

Financial Technology Adoption

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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 13 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 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), 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 financial technologies—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 financial technologies (FinTech)—a category that includes card payments (Einav et al., 2017), mobile money (Suri and Jack, 2016; Yermack, 2018), online lending (Buchak, Matvos, Piskorski and Seru, 2018; Hertzberg, Liberman and Paravisini, 2018), and smartphone financial apps (Gelman et al., 2014; Carlin, Olafsson and Pagel, 2019). These technologies have impacted consumer borrowing (Bartlett, Morse, Stanton and Wallace, 2019; Fuster, Plosser, Schnabl and Vickery, 2019), saving (Blumenstock, Callen and Ghani, 2018), risk sharing (Jack and Suri, 2014; Riley, 2018), and resilience to shocks (Bharadwaj, Jack and Suri, 2019). 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 financial technology 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 financial technology 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 debit cards.¹ Thus, a sufficiently large shock to consumers' adoption could lead to dynamic increases in financial technology adoption on both sides of the market.

The supply-side response to consumers' financial technology adoption and the spillovers onto other consumers have been difficult to address in the literature for four reasons. First, technology adoption is endogenous. To overcome this barrier, I exploit a natural experiment that created an exogenous shock to financial technology 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. The shock created plausibly exogenous variation 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, 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. In the context I study, 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%).²

Third, tracing out 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 within each local market. This rules out the use of policy shocks that affect all participants on one side of

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).

²In 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%.

the local market such as large-scale cost subsidies, or shocks that directly affect the costs and benefits of adoption on both sides of the market such as India's demonetization. In the context I study, 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.

Fourth, in most empirical settings there is a lack of high-quality data on firms' financial technology adoption and outcomes for both firms and other consumers. To overcome this barrier, 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 financial technology 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 (approximately five billion transactions).

For spillovers on 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 retailers, and high-frequency price data at the store by barcode level from a sample of stores.

Firms respond to the policy-induced shock to consumer financial technology adoption by adopting POS terminals to accept card payments. Exploiting the gradual rollout of debit cards 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.³ Adoption continues to increase over time: two years

³Administrative 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.

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 financial technology adoption by increasing their adoption of debit cards. Specifically, 3–6 months after the shock occurs, the number of cards held by other consumers increases by 19%. Two years after the shock, 28% more consumers have adopted cards.⁴

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 13% 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, 2020).⁵

Finally, I estimate producer and consumer surplus. To estimate gains for producers, I use data on the revenues and costs of the universe of retailers in Mexico. Over the five-year period between

⁴Although I cannot directly test whether these cards from other banks are adopted by other consumers or by 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 suggest that in practice, Prospera beneficiaries are not adopting other cards (Section 5.2).

⁵In that paper, we find that the cards do not affect beneficiaries' income, but that beneficiaries do begin saving more in the bank after receiving cards. Furthermore, this increase in formal savings represents an increase in overall savings, financed by a voluntary reduction in current consumption. Consistent with those findings, using a different data set in this paper I also find evidence of a reduction in overall consumption by Prospera beneficiaries as a result of receiving a debit card.

survey rounds, corner store sales increase by 6% more in earlier-treated localities. Corner stores increase the amount of inventory they buy and sell but do not change other input costs such as wages, number of employees, rent, capital, or utilities, which leads to an increase in profits. This does not represent an aggregate gain to producers, however, as increased corner store sales are accompanied by decreased supermarket sales that are very similar in aggregate magnitude.

To estimate consumer surplus from the supply-side POS adoption that occurred as a result of the demand-side policy shock, I combine consumption survey microdata on consumer choices across store types and prices with data on POS adoption and the geocoordinates of all retailers. My estimating equation is derived from a discrete–continuous choice model where consumers decide for each shopping trip which store to go to and how much of each good to purchase. Empirically, supermarkets are farther than corner stores on average and charge more for identical products, but accept card payments and offer other amenities. Corner stores, on the other hand, may or may not accept card payments. Because local prices should respond endogenously to demand at each type of store, I follow Atkin, Faber and Gonzalez-Navarro (2018) in using a region-based Hausman (1996) instrument for prices. Because POS adoption also likely responds endogenously to demand, I use the debit card shock as an instrument for POS adoption—as described above, this instrument is plausibly exogenous and has a strong first stage. 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. Over half of the consumer gains are spillovers to existing card holders and to non-beneficiaries who adopt cards as a result of the shock.

This paper thus makes three main contributions to the literature. First, I investigate how consumers' financial technology adoption filters through markets to affect retail adoption of financial technology, 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 have looked at the effect of debit card adoption on transaction and travel costs to access money at ATMs (Bachas, Gertler, Higgins and Seira, 2018; Schaner, 2017), the use of

debit cards to make in-store purchases (Zinman, 2009), the use of technology to monitor account balances (Bachas, Gertler, Higgins and Seira, 2020; Carlin, Olafsson and Pagel, 2019), 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, 2019; Buchak, Matvos, Piskorski and Seru, 2018; Fuster, Plosser, Schnabl and Vickery, 2019) and other financial service providers (Philippon, 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. 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; Gowrisankaran and Stavins, 2004; 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. One exception is Björkegren (2019), who finds that the majority of consumer surplus from the expansion of rural cell phone towers in Rwanda accrues to users whose coverage was not affected, through network externalities. Another policy shock that has been used, the Indian demonitization, has large direct impacts on both sides of the market and

far-ranging effects not only on FinTech adoption but also on employment, output, and bank credit (Chodorow-Reich, Gopinath, Mishra and Narayanan, 2020); thus, studies exploiting this shock to study FinTech adoption (e.g., Agarwal et al., 2018; Crouzet, Gupta and Mezzanotti, 2020) do not isolate spillovers across the two sides of the market.⁶ 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 retail 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 producer and consumer 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 71%—compared to 50% worldwide (Demirgüç-Kunt et al., 2018). The proportion of the population with a debit or credit card is also highly correlated with income, as shown in Figure 1a. In urban areas, 10% of households at the bottom of the income distribution had a debit or credit card prior to the Prospera debit card rollout, compared to 70% of households at the top of the income distribution.⁷ On the supply side of the market, 32% of retailers in urban areas had adopted POS terminals prior to the rollout, including 26% of corner stores and nearly 100% of supermarkets.

⁶Crouzet, Gupta and Mezzanotti (2020) impose a structural model to estimate spillovers or complementarities between firms, i.e. within the supply side of the market.

⁷Urban areas are defined as localities with at least 15,000 inhabitants. These results are from the Mexican Family Life Survey 2009. The numbers for the bottom and top of the income distribution are based on the local polynomial fit shown in Figure 1a. The average number of households with cards in the bottom and top *quintiles* of the income distribution are 12% and 54% respectively.

Figure 1b shows the cross-sectional municipality-level correlation between adoption on each side of the market: the y-axis shows the proportion of retailers with POS terminals, and the x-axis shows the number of debit cards per person.⁸ Each point on the graph is a municipality, and the size of the points represents population. The evolution of card and POS terminal adoption over time also appears highly correlated: Figure 2 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—descriptively—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 greater than 15,000 inhabitants.⁹

Prospera—formerly known as Progresa and later Oportunidades—is one of the first and largest conditional cash transfer programs worldwide, with a history of rigorous impact evaluation (Parker and Todd, 2017). The program provides cash transfers to poor families with children ages 0–18 or pregnant women. Transfers are conditional on sending children to school and completing preventive health check-ups. The program began in rural Mexico in 1997, and later expanded to urban areas starting in 2002. After its expansion, nearly one-fourth of Mexican households received 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

⁸This graph is based on publicly available data from Mexico’s National Banking Commission, the Comisión Nacional Bancaria y de Valores (CNBV), and Mexico’s 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 or municipalities).

⁹Mexico 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.

per two-month payment period.

Prior to the debit card rollout, beneficiaries 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. . . 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 4.8 kilometers (Bachas, Gertler, Higgins and Seira, 2018); 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, 2020).

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

All beneficiaries in a treated locality receive cards during the same payment period, and although the overall number of beneficiaries in the program increases nationally over time, the rollout was not accompanied by a differential change in the number of beneficiaries or transfer amounts. Furthermore, conditional on being included in the rollout, the timing of when a locality received the card shock is not correlated with pre-rollout levels or trends in financial infrastructure or other locality-level observables (Section 4).

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.

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; here I use the POS terminal fee structure from a large commercial bank in Mexico to illustrate costs. 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 95% of corner stores. In addition, there is a proportional transaction fee that varies by sector and bank; it was 1.75% for retailers at this large commercial bank during the period of the card rollout. For most corner stores, the monthly fee would swamp the transaction fee: as a percent of total (cash and non-cash) sales, the median corner store would pay 0.5% in transaction fees and 3.2% in monthly fees.¹¹

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 corner stores for which the constraint would bind is not conditional on accepting card payments. It is based on a combination of data on the sales of the universe of corner stores from Mexico's Economic Census with the average proportion of transaction value made on cards—conditional on the store accepting cards—from Mexico's National Enterprise Financing Survey, which is 23% for corner stores. The estimate of fees as a fraction of sales are based on the same combination of data sources.

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

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; the only focus group participant willing to quantify this effect 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 A.

3.1 Card Rollout and Beneficiary Card Use

Administrative data from Prospera. Prospera provided confidential data at the locality by two-month payment period level. The data include the number of beneficiaries in the locality and the payment method by which they are paid. Examples of payment methods include cash, bank account without debit card, and bank account with debit card.¹³ These data, which span 2007–2016 and include all 630 of Mexico's urban localities (as well as all rural localities with Prospera beneficiaries), allow me to identify the two-month period during which cards are distributed in

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.

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 the debit card shock with the effect of more cash flowing into the locality.

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 each debit card purchase was made, which allows me to manually classify whether the purchase was made at a supermarket, corner store, or other type of business.

3.2 Data on POS Terminals

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

¹⁴Merchant category codes are four-digit numbers used by the electronic payments industry to categorize merchants. Einav et al. (2017) and Ganong and Noel (2019) also use merchant category codes to define store types and spending categories. Appendix A explains how I map from postal codes, the geographic identifier in this data set, to localities, the relevant geographic area for the card rollout.

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 July 2007 and March 2015. The data include an average of 1.7 million card transactions per day, for a total of 4.7 billion transactions. For each transaction, I know the date of the transaction, amount of pesos spent, the store type (merchant category code) of the business, and the name of 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 financial technology 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 test 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 220 of the 259 localities included in the card rollout. The survey includes comprehensive income and consumption data; importantly, the consumption data

take the form of a consumption diary that allows me to identify unique store trips and that includes the store type at which each good was purchased, the date of the purchase, quantity purchased, and amount spent on each good in pesos.

3.4 Retail Outcomes and Prices

Retail outcomes. Every five 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). Firm type and store type are determined in this data set using six-digit North American Industry Classification System (NAICS) codes.¹⁵ 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 five million total firms; 344,305 of these are corner stores observed in both census waves and 172,441 of those are in the urban localities included in the Prospera card rollout. There are far fewer supermarkets, department stores, and chain convenience stores such as Oxxo and 7-Eleven than corner stores in Mexico: specifically, there are 20,251 supermarkets, department stores, and chain convenience stores included in both survey waves, of which 13,782 are in the urban localities included in the card rollout. The survey includes detailed questions about various components of sales, costs, and inputs. 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 based on the timing of the card rollout.

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

¹⁵Other studies using NAICS codes to distinguish firm types include Mian and Sufi (2014), Giroud and Mueller (2017), and Giroud and Rauh (2019).

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.

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 and subsequent changes in sales across retailers. For each employee with a current or former job, the employer’s four-digit NAICS code is included in the data, which allows me to distinguish between store types for retail employers.

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 urban 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 at each stage of 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.

The rollout across these 259 urban localities had substantial geographic breadth and does not appear to follow a discernible geographic pattern (Figure 3a). By the end of the rollout, over one million beneficiaries had received cards (Figure 3b). Since—as I show below—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.

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 potential 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 that allows me to determine the timing of the card rollout across localities is also at the two-month level).¹⁶ 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

I include 18 months prior to the shock and 24 months after the shock regardless of the data set being used (i.e., $a = -18, b = 24$); when this involves changes in the sample of localities underlying each coefficient (e.g., if a data set begins at the end of 2008, a locality treated in 2009 does not enter into the estimate for $k = -18$ because that locality has no observations in the data set 18 months before it is treated), I also show results for the balanced sample of localities over the more restricted time span for which I can include all localities in the rollout in the estimate of each coefficient. Furthermore, as in most event study specifications (e.g., McCrary, 2007), I do not drop

¹⁶Some 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.

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. 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 CNBV data described in Section 5.2 is at the quarterly level.

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

I conduct two sets of tests to determine whether the timing of the rollout is correlated with trends or levels of financial infrastructure or other locality-level observables. First, Figure 4 shows that the timing is not correlated with pre-trends by showing that $\phi_k = 0$ for all $k < 0$ from (1); I show this for numerous variables from several data sets, including measures of financial service use (POS terminals, debit cards, and credit cards), financial infrastructure (ATMs and bank branches), financial market outcomes (transaction fees at POS terminals), and other economic variables (wages and prices).

Second, I formally test whether, conditional on being included in the rollout, the timing of the rollout is correlated with levels or trends in locality-level observables. 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 and financial service use from Central Bank and CNBV data (POS terminals, bank accounts, bank branches, and ATMs), population from INEGI, number of Prospera beneficiaries from Prospera administrative data, measures of local politics from electoral data (vote share of the president’s political party and whether the mayor is the same party as the president), and 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 using INEGI data. Of the 40 variables, including both levels and trends, only two are correlated with the timing of the rollout, as can be expected by chance: the coefficient on the proportion of households without plumbing is statistically significant at the 5% level and the coefficient on the percent of children not attending school is statistically significant at the 10% level (Table 1).¹⁹

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). For data sets at the annual level, which are used in some of my identification tests, 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.

¹⁹I 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 levels and trends using a discrete-time hazard model. As in Galiani, Gertler and

In addition, I 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 number of Prospera beneficiaries is available only at the annual level in 2007 and 2008.²⁰ Appendix Figure B.1a shows the results: there is no differential change in the number of beneficiaries that occurs at the same time as the card rollout. None of the point estimates either before or after the shock is statistically significant from zero. While I do not have data on the total benefits disbursed in each locality, because benefits are based on a strictly-followed formula, the absence of a differential trend in the number of beneficiary households suggests that there was no differential trend in total transfer payments correlated with the card rollout either.

Another potential confound would be if the rollout of debit cards was correlated with local politics, e.g. if the program decided to first distribute cards in areas where the party in power at the local level was the same party as the one in power at the national level. This does not appear to be the case, however. I use hand-digitized data from municipal elections, which contain vote shares for each party at the municipal level, to construct a variable that equals 1 if the municipal mayor belongs to the PAN party, which was the party of Mexico's president during the debit card rollout. I include this variable in the discrete time hazard estimation in Table 1 and also show that it neither exhibits differential pre-trends nor is impacted by the debit card shock in Appendix Figure B.1b. I also include a variable for the PAN vote share in Table 1 (but cannot conduct an event study for

Schargrodsky (2005) I include a fifth-order polynomial in time. Changes are measured from 2006–2008 whenever possible, and from 2000–2005 (the two most recent pre-rollout population census waves) for the INEGI variables. 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 so their pre-trend cannot be measured for early-treated localities. Nevertheless, exploiting the staggered rollout timing (i.e., that for later-treated localities pre-trends can be measured), Figure 4 shows that there is no differential pre-trend in debit card adoption. ATMs were also not included in the CNBV data until the last quarter of 2008, which is why commercial bank ATMs and government bank ATMs are the only variables in Table 1 that do not include changes in addition to levels.

²⁰The 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.

this variable given the infrequency of elections).

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 withdrawing 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 financial technology 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 Bansefi.

Appendix Figure B.2 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 at each major store type. The two main types of retail stores in Mexico are corner stores and supermarkets: according to the ENIGH household consumption survey, expenditures—regardless of payment method—at corner stores and supermarkets combined are 45% of total consumption and 74% of retail and restaurant consumption.²¹

²¹Retail and restaurant consumption refers to all categories for which the type of establishment is recorded, including consumption at corner stores, supermarkets, open air markets, ambulatory vendors, restaurants, online purchases, etc. It excludes spending that does not take place in establishments such as rent and utility payments. These calcula-

In the Central Bank transactions data, card transactions at corner stores and supermarkets make up 54% of all card transactions at POS terminals.

I estimate (1) with the log number of POS terminals at corner stores, supermarkets, or all other businesses 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 POS 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 5a).²² For supermarkets, all but one pre-treatment coefficient are statistically insignificant from 0, but there is no effect of the card shock (Figure 5b). 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. Similarly, there is neither a pre-trend nor effect of the card shock for all other businesses (Figure 5c).²³

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 commercial 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 commercial bank has told me that they were not aware of the specific details of when

tions are restricted to households in urban localities.

²²For all regressions with coefficients that are changes in logs, if we denote those coefficients as ϕ , the percent changes I report are $100 \times (\exp(\phi) - 1)\%$. Appendix Figure B.3 shows the results from the same specification using levels rather than logs of the number of POS terminals.

²³These results are also shown in table form in Appendix Table B.1.

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 transaction fees charged to retailers, and (ii) increased bank investment in areas that experienced the card shock, which could increase the ease of adopting a POS terminal. I do not find evidence of these types of bank response.

5.2 Spillovers to Other Consumers

I test for two types of spillovers to other consumers. First, do other consumers adopt cards after the Prospera debit 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 caused an increase in the number of retailers with POS terminals. Alternatively, it could occur due to social learning—a possibility that I test in Section 7. Second, do some consumers shift a portion 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) with the log stock of non-Bansefi debit cards as the dependent variable. 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 restricted to urban municipalities included in the card rollout.

Figure 6 and Table 2, column 1 show 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 stock of non-Bansefi cards increases by 19%. Treated localities have 28% more non-Bansefi debit cards two years after the shock.²⁴

One possibility is that new non-Bansefi cards are not spillovers to other consumers, but are in-

²⁴Appendix Figure B.4a shows that the result is robust to restricting to the set of localities and periods for which each coefficient is estimated on the full set of localities. Appendix Figure B.4b shows that results are robust to using the log number of credit and debit cards rather than just debit cards.

stead 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 explore this, I use data from the Payment Methods Survey described in Appendix Section A.11, where Prospera beneficiaries are asked in mid-2012 (after the rollout) if they have a bank account at another bank, which is a prerequisite to having a debit card from another bank. Just 6% of beneficiaries who receive their Prospera benefits by debit card report having an additional bank account at another bank. Because the base of beneficiaries receiving cards is less than half the size of the existing number of households with cards, even if all beneficiary households with accounts at other banks have a card tied to the account and adopted that other card after receiving a Prospera card, beneficiary adoption could explain at most a 3% increase in the number of non-Bansefi cards.

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—taken together—suggests that these alternative channels do not explain much of 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 . I include locality rather than household fixed effects since the survey is a repeated cross-section rather than a panel at the household level.

Table 3 shows how consumers change their consumption in response to the shock, with results from (2) where the dependent variable is log spending at a particular store type. 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 7, we see that although the point estimate of the increase at corner stores is higher than the point estimate of the decrease at supermarkets (columns 1 and 4), 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 household has a credit card with all of the terms on the right-hand side of (2). Specifically, I estimate

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

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

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 for log spending at corner stores and negative for log spending at supermarkets. While the interaction terms are not statistically significant, they have the expected signs, with point estimates suggesting that consumers with credit cards had a 6 percentage point larger increase in spending at corner stores and a 6 percentage point larger decrease in spending at supermarkets than consumers without credit cards (columns 2

and 5 of Table 3).

Next, I test for heterogeneity by whether the household is a Prospera beneficiary (meaning the household would directly receive a card when the shock occurs). As shown in Bachas, Gertler, Higgins and Seira (2020), beneficiaries respond to the shock by *decreasing* total consumption to finance an *increase* in overall savings now that saving in the account is more attractive and since saving informally is difficult. I estimate

$$y_{it} = \xi_{b(i)j(i)} + \eta_{b(i)t} + \gamma D_{j(i)t} + \omega D_{j(i)t} \times \mathbb{I}(\text{Prospera beneficiary})_{it} + \varepsilon_{it}, \quad (4)$$

where the $b(i)$ subscript denotes interacting fixed effects with whether the household is a Prospera beneficiary. While Bachas, Gertler, Higgins and Seira (2020) use data from a panel survey of only Prospera beneficiaries—and hence have more power to detect effects for beneficiary households—consistent with their findings, Prospera beneficiaries in ENIGH decrease their overall consumption ($\gamma + \omega$ is statistically significant at the 10% level; column 9 of Table 3).

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 interact the difference-in-differences specification with income quintile dummies and estimate

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

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 , with $q = 1$ as the omitted category.²⁵

Figure 7 shows how consumers in each quintile of the income distribution change their con-

²⁵Income quintiles are estimated separately within each survey year (i.e., $q = 1$ corresponds to the poorest 20% of households in each survey wave). Since all localities included in (5) 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.

sumption in response to the shock, plotting $\gamma + \psi_q$ for each quintile. The richest quintile of consumers reduce their consumption at supermarkets by 13% and increase their consumption at corner stores by 15% in response to the debit card shock and subsequent POS adoption by corner stores. The second-richest quintile also appears to increase its consumption at corner stores (by 8%, significant at the 10% level), while the results for the poorest three quintiles are statistically insignificant from zero (Figure 7a). This shift 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 7b). 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.

To know whether the richest quintile's change in consumption represents a shift in consumption from supermarkets to corner stores, we need to know baseline consumption shares at each store type. Prior to the card rollout, the richest quintile consumed 24% of total consumption at corner stores and 17% at supermarkets. Thus, the magnitudes of the 15% increase in corner store consumption and 13% decrease in supermarket consumption come fairly close to lining up, each representing 2.2–3.5% of total consumption. Overall spending by the richest quintile might increase slightly (with a statistically insignificant point estimate of a 3% increase). This potential increase could be driven by being more likely to purchase something when it runs out if the corner store accepts cards—rather than waiting until the next planned supermarket trip—or by reduced travel costs from fewer trips to the supermarket easing the budget constraint.

Given the shift in consumption from supermarkets to corner stores by richer consumers, which goods do they consume at corner stores that they previously consumed at supermarkets? Do they also change the type of goods they consume? To answer these questions, I reestimate (5), where the outcome is now log spending on a particular category of goods at a particular store type. Appendix Figure B.5 plots the $\gamma + \psi_5$ coefficients from separate regressions for each product category by store type. I focus on the fifth quintile because this is the group whose consumption shifts from supermarkets to corner stores. The product categories where we see both a statistically sig-

nificant increase in the fifth quintile’s consumption at corner stores and a statistically significant decrease in consumption at supermarkets are grains/tortillas, dairy/eggs, and soda. For other quintiles, on the other hand—where we did not observe a shift in consumption from corner stores to supermarkets—nearly all coefficients are statistically insignificant (Appendix Tables B.2 and B.3). The right column of Appendix Figure B.5 shows results for total consumption across all store types; all but one of the 16 coefficients are statistically insignificant, indicating that households in the richest quintile likely did not substantially change their consumption bundle when substituting some consumption to corner stores (although it does not rule out changes in the particular items consumed within these product categories).

In Section 7, I test whether a portion of the increase in spending by richer consumers at corner stores could be due to (i) increased corner store prices in response to the shock or (ii) 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 these alternative channels.

6 Producer and Consumer Gains

6.1 Retail 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 POS 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 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 or—in a separate regression—to supermar-

kets, and estimate

$$y_{it} = \gamma_i + \delta_t + \sum_k \gamma_k \mathbb{I}(\text{received cards at } k)_{j(i)} \times D_{j(i)t} + \varepsilon_{it} \quad (6)$$

for a number of firm-level outcomes including log sales, log of each of several components of costs, and the inverse hyperbolic sine of profits (for a log-like transformation that allows for negative profit values). 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. In a second specification, I estimate a pooled coefficient for all firms in localities treated 1.5–4.5 years before the 2013 census wave, relative to firms treated 0–1.5 years before.

I find that corner stores in localities treated 3–4.5 years before the second census wave experience increases in sales of 8% on average (statistically significant at the 5% level), while those in localities treated 1.5–3 years before the second census wave have a statistically insignificant point estimate of a 5% sales increase (Table 4, panel A, column 1). The pooled estimate shows that corner stores in earlier-treated localities experience a 6% increase in sales (statistically significant at the 10% level). This increase in sales comes at the expense of supermarkets, which experience a 12% decrease in sales (statistically significant at the 5% level; Table 4, panel B, column 1). While the sales of each supermarket are much higher on average than those of each corner store, there are also thirteen times as many corner stores. In aggregate, the 6% increase in sales at the average corner store and 12% decrease in sales at the average supermarket line up very closely, since aggregate corner store sales are 1.9 times as large as aggregate supermarket sales.

Consistent with the substitution of sales from corner stores to supermarkets, column 2 shows that the amount spent by corner stores on purchasing inventory increases (by 6% in earliest-treated localities, significant at the 10% level), while the amount spent by supermarkets decreases (by 14%

in earliest-treated localities, significant at the 5% level). Corner stores are able to increase their turnover of inventory without a corresponding increase in other input costs (wage costs, number of workers, rent, capital, and electricity; columns 3–7 of Table 4). As a result, corner store profits increase by 19% in earlier-treated localities (panel A, column 8, pooled coefficient). The story that emerges is that corner stores increase their profits by buying and selling more inventory while keeping other input costs 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; this is true of the point estimates, but I do not have enough power to reject that the point estimates are equal.

There is also some evidence that the card shock leads firms to increasingly formalize: column 9 shows the results from (6) where the outcome is a dummy variable equal to 1 if the firm is suspected to be formal, based on whether it charged value-added tax to any of its customers or paid social security benefits for its employees. Using the pooled coefficient, for example, the probability of formalization increases by 2.3 percentage points on a low base of 12.5%. 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 financial technology adoption in their area.²⁶

Retail 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)

²⁶Higher 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, the debit card shock leads to a 13% increase in VAT payments by the firm. I cannot disentangle whether the increase in VAT paid by retailers is due to higher rates of formality or higher profits.

use with Mexico’s micro-CPI data:

$$\log Price_{gst} = \eta_{gs} + \delta_t + \sum_{k=a}^b \phi_k D_{m(s)t}^k + \varepsilon_{gst}, \quad (7)$$

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

Figure 8 shows that there is no price response at corner stores or supermarkets. All of the ϕ_k coefficients are statistically insignificant from zero, both before and after the card shock. Furthermore, using each estimate’s 95% confidence interval, I can rule out price effects outside of the range $[-1.7\%, 1.1\%]$ during the first ten months after the shock and outside of the range $[-2.5\%, 2.4\%]$ during the first two years after the shock.²⁷

Retail 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. We have already seen that corner stores do not appear to adjust wages or the number of employees in the Economic Census data, 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 the quarterly labor force survey to test whether retail employees’ self-reported wages or employment status change 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 municipality 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:

$$\log Monthly\ salary_{it} = \lambda_{m(i)} + \delta_t + \sum_{k=a}^b \phi_k D_{m(i)t}^k + \varepsilon_{it}. \quad (8)$$

²⁷For comparison, Atkin, Faber and Gonzalez-Navarro (2018) find that when Walmart enters a municipality, prices at traditional retailers fall by 3–4%.

I again separately estimate the results for corner store and supermarket employees (which excludes owners of corner stores or supermarkets). The results are shown in Appendix Figure B.6. There is no evidence of a change in monthly wages (which would account for a change in the wage rate or a change in average hours worked) at either type of store.

For employment, the survey includes questions on whether individuals are employed, whether they lost a job or were terminated or left a job, the reason, and the type of employer (using four-digit NAICS codes). I thus estimate (8) separately for corner store and supermarket employees with the dependent variable as a dummy variable for “lost job” which equals 1 if the following conditions are all satisfied: the individual (i) is unemployed when surveyed, (ii) previously was an employee (not owner) at that type of store, and (iii) reported that the job ended because the individual lost the job or was terminated. Individuals currently employed at that store type—as well as individuals formerly employed at that store type who didn’t lose their job—are included in the regression and coded with a 0 for the “lost job” variable. Appendix Figure B.7 shows that the shock does not have a statistically significant effect on workers’ employment status at corner stores or supermarkets.

6.2 Consumer Surplus

The goal of this section is to quantify the consumer gains from the supply-side response to the demand-side policy shock for different types of consumers, and to thereby quantify what fraction of the total consumer gains are spillovers to consumers who did not directly receive cards from the Mexican government. The fraction of total consumer gains that are spillovers will provide a quantitative estimate of how large the indirect network externalities are in this two-sided market.

Specifically, I will estimate consumer gains for three types of consumers who have cards after the policy shock, and thus benefit from the increase in supply-side POS adoption: (i) Prospera beneficiaries; (ii) existing card holders; and (iii) non-beneficiaries who adopt cards in response to the shock. I impose structural assumptions on consumer utility and combine data on consumption and local product prices across store types with data on 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. Supermarkets, on average, are farther than corner stores and charge more for identical products (based on a regression using price data with barcode by locality by week fixed effects), but supermarkets also accept card payments and offer other amenities.²⁸ These other amenities—which could include, for example, greater product variety (as in Atkin, Faber and Gonzalez-Navarro, 2018; Li, forthcoming)—are included in the model as unobservables. Corner stores, on the other hand, may or may not accept card payments.

Thus, the problem facing consumer i who wants to make a trip t to the store and spend a fixed budget y_{it} is to choose between different stores (e.g., supermarket, corner store, open-air market), then determine how much of each good to consume from the store. I assume that consumers have Cobb-Douglas preferences over the goods they consume and also get some utility from store-specific characteristics, possibly including whether the store accepts card payments.²⁹ Specifically, consumer i 's utility from trip t to store s is

$$u_{ist} = \left(\prod_g x_{igst}^{\phi_{a(i)gst}} \right)^{\alpha_{k(i)}} \cdot \exp(\theta_{k(i)} POS_{ist} + \xi_{a(i)k(i)st} + \varepsilon_{ist}), \quad (9)$$

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 purchased by individual i during trip t to store s , $\sum_g \phi_{a(i)gst} = 1 \forall a, s, t$, $POS_{ist} = 1$ if store s at which individual

²⁸Specifically, to compare the prices charged for identical goods at corner stores and supermarkets, I use product by store by week level price quotes, restrict to price quotes from corner stores and supermarkets using six-digit NAICS codes, and estimate $\log Price_{gst} = \lambda_{gj(s)t} + \beta \mathbb{1}(\text{Corner})_s + \varepsilon_{gst}$. I get an estimate for β of -0.051 ($p = 0.046$).

²⁹Atkin, 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).

i makes trip t has adopted a POS terminal, $\xi_{a(i)k(i)st}$ capture preferences over other (potentially unobserved and time-varying) store characteristics and taste shifters that are common within census tract by consumer group by store by time, and ε_{ist} are unobserved individual by store by time shocks.

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 (9) and taking logs:

$$\log u_{ist} = \alpha_{k(i)} \log y_{it} - \alpha_{k(i)} \sum_g \phi_{a(i)gst} \log p_{a(i)gst} + \theta_{k(i)} POS_{ist} + \tilde{\xi}_{a(i)k(i)st} + \varepsilon_{ist}, \quad (10)$$

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

Assuming overall utility for trip t is additively separable in the potential u_{ist} across stores (Domencich and McFadden, 1975), for a particular trip the consumer will choose the store that gives the most utility. Thus, if we define $v_{ist} \equiv \alpha_{k(i)} \log y_{it} - \alpha_{k(i)} \sum_g \phi_{a(i)gst} \log p_{a(i)gst} + \theta_{k(i)} POS_{ist} + \tilde{\xi}_{a(i)k(i)st}$, then the probability of choosing store s over all other stores $r \neq s$ 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 C 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

$$\log \phi_{akst} = -\alpha_k \log P_{ast} + \theta_k \overline{POS}_{z(a)st} + \tilde{\xi}_{akst} - \log \sum_r \exp \gamma_{akrt}. \quad (11)$$

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), $\overline{POS}_{z(a)st}$ is the fraction of stores of type s in postal code $z(a)$ that have POS terminals at time t , and $\gamma_{akst} \equiv -\alpha_k \log P_{ast} + \theta_k \overline{POS}_{z(a)st} + \tilde{\xi}_{akst}$.³⁰ Finally, to remove the $\log \sum_r \exp \gamma_{akrt}$ term, I subtract the log share of spending on the

³⁰By necessity (since 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 postal code. 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

outside option of open-air markets, denoted ϕ_{ak0t} , which I assume do not accept card payments (i.e., $\overline{POS}_{z(a)0t} = 0 \forall z(a), t$).³¹

Thus the estimating equation is

$$\log \phi_{akst} - \log \phi_{ak0t} = -\alpha_k(\log P_{ast} - \log P_{a0t}) + \theta_k \overline{POS}_{z(a)st} + \eta_{j(a)ks} + \delta_{kst} + v_{akst}. \quad (12)$$

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 time fixed effects, where $j(a)$ denotes the locality of census tract a .³²

Given the lack of a change in prices in response to the card shock and the finding that the increase in corner store sales is substituted by a decrease in supermarket sales—both documented in Section 6.1—the supply side is not directly included in the model.

Endogeneity and identification. There are two endogenous variables on the right-hand side of (12): $(\log P_{ast} - \log P_{a0t})$ and $\overline{POS}_{z(a)st}$, as both prices and POS adoption likely respond to 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.³³ If demand shocks consist of independent local (census tract level) and regional components, this instrument will be uncorrelated with v_{akst} . 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.

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.

³¹There 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 FinTech 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.

³²Because the household survey is not a panel of individuals, it is also not a panel at the census tract level. Including census tract by consumer group by store type fixed effects would entail the loss of many observations.

³³There are five official regions in Mexico, defined by the Instituto Federal Electoral.

I instrument for adoption of POS terminals, $\overline{POS}_{z(a)st}$, 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 supermarkets to corner stores. We have already seen in Section 5 that the shock indeed has both of these effects.

Data. Log spending shares are estimated using the ENIGH consumption module described in Section 3.3. While the ENIGH is publicly available, census tract-level geographic identifiers are not; I accessed these identifiers on-site at INEGI.

I identify consumer types $k(i)$ based on questions in the ENIGH. Specifically, the ENIGH asks a number of questions about income and other benefits from Prospera which allow me to identify which households are Prospera beneficiaries. It also asks whether households have credit cards, but does not ask about debit cards. I thus define three consumer types: Prospera beneficiaries, non-beneficiaries with credit cards, and non-beneficiaries without credit cards. The latter group might (unobserved to me) have a debit card prior to the shock, might obtain a debit card only in response to the shock, or might not have a card either before or after the shock. Because debit card adoption responds to the shock but credit card adoption does not, and because the shock was not accompanied by a differential expansion of the Prospera program, the composition of these three groups is not affected by the shock (which could lead to a violation of the exclusion restriction).

Prices are unit values (i.e., total spent on a good divided by quantity purchased) from ENIGH, where the unit value of good g in store type s at time t is averaged across the unit value reported by each household that consumed that good within 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. Furthermore, unit values have been used in many studies to estimate price elasticities (e.g., Deaton, 1988). The weights $\phi_{a(i)gst}$ used to construct the price indices are expenditure shares cal-

culated 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; the data are restricted to food and beverages for this estimation because other consumption categories do not include quantities to calculate unit values. 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 time 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 merchant category codes. 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, described in Appendix Section A.10, where store type is identified using six-digit NAICS codes.

Estimation results. I estimate (12) for the three consumer groups described above; the results are shown in Table 5. First, I find $\hat{\alpha}_k$ of 3.35 for Prospera beneficiaries. 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), which makes it conservative for welfare estimates.³⁴ The estimates of $\hat{\alpha}_k$ for other consumers are lower, at 2.01 for credit card holders and 2.93 for non-beneficiary non-credit card holders. These magnitudes are consistent with richer consumers being less price elastic (although I do not have enough power to reject no

³⁴To 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 (12), we see that $1 - \sigma = -\alpha_k$, or $\sigma = \alpha_k + 1$.

difference between the estimates for each group), and they are also in the range of price elasticity estimates from other studies.

Credit card holders and other non-Prospera beneficiaries (many of whom likely have a debit card, which is not observed in the survey) put a higher value on the store having a POS terminal than Prospera beneficiaries. Specifically, the estimates are 0.58 and 0.55 for credit card holders and other non-beneficiaries (each significant at the 1% level) while the value for Prospera beneficiaries is 0.24 (not statistically significant). 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 29% price reduction for existing credit card holders and to a 7% price reduction for Prospera beneficiaries. Given that nearly 50% of Prospera beneficiaries use their cards for POS transactions (Figure B.2), the price-index equivalent value they derive from a store having a POS terminal *conditional on being a beneficiary who uses the card* is thus about half of the value derived by existing credit card holders.

Consumer surplus. 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

$$\frac{CV_k}{e(P^1, U^0)} \approx -\sum_s \phi_{ks}^1 \left(\frac{P_s^1 - P_s^0}{P_s^1} \right), \quad (13)$$

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 C shows the full derivation of (13). From (11), we can write

$$-\frac{\theta_k}{\alpha_k} = \frac{d \log \phi_{akst} / d \overline{POS}_{ast}}{d \log \phi_{akst} / d \log P_{ast}} = \frac{d \log P_{ast}}{d \overline{POS}_{ast}}. \quad (14)$$

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 (13), where ΔPOS_{ks} is the change in the

fraction of stores of type s at which consumers of type k can use a card, estimated in Section 5.1.

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

$$\left[\sum_s \phi_{ks}^1 \frac{\theta_k}{\alpha_k} \Delta POS_{ks} \right] - \frac{A_k}{y_k}, \quad (15)$$

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 use a revealed preference approach to impose upper and lower bounds on 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.

For existing card holders (i.e., those who already had cards prior to the shock), I use the ϕ_{ks}^1 , θ_k , and α_k estimated for credit card holders. Because other consumers' credit card adoption did not respond to the shock (while debit card adoption did), credit card holders should largely be a subset of existing card holders. I use the observed $\Delta POS_{k,s=corner}$ change in POS terminals at corner stores as a result of the shock from Section 5.1, while $\Delta POS_{k,s=super} = 0$ since supermarkets did not adopt POS terminals in response to the shock. Existing card holders spend 28% of their total expenditures at corner stores after the shock, and I estimate that they experience an increase in consumer surplus of 0.5% as a result of the supply side's POS adoption in response to the demand-side policy shock.

For beneficiaries, ΔPOS_{ks} for both corner stores and supermarkets are the observed post-shock levels of adoption at each store type (since beneficiaries did not have cards to use at either of these store types prior to the shock). Beneficiaries spend 46% of their consumption at corner stores and 12% of their consumption at supermarkets after the shock, and I estimate that their average

increase in consumer surplus is 1.8%. (Note that it is higher than for existing card holders because, although beneficiaries value an individual store having a POS terminal by less, they went from being able to use a card at no stores to being able to use it at many stores—without having to pay any monetary and non-monetary card adoption costs that might have prevented them from adopting a card before.)

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. new adopters 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 they 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 non-beneficiary households adopt a card in response to the Prospera card shock in the data, I use the ϕ_{ks}^1 , θ_k , and α_k estimated for non-beneficiary non-credit card holders. Note that because credit card adoption did not respond to the shock, new debit card adopters are a subset of this group. Under this assumption, these debit card adopters spend on average 37% of their consumption at corner stores, and using the bounds on the cost of adoption, I estimate that new adopters' relative change in consumer surplus is between 0 and 0.4%.

A back-of-the-envelope approximation of the proportion of the change in total consumer surplus made up of spillovers to other consumers is given by a weighted sum of the compensating variation (CV) for existing card holders and new adopters divided by a weighted sum of the CV for these two groups plus Prospera beneficiaries. The weights used in the sum correspond to the fraction of post-shock card holders that belong to each group, which are roughly 29% beneficiaries, 57% existing card holders, and 14% new card adopters.³⁵ Note that the CV terms in this sum are the absolute (rather than relative) changes in consumer surplus; they are thus increasing in total expenditures, which are higher for existing card holders and (to a lesser extent) new adopters

³⁵These fractions are not taken from the ENIGH given the caveats in identifying existing card holders and new card adopters in ENIGH discussed above. Instead, they are generated using various data sets and results from earlier in the paper.

compared to beneficiaries. These two factors—that beneficiaries make up a smaller fraction of post-shock card holders and have lower incomes—imply that even though each individual beneficiary’s proportional change in consumer surplus is higher than that of existing card holders and new adopters, a large fraction of overall consumer surplus goes to non-beneficiaries. Specifically, I estimate that between 55 and 58% of the increase in consumer surplus caused by retailers’ response to the policy of distributing debit cards to cash transfer beneficiaries accrues as spillovers to non-beneficiaries.

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. In conversations with executives at Mexico’s largest commercial bank, however, they noted that they were unaware of when the distribution of Prospera debit cards was occurring in different localities and thus were not responding to the shock. Meanwhile, the bank that distributed the debit cards to beneficiaries is a consumer-facing government social bank and does not offer POS terminals. Here, I test two ways banks could respond to the debit card shock that would potentially affect POS terminal adoption. First, they could decrease the transaction fee charged for each POS transaction. 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.³⁶

Transaction fees. To test whether banks adjusted the POS terminal transaction fees they charge in response to the shock, I use data from Mexico’s Central Bank on the POS transaction fees charged by each bank over time. These fees are regulated by the Central Bank, and although they vary across banks and merchant types (e.g., gas stations, fast food, retail), each bank must set a

³⁶Banks 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 commercial bank, they reported not following these strategies. The monetary costs of adopting a POS terminal—like the transaction fee—are set nationally by each bank.

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

Appendix Figure B.8a 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 POS transaction fee charged by banks 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 B.8b 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 very close to zero, at 0.0002 ($p = 0.99$).

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—are also the channels through which the network externality would occur. Nevertheless, I present a number of tests that, taken together, suggest that word-of-mouth learning is not the main 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 about the *benefits* of using cards, not their existence as a technology.

Heterogeneity by ATM density. Since nearly no Prospera beneficiaries had debit cards prior to receiving one from the program, their value of receiving a debit card (and hence their desire to tell others about these benefits) would be just as high or higher in areas with high ATM density than in areas with low ATM density. Indeed, Bachas, Gertler, Higgins and Seira (2020) document the use of ATMs by Prospera beneficiaries and the related benefits they experience from the decrease in transaction costs to access their transfers. Thus, if the effect were due to social learning about the benefits of debit cards, we would expect the effect in areas with high ATM density to be just as large or larger than the effect in areas with low ATM density.

If, on the other hand, the channel is indirect network externalities, the relative benefit to a non-beneficiary of a store adopting POS would be lower in areas with high ATM density. In other words, if there is an ATM on the same block as every corner store, a consumer would not care as much if the corner store accepts cards or not because she could easily get cash for her purchase from the nearby ATM. Thus, a consumer who didn't want to carry around large amounts of cash would have already adopted a debit card in areas with high ATM density, and thus would not respond to corner store POS adoption by adopting a card.

Table 2, columns 2 and 3, and Figure B.9 show this heterogeneity test, where (1) is run separately for municipalities with below- or above-median baseline ATMs per person. Consistent

with the indirect network externalities channel but inconsistent with the word-of-mouth learning channel, the effect is concentrated in municipalities with below-median ATM density. In those municipalities, the increase in other consumers' debit card adoption is statistically significant in all quarters after the first, and the coefficient two years after the shock represents a 49% increase in other consumers' debit card adoption. In municipalities with above-median baseline ATM density, on the other hand, there appears to be a smaller, immediate 10% increase in debit card adoption (statistically significant at the 10% level in the quarter of the shock) but no increase thereafter: coefficients for later periods remain around 10% but are no longer statistically significant. This smaller, immediate increase in other consumers' card adoption in low-ATM areas could be due to social learning.

Furthermore, when I test the difference in coefficients by interacting D_{jt}^k and δ_t with the above-median ATM density dummy in (1) rather than running separate regressions for above- and below-median municipalities, the coefficients on the interaction terms are statistically significant in five of the nine post-shock periods.

Heterogeneity by 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 was already near-universal prior to the shock, 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 areas where beneficiaries shop at supermarkets. The effect would instead be concentrated in areas 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 shopping patterns of beneficiaries within the first 6 months they have the card, using the Bansefi transactions data described in Section 3.1, to split the municipalities into two equal-sized groups: those in which the proportion of Prospera debit card transactions made at supermarkets is above median, and those in which it is below median.

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 (Table 2, column 4, and Appendix Figure B.10a). 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. In contrast, 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 (Table 2, column 5, and Appendix Figure B.10b).³⁷ When I test the difference in coefficients as above, the coefficients on the interaction terms are statistically significant in three of the nine post-shock periods.

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. If the spillover on other consumers' card adoption were driven by word-of-mouth social learning independent of network externalities 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 year after the card shock in places where we also see no delay in retailers' response to the card shock.

Specifically, I create a dummy variable for whether corner store POS terminal adoption increases within the first year after the shock—which occurs in 157 of the 255 treated municipalities. 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 POS adoption to the municipality rather than locality level since the data on other card adoption are at the municipality level.

Due to the smaller number of municipality by time observations in this test, I create dummy

³⁷Note that many beneficiaries still shop at corner stores in these municipalities as the median municipality-level proportion of Prospera card transactions at supermarkets is 22%, so we would not expect precise 0 point estimates.

variables for just three broad periods: prior to the card shock, less than one year after the card shock, and one year or more after the card shock. I then estimate (1) for the log stock of non-Bansefi debit cards, interacting both D_{jt}^k and δ_t with a dummy for whether there was an increase in corner store POS adoption within the first year after the card shock. The omitted period in the regression is prior to the card shock.

Table B.4 shows the results, with the results from (1) without interaction terms in column 1 for comparison and the interaction results in column 2. In municipalities in which corner stores responded during the first year after the card shock, we also see a 21% increase in other consumers' debit card adoption during the first year after the card shock (significant at the 5% level). After the first year, this increases slightly to 24% more debit cards adopted by other consumers. On the other hand, the interaction terms in column 2 show that in municipalities in which corner stores did not respond during the first year, we see evidence of little or no spillover to other consumers' card adoption over the same time period: the point estimate on the interaction term is -0.18 (statistically significant at the 10% level); summing coefficients suggests a very close to 0 change in other card adoption in municipalities that did not experience a change in POS adoption—specifically, a statistically insignificant point estimate of a 1% change. After at least a year, the interaction term is still large but no longer statistically significant, and summing coefficients suggests a (statistically insignificant) 5% increase in other card adoption after more than a year in the slow-to-respond municipalities.

7.3 Alternative Explanations of Increase in Corner Store Consumption

Prices. In Section 6.1, 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 B.11 and Appendix Table B.5, columns 3 and 4, show that it is not just the amount spent (price \times quantity) at corner stores that increases for the richest quintile, but also the quantity purchased. Specifically, the richest quintile increases quantity purchased from corner stores by 16% and decreases quantity purchased from supermarkets by 15%.

Minimum card payment amounts. In the US, it is common for small retailers to impose a minimum payment amount for payments by credit or debit card (and it is legal for retailers to impose these minimum payment amounts 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 most small retailers face in the US, which includes a fixed cost of \$0.10 per transaction plus a variable cost (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 amount for card payments. In Mexico, on the other hand, the fee structure does not include a fixed cost; instead, there is only a variable cost (which is 1.75% 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 minimum card payment amounts.

It is nevertheless an empirical question whether many Mexican retailers impose minimum card payment amounts in practice. Appendix Figure B.12 shows histograms of debit card transaction amounts for transactions made at POS terminals by all card holders in Mexico, using the Central Bank data on the universe of card transactions at POS terminals described in Section 3.2. There is no evidence that retailers (and especially corner stores) impose a minimum payment: over 20% of all transactions at corner stores are between 0 and 20 pesos, which is less than \$2, and over 50% of all transactions at corner stores are for less than 40 pesos.

7.4 Alternative Explanations of Increase in Corner Store Sales

Misreporting. It is possible that the increase in corner store sales measured using the Economic Census in Section 6.1 is not due to a true increase in sales, but rather due to misreporting. Specifically, corner stores could have been underreporting their sales before adopting a POS terminal, but more accurately reporting their sales after adopting—either due to fear of their reporting in the Economic Census being cross-checked against other data or due to the store owners themselves better tracking sales once they have a POS.

This is unlikely to explain the increase in corner store sales for a number of reasons. First, the pooled coefficient on the increase in corner store sales from Table 4, column 1 suggests a 6% increase in sales. This point estimate is very close to the 7% increase in corner store consumption *reported by consumers*—who would not have an incentive to misreport prior to the store’s POS adoption—in the ENIGH consumption survey (Table 3, column 1). Second, the estimated aggregate increase in corner store sales across all corner stores is very close in magnitude to the estimated aggregate decrease in supermarket sales, as discussed in Section 6.1. Third, corner stores also report an increase in inventory costs of about 4% (consistent with the increase in sales); unlike sales, inventory costs are a category in the Economic Census that the government could have already cross-checked against supplier receipts in theory. Finally, by law no other government agencies are able to see firms’ responses to the Economic Census, and this is carefully communicated to firms prior to their participation in the survey; thus the responses to the Economic Census are not cross-checked against tax filings or electronic sales data.

8 Conclusion

Due to the network externalities of financial technologies—which arise from the interactions between consumers’ and retail firms’ financial technology adoption—the spillovers of consumer FinTech adoption could be large. As a result, assessing the overall effects of increased financial inclusion requires us to quantify not only the direct effect on consumers who adopt financial technologies, but also 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 caused shocks to the adoption of a particular financial technology—debit cards—over time and space. When the Mexican government provided debit cards to existing cash transfer recipients in urban areas, small retailers responded 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 retailers' financial technology adoption in two ways. Some—who likely already shopped at the corner stores that were now adopting POS terminals—adopted debit cards. Richer consumers—who mostly already had cards—shifted 13% of their supermarket consumption to corner stores. Corner stores, in turn, benefited from the demand shock: their profits increased due to their ability to turn over more inventory, increasing both sales and inventory costs 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 (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 simultaneous adoption by many consumers in a local market, 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 and have spillovers back onto the demand side 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 retail firms.

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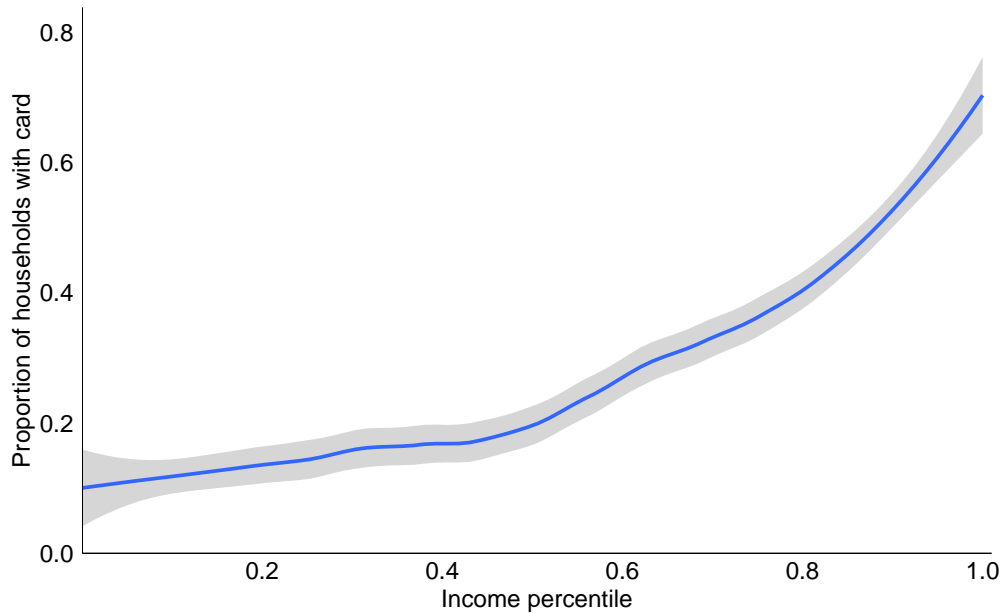
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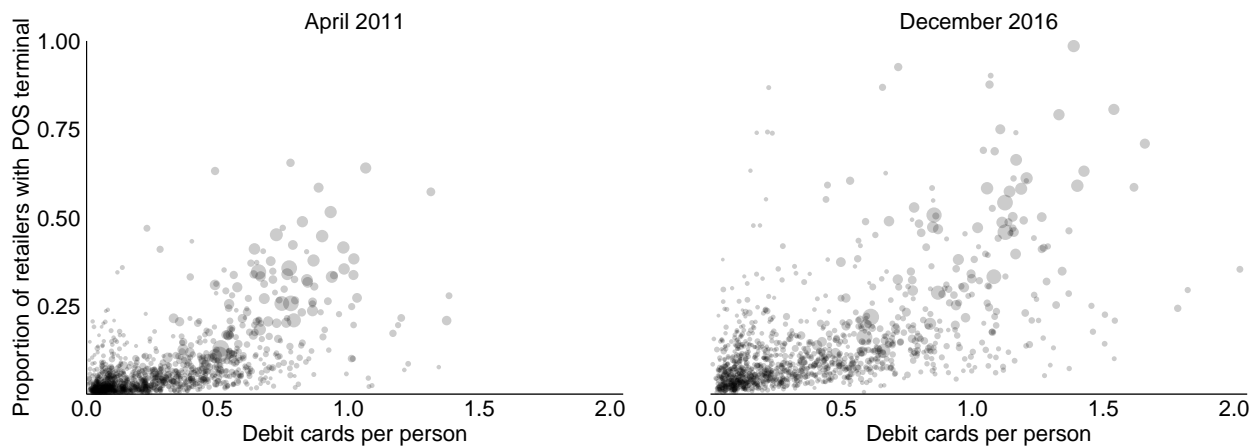
Figures and Tables

Figure 1: Financial technology adoption in Mexico

(a) Proportion of urban households with debit or credit cards



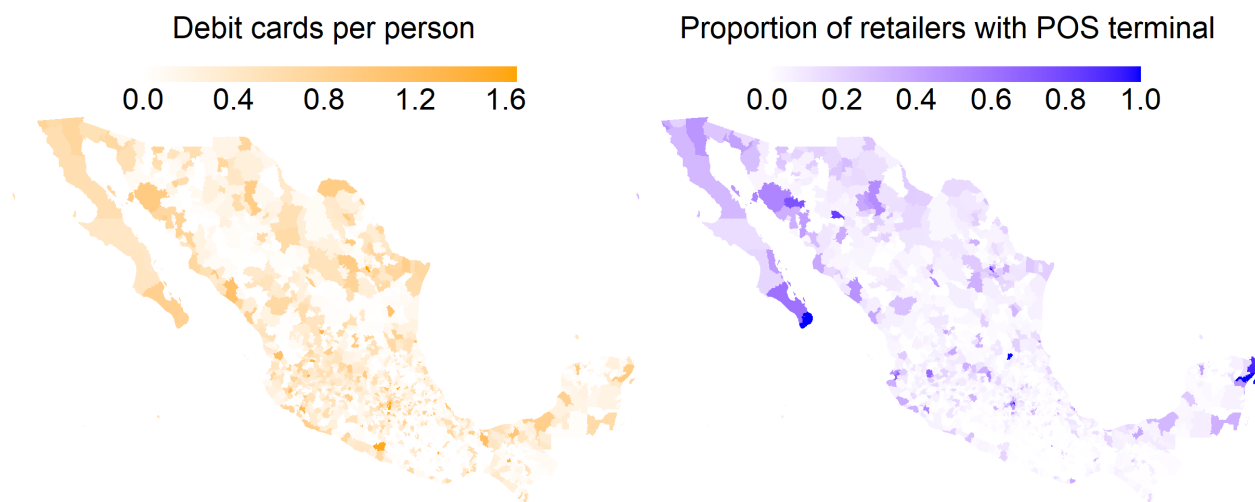
(b) Cross-sectional correlation between adoption of debit cards and POS terminals



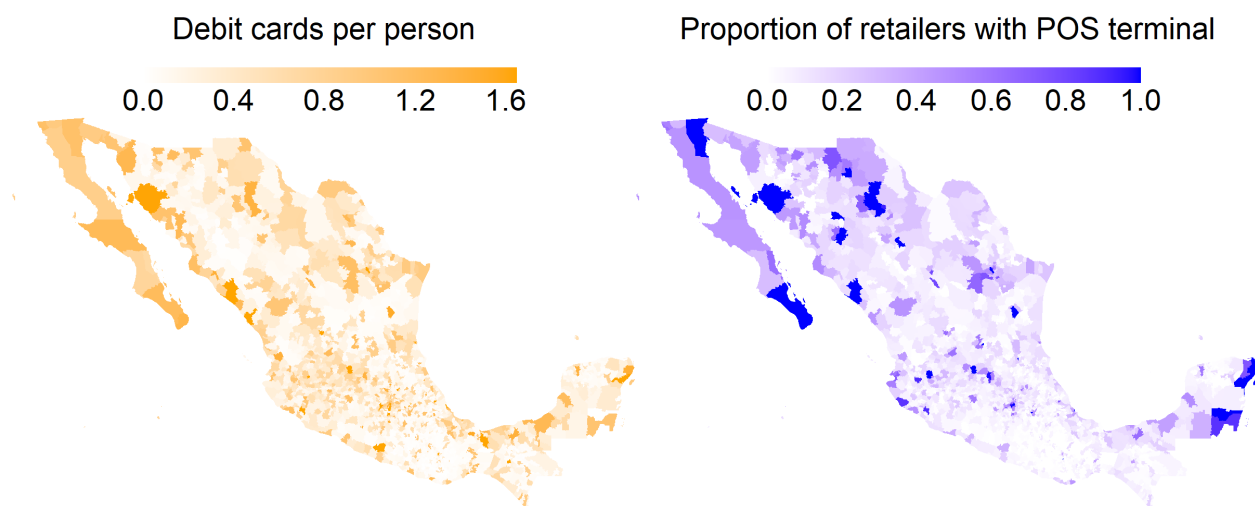
This figure shows that card adoption is highly correlated with income, and that adoption of POS terminals and cards within a municipality are highly correlated. Panel a shows the proportion of urban households with a debit or credit card across the income distribution using data from the 2009 Mexican Family Life Survey. The data are restricted to households in urban localities (i.e., localities with at least 15,000 inhabitants) since the debit card rollout I study occurred in urban localities, and income percentiles are defined within the set of urban households. $N = 4,234$ households. Panel b 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). Each is measured at the municipality level. Each dot is a municipality and the size of the dots is proportional to municipality population. $N = 2,458$ municipalities. For legibility, the top 1% of observations on each axis are excluded.

Figure 2: Concentration of cards and POS terminals over space and time

(a) April 2011



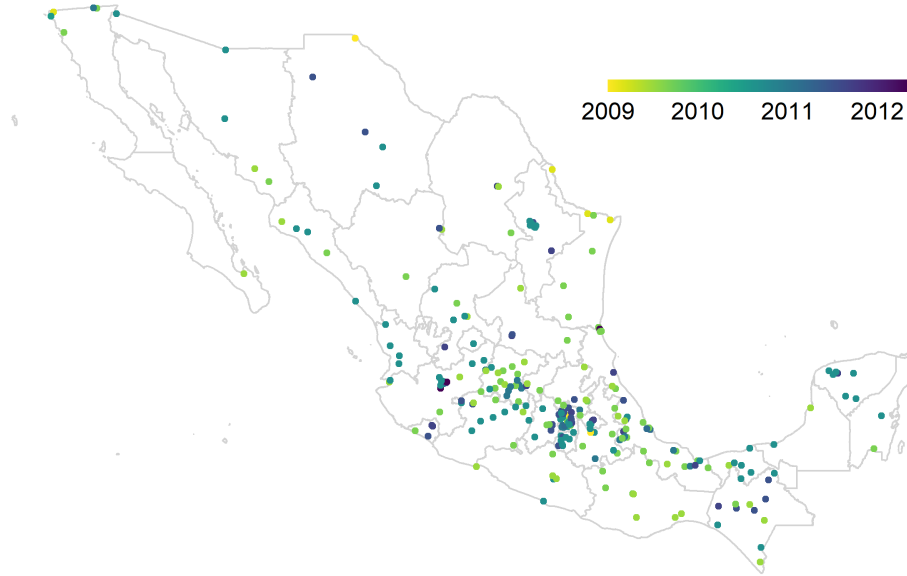
(b) December 2016



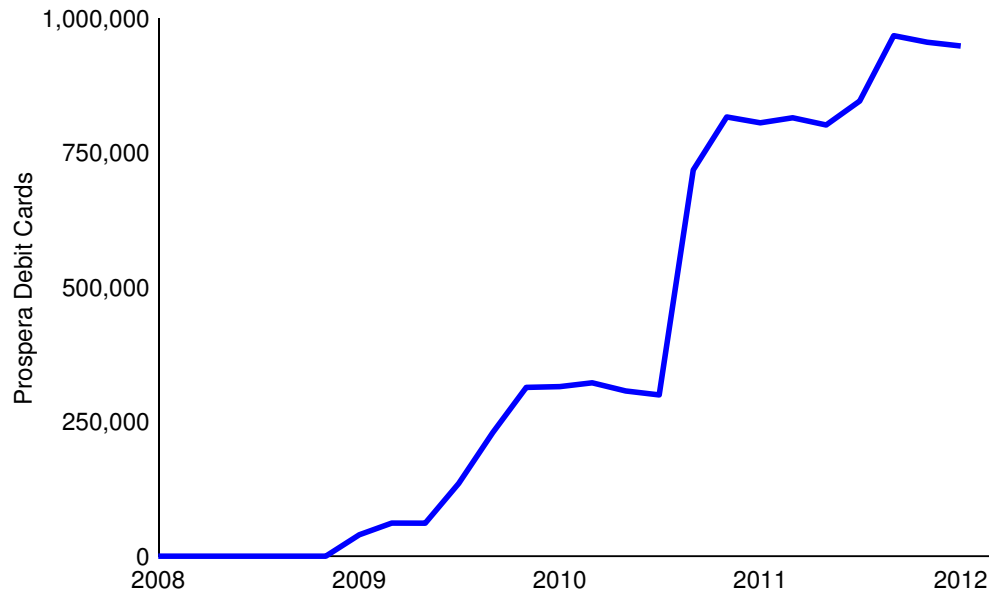
This figure shows the municipality-level number of debit cards per person (constructed as the number of debit cards using CNBV data divided by the population using INEGI data) and proportion of retailers accepting cards (constructed as the number of businesses with POS terminals using CNBV data divided by the number of retailers using INEGI data). The figure also uses municipality shapefiles. $N = 2,458$ municipalities.

Figure 3: Debit card rollout over space and time

(a) Rollout over space and time

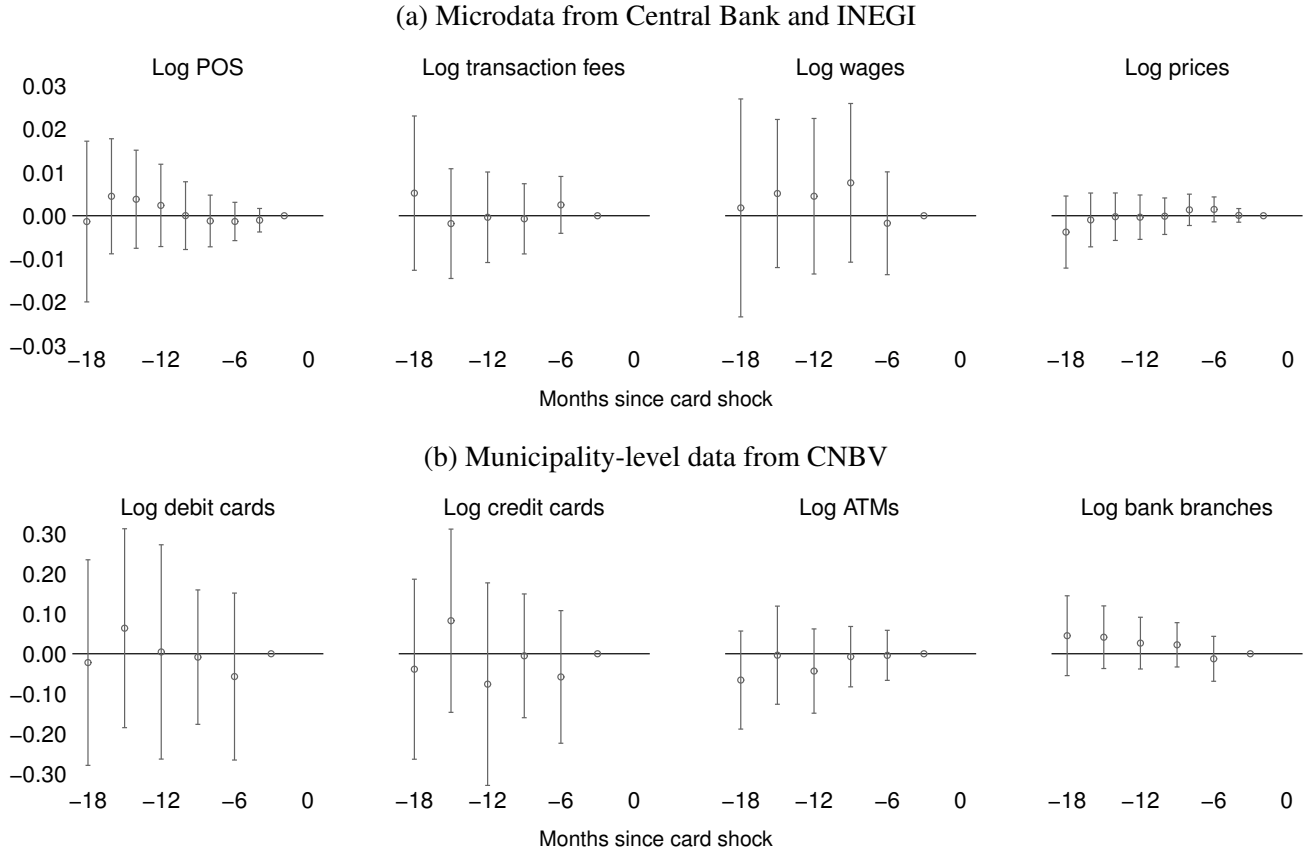


(b) Number of households treated over time



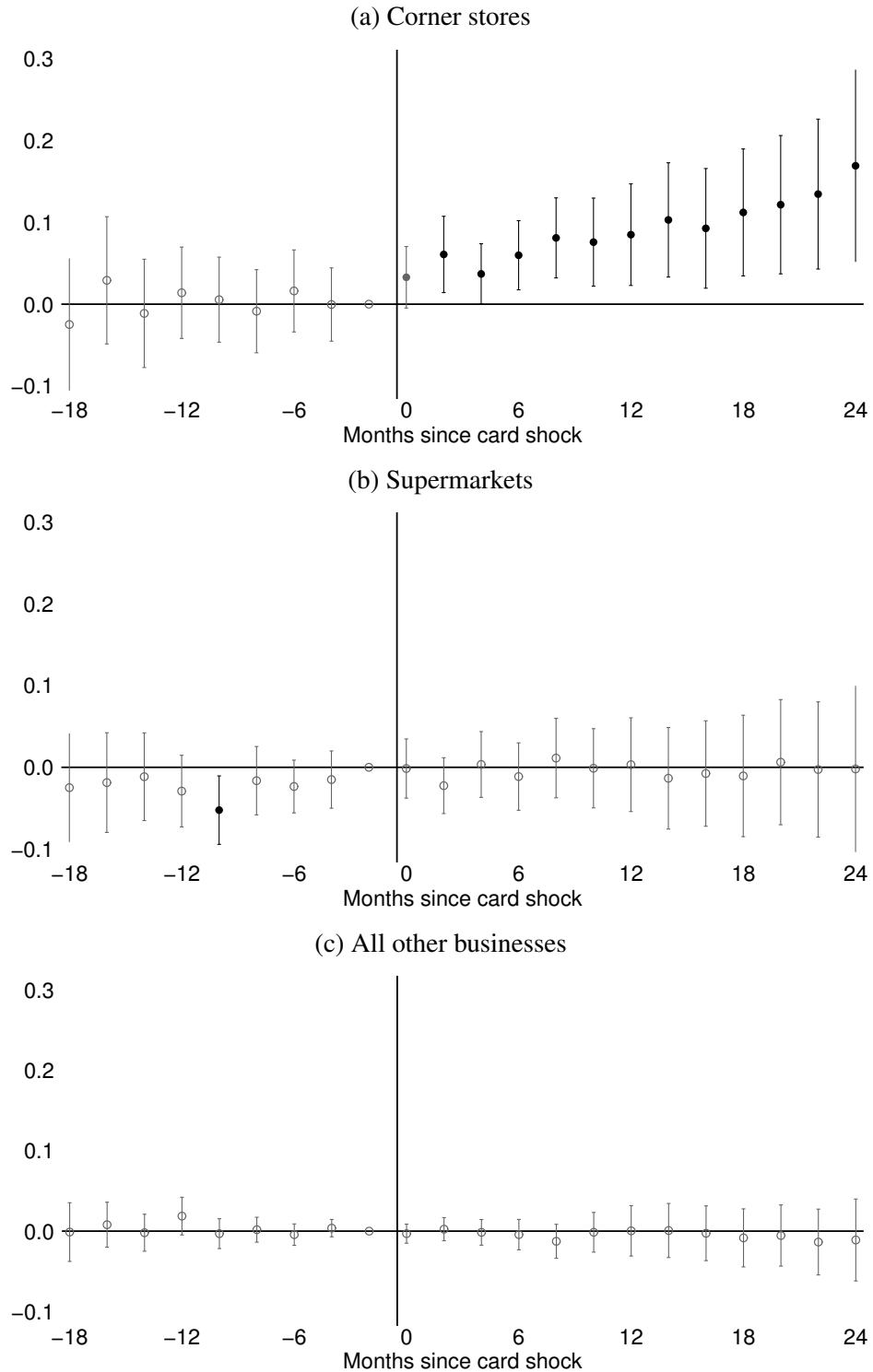
This figure shows when beneficiaries in each urban locality received debit cards from Prospera. It uses administrative data from Prospera on the number of beneficiaries and payment method in each locality during each payment period ($N = 5,690,536$ locality by two-month period observations), which I used to determine which localities were included in the rollout and when the debit card shock occurred in each locality; it also uses locality and state shapefiles.

Figure 4: Balanced pre-trends



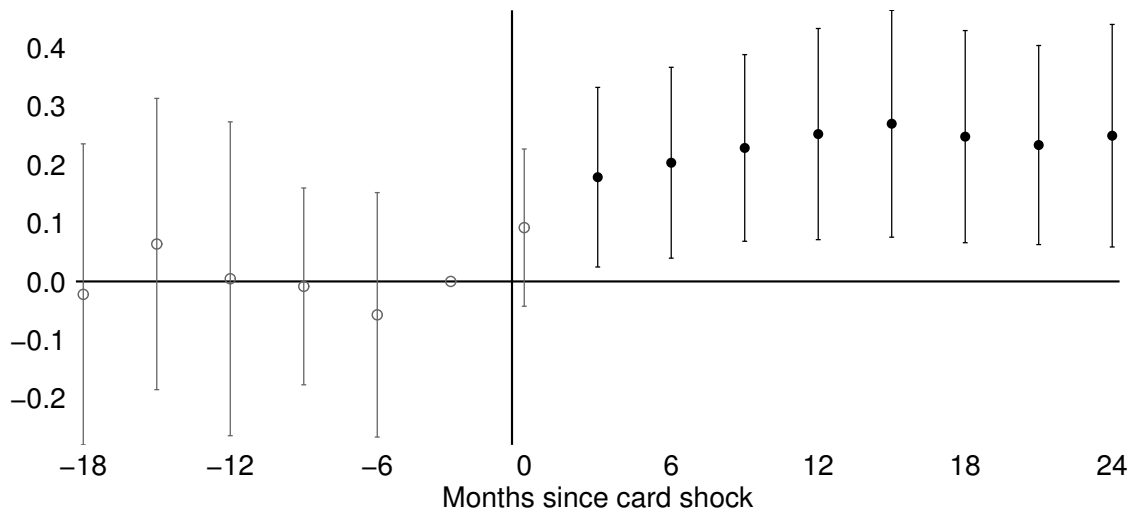
This figure shows parallel pre-trends in variables from data on POS terminal adoptions from Mexico’s Central Bank, data on merchant fees charged by bank over time from Mexico’s Central Bank, data on wages from INEGI’s labor force survey, data on prices from INEGI, and municipality-level data on financial variables (debit cards, credit cards, ATMs, and bank branches) from CNBV. Point estimates are ϕ_k for $k < 0$ from (1), where $k = -1$ is the omitted period. In the POS terminals regression the data are aggregated to the locality level and each observation is a locality by two-month period ($N = 8,806$); in the transaction fees regression the data are aggregated to the municipality level and each observation is a municipality by quarter ($N = 7,823$); in the wages regression each observation is a worker by quarter but since the panel only lasts five quarters for each worker, municipality but not worker fixed effects are included ($N = 1,358,981$); in the prices regression each observation is at the good by store by two-month period level and good by store fixed effects are included ($N = 4,107,314$); in each regression in panel b, each observation is a municipality by quarter ($N = 8,423$). The frequency of ϕ_k coefficients depends on the frequency of each data set. Standard errors are clustered at the locality level, except when data are at the municipality level in which case they are clustered at the municipality level.

Figure 5: Effect of card shock on log POS terminals (event study estimates)



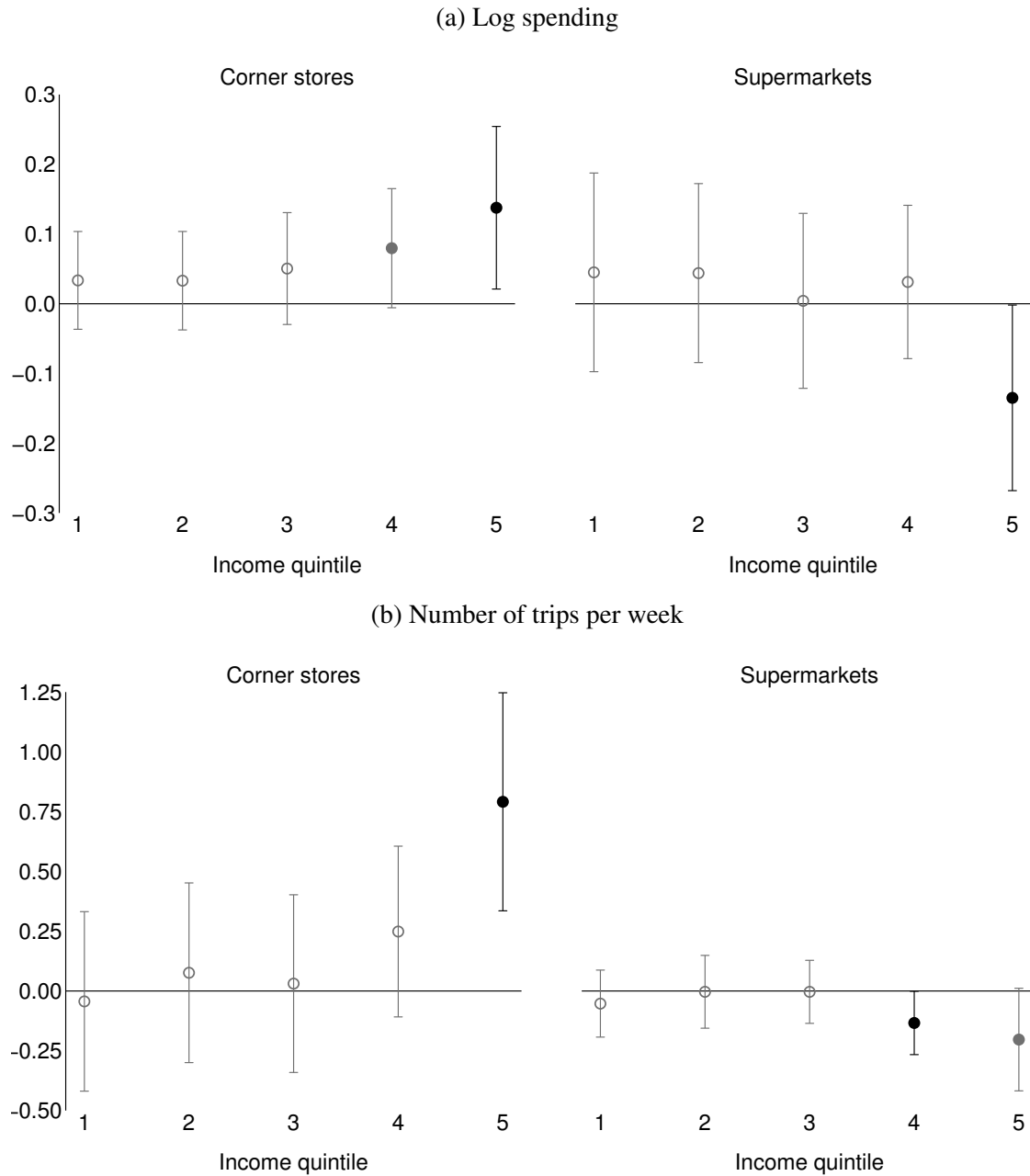
This figure shows the effect of the debit card shock on the stock of point-of-sale (POS) terminals at the two types of retailers that make up the majority of consumption: corner stores (panel a) and supermarkets (panel b), as well as all other businesses (panel c). It graphs the coefficients from (1), where the dependent variable is the log stock of point of sale terminals by type of merchant (corner store, supermarket, or other) in locality j at two-month period t , using data on the universe of POS terminal adoptions and cancellations from Mexico's Central Bank. Observations are at the locality by two-month period level. $N = 8,806$ locality by time observations from 259 localities. Standard errors are clustered at the locality level. The same results can be found in Appendix Table B.1.

Figure 6: Spillover effect to other consumers' card adoption (event study estimates)



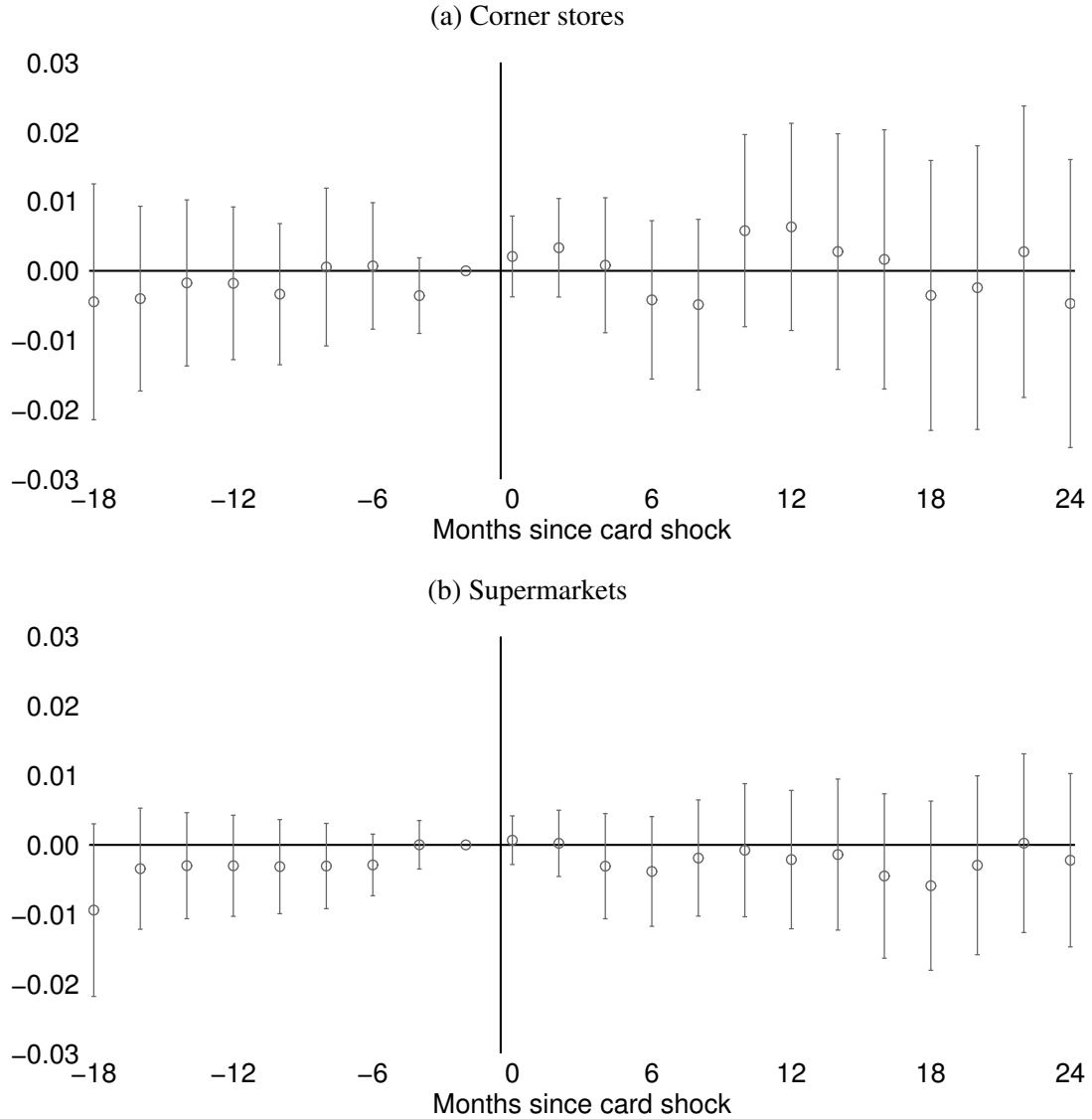
This graph shows that adoption of debit cards at other banks increases after the debit card shock. It graphs the coefficients from (1), where the outcome variable is the log stock of non-Bansefi debit cards in municipality m in quarter t ; this variable comes from the CNBV data. $N = 8,243$ municipality by quarter observations from 255 municipalities. Pooled difference-in-differences coefficient = 0.189^{***} (0.066), or an $\exp(0.189) - 1 = 21\%$ average increase in adoption of debit cards at other banks. Standard errors are clustered at the municipality level. The same results can be found in Table 2.

Figure 7: Spillover effect on consumer spending across store types



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 in the number of trips per week they make to each type of store (panel b). The figure graphs coefficients from (5) where the outcome variable is log spending in pesos at the particular store type (corner stores or supermarkets) in panel a, and number of trips over the course of one week to the particular store type in panel b. It uses data from the ENIGH household income and expenditure survey. $N = 49,810$ households from 220 localities. Standard errors are clustered at the locality level. The same results can be found in Appendix Table B.5.

Figure 8: 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 (7), where the outcome variable is the log price of barcode-level product g at store s at time t . It uses the 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 two-month period observations from 72 municipalities; (b) $N = 979,108$ product by store by two-month period observations from 64 municipalities. Standard errors are clustered at the municipality level.

Table 1: Pre-rollout levels and trends of locality characteristics not correlated with rollout

<i>Panel A: Banco de México, CNBV, population, Prospera, and electoral data</i>			<i>Panel B: INEGI measures used to track development</i>				
Variable	(1) Mean	(2) Standard deviation	(3) Discrete time hazard	Variable	(1) Mean	(2) Standard deviation	(3) Discrete time hazard
Log point-of-sale terminals	5.82	1.84	0.006 (0.007)	% illiterate (age 15+)	6.13	3.94	0.007 (0.005)
Δ log point-of-sale terminals	0.68	0.17	-0.012 (0.026)	Δ % illiterate	-0.01	0.01	-0.757 (1.118)
Log bank accounts	9.97	3.53	0.002 (0.004)	% not attending school (6-14)	4.23	1.94	-0.011* (0.006)
Δ log bank accounts	2.07	4.02	0.001 (0.004)	Δ % not attending school	-0.03	0.02	-0.435 (0.686)
Log commercial bank branches	2.55	1.44	0.014 (0.018)	% without primary education (15+)	40.20	10.18	0.000 (0.003)
Δ log commercial bank branches	0.65	0.97	-0.009 (0.018)	Δ % without primary education	0.17	0.04	0.264 (0.371)
Log government bank branches	0.64	0.59	0.031 (0.019)	% without health insurance	46.51	15.82	0.000 (0.001)
Δ log government bank branches	0.18	0.41	0.001 (0.016)	Δ % without health insurance	-0.05	0.08	-0.003 (0.108)
Log commercial bank ATMs	3.12	1.77	-0.018 (0.013)	% with dirt floor	5.31	5.30	0.000 (0.002)
Log government bank ATMs	0.16	0.37	-0.009 (0.022)	Δ % with dirt floor	-0.02	0.02	0.494 (0.361)
Log population	11.29	1.27	0.016 (0.012)	% without toilet	5.81	3.50	-0.006 (0.004)
Δ log population	0.10	0.18	-0.021 (0.031)	Δ % without toilet	-0.02	0.04	-0.024 (0.167)
Log Prospera beneficiaries	7.09	1.11	-0.003 (0.010)	% without water	6.23	9.00	0.000 (0.001)
Δ log Prospera beneficiaries	0.07	0.38	0.000 (0.015)	Δ % without water	-0.04	0.05	0.088 (0.109)
% vote share PAN	29.01	15.00	0.000 (0.001)	% without plumbing	3.62	6.20	0.004** (0.002)
Δ % vote share PAN	-0.51	17.49	0.001 (0.001)	Δ % without plumbing	-0.06	0.06	0.111 (0.139)
Mayor = PAN (\times 100)	19.31	39.55	0.000 (0.000)	% without electricity	4.32	2.19	0.006 (0.006)
Δ mayor = PAN (\times 100)	-11.97	58.17	0.000 (0.000)	Δ % without electricity	0.02	0.03	0.109 (0.629)
				% without washing machine	33.81	14.47	0.001 (0.001)
				Δ % without washing machine	-0.10	0.05	-0.017 (0.252)
				% without refrigerator	17.31	10.13	-0.002 (0.001)
				Δ % without refrigerator	-0.08	0.06	0.043 (0.268)

Columns 1 and 2 show the mean and standard deviation of levels and changes in locality-level financial infrastructure, population, Prospera beneficiaries, and political measures (panel A), and all characteristics that are used to measure locality-level development by Mexico's National Council for the Evaluation of Social Development (CONEVAL) using data from INEGI's Population Census (panel B). Column 3 tests whether these characteristics predict the timing of when localities receive debit cards as part of the debit card rollout in a single regression (including variables from both panels A and B), using a linear probability discrete time hazard 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 in levels are each measured at the end of 2008 (just prior to the debit card rollout) and their trends (marked with Δ) compare the end of 2008 to the end of 2006. The number of POS terminals is from the POS adoption data from Mexico's Central Bank and includes POS terminals from all merchant categories. Bank accounts, bank branches, and ATMs are from CNBV; I do not include trends in commercial bank ATMs or government bank ATMs because ATMs were only added to the CNBV data in the last quarter of 2008. Population is based on the 2005 Population Census (which is conducted every 5 years) and change in population compares to the 2000 Census. Prospera beneficiaries are based on administrative data from Prospera; the variable in levels is measured at the end of 2008 and the change relative to the end of 2006. Vote share of the PAN party and whether the local mayor is from the PAN party (i.e., the same party as Mexico's president during the debit card rollout) are based on electoral data. Vote share of the PAN party is measured in the most recent pre-rollout election and the change relative to the election before that; whether the mayor is from PAN is measured in 2008 and the change relative to 2006. Levels of all variables in panel B are based on the 2005 Population Census and changes compare to the 2000 Census. $N = 259$ localities in the debit card rollout, and 2,769 locality by two-month-period observations in column 3. Standard errors are clustered at the locality level.

Table 2: Spillovers on other consumers' card adoption (event study estimates)

Months since card shock	(1)	(2)	(3)	(4)	(5)
	Dependent variable: log debit cards (excluding Bansefi cards)				
	Main	Heterogeneity			
		ATM density		Proportion of Prospera transactions at supermarkets	
	< median	> median	< median	> median	
-18 to -16	-0.022 (0.131)	0.111 (0.262)	-0.001 (0.092)	-0.067 (0.193)	-0.093 (0.200)
-15 to -13	0.064 (0.127)	0.188 (0.249)	0.029 (0.062)	-0.068 (0.189)	0.252 (0.216)
-12 to -10	0.005 (0.136)	0.043 (0.256)	0.051 (0.058)	0.170 (0.161)	-0.162 (0.249)
-9 to -7	-0.008 (0.086)	0.030 (0.167)	-0.006 (0.057)	-0.070 (0.141)	0.030 (0.114)
-6 to -4	-0.057 (0.106)	0.089 (0.175)	-0.137 (0.129)	0.036 (0.182)	-0.165 (0.140)
-3 to -1 (omitted)	0	0	0	0	0
0 to 2	0.092 (0.068)	0.105 (0.122)	0.096* (0.054)	0.148 (0.091)	0.069 (0.117)
3 to 5	0.178** (0.078)	0.253* (0.142)	0.085 (0.061)	0.274** (0.115)	0.130 (0.128)
6 to 8	0.203** (0.083)	0.332** (0.146)	0.079 (0.068)	0.378*** (0.129)	0.070 (0.122)
9 to 11	0.229*** (0.081)	0.357** (0.141)	0.078 (0.063)	0.389*** (0.136)	0.101 (0.112)
12 to 14	0.252*** (0.092)	0.393** (0.158)	0.095 (0.068)	0.432*** (0.159)	0.121 (0.119)
15 to 17	0.270*** (0.099)	0.420** (0.169)	0.092 (0.074)	0.460*** (0.169)	0.132 (0.128)
18 to 20	0.248*** (0.092)	0.395** (0.159)	0.092 (0.074)	0.444*** (0.149)	0.110 (0.128)
21 to 23	0.234*** (0.087)	0.360** (0.148)	0.096 (0.072)	0.412*** (0.138)	0.106 (0.125)
24 to 26	0.250** (0.097)	0.401** (0.166)	0.095 (0.082)	0.465*** (0.156)	0.105 (0.138)
<i>N</i> (municipality × quarter)	8,243	4,035	4,208	3,833	3,852
Number of municipalities	255	127	128	119	118
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes
Time (quarter) fixed effects	Yes	Yes	Yes	Yes	Yes

This table shows spillovers within the demand side of the market onto other consumers' adoption of debit cards. It shows the coefficients from (1), where the dependent variable is the log stock of debit cards (excluding debit cards issued by Bansefi) in a municipality by quarter, using data from CNBV. Observations are at the municipality by quarter level since the CNBV data is at the issuing bank by municipality by quarter level. Column 1 shows the main estimates. Columns 2–3 show heterogeneity by ATM density, splitting the sample of municipalities at the median of baseline ATMs per person (measured at the end of 2008, also using CNBV data, and divided by population in INEGI data). Columns 4–5 show heterogeneity by whether Prospera beneficiaries tend to shop at supermarkets. Using Bansefi transactions data, I calculate the fraction of transactions made by Prospera beneficiaries at supermarkets in the first 6 months they have the debit card and split the municipalities at the median. The sum of the number of municipalities in columns 4 and 5 is less than in column 1 because in 18 municipalities no Prospera beneficiaries use the card to make POS transactions during the first 6 months with the card, and hence the heterogeneity variable is missing for those municipalities. Standard errors are clustered at the municipality level.

Table 3: Spillovers on consumption decisions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Dependent variable: log spending at...								
	Corner stores			Supermarkets			Total		
Diff-in-diff	0.067** (0.032)	0.051 (0.033)	0.076** (0.033)	-0.018 (0.043)	0.003 (0.050)	-0.016 (0.045)	0.029 (0.030)	0.029 (0.033)	0.041 (0.030)
Diff-in-diff × has credit card		0.061 (0.040)			-0.058 (0.062)			-0.012 (0.040)	
Diff-in-diff × Prospera beneficiary			-0.127** (0.060)			-0.030 (0.133)			-0.161** (0.063)
P-value DID + (DID × interaction)		[0.009]***	[0.423]		[0.250]	[0.732]		[0.581]	[0.073]*
Number of households	49,810	49,810	49,810	49,810	49,810	49,810	49,810	49,810	49,810
Number of localities	220	220	220	220	220	220	220	220	220
Locality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Locality by card/beneficiary fixed effects		Yes	Yes		Yes	Yes		Yes	Yes
Card/beneficiary by time fixed effects		Yes	Yes		Yes	Yes		Yes	Yes

This table shows the effect of the debit card shock on consumption at corner stores, supermarkets, and total. The outcome variable is log spending from the consumption module of ENIGH (at corner stores in columns 1–3, at supermarkets in columns 4–6, and total—including corner stores, supermarkets, and other venues such as open-air markets—in columns 7–9). Columns 2, 5, and 8 show heterogeneity by whether the household has a credit card, and columns 3, 6, and 9 show heterogeneity by whether the household is a beneficiary of the Prospera program. Standard errors are clustered at the locality level.

Table 4: Retailer outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Log Sales	Log Inventory Costs	Log Wage Costs	Log Number Workers	Log Rent Costs	Log Capital	Log Electricity Costs	asinh Profits	Charged VAT or Paid Social Security
<i>Panel A: Corner stores</i>									
Shock 3–4.5 years ago	0.081** (0.036)	0.059* (0.034)	-0.022 (0.020)	0.000 (0.005)	-0.028 (0.025)	0.047 (0.083)	-0.029 (0.034)	0.212** (0.099)	0.014 (0.009)
Shock 1.5–3 years ago	0.045 (0.037)	0.022 (0.035)	-0.022 (0.019)	0.000 (0.004)	0.022 (0.023)	0.024 (0.089)	0.005 (0.034)	0.143 (0.104)	0.031** (0.012)
Shock 0–1.5 years ago (omitted)	0	0	0	0	0	0	0	0	0
Number of firms	172,441	172,441	172,441	172,441	172,441	172,441	172,441	172,441	172,441
<i>Pooled coefficient</i>									
Shock 1.5–4.5 years ago	0.061* (0.034)	0.039 (0.032)	-0.022 (0.017)	0.000 (0.004)	-0.002 (0.022)	0.035 (0.082)	-0.011 (0.032)	0.175* (0.096)	0.023*** (0.008)
Shock 0–1.5 years ago (omitted)	0	0	0	0	0	0	0	0	0
Number of firms	172,441	172,441	172,441	172,441	172,441	172,441	172,441	172,441	172,441
<i>Panel B: Supermarkets</i>									
Shock 3–4.5 years ago	-0.143** (0.063)	-0.155** (0.062)	-0.151 (0.316)	-0.014 (0.019)	0.314 (0.300)	-0.064 (0.085)	0.180 (0.254)	-0.228 (2.353)	-0.054 (0.082)
Shock 1.5–3 years ago	-0.119* (0.062)	-0.124** (0.062)	-0.346 (0.348)	-0.022 (0.019)	0.135 (0.256)	0.144 (0.116)	0.153 (0.259)	0.149 (2.341)	-0.013 (0.081)
Shock 0–1.5 years ago (omitted)	0	0	0	0	0	0	0	0	0
Number of firms	13,782	13,782	13,782	13,782	13,782	13,782	13,782	13,782	13,782
<i>Pooled coefficient</i>									
Shock 1.5–4.5 years ago	-0.131** (0.058)	-0.140** (0.057)	-0.246 (0.308)	-0.018 (0.019)	0.227 (0.242)	0.037 (0.086)	0.167 (0.253)	-0.045 (2.326)	-0.034 (0.080)
Shock 0–1.5 years ago (omitted)	0	0	0	0	0	0	0	0	0
Number of firms	13,782	13,782	13,782	13,782	13,782	13,782	13,782	13,782	13,782
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table shows that the debit card shock led to an increase in corner store sales at the expense of supermarket sales. Corner stores also increase their inventory costs while keeping other input costs fixed, which leads to an increase in profits. The table shows intent-to-treat estimates of the effect of the card shock on various outcomes listed in the column headings for corner stores (panel A) and supermarkets (panel B), using firm panel data from the 2008 and 2013 Economic Census. It shows results from (6) where the omitted dummy corresponds to localities treated less than 1.5 years before the second census wave. The “charged VAT or paid social security” column is a dummy variable equal to 1 if the firm reports charging any value-added tax (VAT) to customers, or any costs from paying social security for employees. Standard errors are clustered at the locality level.

Table 5: Demand estimation

Dependent variable: log share of expenditures at store type s minus log share at outside option	
(1)	
Prospera beneficiaries	
Log price difference ($-\alpha_k$)	-3.35* (1.93)
Share of stores with POS (θ_k)	0.24 (0.30)
Non-beneficiary non-credit card holders	
Log price difference ($-\alpha_k$)	-2.93** (1.26)
Share of stores with POS (θ_k)	0.55*** (0.21)
Credit card holders	
Log price difference ($-\alpha_k$)	-2.01 (1.29)
Share of stores with POS (θ_k)	0.58*** (0.23)
First-stage joint F-test	46.56
Number of observations	21,775
Locality by consumer type by store type fixed effects	Yes
Store type by consumer type by time fixed effects	Yes

This table shows results from (12), which estimates the price elasticity of consumption across store types and the value of shopping at a store that has adopted a POS terminal. Observations are at the census tract by consumer type by store type by time level. There are two store types, corner stores and super markets (since the third store type, open-air markets, is treated as the outside option). 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. Regions are defined by Mexico's five official electoral regions. The share of stores with POS terminals is instrumented with the debit card shock. Standard errors are clustered at the locality level.

Appendix A Data Appendix

The main data I use include (i) administrative data on the debit card rollout, (ii) transactions-level data from the bank accounts of the cash transfer recipients who received debit cards, (iii) the universe of point-of-sale (POS) terminal adoptions in Mexico, (iv) the universe of debit and credit card transactions at POS terminals (by all card holders, not just Prospera beneficiaries), (v) the number of debit cards and other measures of financial infrastructure and financial service use by bank by municipality by quarter, (vi) household-by-product level consumption and price data from a representative household survey, (vii) high-frequency product-by-store level price data from a sample of retailers, (viii) a panel on sales (including those from cash sales) and costs of the universe of retailers, and (ix) a quarterly labor force survey. This appendix describes each of these data sets, as well as auxiliary data sets I use, in turn.

A.1 Administrative data on debit card rollout

My source of information on the timing of the card rollout is a locality by two-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 5,690,536 locality by two-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.³⁸ 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.

³⁸In 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 A.2, I observe when the beneficiary is switched to a debit card account.

A.2 Transactions of Prospera beneficiaries

These data include the universe of transactions made by cash transfer beneficiaries. The data set includes 106,449,749 transactions from 961,617 accounts. The data include type of transaction (including cash withdrawals, card payments, deposits, interest payments, and fees), amount in pesos, a timestamp, and other details about each transaction. 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 each 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. I use these classifications to create a variable of the proportion of transactions made by Prospera beneficiaries at supermarkets, which I use for a heterogeneity test.

A.3 Universe of POS terminal adoptions

Data on POS terminal adoption comes from Banco de México (Mexico's Central Bank). The data are 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). They also include an anonymized firm ID and the postal code in which the firm is located. Because the card rollout occurred at the locality level and my demand estimation is at the AGEBA (census tract) level, and because neither an official mapping

between localities or AGEBs 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 of each firm and its geocoordinates to determine its AGEB and locality.³⁹ This data set on the geocoordinates of the universe of firms is described in more detail in Section A.10.

A.4 Universe of card transactions at POS terminals

These data include the universe of debit and credit card transactions at POS terminals from July 2007 to March 2015. The data include an anonymized indicator of the acquiring and issuing bank for each transaction, type of business (MCC code), type of card (credit or debit), type of transaction (ON-US or OFF-US, which indicates whether the acquiring and issuing bank are on two separate networks), the date of the transaction, amount in pesos, and a string variable with the locality name. I match the locality strings to INEGI locality codes using a crosswalk created by the Central Bank that accounts for the many typos in the locality strings. The data do not include identifiers that can be used to link transactions made on the same card nor at a particular business. The data set includes 4,718,690,034 observations (transactions).

A.5 Quarterly data 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. The data also include the number of POS terminals, but not by type of business and only since April 2011. I use these data to (i) present descriptive statistics on financial technology adoption on the two sides of the market, (ii) test whether the card rollout is correlated with pre-treatment levels and trends of financial infrastructure, and (iii)

³⁹Shapefiles for a partial set of postal codes are available at https://www.correosdemexico.gob.mx/SSLServicios/ConsultaCP/CodigoPostal_Exportar.aspx, but a substantial fraction of postal codes are not included in the data set. 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.

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 2,458 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. These data also include the number of POS terminals (not differentiated by type of store), but only since April 2011, which is why the descriptive figures comparing card and POS adoption across all municipalities in Mexico begin in April 2011. Other variables, such as the number of bank accounts and number of bank branches, extend back to 1995.

A.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-air market, corner store, supermarket, etc. I use these data to construct a measure of total spending at each of the different types of store. The survey also includes a detailed income

module, which allows me to measure household income per capita, which I use to test for heterogeneity throughout the income distribution. In addition, I use data in the survey about whether a household is a Prospera beneficiary (based on questions about income, scholarships, or health services received through Prospera) and whether a household has a credit card—as the survey does not ask about debit card or bank account ownership.

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 below-municipality level geographic identifiers which are 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 in the demand estimation in Section 6.2. Within Mexico’s 630 urban localities, there are 61,424 AGEBS, making them roughly analogous to census tracts in the US.

A.7 Economic Census on the universe of retailers

Every five 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 waves 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, about 2 million of which are retailers. Of the 2 million retailers, about 1 million are also observed in the 2013 census, indicating that they survived over the five year period between census waves. This rate of firm survival is consistent with estimates of firm survival in developing countries (McKenzie and Paffhausen, 2019). Of the

retailers observed in both census waves, 344,305 are corner stores and 172,441 of those are in the urban localities included in the Prospera card rollout; 20,251 are supermarkets, department stores, and chain convenience stores included in both survey waves, of which 13,782 are in the urban localities included in the card rollout.

The Economic Census includes many questions about costs by category, revenue by category, years in business, number of employees, loans, inventory, assets, and locality. The survey does not include a question about whether the firm is formal, but I construct a proxy for formality based on whether the firm charged VAT to its customers. Store types are determined using six-digit NAICS codes.

A.8 High-frequency product-by-store price panel

Mexico collects weekly price estimates for food products and biweekly price estimates for other products to construct its consumer price index (CPI). I use the store by product by week price microdata to test whether the debit card shock had a general equilibrium price effect. Until 2010 the data were collected by Mexico's Central Bank, and from 2011 on they were 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.⁴⁰ Because the Mexican government focuses on the largest urban areas when collecting price data for its CPI, most stores are in urban municipalities included in the card rollout: after removing stores in other municipalities, there are still 4.9 million product by store by two-month period observations. I further restrict the analysis to the category of goods encompassing food, beverages, alcohol, and tobacco, as this

⁴⁰For 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 A.5.

is likely the main type of product for which consumers are deciding between purchasing at the supermarket and corner store.⁴¹ Finally, I restrict to a balanced sample of products so that results are not affected by a change in the composition of products included in the price list. This leaves 531,762 price quotes from corner stores and 979,108 price quotes from supermarkets in the final data set used for the event study regressions.

A.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 data set includes questions about wages, current and former jobs, reason for termination of a previous job, municipality, and includes four-digit NAICS codes that I use to determine the type of store at which retail employees work. I use data spanning 2005–2016, which include over 20 million individual by quarter observations. After restricting to urban localities included in the debit card rollout, there are 83,222 employees employed at corner stores at the time they are surveyed and 96,380 employees employed at supermarkets. For the analysis of probability of losing a job, I include employees either currently or formerly employed at these store types, of which there are 95,539 corner store employees and 98,706 supermarket employees in the data. These samples exclude owners of corner stores and supermarkets, who I identify in the data using a question about whether the worker has a boss.

A.10 Auxiliary administrative data

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

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

rollout) to test whether the card rollout is correlated with locality characteristics. I also measure changes in these variables relative to the same variables from the 2000 population census. I use the same characteristics that are used to measure locality-level development by INEGI and Mexico's National Council for the Evaluation of Social Development (CONEVAL). These locality-level measures based on the population census are publicly available from <https://www.inegi.org.mx/programas/ccpv/2005/default.html#Microdatos> and <https://www.inegi.org.mx/programas/ccpv/2000/default.html#Microdatos>.

Shapefiles. I use polygons corresponding to the border of each state, municipality, locality, and AGEB (census tract) for several figures in the paper, and to create the mapping between localities/AGEBs and postal codes. These shapefiles are publicly available from INEGI.

Geocoordinates of the universe of retail firms in Mexico. These data are a directory of all firms in Mexico, including the name and six-digit NAICS code of the firm (which allow me to identify the type of store), its postal code, and exact geocoordinates. This directory is publicly available and is thoroughly updated after each Economic Census. I combine these data with AGEB shapefiles to (i) create a mapping between AGEBs, localities, and postal codes since some of the Central Bank data are at the postal code level and (ii) 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 at the postal code level, after merging the data with the number of retailers with POS terminals from the Central Bank data.

To merge with the Central Bank data and construct the proportion of each type of retailer that accepts cards at the postal code level, I restrict these data to firms that were included in the data set prior to the card rollout, which correspond to the 2008 Economic Census. After making this restriction, there are 4,287,463 total firms in the data, 1,888,460 of which are retailers.

Postal code to municipality mapping. While a postal code to locality mapping is not available, a postal code to municipality mapping is available from Mexico's postal service (SEPOMEX). I use this mapping when I need a mapping between municipalities and postal codes.

Transaction fees at POS terminals. Mexico’s Central Bank publishes data on the POS terminal transaction fees charged by each bank by sector over time. These data are publicly available.

Elections data. I use elections data that were hand-digitized from pdfs recording polling station level election results (i.e., number of votes for each party) obtained from the electoral commissions of each state in Mexico. The data include vote shares for each party and span 2004–2014. After aggregating to the municipality by election by party level, the data include 34,803 observations. I use these data to both measure the vote share for the PAN party (the same party as the president of Mexico during the debit card rollout) in each election, as well as construct a municipality-by-year dummy variable for whether the municipal mayor belongs to the PAN party.

A.11 Auxiliary survey data

Global Findex. I use the 2017 Global Findex microdata (Demirgüç-Kunt et al., 2018) to calculate the proportion of adults in Mexico and worldwide that do not have a bank or mobile money account. The survey includes 1,000 respondents in Mexico and 154,923 total respondents worldwide.

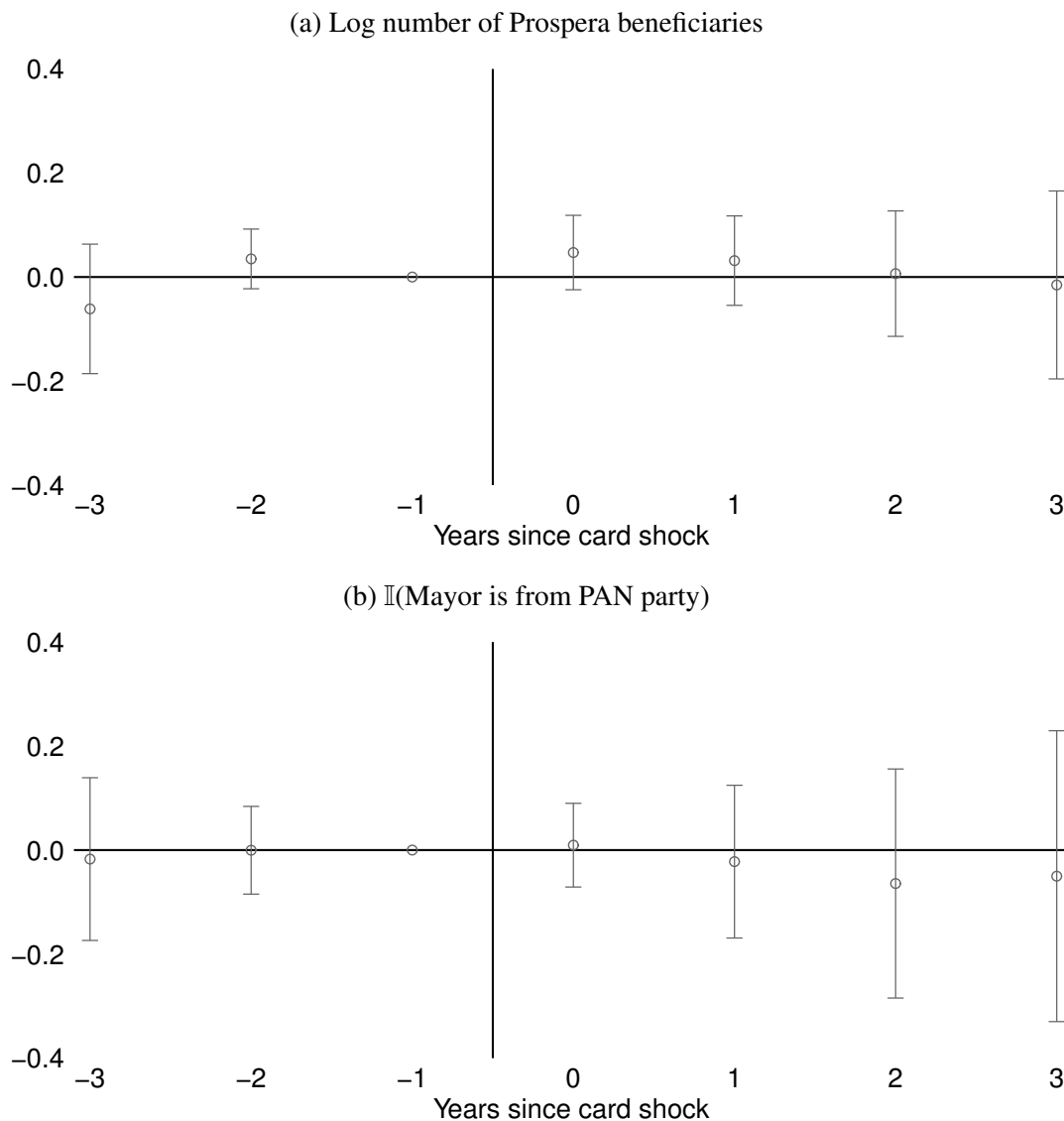
Mexican Family Life Survey. This survey has more detailed information about debit and credit card ownership than other household surveys in Mexico. The most recent wave of the Mexican Family Life Survey was conducted in 2009, prior to the debit card rollout in nearly all localities included in the rollout. The survey also includes detailed questions about income, as well as numerous other survey modules. I use the survey 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 9,205 households; because the survey oversampled rural areas, just 4,234 of these households live in urban areas, which is the sample I use for the summary statistics presented in the paper. These data are publicly available from <http://www.ennvih-mxfls.org/>, with the exception of the questions about whether a household is a Prospera beneficiary and the income they receive from the program. To include that income in the household income aggregate, I requested and received these additional variables from the data provider.

National Enterprise Financing Survey. This survey of 3,469 firms was conducted jointly by CNBV and INEGI, and I accessed the data on-site at INEGI. The data set includes a number of questions about the banking, financing, and payment methods used by small businesses. I use it for descriptive statistics on the fraction of firms of each type that accept card payments and the fraction of transactions that are paid by card conditional on a store accepting card payments.

Payment Methods Survey of Prospera beneficiaries. This publicly-available 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. 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. The survey includes questions about beneficiaries' use of financial services and their satisfaction with the debit cards.

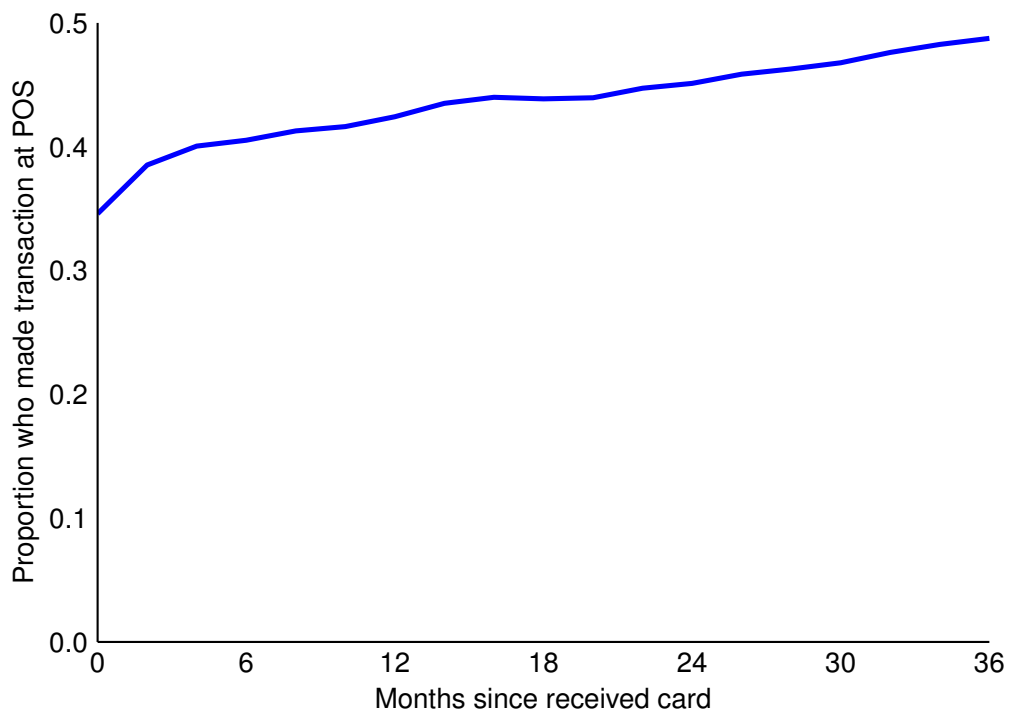
Appendix B Appendix Figures and Tables

Figure B.1: Rollout not correlated with number of beneficiaries or political party



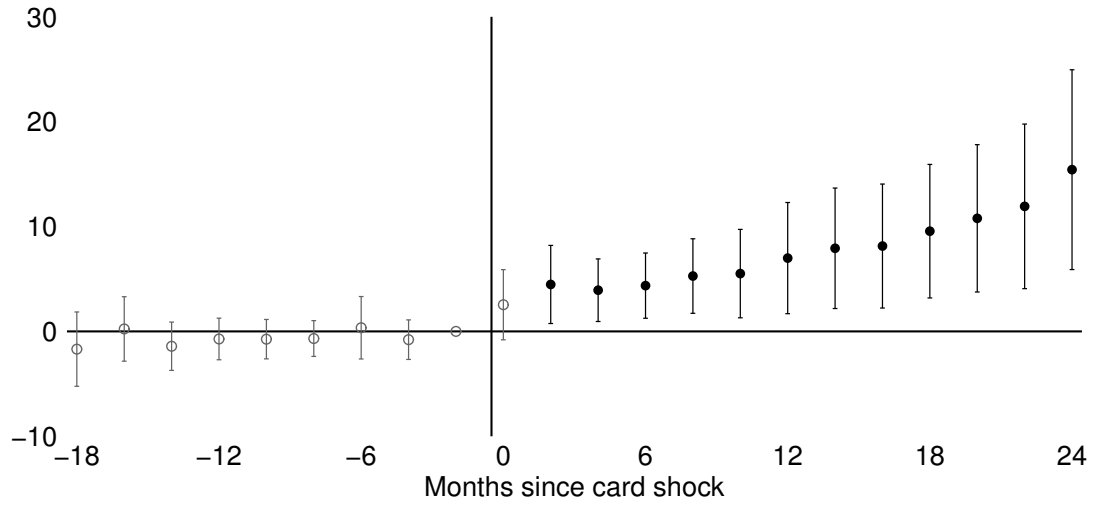
This figure shows that the rollout of debit cards is not correlated with changes in the number of beneficiaries or in the political party in power at the local level. Panel a shows the coefficients from (1), where the outcome is the log number of Prospera beneficiaries in locality j during the last two-month period of year t , using administrative data from Prospera on the number of beneficiaries in each locality over time (available by year prior to 2009 and by two-month period from 2009 on). $N = 2,590$ locality by year observations in 259 treated localities. Standard errors are clustered at the locality level. Panel b shows the coefficients from (1), where the outcome is a dummy variable equal to one if the municipal mayor is from the PAN, the party of the country's president during the card rollout, in municipality m during year t . The estimation uses hand-digitized data on vote shares from municipal elections. $N = 2,805$ municipality by year observations in 255 municipalities. Standard errors are clustered at the municipality level.

Figure B.2: Proportion of Prospera card holders who make transactions at POS terminals



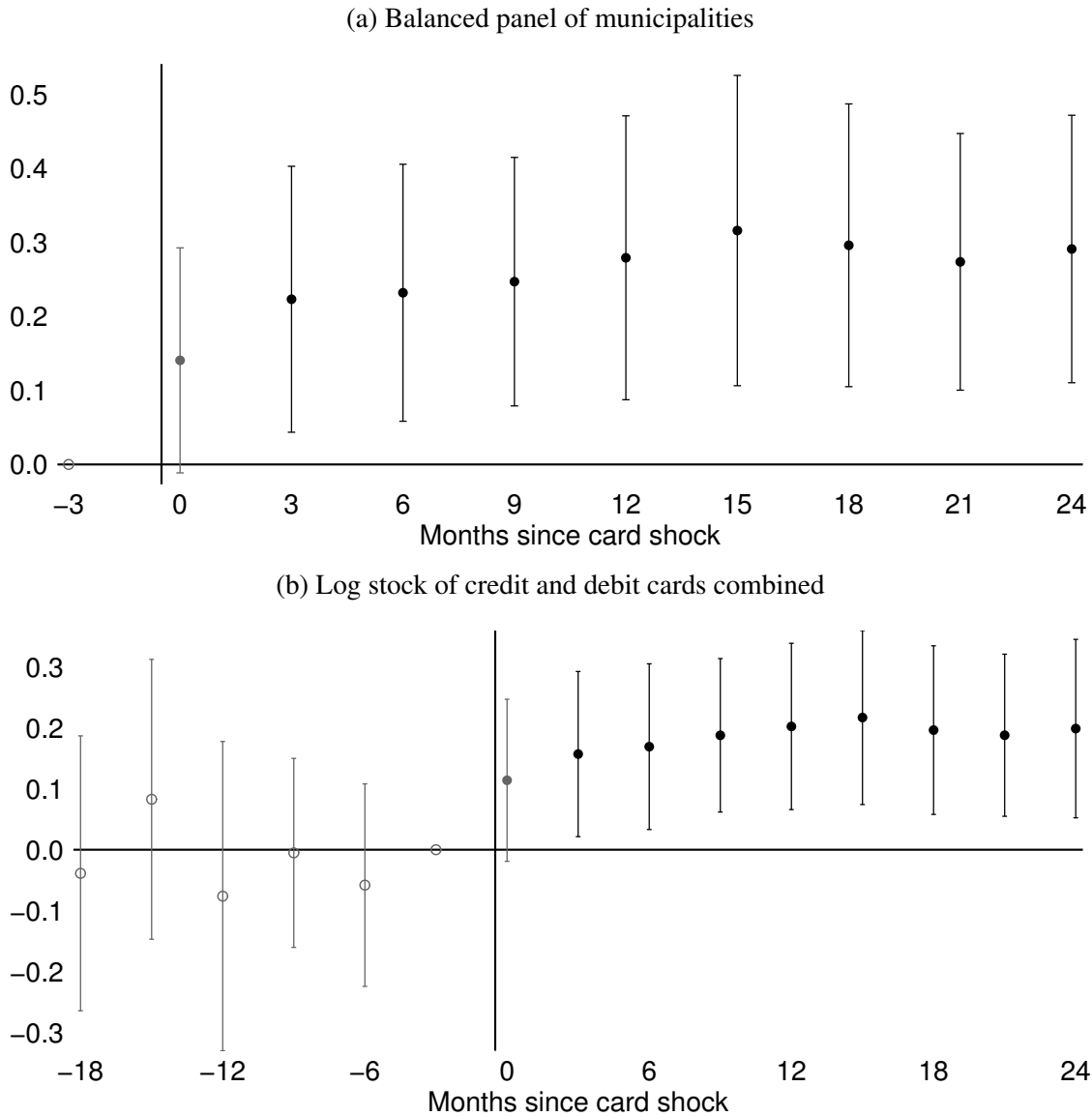
This figure shows the proportion of Prospera card holders who make at least one transaction at a POS terminal using their card during each two-month period. Periods are binned in two-month intervals because the cash transfer is paid every two months. The figure uses Bansefi transactions data with $N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

Figure B.3: Effect of card shock on corner store POS adoption **in levels** (event study estimates)



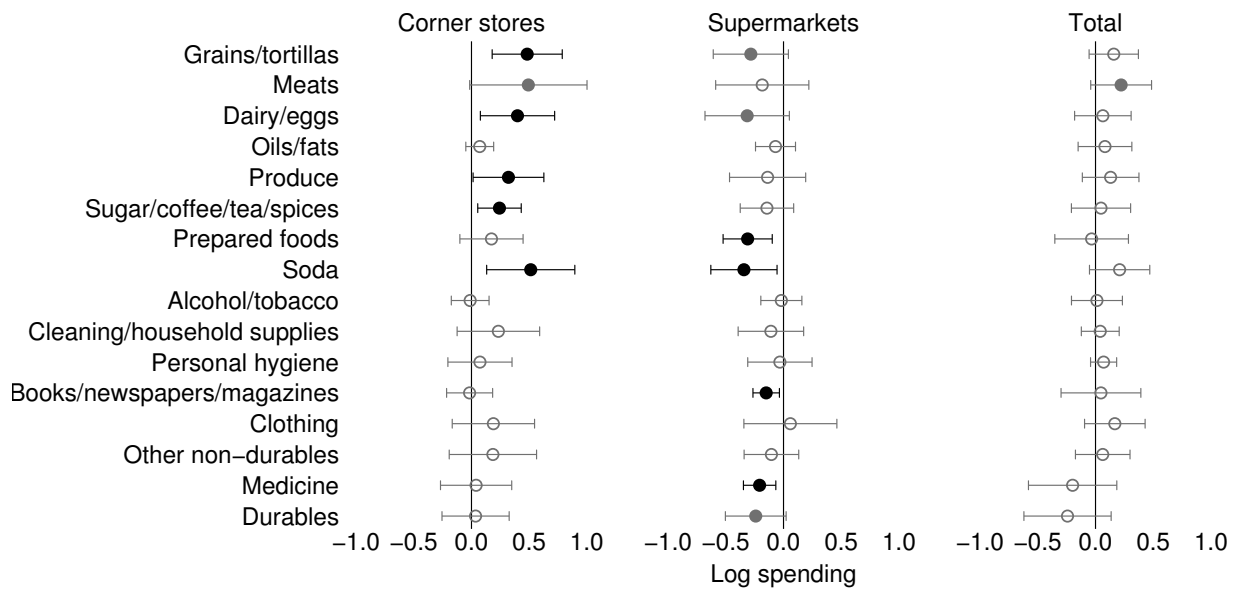
This figure shows the effect of the debit card shock on the stock of point-of-sale (POS) terminals at corner stores, measured in levels. It graphs the coefficients from (1), where the dependent variable is the number of corner stores with point of sale terminals. Observations are at the locality by two-month period level. $N = 8,806$ locality by time observations from 259 localities. Standard errors are clustered at the locality level.

Figure B.4: Robustness of spillover effect to other consumers' card adoption



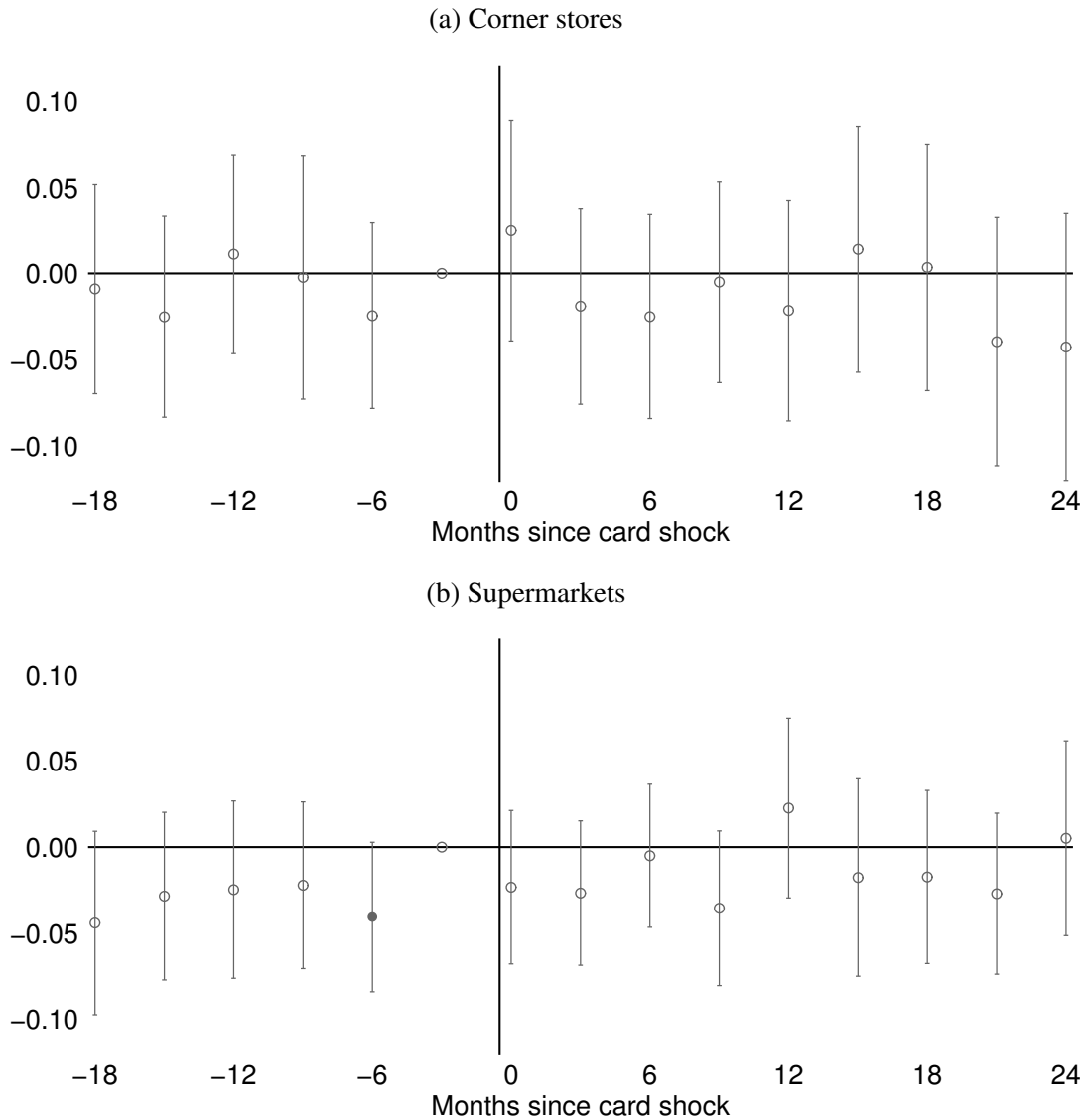
This figure shows robustness of the spillover effect on other consumers' card adoption, showing coefficients from (1) using the CNBV data. Panel a uses the same outcome variable as Figure 6—the log stock of non-Bansefi debit cards in municipality m in quarter t —but in the estimation uses only the relative periods for which the full sample of 255 municipalities is available. (Because the data begin in the last quarter of 2008 and the rollout begins in the first quarter of 2009, pre-trends cannot be shown in this graph beyond the omitted $k = -1$ period. This is in contrast to the pre-trends for POS terminals in Figure 5, which are already based on a balanced panel since the POS data begin in 2006, i.e. three years before the rollout began.) $N = 5,100$ municipality by quarter observations from 255 municipalities. Panel b shows the adoption of debit *and* credit cards, i.e. the outcome variable is the log stock of non-Bansefi debit and credit cards in municipality m in quarter t . $N = 8,243$ municipality by quarter observations from 255 municipalities. Standard errors are clustered at the municipality level.

Figure B.5: Effect on fifth quintile’s log spending by product category and store type



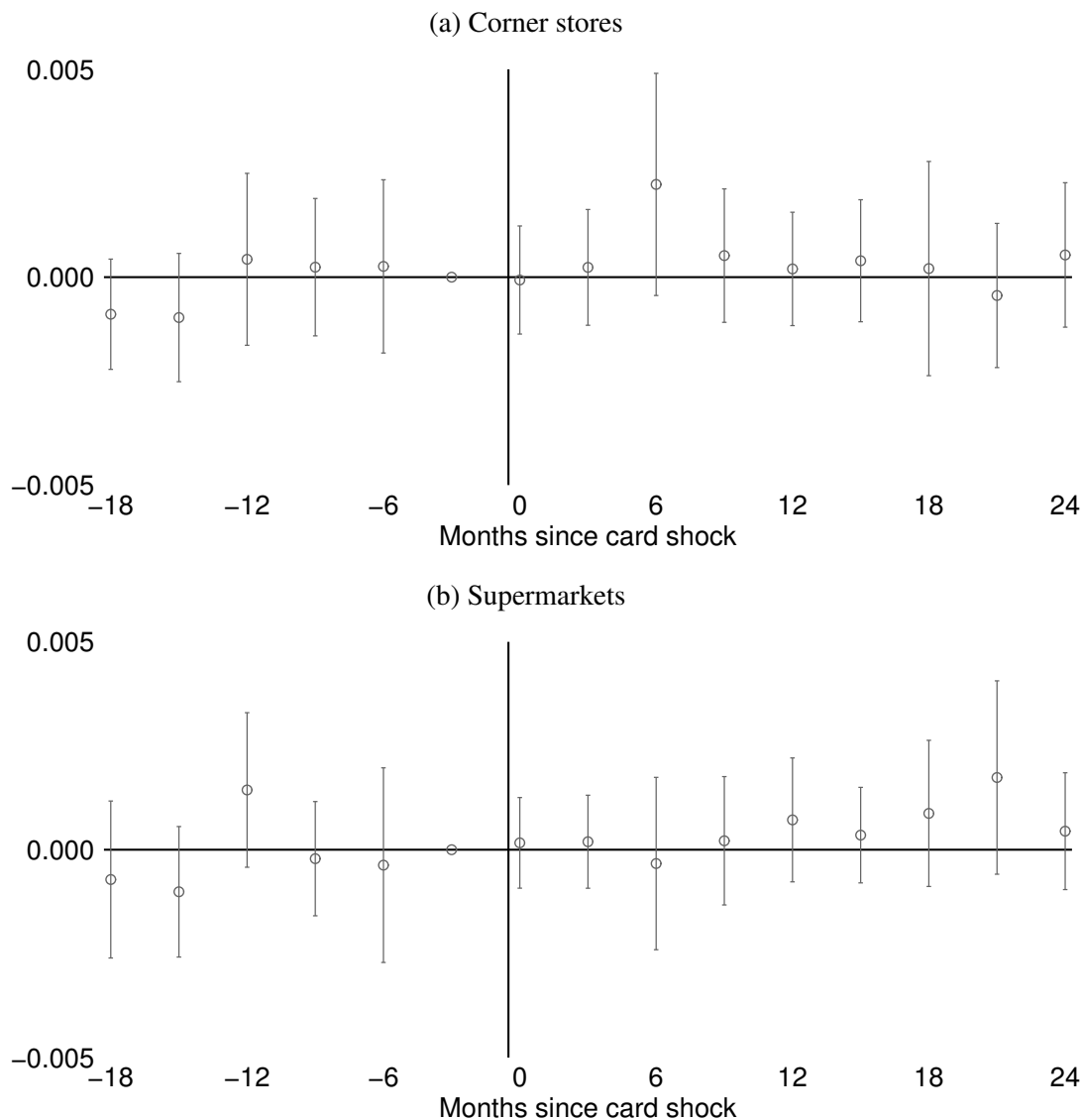
This figure shows a breakdown by product category of the partial shift in the fifth quintile’s consumption from supermarkets to corner stores. Each coefficient is $\gamma + \psi_5$ from a separate regression using specification (5), where the outcome is log spending on a particular product category (rows of the figure) at a particular store type (columns of the figure) from the consumption module of ENIGH. The “total” column includes spending not only at corner stores and supermarkets but also at other types of stores such as open-air markets. Each regression has $N = 49,810$ households from 220 localities. Standard errors are clustered at the locality level.

Figure B.6: Effect of card shock on log 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 (8), where the outcome is log monthly wages of individual i in municipality m during quarter t , using Mexico's quarterly labor force employment survey. (a) $N = 83,222$ individual by quarter observations of individuals employed at corner stores (excluding store owners) in 250 treated municipalities; (b) $N = 96,380$ individual by quarter observations of individuals employed at supermarkets (excluding store owners) in 244 treated municipalities. Standard errors are clustered at the municipality level.

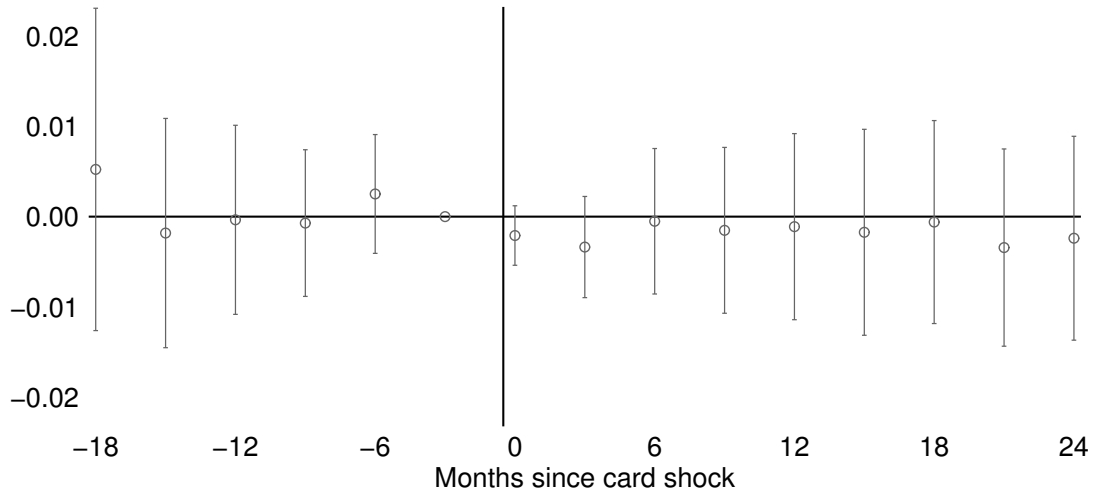
Figure B.7: Effect of card shock on probability of losing job (event study estimates)



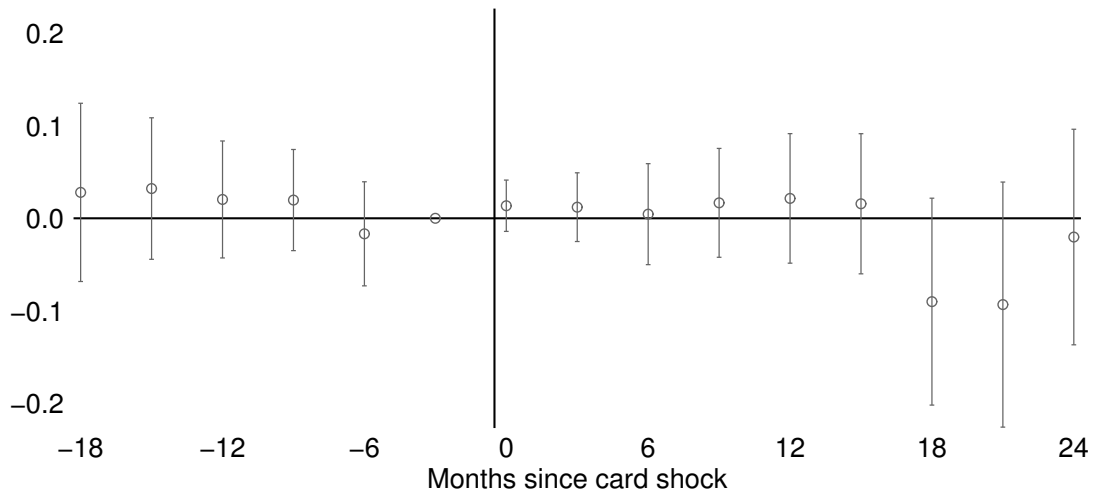
This figure shows that the rollout of debit cards did not have an effect on employment using Mexico’s quarterly labor force employment survey. It shows the coefficients from (8), where the outcome is instead a dummy variable equal to 1 if the following conditions are all satisfied: the individual (i) is unemployed when surveyed, (ii) previously was an employee (not owner) at that type of store, and (iii) reported that the job ended because the individual lost the job or was terminated. Individuals currently employed at that store type—as well as individuals formerly employed at that store type who didn’t lose their job—are included in the regression and coded with a 0 for the “lost job” variable. (a) $N = 95,539$ individual by quarter observations of individuals currently or previously employed at corner stores in 250 treated municipalities; (b) $N = 98,706$ individual by quarter observations of individuals currently or previously employed at supermarkets in 245 treated municipalities. Standard errors are clustered at the municipality level.

Figure B.8: Lack of bank response to card shock

(a) Log per-transaction merchant fee charged for POS terminal use

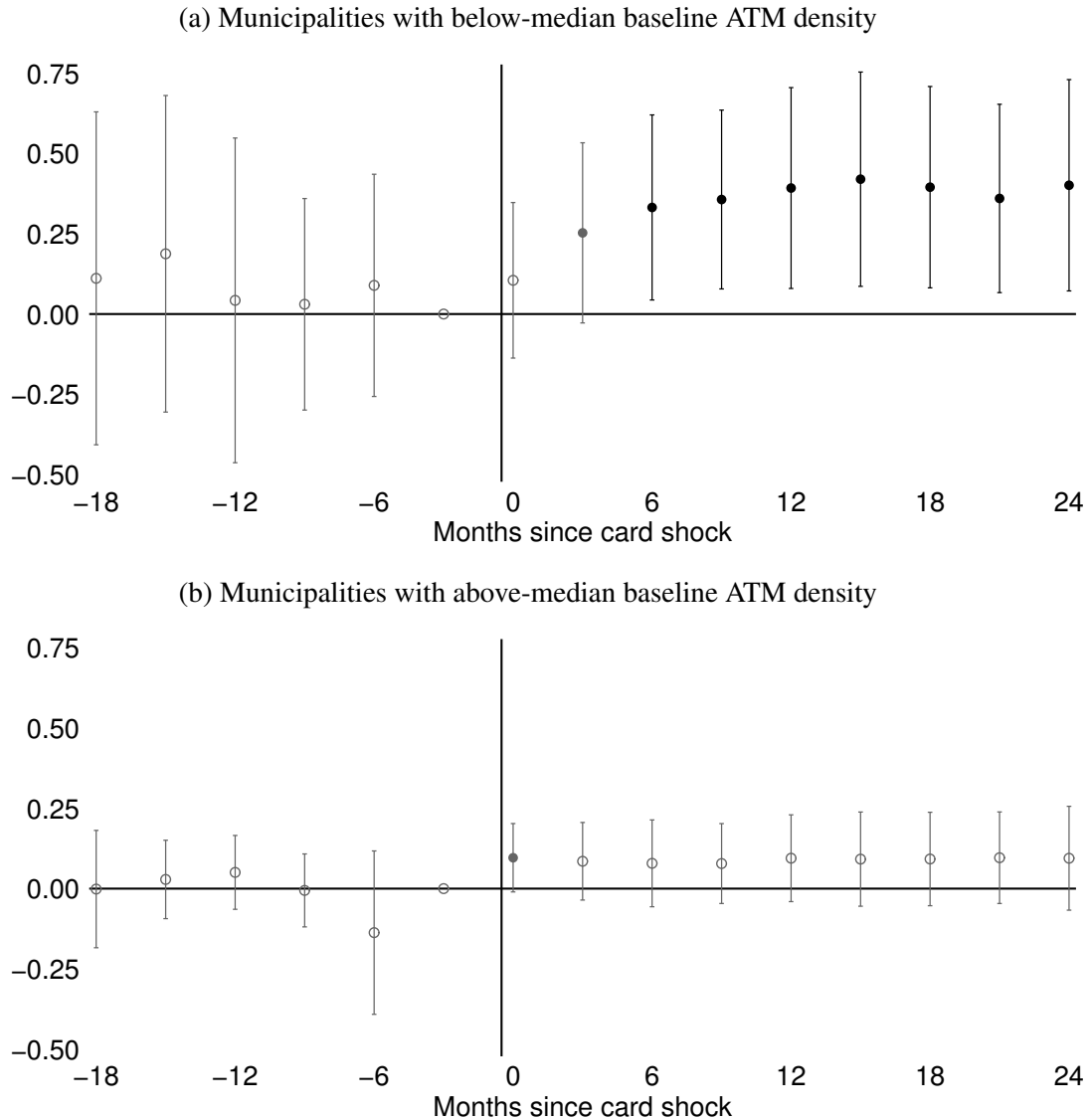


(b) Log number of commercial bank branches



This figure tests for a response by commercial banks to the debit card shock. Panel a shows that the per-transaction merchant fee charged by banks for retailers' use of POS terminals does not change in response to the card shock. It shows coefficients from (1) where the outcome variable is the log of the per-transaction fee (e.g. a 2.75% fee would be coded in the data as $\log(2.75)$), averaged over all banks with a presence within municipality m at time t , using data on these fees at each bank over time from Mexico's Central Bank. $N = 7,823$ municipality by quarter observations from 255 municipalities. Panel b shows that that the number of commercial bank branches does not change in response to the card shock. It shows coefficients from (1) where the outcome variable is the log number of commercial bank branches in the municipality, using quarterly data from CNBV. $N = 8,243$ municipality by quarter observations from 255 municipalities. Standard errors are clustered at the municipality level.

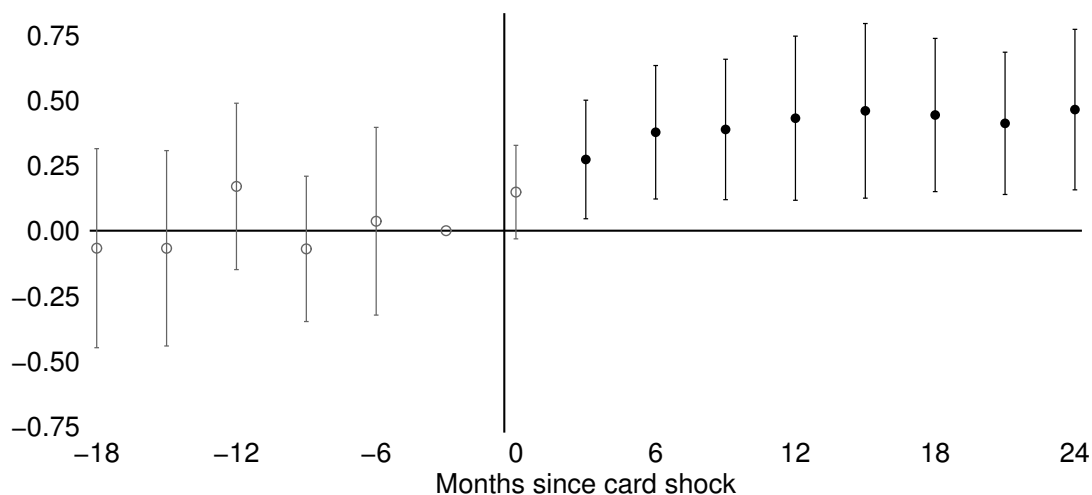
Figure B.9: Heterogeneous spillover effect on card adoption by ATM density



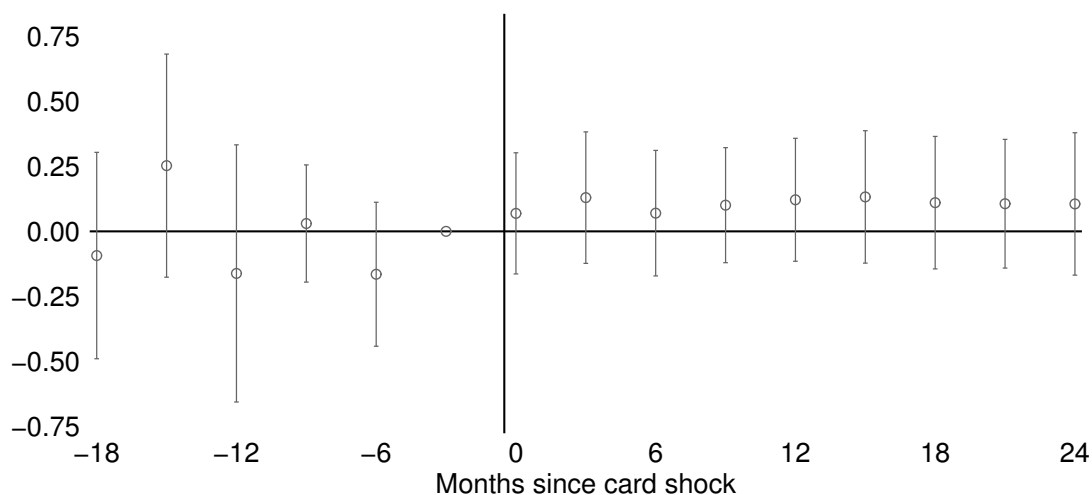
This figure shows that the spillovers on other consumers' card adoption appear to be concentrated in municipalities with low ATM density, suggesting that the effect on other consumers' card adoption is driven by network externalities rather than word-of-mouth learning. It graphs coefficients from (1), where the outcome variable is the log stock of non-Bansefi debit cards in municipality m in quarter t . Panel a restricts to municipalities with below-median baseline ATM density and panel b to above-median baseline ATM density, where baseline ATM density is measured using the last quarter of 2008 in the CNBV data, divided by population in INEGI data. (a) $N = 4,035$ municipality by quarter observations from 127 municipalities. (b) $N = 4,208$ municipality by quarter observations from 128 municipalities. Standard errors are clustered at the municipality level. The same results can be found in Appendix Table B.5.

Figure B.10: Heterogeneous spillover effect on card adoption by beneficiary shopping patterns

(a) Municipalities with below-median proportion of beneficiary transactions at supermarkets

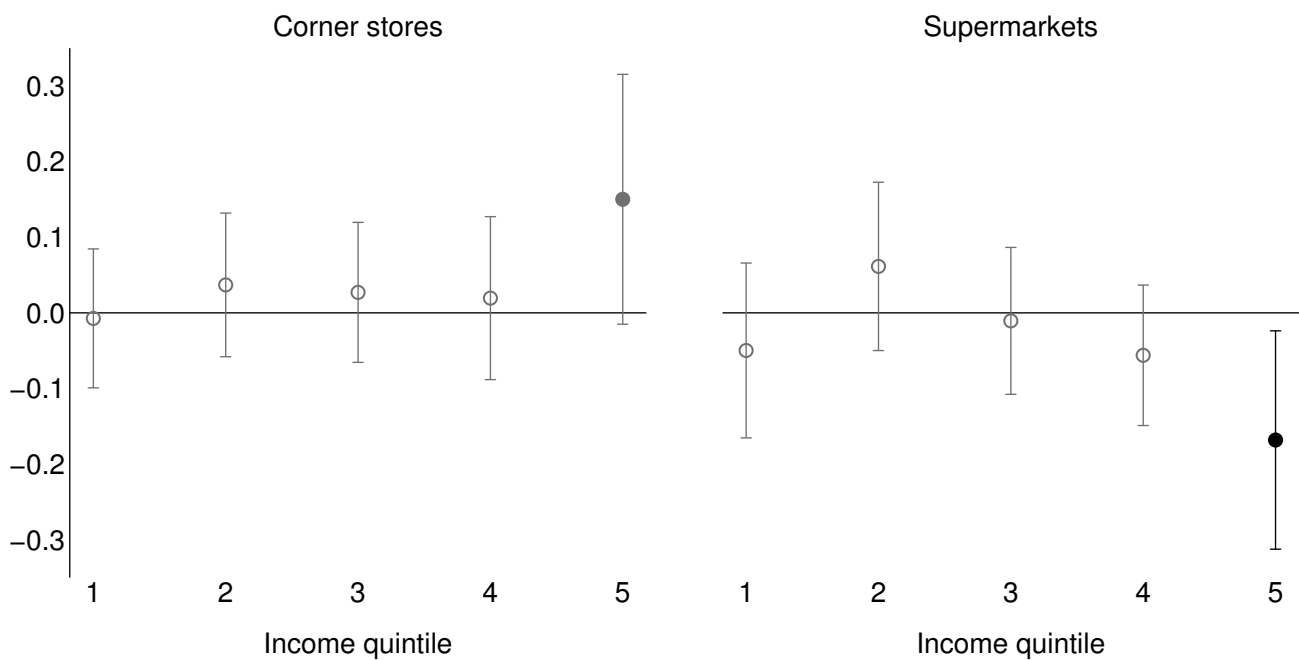


(b) Municipalities with above-median proportion of beneficiary transactions at supermarkets



This figure shows that the spillovers on other consumers' card adoption appear to be concentrated in municipalities 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 the outcome variable is the log stock of non-Bansefi debit cards in municipality m in quarter t . Panel a restricts to municipalities with below-median beneficiary card spending at supermarkets and panel b to above-median beneficiary card spending at supermarkets. The heterogeneity measure is constructed as the proportion of card transactions during their first 6 months with the card that Prospera beneficiaries make at supermarkets, using the Bansefi transactions data, while the outcome variable is from CNBV data. (a) $N = 3,833$ municipality by quarter observations from 119 municipalities. (b) $N = 3,852$ municipality by quarter observations from 118 municipalities. The sum of the number of municipalities in panels a and b is less than 255 because in 18 municipalities no Prospera beneficiaries use the card to make POS transactions during the first 6 months with the card, and hence the heterogeneity variable is missing for those municipalities. Standard errors are clustered at the municipality level. The same results can be found in Appendix Table B.5.

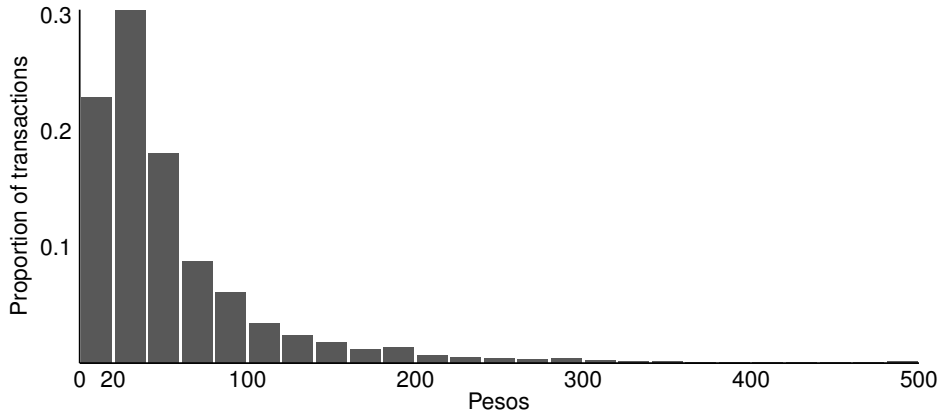
Figure B.11: Effect of corner store POS adoption on quantity consumed



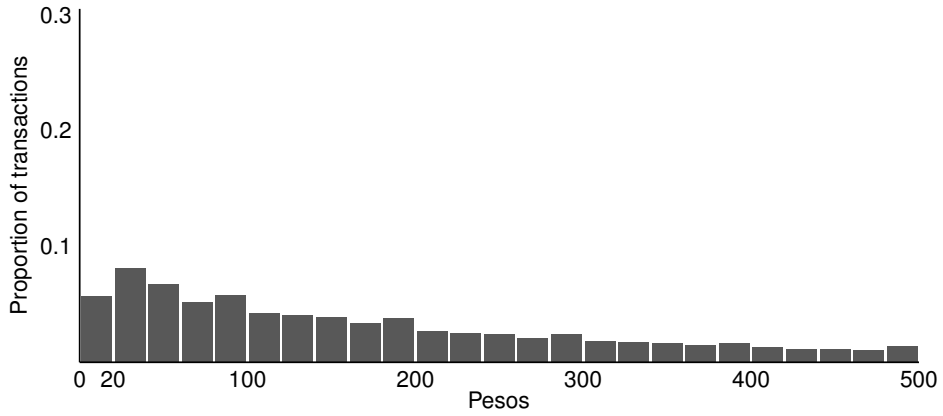
This figure shows that richer consumers substitute some of the quantity (measured in kilograms and liters) that they purchase from supermarkets to corner stores. This suggests that the results in Figure 7a are not explained by prices. The figure graphs coefficients from (5) where the outcome variable is $\log(\text{kilograms} + \text{liters purchased})$ at the particular store type (corner stores or supermarkets). $N = 49,810$ households from 220 localities. Standard errors are clustered at the locality level.

Figure B.12: Histograms of transaction amounts (transactions at POS terminals)

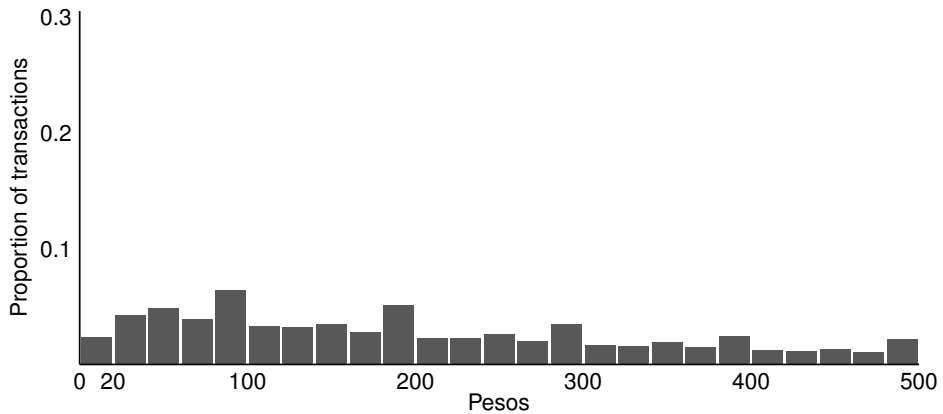
(a) Corner stores



(b) Supermarkets



(c) All other businesses



This figure shows that retailers likely do not impose minimum transaction amounts because a substantial proportion of transactions are made for very small amounts, especially at corner stores (20 pesos is less than \$2). It graphs the histogram of transaction amount sizes using the universe of card transactions at POS terminals. Transactions above 500 pesos are excluded from the histograms since they represent just 0.4% of transactions at corner stores (but 23% and 31% of transactions at supermarkets and all other businesses, respectively). $N = 4,718,690,034$ transactions.

Table B.1: Effect of card shock on log POS terminals (event study estimates)

Months since card shock	Corner stores	Supermarkets	All other businesses
-18 to -17	-0.025 (0.041)	-0.025 (0.034)	-0.001 (0.019)
-16 to -15	0.029 (0.040)	-0.019 (0.031)	0.008 (0.014)
-14 to -13	-0.011 (0.034)	-0.012 (0.027)	-0.002 (0.012)
-12 to -11	0.014 (0.028)	-0.029 (0.022)	0.019 (0.012)
-10 to -9	0.005 (0.026)	-0.052** (0.021)	-0.003 (0.009)
-8 to -7	-0.009 (0.026)	-0.016 (0.021)	0.002 (0.008)
-6 to -5	0.016 (0.026)	-0.024 (0.016)	-0.004 (0.007)
-4 to -3	0.000 (0.023)	-0.015 (0.018)	0.004 (0.005)
-2 to -1 (omitted)	0	0	0
0 to 1	0.033* (0.019)	-0.001 (0.018)	-0.003 (0.006)
2 to 3	0.061** (0.024)	-0.023 (0.017)	0.002 (0.007)
4 to 5	0.037** (0.019)	0.003 (0.020)	-0.002 (0.008)
6 to 7	0.060*** (0.022)	-0.011 (0.021)	-0.004 (0.010)
8 to 9	0.081*** (0.025)	0.011 (0.025)	-0.013 (0.011)
10 to 11	0.076*** (0.027)	-0.001 (0.025)	-0.001 (0.013)
12 to 13	0.085*** (0.032)	0.003 (0.029)	0.000 (0.016)
14 to 15	0.103*** (0.036)	-0.013 (0.032)	0.001 (0.017)
16 to 17	0.093** (0.037)	-0.008 (0.033)	-0.003 (0.017)
18 to 19	0.112*** (0.040)	-0.011 (0.038)	-0.008 (0.018)
20 to 21	0.122*** (0.043)	0.006 (0.039)	-0.005 (0.019)
22 to 23	0.135*** (0.047)	-0.003 (0.042)	-0.014 (0.021)
24 to 25	0.169*** (0.060)	-0.002 (0.052)	-0.011 (0.026)
<i>N</i> (locality × 2-month period)	8,806	8,806	8,806
Number of localities	259	259	259
Locality fixed effects	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes

This table shows the point estimates and standard errors from Figure 5. It shows the coefficients from (1), where the dependent variable is the log number of point of sale terminals by type of merchant (corner store, supermarket, or other). Observations are at the locality by two-month period level. Standard errors are clustered at the locality level.

Table B.2: Changes in log spending by category and store type: food

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Grains/ tortillas	Meats	Dairy/ eggs	Oils/ fats	Produce	Sugar/ coffee/tea/ spices	Prepared foods	Soda	Alcohol/ tobacco
<i>Panel A: Corner stores</i>									
Quintile 1	-0.051 (0.096)	0.145 (0.175)	0.292* (0.154)	-0.015 (0.101)	0.307** (0.137)	0.329** (0.145)	-0.031 (0.149)	-0.060 (0.141)	0.089 (0.057)
Quintile 2	0.033 (0.099)	0.080 (0.167)	0.068 (0.130)	0.159** (0.078)	0.052 (0.120)	0.055 (0.115)	-0.243* (0.145)	0.180 (0.136)	0.070 (0.071)
Quintile 3	-0.025 (0.109)	0.003 (0.160)	0.140 (0.132)	0.097 (0.073)	0.221* (0.133)	0.135 (0.124)	-0.056 (0.129)	0.191 (0.129)	0.128 (0.079)
Quintile 4	0.144 (0.103)	0.167 (0.145)	0.162 (0.124)	0.013 (0.074)	0.130 (0.145)	0.019 (0.102)	-0.060 (0.150)	0.234* (0.131)	-0.053 (0.079)
Quintile 5	0.483*** (0.154)	0.493* (0.258)	0.399** (0.163)	0.072 (0.061)	0.321** (0.156)	0.243** (0.096)	0.173 (0.139)	0.514*** (0.194)	-0.011 (0.083)
Baseline mean	5.772	4.289	4.765	0.740	3.660	1.683	2.501	4.332	0.580
Number of observations	49,810	49,810	49,810	49,810	49,810	49,810	49,810	49,810	49,810
Number of localities	220	220	220	220	220	220	220	220	220
<i>Panel B: Supermarkets</i>									
Quintile 1	-0.024 (0.142)	-0.013 (0.122)	-0.092 (0.121)	0.018 (0.079)	-0.069 (0.138)	0.011 (0.099)	0.004 (0.069)	-0.096 (0.089)	0.039 (0.036)
Quintile 2	0.210 (0.151)	0.151 (0.128)	0.161 (0.132)	0.121* (0.068)	0.086 (0.143)	0.250** (0.120)	-0.024 (0.073)	0.009 (0.093)	-0.007 (0.038)
Quintile 3	-0.034 (0.125)	0.121 (0.123)	-0.070 (0.121)	0.076 (0.073)	-0.004 (0.111)	0.207* (0.113)	-0.022 (0.071)	-0.004 (0.087)	0.095* (0.054)
Quintile 4	-0.030 (0.113)	0.057 (0.141)	-0.167 (0.108)	-0.087 (0.071)	-0.049 (0.124)	-0.048 (0.092)	0.013 (0.088)	-0.125 (0.095)	-0.092* (0.054)
Quintile 5	-0.283* (0.165)	-0.184 (0.205)	-0.315* (0.185)	-0.069 (0.088)	-0.138 (0.167)	-0.144 (0.117)	-0.311*** (0.108)	-0.343*** (0.145)	-0.019 (0.090)
Baseline mean	2.065	2.122	2.042	0.542	1.895	0.956	0.634	1.311	0.242
Number of observations	49,810	49,810	49,810	49,810	49,810	49,810	49,810	49,810	49,810
Number of localities	220	220	220	220	220	220	220	220	220
<i>Panel C: Total</i>									
Quintile 1	0.080 (0.054)	0.066 (0.115)	0.181* (0.102)	-0.018 (0.108)	0.106 (0.091)	0.296** (0.131)	-0.149 (0.170)	-0.083 (0.135)	0.122* (0.071)
Quintile 2	0.054 (0.043)	0.200 (0.126)	0.108 (0.080)	0.341*** (0.096)	0.122 (0.086)	0.263** (0.127)	-0.388** (0.169)	0.195* (0.104)	0.067 (0.084)
Quintile 3	0.070 (0.071)	0.084 (0.125)	0.085 (0.098)	0.155 (0.105)	0.128 (0.118)	0.364** (0.158)	-0.105 (0.145)	0.235** (0.109)	0.194** (0.097)
Quintile 4	0.114* (0.064)	0.270** (0.126)	0.098 (0.102)	-0.072 (0.099)	0.235** (0.105)	0.003 (0.128)	-0.173 (0.154)	0.144 (0.111)	-0.094 (0.090)
Quintile 5	0.158 (0.108)	0.222* (0.134)	0.064 (0.124)	0.082 (0.118)	0.131 (0.124)	0.047 (0.130)	-0.034 (0.162)	0.209 (0.132)	0.012 (0.112)
Baseline mean	6.601	6.190	6.017	1.423	5.909	2.695	3.499	5.217	0.777
Number of observations	49,810	49,810	49,810	49,810	49,810	49,810	49,810	49,810	49,810
Number of localities	220	220	220	220	220	220	220	220	220
Locality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quintile × time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table shows a breakdown by product category of the shifts in consumption by income quintile. It includes *food* product categories; non-food product categories are in Appendix Table B.3. Each column and panel shows coefficients from a separate regression using specification (5), where the outcome is log spending on a particular product category from the consumption module of ENIGH. The “total” in panel C includes spending not only at corner stores and supermarkets but also at other types of stores such as open-air markets.

Table B.3: Changes in log spending by category and store type: non-food

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Cleaning/ household supplies	Personal hygiene	Books/ newspapers/ magazines	Cooking/ heating fuel	Clothing	Other non-durables	Medicine	Durables
<i>Panel A: Corner stores</i>								
Quintile 1	-0.093 (0.141)	-0.167 (0.164)	0.062 (0.054)	-0.148 (0.268)	0.368* (0.190)	-0.076 (0.273)	-0.090 (0.138)	0.033 (0.111)
Quintile 2	-0.096 (0.151)	-0.162 (0.168)	0.055 (0.051)	0.073 (0.225)	0.157 (0.166)	0.110 (0.227)	-0.024 (0.151)	-0.023 (0.108)
Quintile 3	-0.153 (0.152)	0.060 (0.154)	0.054 (0.062)	0.146 (0.241)	0.083 (0.175)	0.280 (0.232)	0.195 (0.149)	0.085 (0.147)
Quintile 4	-0.114 (0.134)	-0.139 (0.156)	0.044 (0.063)	0.262 (0.257)	0.184 (0.149)	0.298 (0.228)	-0.036 (0.169)	-0.003 (0.130)
Quintile 5	0.233 (0.181)	0.074 (0.141)	-0.016 (0.101)	0.178 (0.225)	0.190 (0.181)	0.186 (0.192)	0.040 (0.157)	0.036 (0.148)
Baseline mean	2.753	4.045	0.519	1.775	3.846	3.670	2.582	1.622
Number of observations	49,810	49,810	49,810	49,810	49,810	49,810	49,810	49,810
Number of localities	220	220	220	220	220	220	220	220
<i>Panel B: Supermarkets</i>								
Quintile 1	0.128 (0.168)	0.240 (0.150)	0.012 (0.021)	-0.002 (0.021)	0.089 (0.106)	-0.023 (0.059)	0.038 (0.023)	-0.066 (0.083)
Quintile 2	0.214* (0.130)	0.139 (0.146)	-0.016 (0.027)	0.028 (0.025)	0.078 (0.131)	0.056 (0.078)	0.018 (0.034)	-0.084 (0.088)
Quintile 3	0.309* (0.160)	0.293* (0.151)	-0.012 (0.030)	0.040 (0.031)	0.031 (0.112)	0.119 (0.073)	-0.013 (0.032)	0.060 (0.106)
Quintile 4	0.217** (0.109)	0.130 (0.108)	-0.047 (0.040)	-0.040 (0.035)	0.390** (0.167)	0.069 (0.083)	-0.033 (0.033)	-0.148 (0.114)
Quintile 5	-0.109 (0.144)	-0.031 (0.141)	-0.150** (0.058)	-0.017 (0.042)	0.059 (0.204)	-0.105 (0.120)	-0.207*** (0.071)	-0.241* (0.133)
Baseline mean	4.623	5.311	0.188	0.116	2.406	0.854	0.151	1.173
Number of observations	49,810	49,810	49,810	49,810	49,810	49,810	49,810	49,810
Number of localities	220	220	220	220	220	220	220	220
<i>Panel C: Total</i>								
Quintile 1	0.047 (0.054)	0.033 (0.057)	-0.267 (0.203)	-0.281 (0.287)	0.096 (0.172)	-0.145 (0.209)	-0.109 (0.179)	-0.169 (0.143)
Quintile 2	0.014 (0.051)	-0.029 (0.050)	-0.298 (0.207)	-0.128 (0.218)	0.036 (0.129)	-0.057 (0.123)	-0.006 (0.162)	-0.182 (0.150)
Quintile 3	0.070 (0.052)	0.028 (0.059)	0.002 (0.194)	-0.080 (0.226)	-0.078 (0.154)	-0.014 (0.136)	0.227 (0.174)	-0.016 (0.172)
Quintile 4	0.074 (0.053)	0.035 (0.055)	-0.012 (0.182)	-0.123 (0.251)	0.215 (0.134)	0.020 (0.121)	-0.040 (0.192)	-0.255 (0.190)
Quintile 5	0.041 (0.083)	0.070 (0.057)	0.047 (0.175)	-0.338 (0.285)	0.167 (0.133)	0.062 (0.120)	-0.198 (0.194)	-0.242 (0.192)
Baseline mean	6.427	6.822	4.143	5.915	5.728	7.718	3.372	2.826
Number of observations	49,810	49,810	49,810	49,810	49,810	49,810	49,810	49,810
Number of localities	220	220	220	220	220	220	220	220
Locality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quintile × time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table shows a breakdown by product category of the shifts in consumption by income quintile. It includes *non-food* product categories; food product categories are in Appendix Table B.2. Each column and panel shows coefficients from a separate regression using specification (5), where the outcome is log spending on a particular product category from the consumption module of ENIGH. The “total” in panel C includes spending not only at corner stores and supermarkets but also at other types of stores such as open-air markets.

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

	(1)	(2)
< 1 year after shock	0.149** (0.060)	0.193** (0.076)
≥ 1 year after shock	0.170* (0.089)	0.212* (0.114)
< 1 year after shock \times ≥ 1 year delay in POS response		-0.180* (0.106)
≥ 1 year after shock \times ≥ 1 year delay in POS response		-0.167 (0.150)
<i>N</i> (municipality \times 6-month period)	3,290	3,290
Number of municipalities	255	255
Municipality fixed effects	Yes	Yes
Time fixed effects	Yes	Yes
Time \times ≥ 1 year delay in POS response fixed effects		Yes

This table shows heterogeneity in the spillovers on card adoption based on whether there was a delay in corner stores' POS response to the shock. It combines the CNBV and Central Bank data and shows results from a regression of the log stock of non-Bansefi debit cards in municipality m during 6-month period t on a set of dummies for broad categories of time: periods preceding the Prospera card shock (omitted dummy), less than one year after the card shock, and greater than one year after the card shock (column 1) and these variables interacted with a dummy for whether there was a corner store POS adoption response within the first year after the card shock using the Central Bank data (column 2). Time periods are aggregated to the 6-month level since the table relies on quarterly data on the stock of non-Bansefi debit cards and the data on POS adoption which were aggregated to the 2-month level. Standard errors are clustered at the municipality level.

Table B.5: Changes in consumption and number of trips by quintile

	(1)		(2)		(3)		(4)		(5)		(6)	
	Log spending		Log quantity		Log quantity		Log quantity		Number of trips		Number of trips	
	Corner store	Supermarket	Corner store	Supermarket	Corner store	Supermarket	Corner store	Supermarket	Corner store	Supermarket	Corner store	Supermarket
Quintile 1	0.034	0.045	-0.007	-0.050	-0.044	-0.053	(0.036)	(0.072)	(0.047)	(0.059)	(0.190)	(0.071)
Quintile 2	0.033	0.044	0.037	0.061	0.076	-0.003	(0.036)	(0.065)	(0.048)	(0.056)	(0.191)	(0.077)
Quintile 3	0.050	0.004	0.027	-0.011	0.031	-0.004	(0.041)	(0.064)	(0.047)	(0.049)	(0.189)	(0.067)
Quintile 4	0.080*	0.031	0.019	-0.056	0.249	-0.134**	(0.043)	(0.056)	(0.055)	(0.047)	(0.181)	(0.067)
Quintile 5	0.138**	-0.135**	0.150*	-0.168**	0.791***	-0.203*	(0.059)	(0.068)	(0.084)	(0.073)	(0.231)	(0.109)
P-values comparing quintiles												
1 vs. 5	[0.060]*	[0.090]*	[0.084]*	[0.234]	[0.008]***	[0.246]	[0.070]*	[0.081]*	[0.223]	[0.017]**	[0.034]**	[0.117]
2 vs. 5	[0.135]	[0.140]	[0.179]	[0.084]*	[0.018]**	[0.118]	[0.337]	[0.018]**	[0.161]	[0.014]**	[0.589]	
3 vs. 5												
4 vs. 5												
Baseline mean	8.626	7.786	2.533	0.870	7.432	0.886						
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						
Quintile × time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes						

This table shows the point estimates and standard errors from Figure 7 and Appendix Figure B.11. Each column is from (5) where the outcome variable is log spending in pesos at the particular store type (corner stores or supermarkets) in columns 1–2, log(kilograms + liters purchased) at the particular store type (restricted to goods with quantities purchased recorded in the consumption data) in columns 3–4, and number of trips over the course of one week to the particular store type in columns 5–6. Standard errors are clustered at the locality level.

Appendix C Demand Estimation

This appendix provides more detail on the derivation of (11) and (13) to estimate the consumer gains from the policy shock.

Log share equation. Starting from (10),

$$\log u_{ist} = \underbrace{\alpha_{k(i)} \log y_{it} - \alpha_{k(i)} \sum_g \phi_{a(i)gst} \log p_{a(i)gst} + \theta_{k(i)} POS_{ist} + \tilde{\xi}_{a(i)k(i)st}}_{\equiv v_{ist}} + \varepsilon_{ist}, \quad (16)$$

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

$$\begin{aligned} \pi_{ist} &= \int_{\varepsilon} \int_{POS} \mathbb{I}(\varepsilon_{ikt} < \varepsilon_{ist} + v_{ist} - v_{ikt}) f(POS) f(\varepsilon) dPOS d\varepsilon \\ &= \int_{\varepsilon} \mathbb{I}(\varepsilon_{ikt} < \varepsilon_{ist} + \gamma_{a(i)k(i)st} - \gamma_{a(i)k(i)rt}) f(\varepsilon) d\varepsilon, \end{aligned} \quad (17)$$

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 $\overline{POS}_{z(a(i))st}$ 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.⁴²

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

$$\pi_{ist} = \frac{\exp(\gamma_{a(i)k(i)st})}{\sum_r \exp(\gamma_{a(i)k(i)rt})} \quad (18)$$

(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 $i \in (a, k)$.

⁴²I 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 $x_{igst} = \phi_{a(i)gst} y_{it} / p_{a(i)gst}$ and recalling $\sum_g \phi_{a(i)gst} = 1$, or by noting that the first order condition for the budget constraint gives $y_{it} = \sum_g p_{agst} x_{igst}$.

Substituting ϕ_{akst} into (18) and taking logs gives (11).

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,

$$CV \approx - \sum_s x_s^1 (P_s^1 - P_s^0) \approx - \sum_s P_s^1 x_s^1 \left(\frac{P_s^1 - P_s^0}{P_s^1} \right). \quad (19)$$

To obtain the proportional change in consumer surplus, divide both sides by expenditures after the change, $e(P^1, U^0)$, which gives (13).