Einav et al. (2017) and Ganong and Noel (2018), while studies using NAICS codes to distinguish firm types include Mian and Sufi (2014), Giroud and Mueller (2017), and Giroud and Rauh (forthcoming).

which makes it conservative for welfare estimates.³¹ We can interpret $-\theta_k/\alpha_k$ as the price index equivalent value of all stores adopting POS relative to a scenario in which no stores have adopted POS: this extreme change in technology adoption would be equivalent, from a welfare perspective, to a 30% price reduction.

Next, I estimate (9) with two consumer groups: credit card holders with above and below median income.³² This type of consumer heterogeneity is important to estimate for two reasons. First, because θ_k is only defined for always-takers, but because I will need to assume for estimating consumer surplus that beneficiaries and compliers have the same θ_k as always-takers, it is useful to test whether θ_k varies across income within credit card holders. Second, I test whether price elasticities vary by income; poorer consumers may be more price elastic and thus have a higher α . A caveat in this estimation is that many census tracts do not have consumers belonging to both groups and are hence dropped from the estimation. Column 2 of Table 3 shows the results.

I find a similar θ_k for the poorer half of consumers ($\hat{\theta}_k = 0.80$) relative to the pooled estimation from column 1, and no difference in the value of POS for richer and poorer cardholders (the coefficient on the term interacting the share of stores with POS and a dummy for above-median income is 0.01). This is reassuring: to make traction on estimating the consumer surplus of the policy for different consumer groups, I will need to assume that conditional on having a card, beneficiaries who received a card and non-beneficiary compliers who adopted in response to the shock—who are on average poorer than always-takers—value POS the same as always-takers. The estimates of α_k separately for richer and poorer cardholders are not statistically significant, but the magnitudes are reasonable: for poorer cardholders (who are generally more price elastic), the point estimate of $\hat{\alpha}_k$ is 3.76, while it is 2.20 for richer cardholders.

³¹To see that $\alpha_k + 1$ gives the elasticity of substitution across store types under CES, consider the simplified model with a composite good x_s available from each store type s and CES utility function $U(\mathbf{x}) = \left(\sum_s x_s^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}}$, where σ is the elasticity of substitution. The first order conditions from maximizing utility subject to a linear budget constraint lead to the following expression for quantities consumed at store types s and 0: $(x_s/x_0)^{-1/\sigma} = p_s/p_0$. Multiplying both sides by $(p_s/p_0)^{-1/\sigma}$, taking logs, and simplifying gives $\log(p_s x_s/p_0 x_0) = (1 - \sigma) \log(p_s/p_0)$. Finally, dividing the numerator and denominator in the left-hand side by total expenditures, $\log \phi_s - \log \phi_0 = (1 - \sigma)(\log p_s - \log p_0)$, where ϕ_s is the share of expenditures at store type s . Comparing this to (9), we see that $1 - \sigma = -\alpha_k$, or $\sigma = \alpha_k + 1$.

³²Median income is defined among credit card holders within each survey wave.

Welfare. Following Atkin, Faber and Gonzalez-Navarro (2018), a first-order approximation of the proportional change in consumer surplus induced by a price change is given by

$$\frac{CV_k}{e(P^1, U^0)} \approx -\sum_s \phi_{ks}^1 \left(\frac{P_s^1 - P_s^0}{P_s^1} \right), \quad (10)$$

where CV_k denotes the compensating variation for consumer group k , e is the expenditure function, and ϕ_{ks}^1 is the expenditure share of consumer group k at store type s after the change. Appendix A shows the full derivation of (10). From (8), we can write

$$-\frac{\theta_k}{\alpha_k} = \frac{d \log s_{akst} / d \overline{POS}_{akst}}{d \log s_{akst} / d \log P_{ast}} = \frac{d \log P_{ast}}{d \overline{POS}_{akst}}. \quad (11)$$

Thus $-(\theta_k/\alpha_k)$ gives the price index equivalent of a change from a world in which no stores have adopted POS terminals to one in which all stores have adopted POS terminals; therefore, we can replace $(P_s^1 - P_s^0)/P_s^1 \approx d \log P_s \approx -(\theta_k/\alpha_k) \Delta POS_{ks}$ in (10), where ΔPOS_{ks} is the change in the fraction of stores of type s at which consumers of type k can use a card.

The proportional change in consumer surplus from the supply-side's response to the demand-side policy shock, estimated for those with cards after the shock (who either already had cards, received cards from the government, or adopted cards in response to the shock), is thus approximately

$$\left[\sum_s \phi_{ks}^1 \frac{\theta_k}{\alpha_k} \Delta POS_{ks} \right] - \frac{A_k}{y_k}, \quad (12)$$

where A_k is the cost of card adoption paid by consumer group k , and y_k is total expenditures. If consumer group k already had cards, ΔPOS_{ks} is the change in the concentration of POS terminals and $A_k = 0$ since the adoption cost was already paid in a previous period. If consumer group k previously did not have cards, ΔPOS_{ks} is the fraction of stores with POS after the shock, given that before the shock these consumers did not have cards and hence experienced $POS_{ks} = 0$. For consumers who receive cards from the program I assume $A_k = 0$, while I impose upper and lower bounds for A_k for consumers who did not receive cards from the program but adopt now that they

can use a card at more corner stores.

Using the change in POS terminals at corner stores as a result of the shock, I estimate that existing cardholders—who spend 25% of their total expenditures at corner stores—experience an increase in consumer surplus of 0.4% as a result of the supply-side’s POS adoption in response to the demand-side policy shock. To estimate the consumer surplus for beneficiaries, I assume they have the same θ_k as existing cardholders, which is reasonable given that below-median income credit cardholders and above-median income credit cardholders appear to have very similar values of θ_k in Table 3 column 2. Nevertheless, beneficiaries still have lower incomes than below-median income always-takers, and thus beneficiaries might have a lower θ_k and likely have a higher α (i.e., are more price elastic).³³ Both of these will mean a higher θ_k/α_k for beneficiaries, in which case my assumption overestimates the change in consumer surplus of beneficiaries. This in turn would lead to a *conservative estimate* of the percent of total benefits that are spillovers.

Under the assumption that the 40% of beneficiaries who use their debit cards to make POS transactions have the same θ_k and α_k as cardholders (while beneficiaries who do not use their debit cards to make POS transactions have $\theta_k = 0$), the average increase in consumer surplus for beneficiaries is 2.7%.³⁴ Beneficiaries spend 42% of their consumption at corner stores after the shock and benefit from going from being able to use a card nowhere (since they did not have cards) to being able to use cards at the post-shock fraction of stores that accept cards.

Finally, to estimate the benefits to new card adopters, I need to make an assumption about the costs of adoption A_k . By revealed preference, I can bound the cost of adoption: an upper bound on the cost of adoption is $A_k^U/y_k = \sum_s \phi_{ks}^1(\theta_k/\alpha_k)\Delta POS_{ks}$, i.e. non-beneficiary “compliers” are exactly on the margin of adoption after the shock, and thus have no change in consumer surplus once they pay the cost of adoption. A lower bound assumes that compliers were exactly on the margin of adopting before the shock: $A_k^L/y_k = \sum_s \phi_{ks}^1(\theta_k/\alpha_k)POS_s^0$. Since I do not observe which households are compliers in the data, I assume that they have the same spending share as non-beneficiary non-

³³Median income per capita of beneficiary households is 1600 pesos per month, while the 25th percentile of income for households with a credit card (and thus the median within below-median households) is 5100 pesos per month.

³⁴The 40% figure is taken from the Bansefi data six months after cards are received.

credit card holders (note that compliers are a subset of this group). Under this assumption, these card adopters spend 32% of their consumption at corner stores. Using the bounds on the cost of adoption, I estimate that non-beneficiary compliers' change in consumer surplus is between 0 and 0.6%.

Combining these benefits and summing absolute (rather than relative) changes in consumer surplus across individuals, a back-of-the-envelope approximation of the proportion of consumer surplus that is spillovers to other consumers is given by

$$\frac{\psi_{\text{always-takers}} CV_{\text{always-takers}} + \psi_{\text{compliers}} CV_{\text{compliers}}}{\psi_{\text{beneficiaries}} CV_{\text{beneficiaries}} + \psi_{\text{always-takers}} CV_{\text{always-takers}} + \psi_{\text{compliers}} CV_{\text{compliers}}}, \quad (13)$$

where ψ_k for $k \in \{\text{beneficiaries, always-takers, compliers}\}$ refers to the fraction of post-shock cardholders that belong to each group. These fractions are roughly 29% beneficiaries, 57% always-takers (existing cardholders), and 14% compliers (new card adopters).³⁵ Note that the CV_k terms in (13) are the absolute (rather than relative) changes in consumer surplus; they are thus increasing in total expenditures, which are higher for existing cardholders and (to a lesser extent) compliers compared to beneficiaries. These two factors—that beneficiaries make up a smaller fraction of post-shock cardholders and have lower incomes—imply that even though each individual beneficiary's proportional change in consumer surplus is substantially higher than that of always-takers and compliers, a large fraction of overall consumer surplus goes to non-beneficiaries. Specifically, I estimate that between 43 and 47% of the increase in consumer surplus caused by the policy of distributing debit cards to cash transfer beneficiaries accrues as spillovers to non-beneficiaries.

6.2 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

³⁵These fractions are not taken from the ENIGH, but are generated using various data sets and results from earlier in the paper.

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

$$y_{it} = \gamma_i + \delta_t + \sum_k \gamma_k \mathbb{I}(\text{received cards at } k)_{j(i)} \times D_{j(i)t} + \varepsilon_{it} \quad (14)$$

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. It is worth noting that any gains to corner stores likely come at the expense of supermarkets, and thus likely do not represent changes in aggregate consumer surplus.³⁶

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 4, 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.³⁷

³⁶Results for supermarkets are extremely noisy, which is not surprising given that there are 40 times more corner stores than supermarkets, and that the size distribution of supermarkets has much higher variation.

³⁷Because these results rely on a balanced sample of retailers, I test whether treatment differentially affects the

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 (14)—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 4. 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).³⁸ 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 4 show the results. The story that emerges is that corner stores increase their profits by increasing both their purchases of and revenues from merchandise

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.6 shows that earlier vs. later treatment did not have a statistically significant effect on the probability of survival.

³⁸The 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).

while keeping other input costs (wages, rent, and number of employees) fixed. It is possible that a portion of the profits increase is due to other factors related to the demand shock they experience: for example, richer customers likely buy higher-margin products. If this were the case, the increase in merchandise sales should exceed the increase in merchandise costs; while this is true of the point estimates in each specification, I do not have power to reject that the point estimates are equal.

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 points on a low base of 12.5%, or a 20% increase in formality.³⁹ 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.⁴⁰

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. 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:

$$\log Price_{gst} = \eta_{gs} + \delta_t + \sum_{k=a}^b \phi_k D_{m(s)t}^k + \epsilon_{gst}, \quad (15)$$

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

³⁹Because 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.

⁴⁰Higher 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.

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.⁴¹

Retailer wages and employment. Welfare could also be affected by the debit card shock if either corner stores or supermarkets adjust wages or employment in response to consumers' changes in demand in response to the card shock and subsequent corner store POS adoption. We have already seen that corner stores do not appear to adjust wages or the number of employees, but because there are far fewer supermarkets than corner stores, the same outcomes for supermarkets have such large standard errors that the analogous tests for supermarkets are uninformative. Thus, I now turn to a quarterly labor force survey to test whether retail employee's self-reported wages or employment status changes as a result of the shock. Although the data are a rotating panel at the individual level, I continue using an event-study specification with locality fixed effects given that the rotating panel only lasts for five quarters for each individual.

For wages, I estimate the following variant of (1) where outcomes are now at the individual level:

$$\log \text{Monthly salary}_{it} = \lambda_{j(i)} + \delta_t + \sum_{k=a}^b \phi_k D_{j(i)t}^k + \varepsilon_{it}. \quad (16)$$

I again separately estimate the results for corner store and supermarket employees, using the NAICS codes to classify firm types in the survey.⁴² The results are shown in Appendix Figure C.7. There is no evidence of a change in monthly wages (which would account for a change in the wage rate or a change in hours assigned to employees) at either type of store.⁴³

For employment, the survey includes questions on whether the individual ever left a job or was terminated, the reason, and the sector of that job. I thus estimate (16) with the dependent variable

⁴¹For comparison, Atkin, Faber and Gonzalez-Navarro (2018) find that when Walmart enters a municipality, prices at traditional retailers fall by 3–4%.

⁴²Corner store owners (and, rarely observed super market owners) are excluded.

⁴³The survey asks individuals how much they were most recently paid and for what time period; I convert these to monthly salaries (for example, if the employee reports their payment for the past week's work, I multiply this by 52/12 to get the monthly salary).

as a dummy variable for “lost corner store/supermarket job” where that variable is coded as 1 if the individual previously had a job at that type of store, reported that the job ended because the individual lost it or was terminated or the business closed, and is still unemployed at the time of the survey. Individuals currently employed at corner stores or supermarkets—as well as individuals who lost a job at that store type but are currently employed elsewhere—are included in the regression and coded with a 0 for the “lost job” variable. Appendix Figure C.8 shows that the shock does not appear to have had a statistically significant effect on individuals’ employment status at corner stores or super markets.

7 Alternative Explanations

7.1 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.⁴⁴

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

⁴⁴Banks 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.

across the fees charged by each issuing bank with a branch presence in the municipality.

Appendix Figure C.9 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.

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 anticipating 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.10 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).

7.2 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.12a 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.⁴⁵ This is inconsistent with word-of-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.12b). 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.⁴⁶ 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 municipal-

⁴⁵Note that some beneficiaries still shop at corner stores in these municipalities, so we would not expect precise 0 point estimates.

⁴⁶The median is defined within the subset of localities where beneficiaries prefer to shop at supermarkets.

ities where beneficiaries preferred corner stores but their contacts' baseline adoption was similar or higher. Appendix Figure C.13 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 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 5 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%.⁴⁷

7.3 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

⁴⁷This estimate is statistically insignificant from zero, which is not surprising given the small number of municipalities (21) in this regression.

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 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 6.2, 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 an additional test 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.11 shows that it is not just the amount spent ($\text{price} \times \text{quantity}$) 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.14 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.

8 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 the direct effect on consumers who adopt financial technologies, but also on how the supply side of the market responds to their adoption 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 12% 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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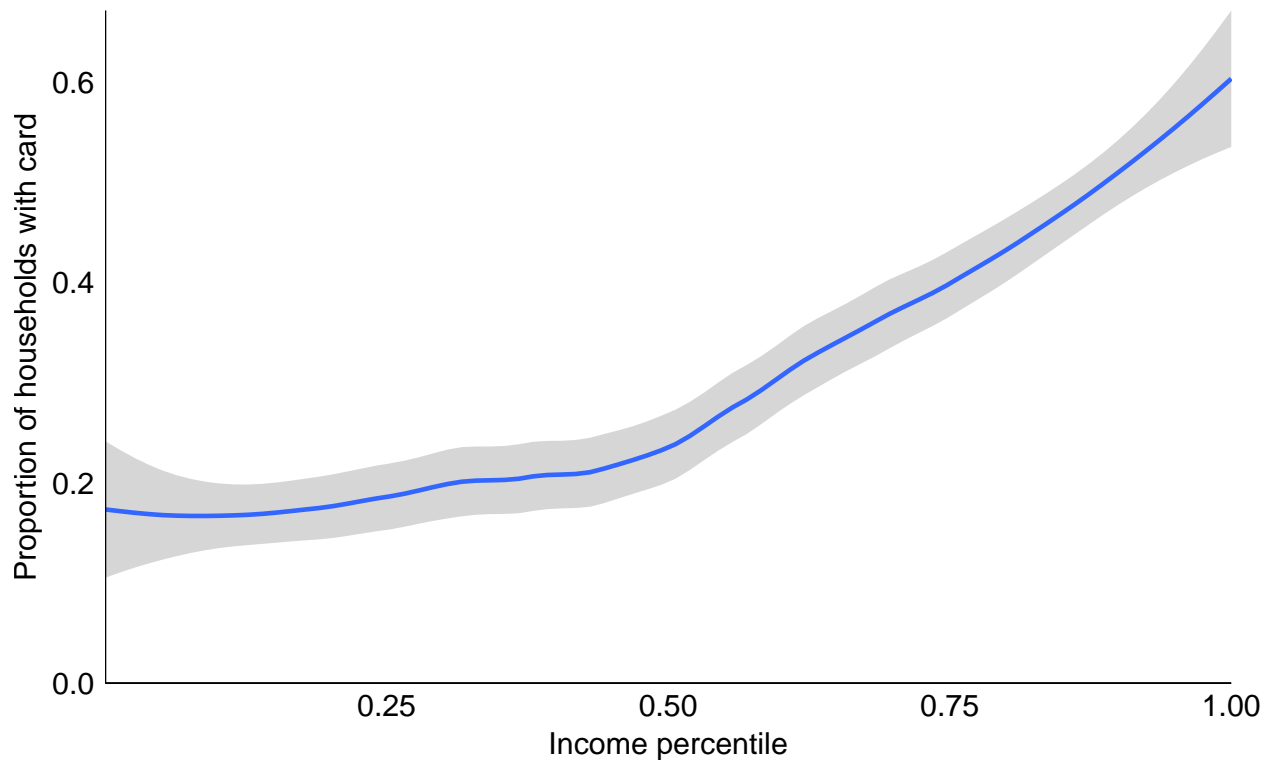
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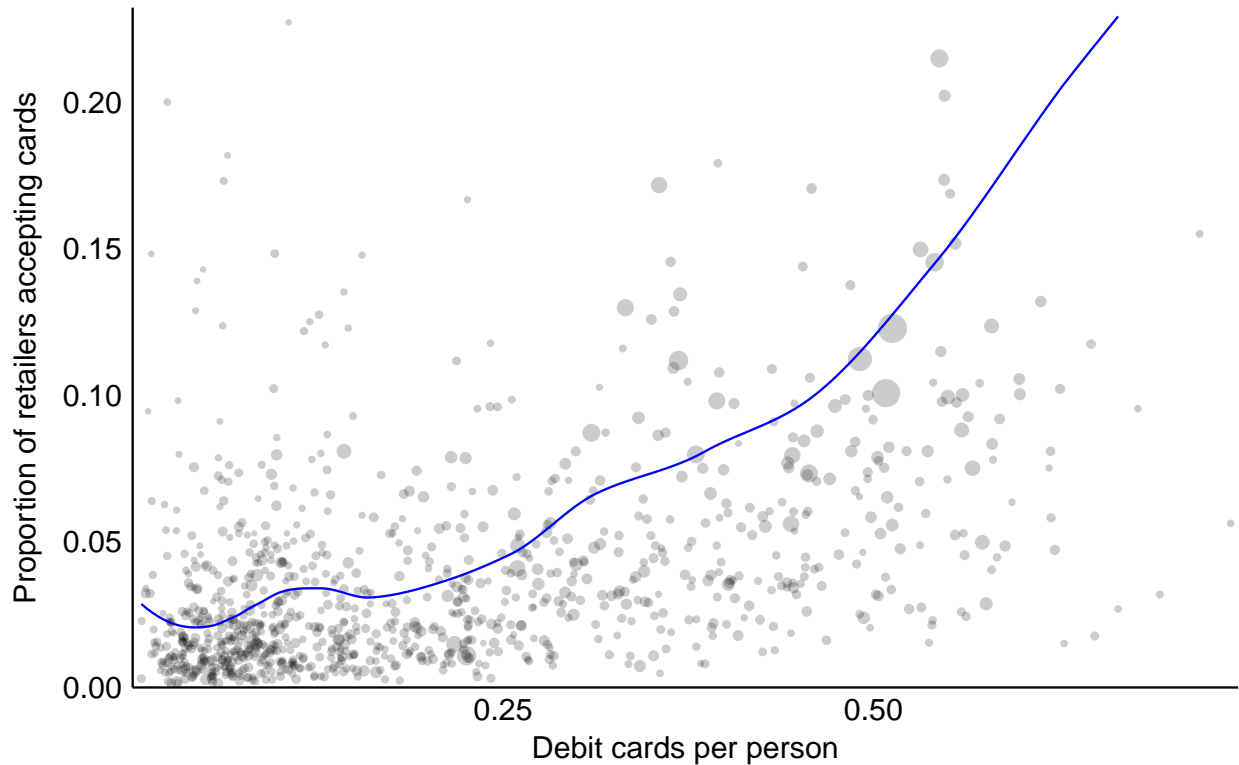
Figures and Tables

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.

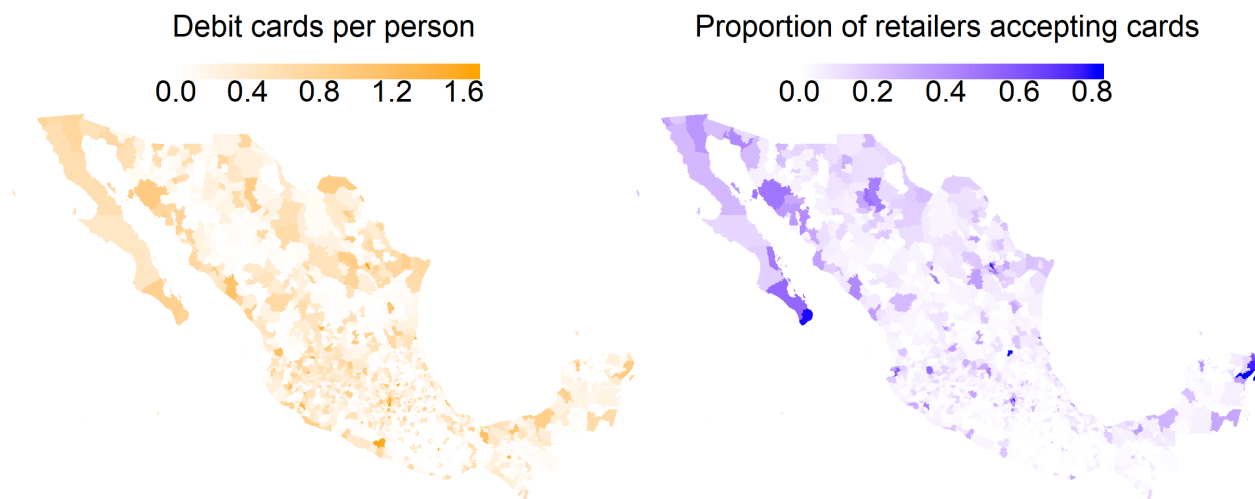
Figure 2: Cross-sectional correlation between adoption of cards and POS terminals



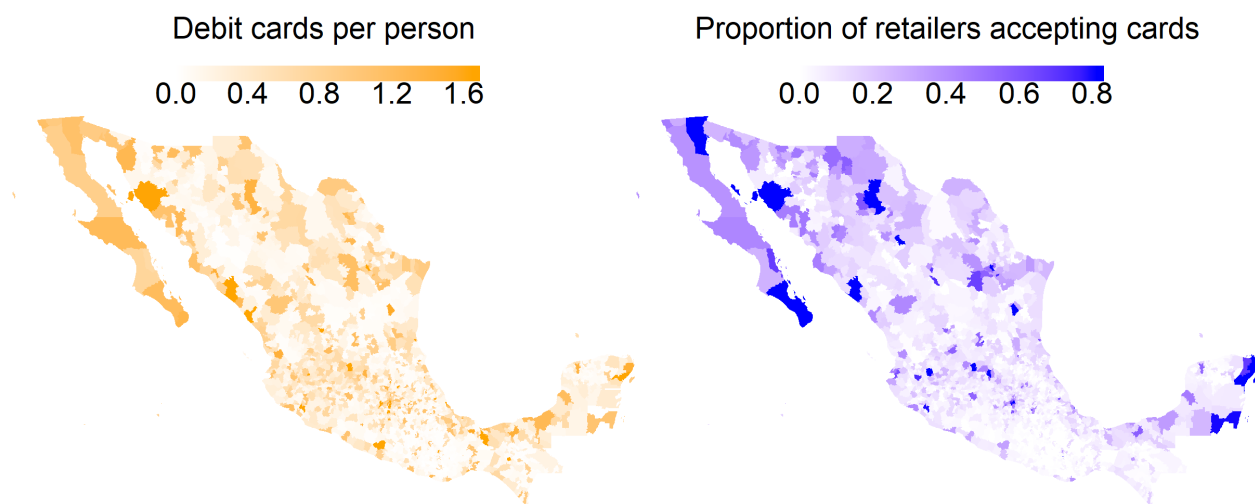
This figure shows the proportion of retailers accepting cards (constructed as the number of businesses with POS terminals using CNBV data divided by the number of retailers using INEGI data) and the number of debit cards per person (constructed as the number of debit cards using CNBV data divided by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



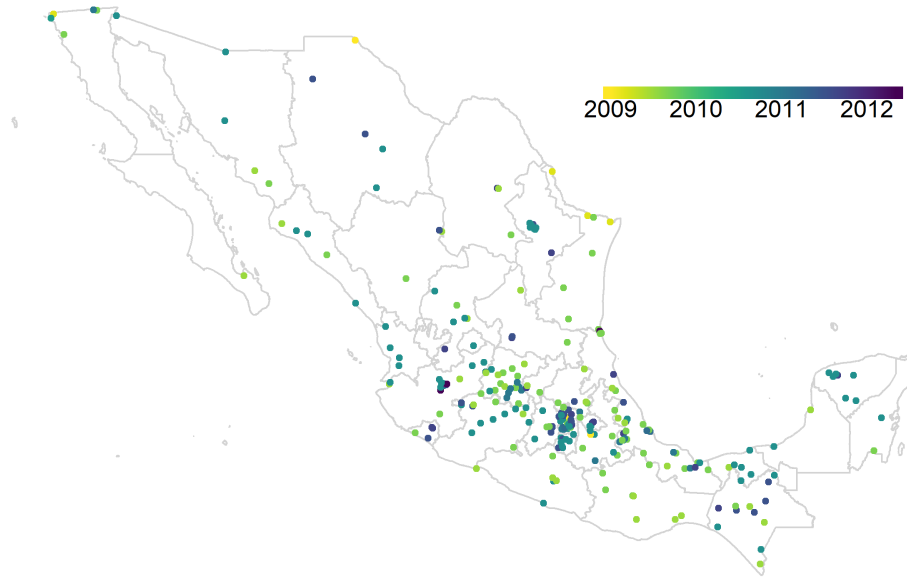
(b) December 2016



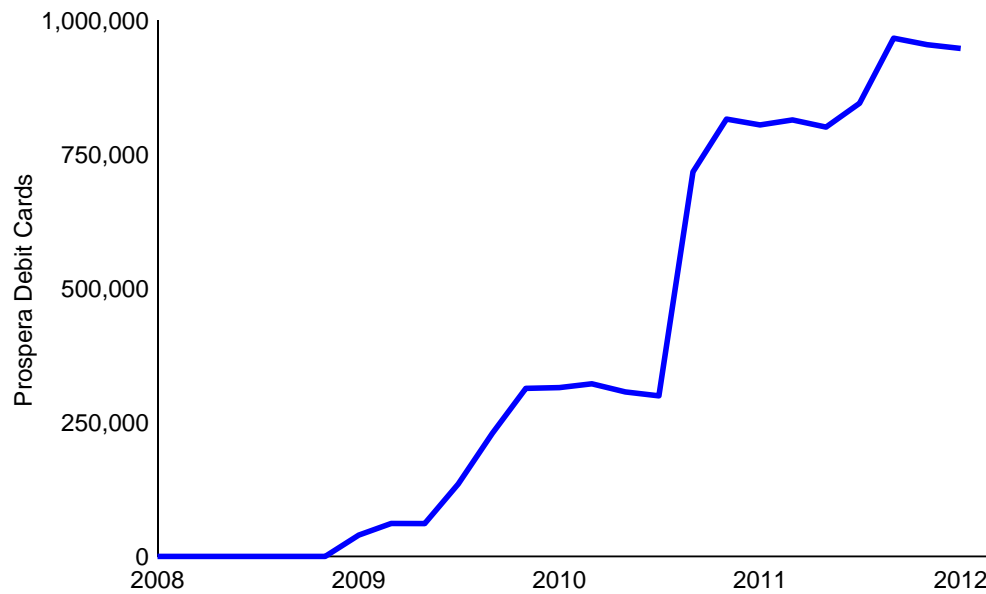
This figure shows the municipality-level number of debit cards per person (constructed as the number of debit cards using CNBV data divided by the population using INEGI data) and proportion of retailers accepting cards (constructed as the number of businesses with POS terminals using CNBV data divided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

Figure 4: Debit card rollout over space and time

(a) Rollout over space and time

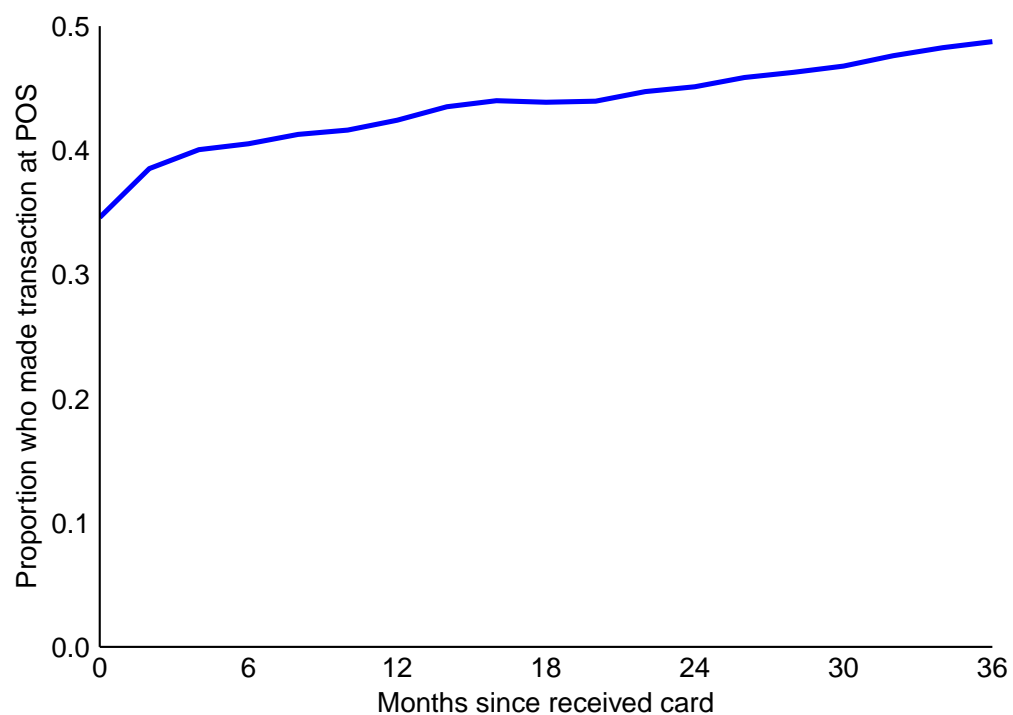


(b) Number of households treated over time



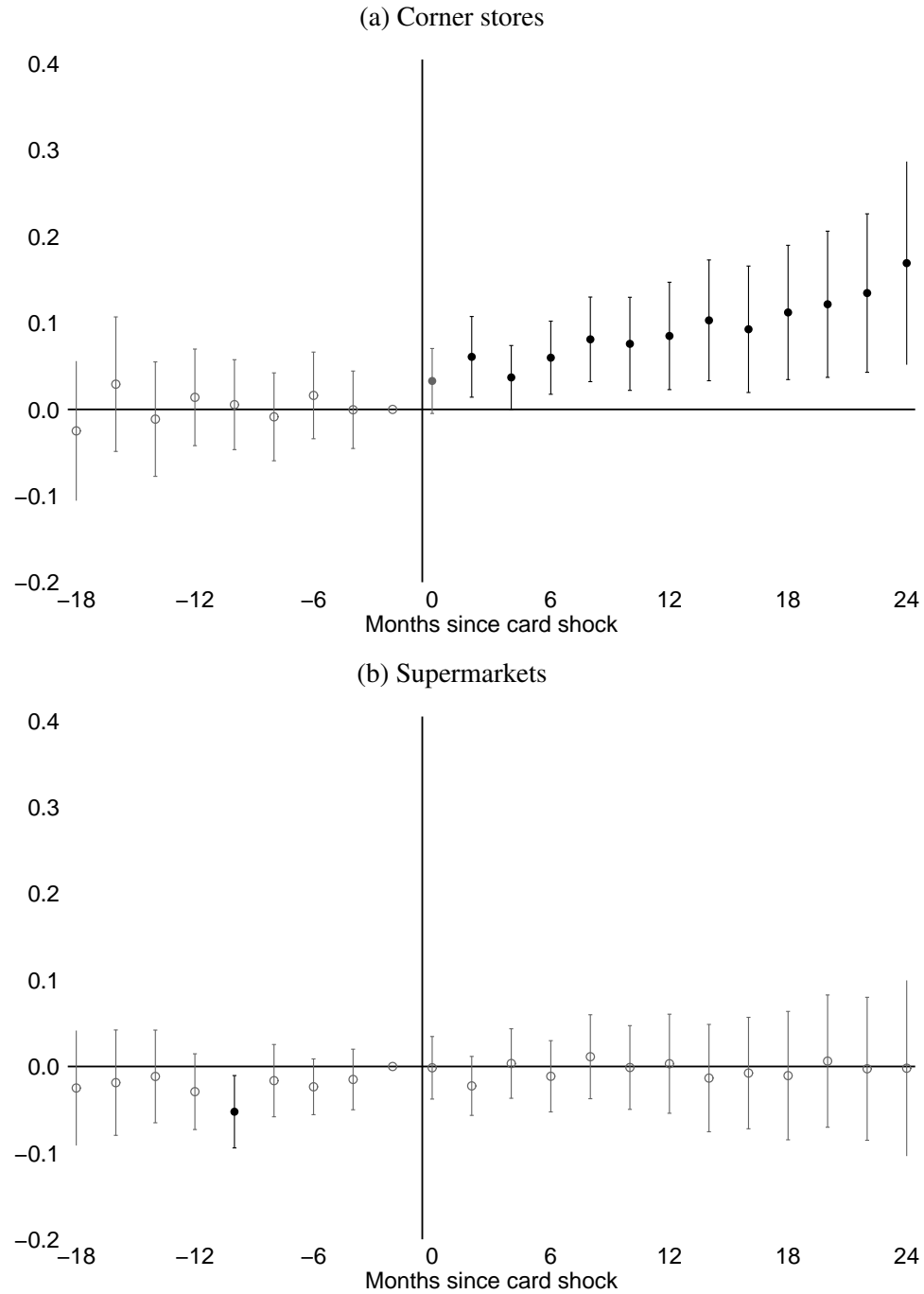
This figure shows when beneficiaries in each urban locality received debit cards from Prospera. It uses administrative data from Prospera on the number of beneficiaries and payment method in each locality during each payment period, which I used to impute when the debit card shock occurred in each locality; 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



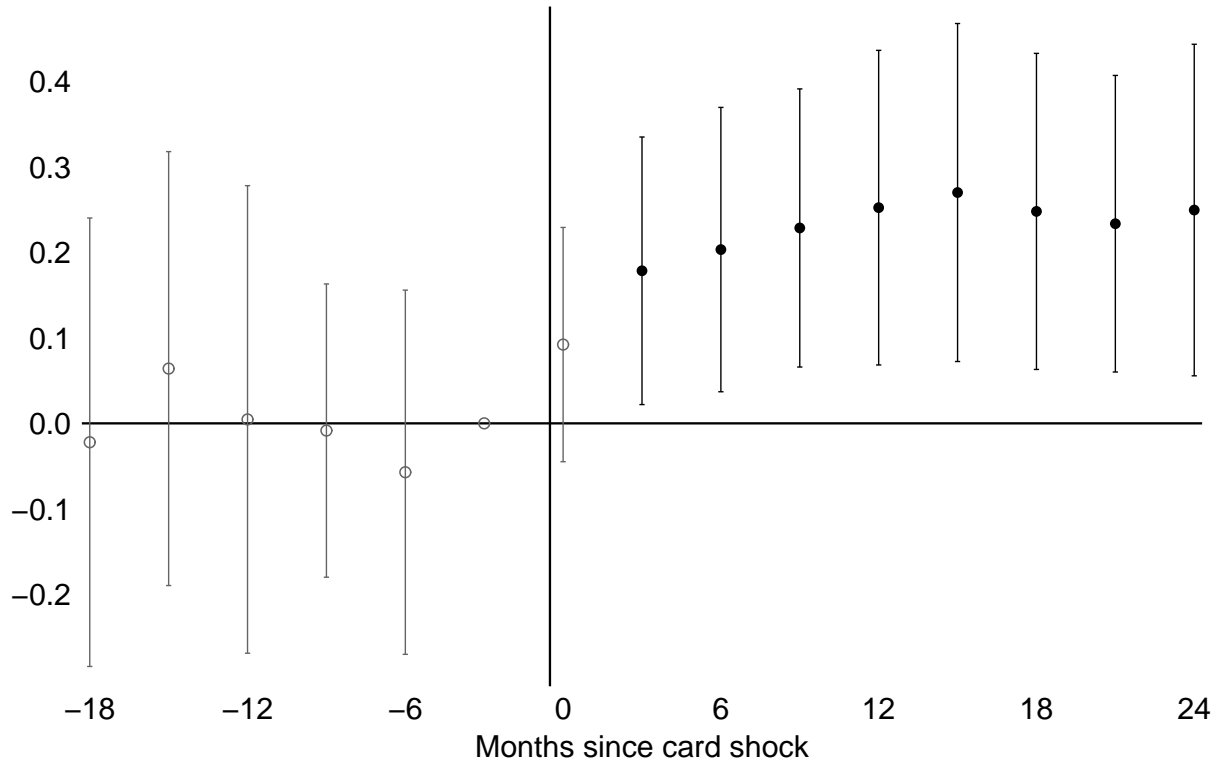
This figure shows the proportion of Prospera cardholders who make at least one transaction at a POS terminal using their card during each two-month period. Periods are binned in two-month intervals because the cash transfer is paid every two months. Source: data on 106,449,749 transactions from 961,617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

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



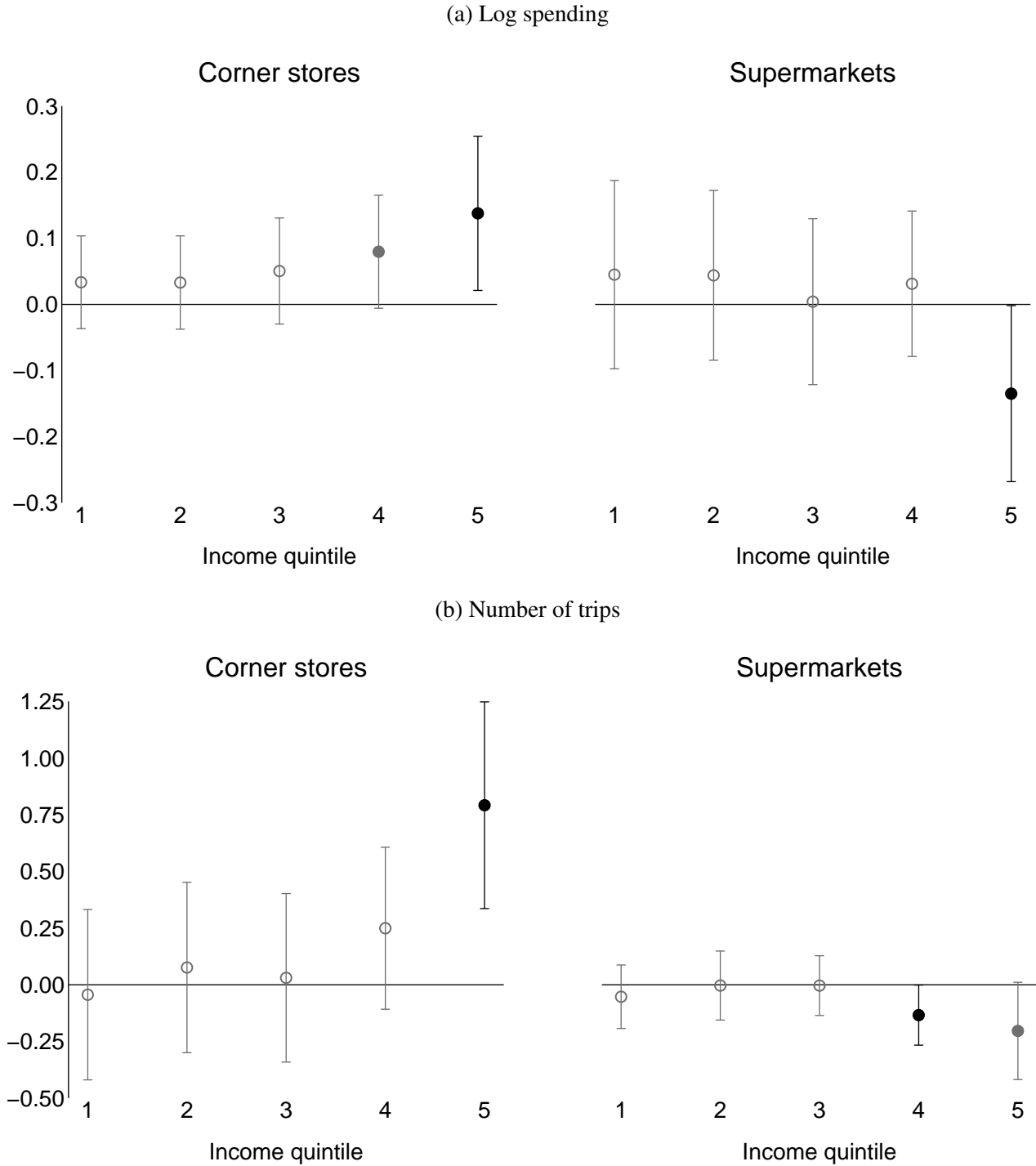
This figure shows the effect of the debit card shock on the stock of point-of-sale (POS) terminals at corner stores (panel a) and supermarkets (panel b). Corner stores respond to the debit card shock by adopting POS terminals. It graphs the coefficients from (1), where the dependent variable is the log 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. Standard errors are clustered at the locality level. The same results can be found in table form in Appendix Table C.1.

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



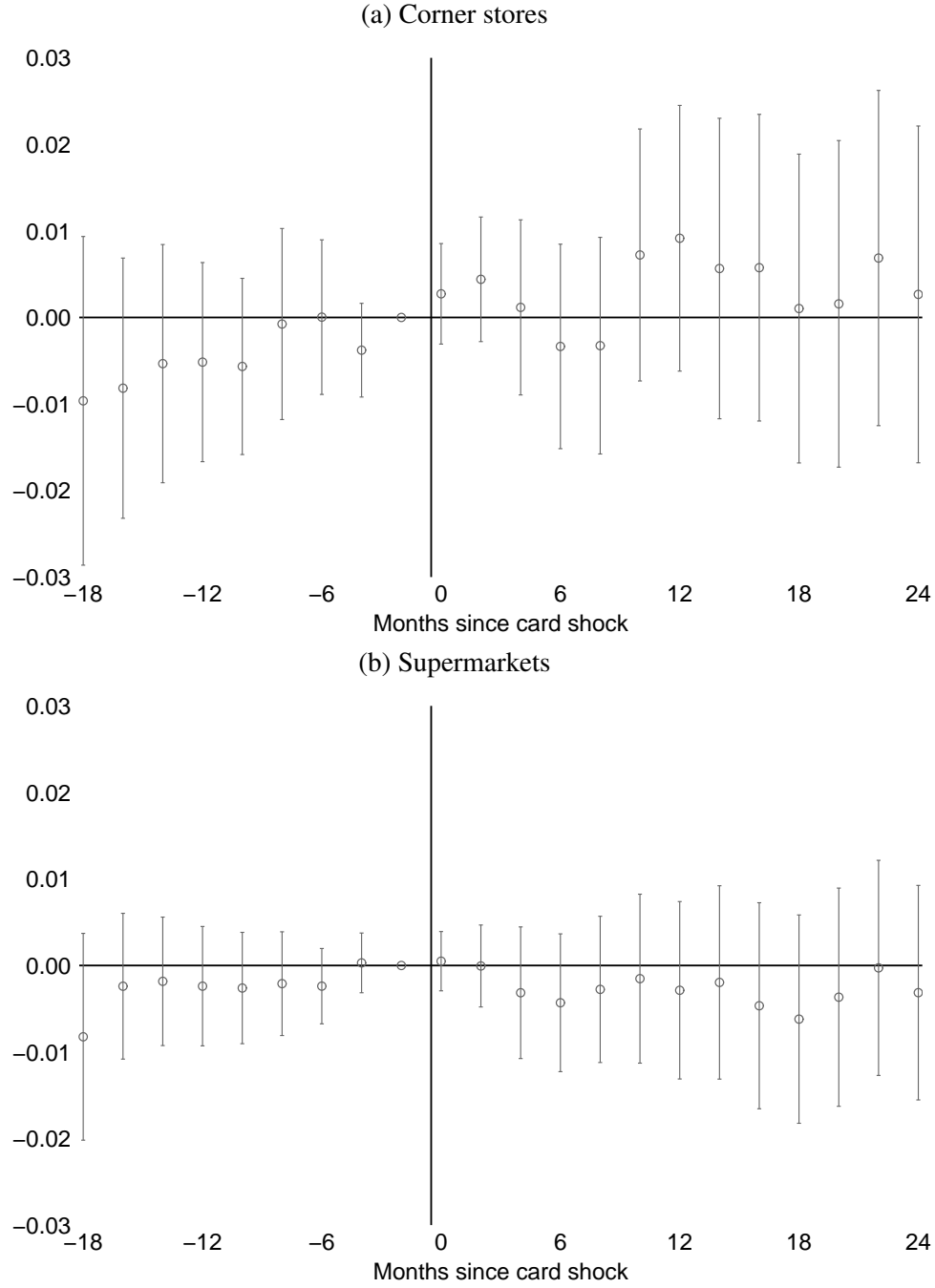
This graph shows that adoption of debit cards at other banks increases after the debit card shock. It graphs the coefficients from (1), where the outcome variable is the log stock of debit cards issued by banks other than Bansefi in municipality m in quarter t ; this variable comes from the CNBV data.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

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



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. Standard errors are clustered at the locality level.

Figure 9: Price effects



This figure shows that neither corner stores nor supermarkets change prices in response to the debit card shock. It shows the results from (15), 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) | (2) | (3) | (4) |
|---------------------------------------|-------|-----------------------|--|---|
| | Mean | Standard Deviation | Discrete Time Hazard Linear Probability | Discrete Time Hazard Proportional Hazard |
| Log point-of-sale terminals | 4.47 | 2.11 | -0.0003 (0.0093) | 0.0047 (0.0837) |
| Δ Log point-of-sale terminals | 0.81 | 0.38 | -0.0266 (0.0179) | -0.2534 (0.1549) |
| Log bank accounts | 9.27 | 3.27 | 0.0062 (0.0054) | 0.0535 (0.0451) |
| Δ Log bank accounts | 1.78 | 3.61 | 0.0055 (0.0067) | 0.0620 (0.0562) |
| Log commercial bank branches | 2.58 | 1.42 | -0.0215 (0.0185) | -0.2047 (0.1495) |
| Δ Log commercial bank branches | 0.61 | 0.95 | -0.0224 (0.0248) | -0.2581 (0.2192) |
| Log Bansefi bank branches | 0.58 | 0.41 | 0.0072 (0.0246) | 0.0812 (0.2039) |
| Log commercial bank ATMs | 3.15 | 1.74 | 0.0108 (0.0095) | 0.0980 (0.0940) |
| Log population | 11.26 | 1.24 | 0.0101 (0.0160) | 0.0919 (0.1340) |
| % illiterate (age 15+) | 6.14 | 3.69 | 0.0005 (0.0049) | 0.0053 (0.0422) |
| % not attending school (age 6-14) | 4.15 | 1.65 | 0.0006 (0.0092) | -0.0013 (0.0808) |
| % without primary education (age 15+) | 40.98 | 9.59 | 0.0015 (0.0019) | 0.0139 (0.0169) |
| % without health insurance | 45.68 | 16.15 | -0.0011 (0.0008) | -0.0104 (0.0065) |
| % with dirt floor | 5.28 | 4.83 | 0.0051** (0.0024) | 0.0497** (0.0197) |
| % without toilet | 5.89 | 3.60 | -0.0061 (0.0041) | -0.0500 (0.0344) |
| % without water | 6.45 | 9.12 | -0.0007 (0.0010) | -0.0060 (0.0094) |
| % without plumbing | 3.94 | 6.39 | 0.0020 (0.0015) | 0.0164 (0.0129) |
| % without electricity | 4.29 | 2.24 | 0.0049 (0.0047) | 0.0426 (0.0403) |
| % without washing machine | 33.64 | 14.33 | -0.0003 (0.0010) | -0.0038 (0.0093) |
| % without refrigerator | 16.80 | 9.73 | 0.0006 (0.0016) | 0.0034 (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** (0.032) | 0.049 (0.033) | −0.018 (0.043) | 0.011 (0.047) | 0.029 (0.030) | 0.031 (0.030) |
| Diff-in-diff × has credit card | | 0.071* (0.040) | | −0.043 (0.059) | | 0.009 (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: Demand estimation

| Dependent variable: log share of expenditures at store type s minus log share at outside option | | |
|---|-------------------|-------------------|
| | (1) | (2) |
| Log price difference ($-\alpha$) | -3.23** (1.56) | -3.76 (4.33) |
| Log price difference $\times \mathbb{I}(\text{Income} > \text{median})$ | | 1.56 (3.94) |
| Share of stores with POS (θ) | 0.93*** (0.03) | 0.80*** (0.33) |
| Share of stores with POS $\times \mathbb{I}(\text{Income} > \text{median})$ | | 0.01 (0.36) |
| First-stage joint F-test | 26.80 | 25.64 |
| Number of observations | 6,454 | 8,190 |
| Locality by store type fixed effects | Yes | |
| Locality by store type by $\mathbb{I}(\text{Income} > \text{median})$ fixed effects | | Yes |
| Store type by time fixed effects fixed effects | Yes | |
| Store type by time fixed effects by $\mathbb{I}(\text{Income} > \text{median})$ fixed effects | | Yes |

This table shows results from (9), which estimates the price elasticity of consumption across store types and the value of shopping at a store that has adopted a POS terminal. Only households with a credit card are included when constructing the log expenditure shares at the census tract (by consumer type) level. Observations are at the census tract by store type by time level in column 1 and the census tract by consumer type by store type by time level in column 2. Census tracts are only included if there is a positive amount of spending at all three store types within a survey wave by consumer type. The number of observations in column 2 is not double that of column 1 because many census tracts only have one of the two consumer types. There are two store types, corner stores and super markets (since the third store type, open-air markets, is treated as the outside option). In column 1 there is only one consumer type (all credit card holders) while in column 2 there are two consumer types, credit card holders with above and below median income. Median income is calculated among credit card holders and within the survey wave. To deal with endogeneity, prices are instrumented by a variant of the Hausman (1996) price index used by Atkin, Faber and Gonzalez-Navarro (2018): a within-region leave-one-tract-out price average. The share of stores with POS terminals is instrumented with the debit card shock. Regions are defined by Mexico's five official electoral regions. Standard errors are clustered at the locality level.

Table 4: Corner store outcomes

| | (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 |
|--|-------------------------|------------------------------------|------------------------------------|-----------------------------|--------------------|----------------------------|--|
| <i>Panel A: Last localities to receive cards as control</i> | | | | | | | |
| Shock 3–4.5 years ago | 0.238*** (0.082) | 0.049 (0.033) | 0.027 (0.029) | −0.010 (0.019) | −0.015 (0.041) | −0.017 (0.038) | 0.017* (0.009) |
| Shock 1.5–3 years ago | 0.169** (0.085) | 0.026 (0.033) | 0.005 (0.030) | −0.012 (0.019) | 0.019 (0.039) | −0.010 (0.034) | 0.032*** (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 |
| <i>Panel B: Pooled diff-in-diff, last localities to receive cards as control</i> | | | | | | | |
| Shock 1.5–4.5 years ago | 0.201*** (0.076) | 0.036 (0.029) | 0.016 (0.026) | −0.011 (0.016) | 0.003 (0.038) | −0.014 (0.034) | 0.025*** (0.009) |
| Number of firms | 270,673 | 270,673 | 270,673 | 270,673 | 270,673 | 270,673 | 270,673 |
| <i>Panel C: Non-treated localities as control</i> | | | | | | | |
| Shock 3–4.5 years ago | 0.198*** (0.059) | 0.051** (0.023) | 0.041** (0.021) | −0.010 (0.014) | −0.032 (0.020) | 0.001 (0.022) | −0.002 (0.007) |
| Shock 1.5–3 years ago | 0.112* (0.062) | 0.026 (0.023) | 0.018 (0.022) | −0.012 (0.014) | 0.003 (0.016) | 0.009 (0.014) | 0.015 (0.010) |
| Shock 0–1.5 years ago | −0.074 (0.069) | −0.011 (0.024) | −0.002 (0.022) | −0.012 (0.016) | −0.002 (0.029) | 0.015 (0.027) | 0.000 (0.011) |
| Number of firms | 532,374 | 532,374 | 532,374 | 532,374 | 532,374 | 532,374 | 532,374 |
| <i>Panel D: Pooled diff-in-diff, non-treated localities as control</i> | | | | | | | |
| Shock 0–4.5 years ago | 0.139*** (0.050) | 0.034** (0.017) | 0.027* (0.016) | −0.011 (0.010) | −0.013 (0.013) | 0.006 (0.013) | 0.007 (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 (14) 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 (14) 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 5: 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** (0.049) | −0.020 (0.034) | 0.043** |
| Subsequent periods | 0.234* (0.136) | 0.124 (0.147) | 0.583 |
| 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 Demand Estimation

This appendix provides more detail on the derivation of (8) and (10) to estimate the consumer gains from the policy shock.

Log share equation. Starting from (7),

$$\log u_{ist} = \underbrace{\alpha_{k(i)} \log y_{it} - \alpha_{k(i)} \sum_g \phi_{a(i)gst} \log p_{a(i)gst} + \theta_{k(i)} POS_{ist} + \tilde{\xi}_{a(i)k(i)st} + \varepsilon_{ist}}_{\equiv v_{ist}}, \quad (17)$$

the probability that consumer i makes trip t to store type s over all other store types $r \neq s$ is $\pi_{ist} = \text{Prob}(u_{ist} > u_{irt} \forall r \neq s) = \text{Prob}(\varepsilon_{irt} < \varepsilon_{ist} + v_{irt} - v_{ist} \forall r \neq s)$. Integrating over the probability distribution that the store i chooses is revealed to have adopted POS and the stochastic error term,

$$\begin{aligned} \pi_{ist} &= \int_{\varepsilon} \int_{POS} \mathbb{I}(\varepsilon_{ikt} < \varepsilon_{ist} + v_{ist} - v_{ikt}) f(POS) f(\varepsilon) dPOS d\varepsilon \\ &= \int_{\varepsilon} \mathbb{I}(\varepsilon_{ikt} < \varepsilon_{ist} + \gamma_{a(i)k(i)st} - \gamma_{a(i)k(i)rt}) f(\varepsilon) d\varepsilon, \end{aligned} \quad (18)$$

where $\gamma_{a(i)k(i)st} \equiv -\alpha_{k(i)} \log P_{a(i)st} + \theta_{k(i)} \overline{POS}_{z(a(i))k(i)st} + \tilde{\xi}_{a(i)k(i)st}$ and $\overline{POS}_{z(a(i))k(i)st}$ denotes the fraction of stores of type s that have adopted POS terminals at time t in postal code $z(a(i))$ in which individual i lives, and that individuals of consumer group k can use.⁴⁸

Assuming that ε is distributed extreme value 1, the probability that individual i chooses store type s for trip t is

$$\pi_{ist} = \frac{\exp(\gamma_{a(i)k(i)st})}{\sum_r \exp(\gamma_{a(i)k(i)rt})} \quad (19)$$

(Domencich and McFadden, 1975). Noting that in expectation, the fraction of consumer trips by type k in area a at store type s is equal to the probability that any particular consumer of type k in area a selected s for a particular trip, we can write the fraction of consumer trips to store type s in area a as $\pi_{akst} = \pi_{ist}$ for $i \in (a, k)$.

⁴⁸I observe POS adoption at the level of the postal code. Postal codes are larger than census tracts but smaller than localities.

Since a consumer's expected spending at store type s during a particular trip will equal the probability she made the trip times $\sum_g p_{agst} x_{igst}$, we have that the expected share of expenditures by consumer type k at store type s in area a at time t , denoted ϕ_{akst} , are

$$\phi_{akst} = \frac{\sum_{i \in (a,k)} \pi_{akst} \sum_g p_{agst} x_{igst}}{\sum_{i \in (a,k)} y_{it}} = \pi_{akst} \frac{\sum_{i \in (a,k)} \sum_g p_{agst} x_{igst}}{\sum_{i \in (a,k)} y_{it}} = \pi_{ist} \text{ for } i \in (a,k)$$

where we can pull the π_{akst} out of the summation because it does not depend on i , and the last equality arises from plugging in the Marshallian demand x_{igst} , or from noting that $\sum_g p_{agst} x_{igst} = y_{it}$ for each i from the first order condition on their budget constraint.

Substituting ϕ_{akst} into (19) and taking logs gives (8).

Consumer surplus. The change in consumer surplus from a change in prices can be calculated using the compensating variation:

$$CV = e(P^0, U^0) - e(P^1, U^0).$$

Following Atkin, Faber and Gonzalez-Navarro (2018), I take a first-order Taylor expansion of $e(P^0, U^0)$ around P^1 prices:

$$\begin{aligned} CV &\approx \left[e(P^1, U^0) + \sum_s \frac{\partial e(P^1, U^0)}{\partial P_s} (P_s^0 - P_s^1) \right] - e(P^1, U^0) \\ &\approx - \sum_s \frac{\partial e(P^1, U^0)}{\partial P_s} (P_s^1 - P_s^0). \end{aligned}$$

Using Shephard's lemma and duality,

$$CV \approx - \sum_s x_s^1 (P_s^1 - P_s^0) \approx - \sum_s P_s^1 x_s^1 \left(\frac{P_s^1 - P_s^0}{P_s^1} \right). \quad (20)$$

To obtain the proportional change in consumer surplus, divide both sides by expenditures after the change, $e(P^1, U^0)$, which gives (10).

Appendix B Data Appendix

The main 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, (vii) a panel on profits (including those from cash sales) of the universe of retailers, and (viii) a quarterly labor force survey. This appendix describes each of these data sets, as well as auxiliary data sets I use, 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.⁴⁹ 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.

⁴⁹In 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.

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 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).⁵⁰ 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.⁵¹ This data set on the geocoordinates of the universe of firms is described in more detail in Section B.11.

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

⁵⁰Merchant 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.

⁵¹Shapefiles 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/>.

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 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.⁵²

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

⁵²Within Mexico's 630 urban localities, there are 61,424 AGEBS, making them roughly analogous to census tracts in the US.

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

I again restrict the data to municipalities included in the card rollout.⁵³ 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.⁵⁴ 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

⁵³For 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.

⁵⁴The other product categories are clothing, shoes and accessories; housing; furniture, appliances, and domestic products; health and personal hygiene; transport; education and recreation; and other.

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 level in the data; corner stores (together with other small specialty stores) and supermarkets are distinguished at three-digit level.⁵⁵

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 Quarterly labor force survey

Mexico's quarterly labor force survey, the Encuesta Nacional de Ocupación y Empleo (ENOE), conducted by INEGI, includes about 400,000 individuals in each survey wave. It is a rotating panel where individuals are included for five consecutive quarters. The survey asks questions about employment status and wages. I use data spanning 2005–2016, and therefore includes over 20 million individual by quarter observations.

B.10 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

⁵⁵Corner 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.

(CONEVAL).

B.11 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 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.12 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

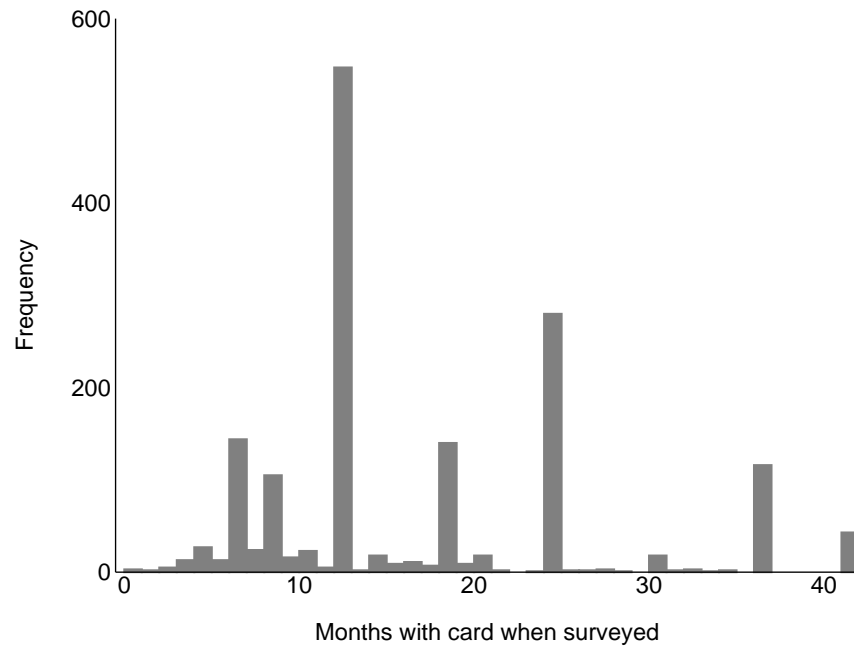
included in the rollout. I thus use it for summary statistics prior to the card rollout, such as the proportion of households with a debit or credit card across the income distribution. The 2009 wave includes 9,436 households; because the survey oversampled rural areas, just 4,232 of these households 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.1 shows their self-reported amount of time with the card at the time of the survey.⁵⁶ The 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.

⁵⁶I 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.

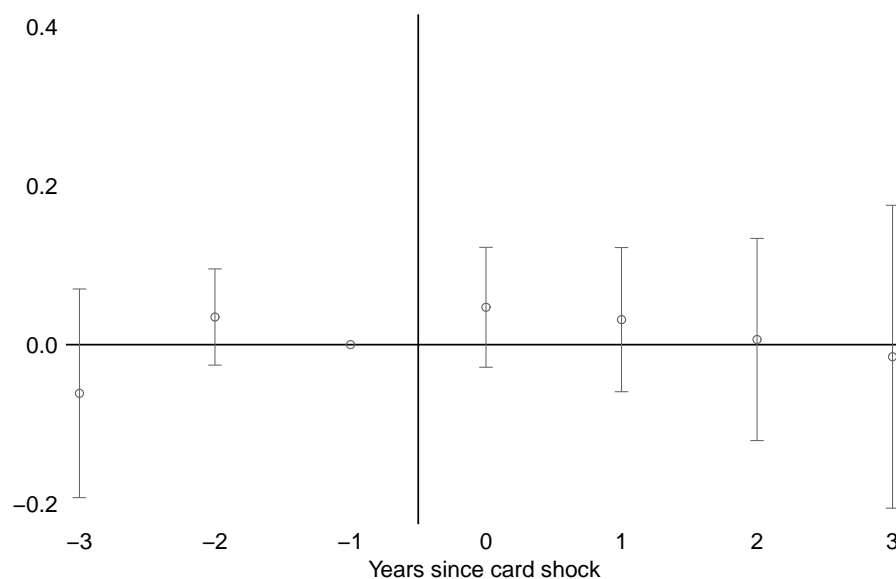
Appendix C Appendix Figures and Tables

Figure C.1: Months with debit card at time of Payment Methods Survey



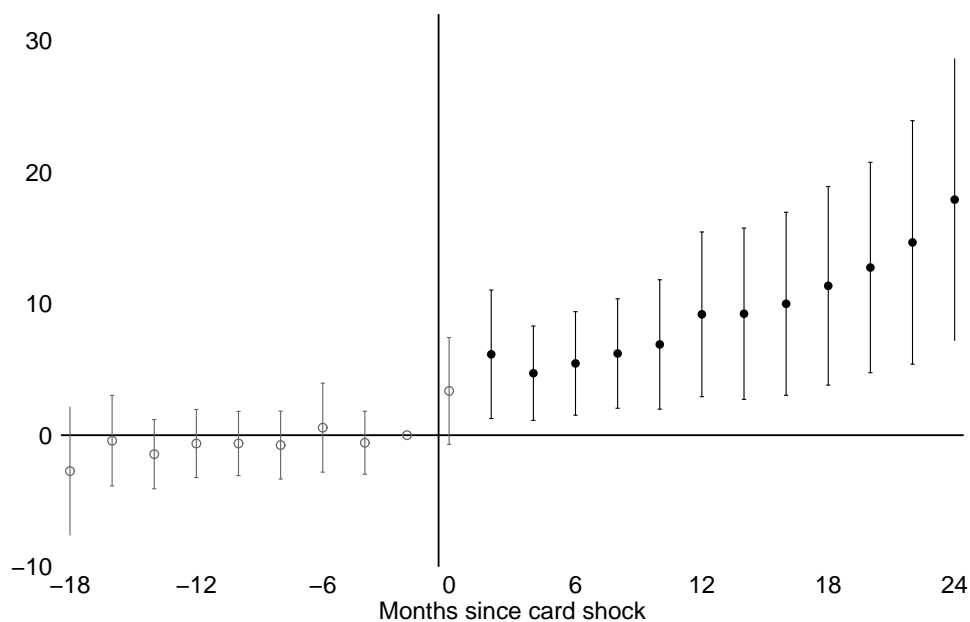
This figure shows how long ago households had received Bansefi debit cards before being surveyed in the Payment Methods Survey. I use self-reported months with the card from the survey. $N = 1,617$ beneficiaries.

Figure C.2: Number of beneficiaries not correlated with rollout



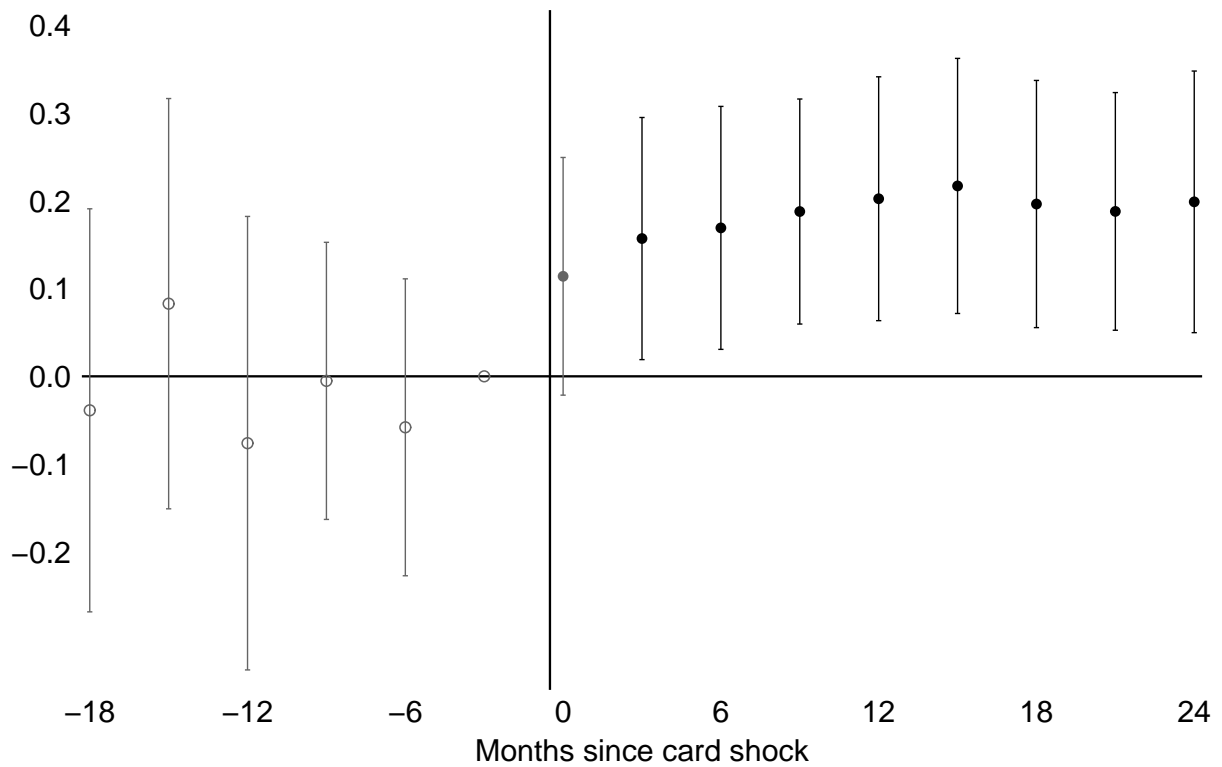
This figure shows that the rollout of debit cards is not correlated with changes in the number of beneficiaries. It shows the coefficients from (1), where the outcome is the log number of Prospera beneficiaries in locality j during the last two-month period of year t . The estimation uses administrative 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.3: Effect of card shock on corner store POS adoption **in levels** (event study estimates)



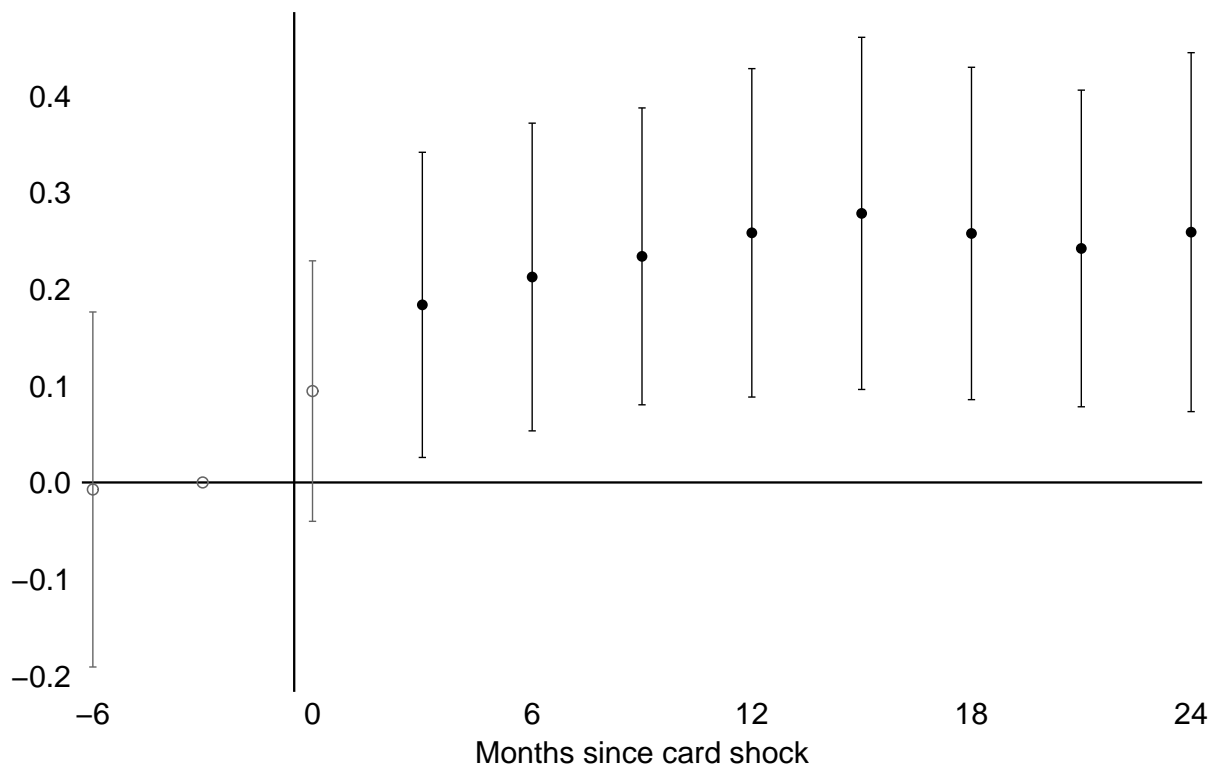
This figure shows the effect of the debit card shock on the stock of point-of-sale (POS) terminals at corner stores, measured in levels. See the notes of the corresponding figure in logs (Figure 6) for more detail.

Figure C.4: Spillover effect to other consumers' **credit and debit** card adoption (event study estimates)



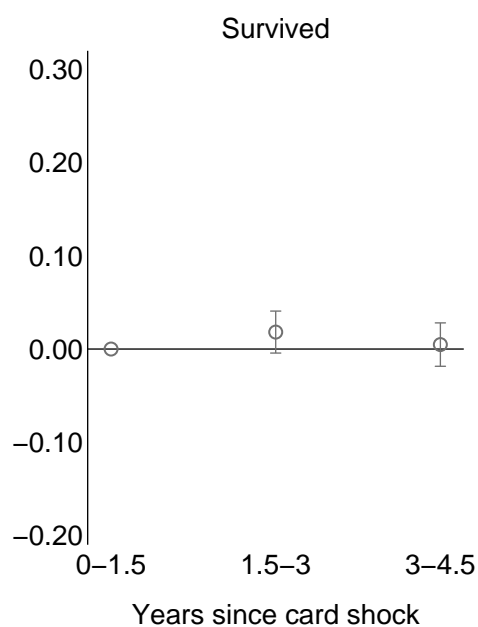
This figure shows the adoption of debit *and* credit cards at other banks relative to the debit card shock. See the notes of the corresponding figure for only debit cards (Figure 7) for more detail.

Figure C.5: Spillover effect to other consumers' card adoption (**balanced sample of municipalities**)



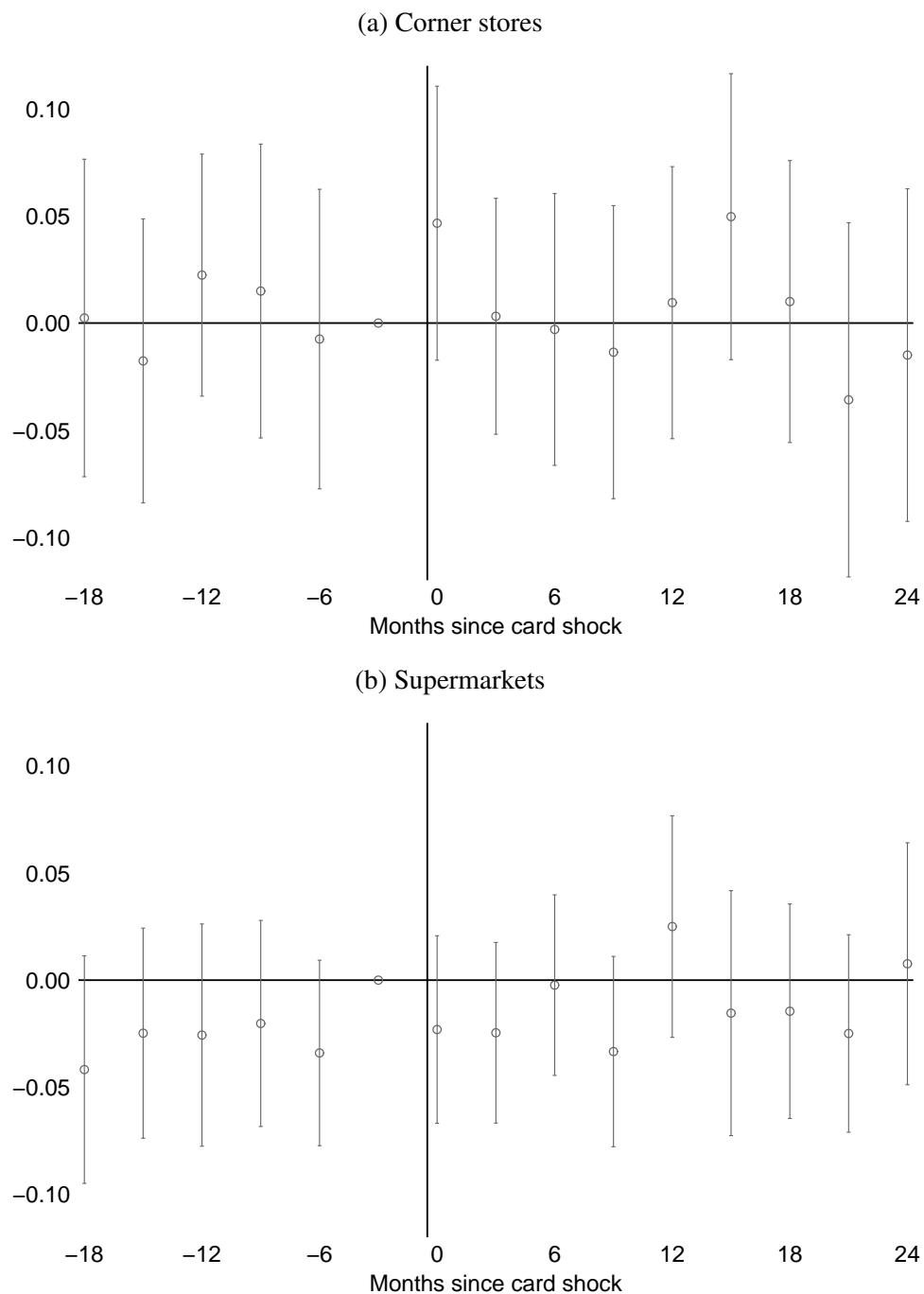
This figure shows the adoption of debit cards at other banks relative to the debit card shock, restricting the sample to the sample of localities that can be observed for all the periods in the graph. Because the data begin in the last quarter of 2008 and the card rollout began in 2009, this requires 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.6: Effect of card shock on probability of survival



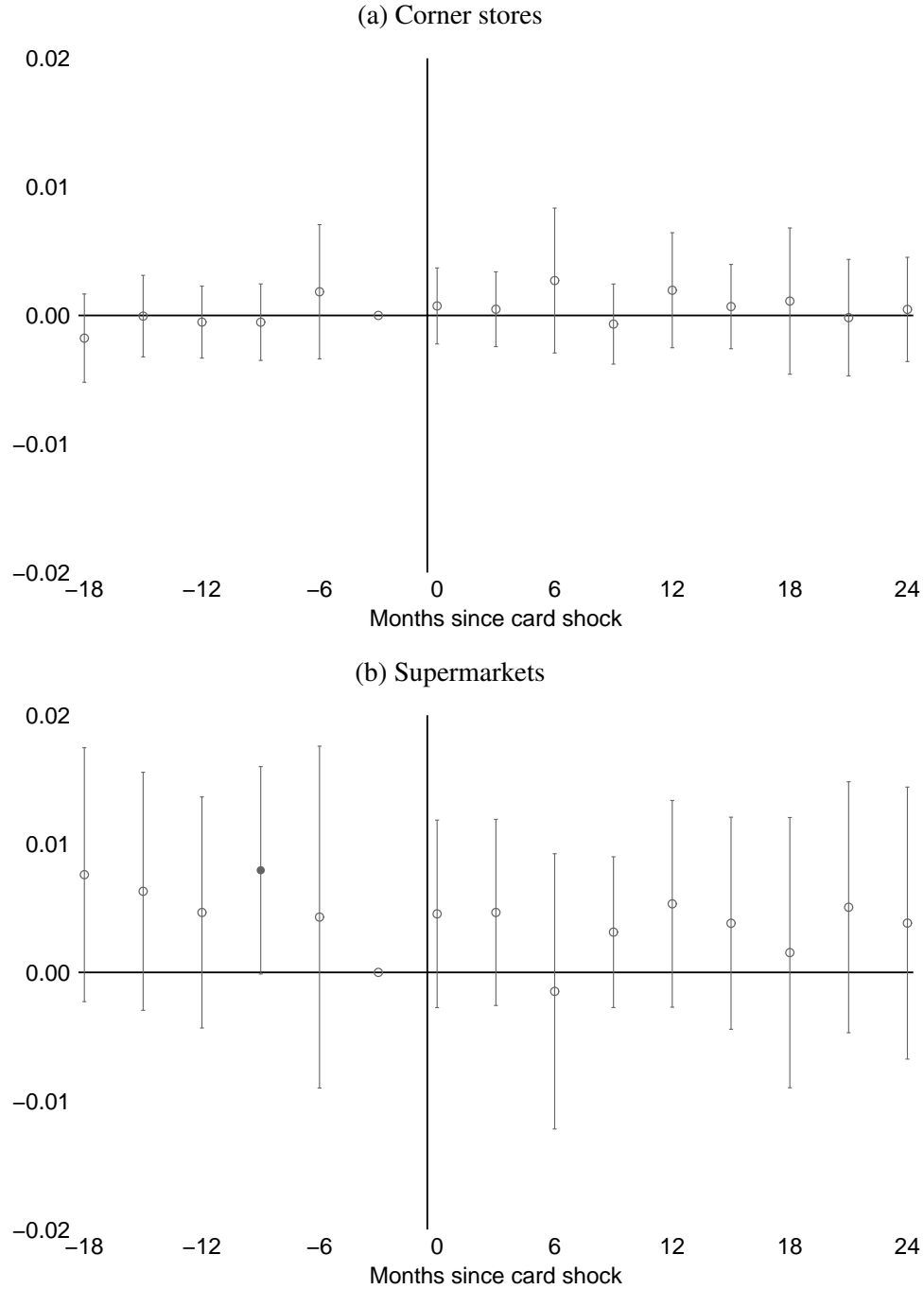
This figure shows γ_k coefficients from $Survived_i = \alpha + \sum_k \gamma_k \mathbb{I}(\text{received cards at } k)_{j(i)} + \epsilon_{it}$, where the omitted k corresponds to localities treated 0–1.5 years before the 2013 survey wave (in other words, the latest-treated localities are controls). The 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.7: Effect of card shock on retail wages (event study estimates)



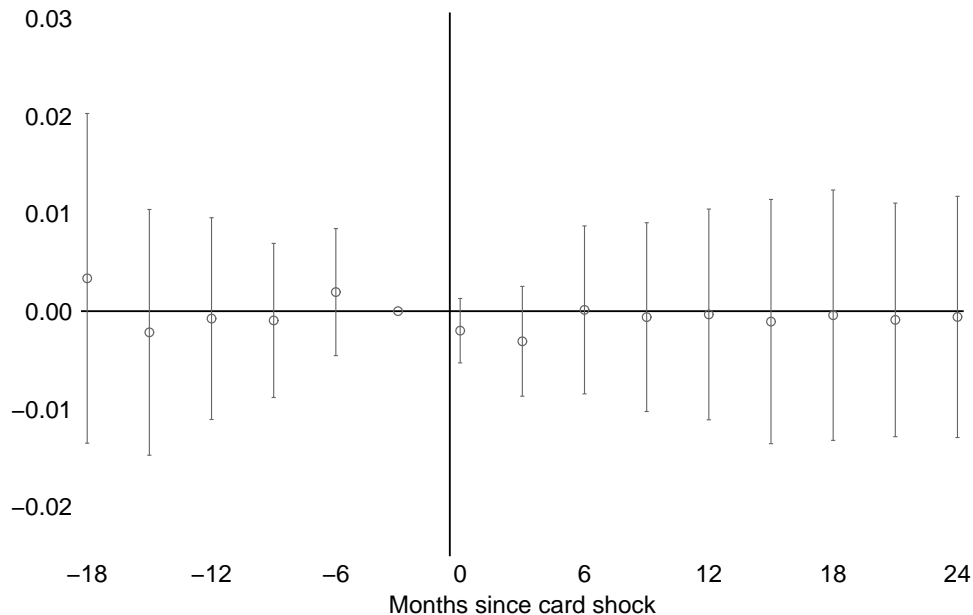
This figure shows that the rollout of debit cards did not have an effect on retail wages. It shows the coefficients from (16), where the outcome is log monthly wages of individual i in locality j during quarter t . The estimation uses Mexico's quarterly labor force employment survey. (a) $N = 88,668$ individual by quarter observations of individuals employed at corner stores (excluding store owners) in 253 treated localities; (b) $N = 96,457$ individual by quarter observations of individuals employed at supermarkets (excluding store owners) in 253 treated localities. Standard errors are clustered at the locality level.

Figure C.8: Effect of card shock on probability of losing job (event study estimates)



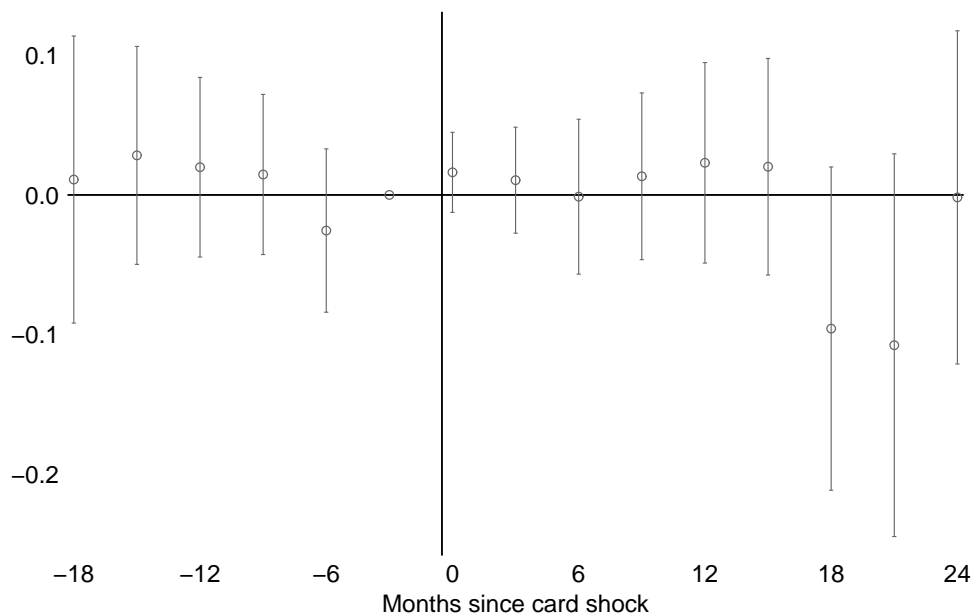
This figure shows that the rollout of debit cards did not have an effect on retail wages. It shows the coefficients from (16), where the outcome is a dummy variable equal to 1 if the individual previously had a job at that type of store, reported that the job ended because the individual lost it or was terminated or the business closed, and is still unemployed at the time of the survey. Individuals currently employed at corner stores or supermarkets—as well as individuals who lost a job at that store type but are currently employed elsewhere—are included in the regression and coded with a 0 for the “lost job” variable. The estimation uses Mexico’s quarterly labor force employment survey. (a) $N = 116,210$ individual by quarter observations of individuals currently or previously employed at corner stores in 253 treated localities; (b) $N = 135,540$ individual by quarter observations of individuals currently or previously employed at supermarkets in 253 treated localities. Standard errors are clustered at the locality level.

Figure C.9: Effect of card shock on per-transaction merchant fee at POS terminals



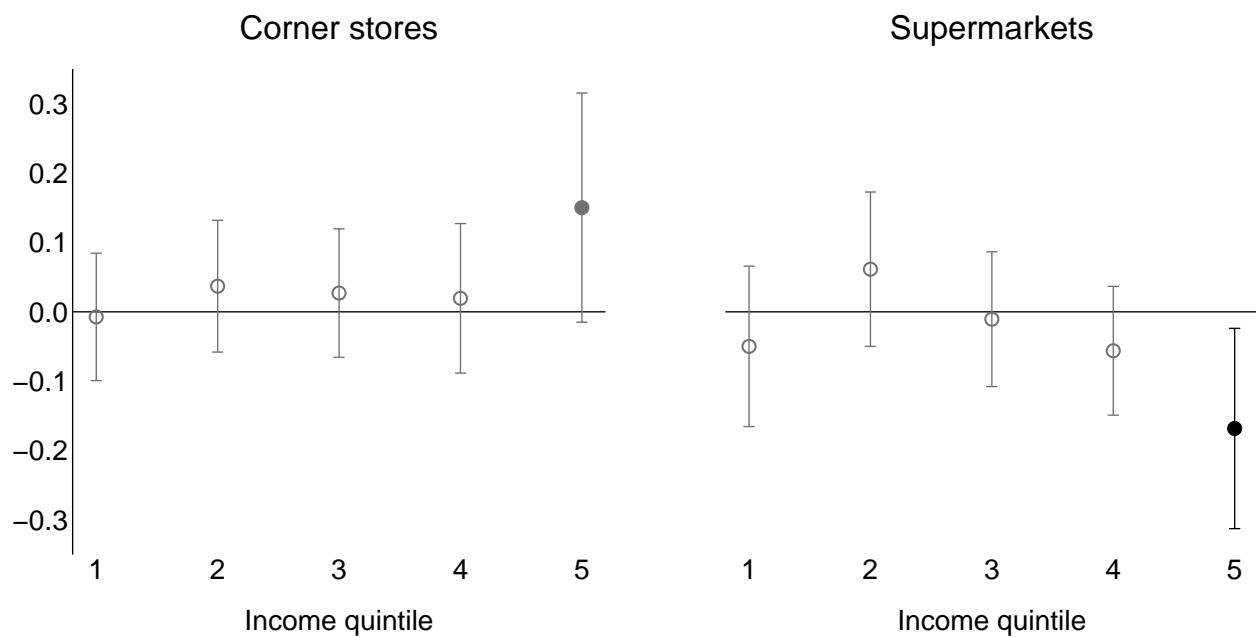
This figure shows that the POS terminal merchant fee charged by banks does not change in response to the card shock. The data include merchant fee rates by bank and were obtained from Mexico's Central Bank. $N = 4574$ municipality by quarter observations from 255 municipalities.

Figure C.10: 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.

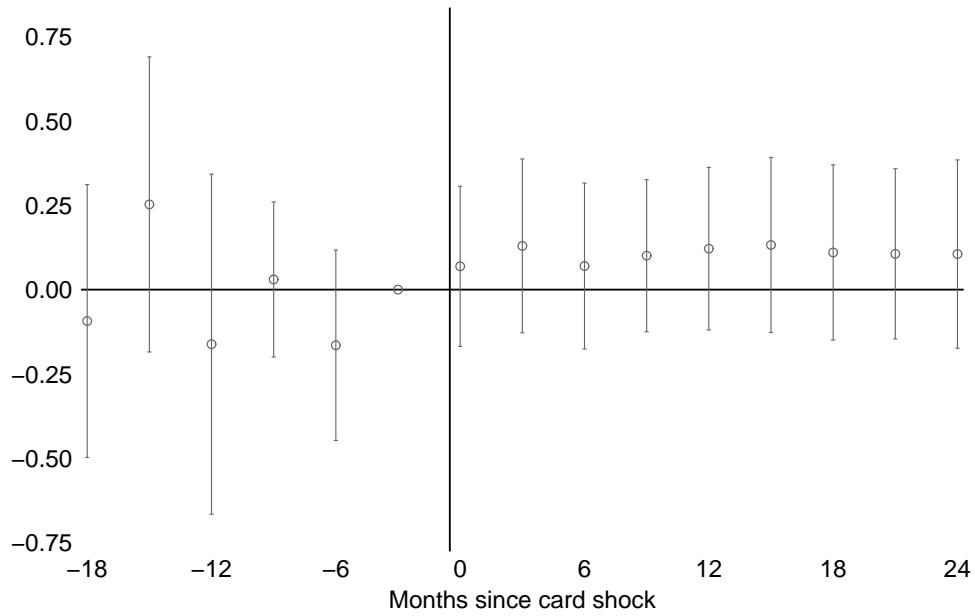
Figure C.11: Effect of corner store POS adoption on quantity consumed



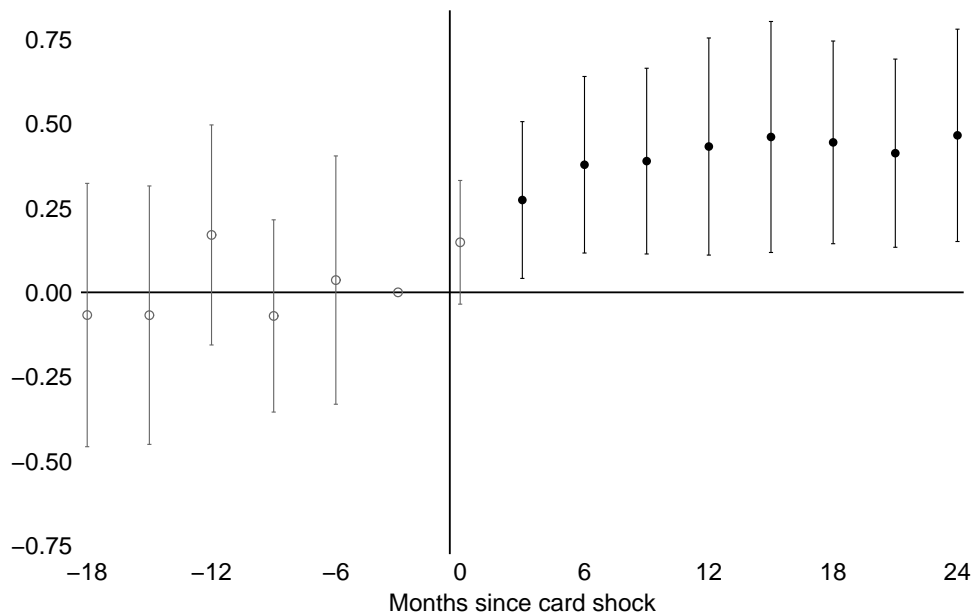
This figure shows that richer consumers substitute some of the quantity (measured in kilograms and liters) that they purchase from supermarkets to corner stores. This suggests that the results are not explained by prices. The figure graphs coefficients from (4) where the outcome variable is $\log(\text{kilograms}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

Figure C.12: Other consumers' card adoption

(a) Municipalities where beneficiaries prefer supermarkets

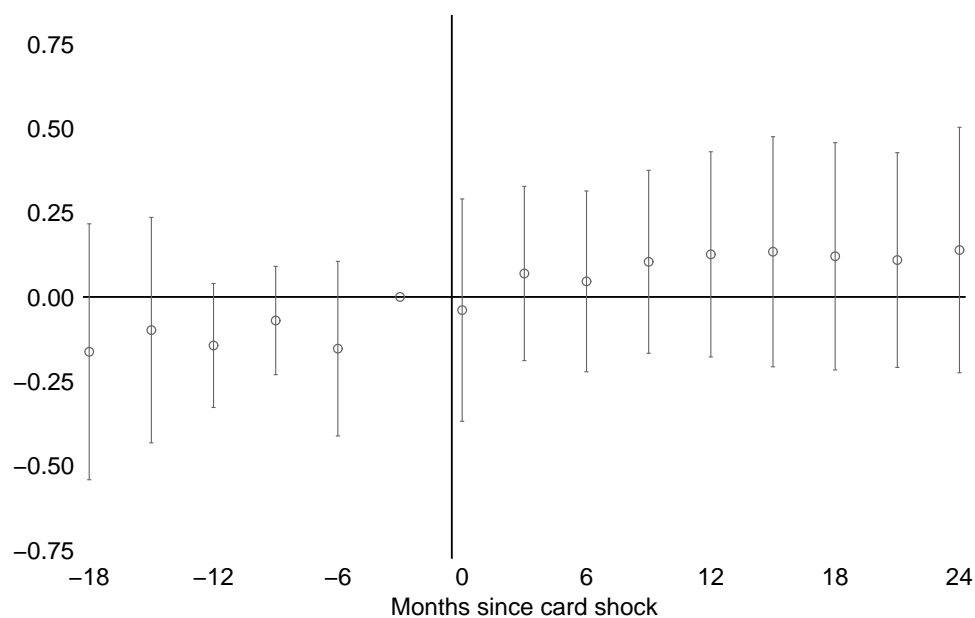


(b) Municipalities where beneficiaries prefer corner stores



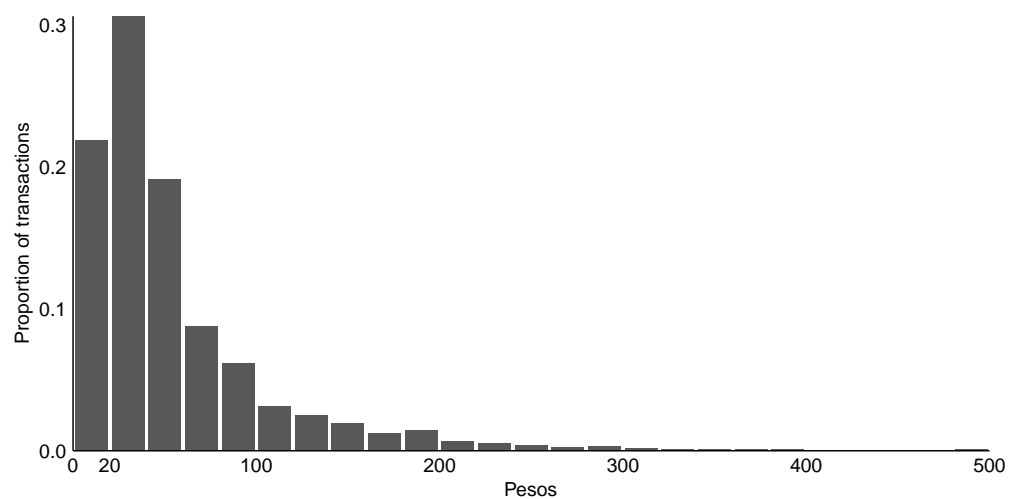
This figure shows that the spillovers on other consumers' card adoption are concentrated in localities where beneficiaries use their cards relatively more at corner stores, suggesting that the effect on other consumers' card adoption is driven by network externalities rather than word-of-mouth learning. 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.13: 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.12 on data sources.

Figure C.14: 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 universe of card transactions at POS terminals. $N = 6,940,166,777$ transactions.

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

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