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Financial Technology Adoption: Network Externalities of Cashless Payments in Mexico†

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Do coordination failures constrain financial technology adoption? Exploiting the Mexican government's rollout of 1 million debit cards to poor households from 2009 to 2012, I examine responses on both sides of the market and find important spillovers and distributional impacts. On the supply side, small retail firms adopted point-of-sale terminals to accept card payments. On the demand side, this led to a 21 percent increase in other consumers' card adoption. The supply-side technology adoption response had positive effects on both richer consumers and small retail firms: richer consumers shifted 13 percent of their supermarket consumption to small retailers, whose sales and profits increased. (*JEL* E42, L25, L81, O14, O33)

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 consumers and retail firms (Jack and Suri 2014; Agarwal et al. 2020). Because payment technologies

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feature two-sided markets, however, coordination failures can constrain adoption. 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] These indirect network externalities can lead to multiple adoption equilibria, where moving to the Pareto-dominating equilibrium requires coordination (Katz and Shapiro 1986; Gowrisankaran and Stavins 2004).

The magnitude of these externalities and resulting spillovers of financial technology adoption within and across the two sides of the market have been difficult to study for three main reasons. First, technology adoption is typically endogenous. Second, because supply-side adoption of the corresponding technology could require consumer adoption to reach a certain threshold before retailer adoption is optimal, any exogenous shock to consumer adoption would need to be large and coordinated within the local market. Third, quantifying indirect network externalities within one side of the market requires a shock that directly affects only a subset of consumers (or firms), ruling out large-scale adoption subsidies that affect an entire side of the market.

I exploit large localized shocks to consumers' adoption of a particular payment technology (debit cards) to trace out the supply- and demand-side spillovers of coordinated technology adoption in a two-sided market. Between 2009 and 2012, the Mexican government disbursed about 1 million debit cards as the new payment method for its large-scale conditional cash transfer program, Prospera. I find that small retailers responded to these large local shocks to consumer debit card adoption by adopting point-of-sale (POS) terminals to accept card payments, while large retailers such as supermarkets already had near-universal adoption of POS terminals. I then examine how this supply-side response fed back to the demand side, finding that it led to an increase in other consumers' debit card adoption and a partial shift in richer households' consumption from large to small retailers now that they could use debit cards at more small retailers. Consistent with this shift in consumption, I find that small retailers' sales and profits increased, while large retailers' sales decreased.

The government's rollout of debit cards to cash transfer recipients has a number of notable features that make it ideal for tracing out the supply- and demand-side responses to a shock to financial technology adoption. First, the shock was large within the local market: in the median treated locality, it directly increased the proportion of households with a debit card by 18 percentage points (48 percent) in one week.[2] Second, the shock only reduced the cost of debit card adoption for a subset of consumers (specifically, beneficiaries of Mexico's cash transfer program), which

[1] Katz and Shapiro (1985) distinguish *indirect* network externalities—which arise in two-sided markets—from *direct* network externalities. A direct network externality arises from a product such as the telephone, where users benefit directly from other consumers' adoption of the technology. An indirect network externality arises from two-sided markets: a debit card user does not benefit directly from other consumers' adoption of debit cards but rather through the effect of other consumers' adoption of cards on the probability that retailers adopt technology to accept card payments.

[2] In the median treated locality, 36 percent 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 percent. Cash transfer recipients were not forced to use the card: after receiving the debit card, they could still travel to a Bansefi bank branch and withdraw cash with a bank teller, as they did prior to receiving the debit card.

allows me to isolate spillover effects on other consumers whose cost of adoption did not change. Third, 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 pretreatment trends in financial infrastructure and other locality characteristics.

An additional challenge to studying the network externalities and spillovers of financial technology adoption is that in most empirical settings there is a lack of high-quality data on firms' technology adoption and on 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 seven additional datasets on both consumers and retailers. The key dataset on supply-side financial technology adoption is a confidential dataset on the universe of POS terminal adoptions by retailers over a 12-year period, accessed on-site at Mexico's Central Bank. For spillovers on other consumers, the two key datasets that I use are quarterly data on the number of debit cards at the bank-by-municipality level and a nationally representative consumption survey that can be used to identify unique trips to different types of stores. I complement these with four additional confidential datasets: transaction-level data on the universe of debit and credit card transactions at POS terminals over eight years; 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-bar-code level from a sample of stores.

Small retail firms responded to the shock to consumer debit card 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 store owners with POS terminals increased by 3 percent during the two-month period in which the shock occurred.[3] Adoption continued to increase over time: two years after the shock, 18 percent more corner stores had adopted POS terminals in treated localities (relative to localities that had yet to be treated). There is no effect among supermarkets, which already had high levels of POS adoption prior to the shock.

The shock to consumer card adoption and subsequent adoption of POS terminals by small retailers had 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 responded to the increase in financial technology adoption by increasing their adoption of debit cards. Specifically, nearly six months after the shock occurred, the number of cards held by other consumers increased by 19 percent. Two years after the shock, 28 percent more consumers had adopted cards. Heterogeneity tests show that there was no statistically significant difference in the spillover on other consumers' debit card adoption based on the areas' social connectedness, whereas the effect was larger in areas with below-median ATM density and areas where beneficiaries were less likely to shop at supermarkets. Taken together, these heterogeneity tests provide evidence that the spillover on debit card adoption was likely driven at least partly by indirect network externalities, rather than only through word-of-mouth

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

learning about the advantages of cards. Combining the large direct shock to debit card adoption and its spillover effect on other consumers' adoption, debit card adoption in the median treated locality increased from 36 percent to 63 percent in just one year; for comparison, in the absence of a large, coordinated shock, it took China and the United States each about six years to achieve similar increases in debit card adoption.[4]

The adoption of POS terminals by small retailers also affected where consumers shopped. The richest quintile of all consumers, who were substantially more likely to have cards before the shock, substituted about 13 percent of their total supermarket consumption to corner stores after the increased POS adoption by 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 made, 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 occurred 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 et al. 2021).[5]

To estimate the effects of POS terminal adoption on small retailers, I use data on the revenues and costs of the universe of retailers in Mexico. Over the 5-year period between survey rounds, corner store sales increased by 6 percent more in earlier-treated localities. Corner stores increased the amount of inventory they bought and sold without increasing other input costs such as wages, number of employees, rent, capital, or utilities, which led to an increase in their profits. This does not represent an aggregate gain for retailers, however, as increased corner store sales were accompanied by decreased supermarket sales that are very similar in aggregate magnitude. The shift in sales from supermarkets to corner stores has distributional implications, as corner stores are substantially smaller than supermarkets and corner store owners are lower in the income distribution than supermarket owners.

Finally, to explore whether coordination failures constrain financial technology adoption, I conducted a survey of corner store owners in urban localities that were not included in the debit card rollout but that currently have similar levels of debit card and POS adoption as the localities included in the rollout had just before the shock. I use the survey to compare corner store owners' expectations about the effect of POS adoption on profits to the treatment effect of the debit card shock on corner store profits. Only 11–16 percent of corner store owners predict a larger change in profits than the average treatment effect of the shock. This is evidence of a coordination failure: in the absence of a shock to debit card adoption, the vast

[4] In China, debit card adoption increased from 41 percent in 2011 to 67 percent in 2017 (Demirgüç-Kunt et al. 2018). In the United States, adoption increased from 34 percent in 1998 to 59 percent in 2004 (Mester 2009). I assume that debit cards adopted from other banks were adopted by other consumers rather than by Prospera beneficiaries and their household members; this assumption is supported by survey data, which show that Prospera households did not adopt cards from other banks (Section IVB).

[5] In that paper, we find that the cards did not affect beneficiaries' income but that beneficiaries did 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 dataset in this paper, I also find evidence of a reduction in overall consumption by Prospera beneficiaries as a result of receiving a debit card.

majority of corner store owners estimate a lower change in profits than the treatment effect of the shock. This coordination failure could arise due to a combination of a classical coordination failure—where the benefits of adopting a POS terminal are only sufficiently large after a high enough fraction of consumers have adopted POS terminals—and due to biased expectations about the benefits of adopting a POS terminal. The survey provides suggestive evidence that corner store owners do underestimate how many new customers would come to the store if they adopted, which would exacerbate the coordination failure by making fewer corner stores adopt than is optimal in the absence of a shock.

This paper makes three main contributions. First, by combining large local shocks and several rich sources of confidential microdata on consumers and retailers, I am able to trace out how shocks to consumers' financial technology adoption filtered through markets to affect retail adoption of financial technology, as well as how this supply-side response spilled over onto other consumers' technology adoption and consumption across stores. Most research on the effects of financial technologies, on the other hand, has focused on direct effects for households who adopt (e.g., Dupas and Robinson 2013; Schaner 2017; Callen et al. 2019; Breza, Kanz, and Klapper 2020) or on information spillovers across households (Banerjee et al. 2013). Two closely related papers study the network effects of technology adoption. Jack and Suri (2014) find that mobile money increased households' ability to share risk by reducing the transaction costs of transferring money. Björkegren (2019) uses rich data from mobile phone call records to quantify the network effects of mobile phone adoption in Rwanda. Both of these papers focus on *direct* network externalities across households, whereas I study *indirect* network externalities and coordination failures arising from a two-sided market.[6]

Second, I provide empirical evidence that coordination failures constrain adoption of a technology with indirect network externalities. In particular, many small retail firms did not find it optimal to adopt POS terminals until there was a coordinated shock to demand-side adoption of debit cards. Surveys reveal that in the absence of this shock to debit card adoption, the vast majority of corner store owners predict low changes to profits if they adopt a POS terminal. The literature on constraints to firm technology adoption has focused on several other barriers, including information constraints (Bloom et al. 2013; Giorcelli 2019), credit constraints (Banerjee and Duflo 2014; Bruhn, Karlan, and Schoar 2018), lack of trust (Gertler et al. 2022), and misaligned incentives within the firm (Atkin et al. 2017; DellaVigna and Gentzkow 2019). I further find suggestive evidence that the coordination failures that constrain firms' financial technology adoption are exacerbated by firms underestimating a particular benefit of POS adoption: attracting new customers who prefer to pay by card.

Third, I quantify the distributional impacts for both households and retail firms of a large increase in poor households' financial technology adoption: small retailers and richer consumers benefited substantially from the shock, as richer consumers responded to small retailers' adoption of POS terminals by shifting part of their

[6] A set of papers on India's demonetization also study both sides of financial technology markets (e.g., Agarwal et al. 2018; Crouzet, Gupta, and Mezzanotti 2023). Because demonetization had large direct impacts on both sides of the market and also directly impacted employment, output, and bank credit (Chodorow-Reich et al. 2020), studies exploiting this shock do not isolate spillovers across the two sides of the market.

supermarket consumption to corner stores. This relates to a growing literature on the distributional impacts of various shocks on retail firms throughout the firm size distribution, and on the households who shop at these retailers (Atkin, Faber, and Gonzalez-Navarro 2018; Faber and Fally 2022). Furthermore, this finding speaks to the political economy of government policy to subsidize financial inclusion. Specifically, such spending may be politically popular given that it not only benefits poor households by reducing their transaction costs of saving (Bachas et al. 2021) and enabling them to shop with a debit card but also through its effects on retailers' financial technology adoption and the resulting benefits for richer households.

I. 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 percent, compared to 50 percent 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 1, panel A. In urban areas, 12 percent of households in the bottom income quintile had a debit or credit card prior to the Prospera debit card rollout, compared to 54 percent of households in the top income quintile. On the supply side of the market, 32 percent of retailers in urban areas had adopted POS terminals prior to the rollout, including 26 percent of corner stores and nearly 100 percent of supermarkets.

Figure 1, panel B 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.[7] 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 is correlated: the municipalities that had large increases in debit card adoption also had large increases in POS terminal adoption.

A. *Shock to Debit Card Adoption*

Between 2009 and 2012, the Mexican government rolled out debit cards to existing beneficiaries of its conditional cash transfer program Prospera in urban localities, defined as localities with at least 15,000 inhabitants. Prior to the debit card rollout, these beneficiaries already received cash benefits deposited directly into formal savings accounts without debit cards. To access their cash transfers prior to receiving a card, they would travel to a Bansefi branch and withdraw cash with a bank teller. 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 cash from

[7] I use the number of debit cards per person rather than the number of individuals with debit cards because the latter is not available (except in household surveys, which do not include the universe of households or municipalities).

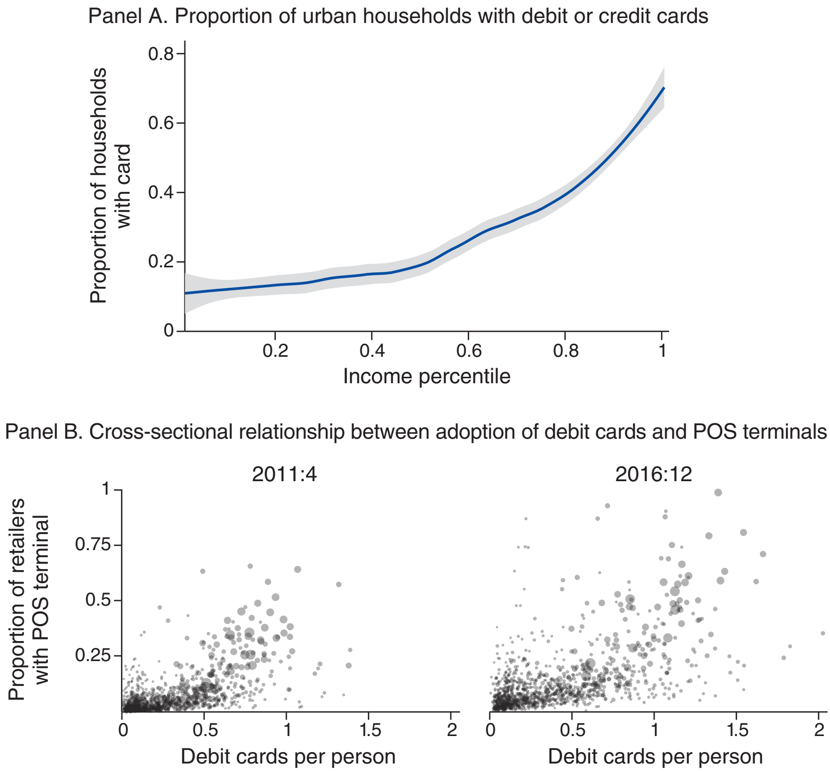

FIGURE 1. FINANCIAL TECHNOLOGY ADOPTION IN MEXICO

Notes: 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. *Observations* = 4,234 households. Panel B shows the proportion of retailers accepting cards (constructed as the number of businesses with POS terminals using National Banking and Securities Commission (CNBV) data divided by the number of retailers using Mexico's National Statistical Institute (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. *Observations* = 2,458 municipalities. For legibility, the top 1 percent of observations on each axis are excluded.

any bank's ATM and to make purchases at POS terminals at any merchant accepting Visa.[8] Beneficiaries were not required to use the card (either at ATMs or POS terminals), however; they could still travel to a Bansefi bank branch and withdraw cash with a bank teller, as they did prior to receiving the debit card.

Prior to this policy change in Mexico, several other countries had already shifted to using cashless payments for their social programs, including the United States in the 1990s (Wright et al. 2017) and Argentina, Brazil, Colombia, the Dominican Republic, Jamaica, and Pakistan in the 2000s (Pickens, Porteous, and Rotman 2009). In some of these countries, however, the cards could only be used to access cash at

[8] The cards could also be used to make online purchases, but online purchases were rare during this time period in Mexico, accounting for less than 0.1 percent of all retail consumption.

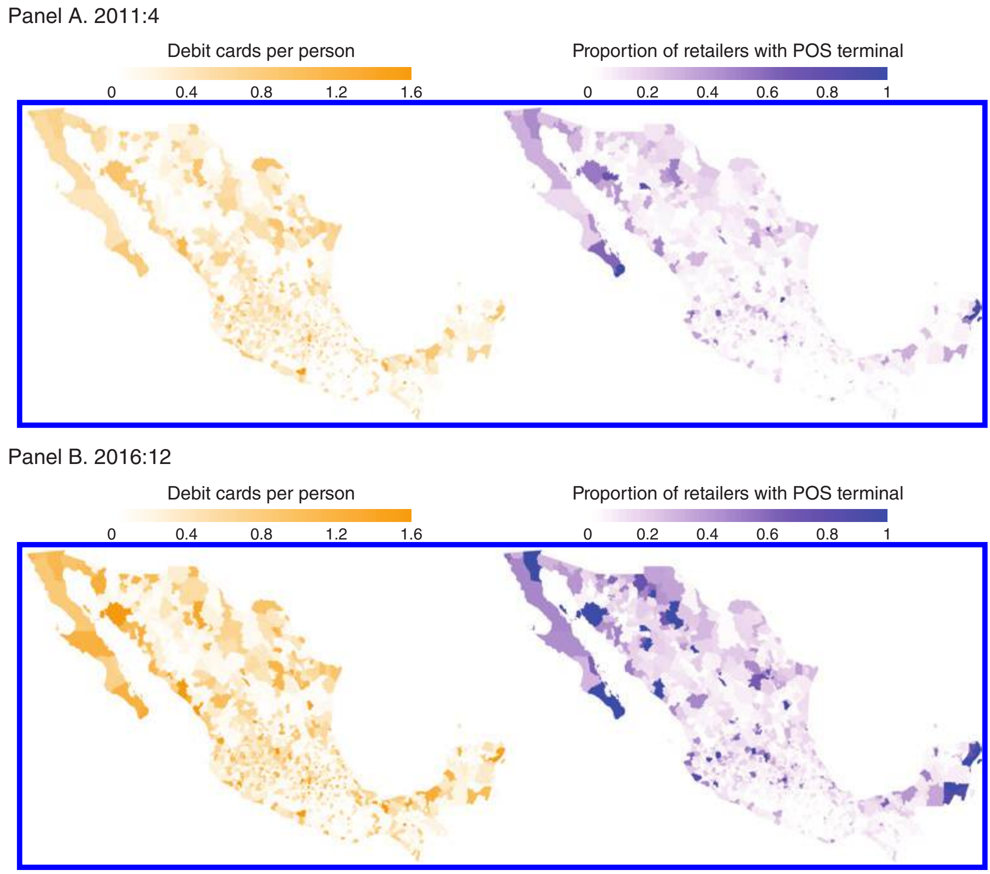

Figure 2. Concentration of Cards and POS Terminals over Space and Time

Notes: 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. *Observations* = 2,458 municipalities.

banking agents or ATMs but not to make purchases at POS terminals; in others, they could only be used to make POS transactions for a limited set of goods at approved retailers. Other countries such as Chile and India have more recently distributed debit cards tied to bank accounts at a large scale. In India, where 190 million debit cards were distributed by the government between 2014 and 2016, households in areas that were more exposed to the debit card rollout experienced a greater increase in access to formal credit (Agarwal et al. 2017).

Mexico's Prospera program—formerly known as Progresa and during the debit card rollout as 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 checkups. The program began in rural Mexico in 1997 and later expanded to urban areas starting in 2002. By 2008 (just prior to the debit card rollout), Prospera had reached its desired coverage of households, with nearly one-fourth of Mexican households receiving benefits, and the number of beneficiaries was growing only slowly at less than 2 percent per year. Beneficiary households receive payments every two months, and payments are always made to women except in the case of single fathers. The transfer amount depends on the number of children in the household, and during the time of the card rollout averaged US$150 per two-month payment period.

The formal savings accounts in which Prospera beneficiaries were already receiving their transfers prior to the debit card rollout were 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"(Diario Oficial de la Federación 2014). 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 was 4.8 kilometers (Bachas et al. 2018); possibly as a result of these indirect transaction costs, prior to receiving a debit card, 90 percent of beneficiaries made one trip to the bank per payment period, withdrawing their entire transfer (Bachas et al. 2021).

The government's primary motive for distributing debit cards was to reduce the time and travel costs incurred by beneficiaries to access their transfers, by enabling them to withdraw funds from any bank's ATM.[9] The Bansefi and Prospera leaders that I spoke to expected beneficiaries to use their debit cards to withdraw cash at ATMs but did not expect many of them to use the cards to make transactions at POS terminals since these were poor individuals who were less familiar with financial technologies and who likely shopped at retailers that did not have POS terminals. These government officials were surprised when I showed them—using the transaction-level data Bansefi had shared with me—that immediately after receiving a card, about 35 percent of beneficiaries used their cards to make POS transactions, and that the proportion actively using the cards at POS terminals increased steadily over time, reaching 47 percent of beneficiaries after they had the card for three years (online Appendix Figure A.1).

In addition to about 35 percent of beneficiaries using the cards to make transactions at POS terminals within the first two months after receiving them, most beneficiaries also began using the debit cards right away to withdraw cash at ATMs: 87 percent made at least one withdrawal at an ATM in the first two-month period after receiving the card. The proportion using ATMs fell over the next three years to 72 percent, as some beneficiaries shifted to using the debit cards exclusively at POS terminals. On average (including those who did not transact at corner store POS

[9] 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. Furthermore, prior to the rollout of debit cards, it was not possible for beneficiaries to have the transfers automatically deposited in or automatically transferred to a debit card–eligible Bansefi account or to an account at another bank. Thus, if a beneficiary wanted to voluntarily adopt a debit card prior to the rollout, she would still have to travel to the bank branch every two months to manually transfer her benefits from the account in which they were automatically deposited to the debit card–enabled account.

terminals), beneficiaries initially spent MXN 128 per two-month period at corner store POS terminals, which can be compared to an average Prospera transfer amount of MXN 1,636 in the two-month period in which they received cards. Conditional on making a POS transaction at a corner store, 25 percent of total withdrawals from the account (including withdrawals at a bank branch, withdrawals at an ATM, and spending on the card) were POS transactions at corner stores. This increased over time and reached 31 percent of their total withdrawals after having the card for three years (online Appendix Figure A.1). The story that emerges from these descriptive statistics is that many beneficiaries began using the debit cards right away for both cash withdrawals from ATMs and transactions at POS terminals; over time, there was a gradual shift toward more POS transactions and fewer ATM withdrawals, although many beneficiaries continued using a combination of both.

All beneficiaries in a treated locality received cards during the same payment period, and administrative data from Bansefi show that cards were generally distributed during the first week of the payment period. Although the overall number of beneficiaries in the program was increasing nationally over time at a rate of 2 percent per year, the rollout was not accompanied by a differential change in the number of beneficiaries or transfer amounts (online Appendix C.2 and online Appendix Figure A.3, panel A). 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 III).

B. *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.[10] The terminal has a low up-front 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 percent of corner stores. In addition, there is a proportional transaction fee that varies by sector and bank; it was 1.75 percent 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 noncash) sales, the median corner store would pay 0.5 percent in transaction fees and 3.2 percent in monthly fees.[11]

In addition to these direct financial costs, there are potential indirect costs. First, acquiring a POS terminal requires having or opening an account with the bank issuing the terminal and signing a contract with the bank to obtain the POS

[10] I use a particular large commercial bank to illustrate because their full fee structure is publicly available at https://www.bbva.mx/empresas/productos/cobros-y-pagos/terminal-punto-de-venta.html. For other banks, while I have data on their transaction fee from Mexico's Central Bank, I do not have data on their full fee structure for POS terminals.

[11] The 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 percent for corner stores. The estimate of fees as a fraction of sales is based on the same combination of data sources.

terminal. 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, 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. 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 have been limited.[12]

The perceived benefits of POS adoption, reported by retailers in focus groups and surveys I conducted, 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 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. The most common responses on the benefits of POS adoption in the survey were increased sales and number of customers. Furthermore, 54 percent of corner store owners who had adopted POS terminals reported higher sales after adoption. In addition, 51 percent of corner store owners reported attracting new customers once they began accepting card payments. The majority (65 percent) of corner store owners who had adopted a POS terminal also reported that prior to adopting, they would lose potential sales when customers were not carrying cash at the time. The effects of these forces on merchant POS terminal adoption and consumer card adoption are modeled theoretically by Rochet and Tirole (2002).

II. Data

I combine administrative data on the debit card rollout with a rich collection of microdata from Mexico. These datasets 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 III, I restrict each dataset to the subsample corresponding to urban localities included in Prospera's debit card rollout. I describe each of the main datasets in this section and provide more detail in online Appendix B.

A. *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 (Prospera 2007–2016). Examples of payment methods include cash, bank account without

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

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 were distributed in each locality. In addition, they allow me to test whether the card rollout was accompanied by an expansion of the number of Prospera beneficiaries, which would be a threat to identification, as it would confound the effect of 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 (Bansefi 2007–2015). 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 dataset includes 106 million transactions. I use this dataset to measure whether the beneficiaries who directly received cards from Prospera used 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.

B. *Data on POS Terminals*

Universe of POS Terminal Adoptions: Data on POS terminal adoption were accessed on-site at Banco de México, Mexico's Central Bank. The dataset includes all changes to POS contracts between retailers and banks from 2006 to 2017, where contract changes include adoptions of POS terminals, cancellations, and changes to the fee structure or other contract parameters (Banco de México 2006–2017). The data include the store type (more precisely, the merchant category code) and a geographic identifier (postal code).[14] In total, the dataset includes over 5 million contract changes, 1.7 million of which are adoptions. I use both the adoptions and cancellations—combined with another dataset 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 dataset with the stock of POS terminals in each locality by store type by two-month period.

Universe of Card Transactions at POS Terminals: These data were also accessed on-site at Mexico's Central Bank and include card transactions made at a POS terminal between July 2007 and March 2015 (Banco de México 2007–2015). 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. The data only include the universe of

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

[14] Merchant category codes are four-digit numbers used by the electronic payments industry to categorize merchants. Dolfen et al. (2023) and Ganong and Noel (2019) also use merchant category codes to define store types and spending categories. Online Appendix B explains how I map from postal codes, the geographic identifier in this dataset, to localities, the relevant geographic area for the card rollout.

card transactions through mid-2013, as some banks shifted to a different transaction clearinghouse not included in the data. Since the debit card rollout lasts through mid-2012, my event studies using transactions data thus only include one year of posttreatment results to ensure that changing coefficients over time in the event study are not driven by dramatic changes to the sample underlying each coefficient.

C. *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 (CNBV 2008–2016).[15] 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. The data on number of other consumers' debit cards are measured as stocks as of the last day of each quarter. 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 the publicly available version 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.[16] The survey includes comprehensive income and consumption data at the household level; 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 (INEGI 2006–2014).

Google Searches for Supermarkets: I use data on Google searches for large supermarket chains in Mexico (Higgins 2024) to corroborate the findings from the consumption survey with higher-frequency data. While Google Trends data are not available at a geographic level below the state level in Mexico for the relevant time

[15] Gender-disaggregated data on the number of debit cards are only provided by CNBV starting in 2018, so it is not possible to test whether there were gender differences in the spillover effect on other consumers' debit card adoption.

[16] Online Appendix Table A.1 shows the distribution of when the localities of the 49,810 surveyed households were treated and when the households were surveyed.

period, people may search for a combination of the store name and locality name if they are searching for store locations or hours. Thus, I first query Google Trends to determine which were the three most common supermarket chains that people searched for on Google in Mexico prior to the debit card rollout. I then take the three most common supermarket chains and conduct queries on the frequency of Google searches for "[store name] [locality name]" to create a month-by-locality-level dataset on Google searches. The data span the same time period as the Central Bank data on POS terminal adoptions (2006–2017). More detail on the construction of this dataset is provided in online Appendix B.7. I also show in online Appendix B.7 that Google searches for corner stores were much less common than for supermarkets; thus, I only collect data on Google searches for supermarkets.

D. *Retail Outcomes and Prices*

Retail Outcomes: Every five years, INEGI conducts an Economic Census of the universe of firms in Mexico (INEGI 1993–2013). This census includes all retailers, regardless of whether they are formally registered (with the exception of vendors who do not have a fixed business establishment, such as street vendors). Firm type and store type are determined in this dataset using six-digit North American Industry Classification System (NAICS) codes.[17] 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.[18] Each wave includes about 4 million total firms; 354,820 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 20,879 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 revenues and costs.

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 nonfood items). Importantly, the goods are coded at the bar code–equivalent level (such as "600ml bottle of Coca-Cola"), which helps to disentangle price and quality differences between different types of stores; for example, larger stores sell larger pack sizes or higher-quality varieties (Atkin, Faber, and Gonzalez-Navarro 2018). After averaging price quotes across two-month periods for consistency with Prospera's payment periods, the dataset includes 5.4 million observations from 2002 to 2014 (Banco de México and INEGI 2002–2014).

[17] Note that Mexico's NAICS codes, available at https://www.inegi.org.mx/app/scian/, differ from the United States' NAICS codes used to classify firms in the United States (e.g., in Mian and Sufi 2014).

[18] In addition to using the 2008 and 2013 waves for the main regressions using Economic Census data, I use the 1993–2008 waves to test for parallel trends in all of the outcome variables at the locality level, comparing earlier- and later-treated localities. (It is not possible to test for parallel trends at the firm level given that the firm identifier was introduced in 2008.)

E. *Survey of Corner Stores*

I conducted in-person surveys of 1,760 corner store owners to better understand whether coordination failures constrain financial technology adoption (Higgins 2024). The survey was conducted from June to August 2022, and the corner stores in the sample are from 29 urban localities that were *not* included in the debit card rollout. I sampled localities that currently have similar levels of debit card and POS adoption as the localities included in the rollout had just before the shock. Specifically, I sample localities and corner stores such that the bivariate distribution of municipality-level debit card and retail POS adoption faced by the surveyed corner stores (measured at the end of 2021) matches the corresponding distribution that was faced by corner stores when they experienced the debit card rollout in their locality (measured in the quarter prior to the debit card shock happening in their locality). I provide more detail about the survey and sampling procedure in online Appendix B.10.

III. Identification

Prior to the debit card rollout, Prospera determined that it was only worthwhile to distribute debit cards in urban localities with sufficient ATM infrastructure since the primary objective was to reduce the time and travel costs incurred by beneficiaries to access their transfers. The government selected, ex ante, 259 of Mexico's 630 urban localities to be included in the rollout and intended for the nonselected localities to never receive Prospera debit cards; ex post, the nonselected localities never received debit cards.[19] Among the 259 selected localities, cards could not be distributed to all localities at once due to capacity constraints, which is why the government rolled out cards over time. In extensive conversations with me, Bansefi and Prospera officials explained that they wanted the localities that received cards at each stage of the rollout to be similar so that they could test their administrative procedures for the rollout with a quasi-representative sample. They did not expect the distribution of cards to have spillovers on banks' investments in ATM or branch infrastructure (and this expectation was accurate, as shown in Bachas et al. 2021) and were not thinking about spillovers on POS terminal adoption since they did not expect many beneficiaries to use the cards at POS terminals.

The rollout across these 259 urban localities had substantial geographic breadth and does not appear to follow a discernible geographic pattern (online Appendix Figure A.2, panel A). During the rollout, different localities were treated at different points in time, and cards were distributed to all beneficiaries in a particular locality during the same week; by the end of the rollout, over 1 million beneficiaries had received cards (online Appendix Figure A.2, panel B). 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.

[19] Mexico had 195,933 total localities in 2010, but the vast majority are rural and semi-urban localities, defined as having fewer than 15,000 inhabitants; 630 of Mexico's localities are urban.

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

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

In most cases, the outcome y_{jt} is for locality j, and I aggregate high-frequency variables to the two-month period t since Prospera is paid every two months (and the administrative data that allow me to determine the timing of the card rollout across localities are also at the two-month level). The estimating equation includes locality fixed effects λ_j to capture arbitrary time-invariant heterogeneity across localities and time fixed effects δ_t to capture overall time trends. D_{jt}^k is a relative event-time dummy that equals 1 if locality j received the debit card shock exactly k months ago (or will receive the shock $|k|$ months in the future when $k < 0$). I include 18 months prior to the shock and 24 months after the shock regardless of the dataset being used (i.e., $a = -18, b = 24$). Additional details about this specification are included in online Appendix C.1.[20]

I conduct three 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 3 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 datasets, including measures of financial technology adoption (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). Online Appendix Figure A.3 shows that the timing is also uncorrelated with trends in the number of Prospera beneficiaries and with the political party in power at the local level; it also shows that there was no change in these variables as a result of the card shock.

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 (see online Appendix C.3 for details). I include measures of pre-rollout levels and trends in financial technology and infrastructure 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 the Mexican government 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

[20] The issues with two-way fixed effects estimators highlighted by Goodman-Bacon (2021) apply to difference-in-differences regressions of the form $y_{jt} = \lambda_j + \delta_t + \beta D_{jt} + \varepsilon_{jt}$ but do not apply to (1). Indeed, one of the solutions suggested by Goodman-Bacon (2021) is to instead estimate an event study difference-in-differences specification of the form in (1). The methods proposed by Freyaldenhoven, Hansen, and Shapiro (2019) do apply to panel event study designs but focus on the case where pre-trends exist; here, there is no evidence of pre-trends across a range of variables from several datasets.

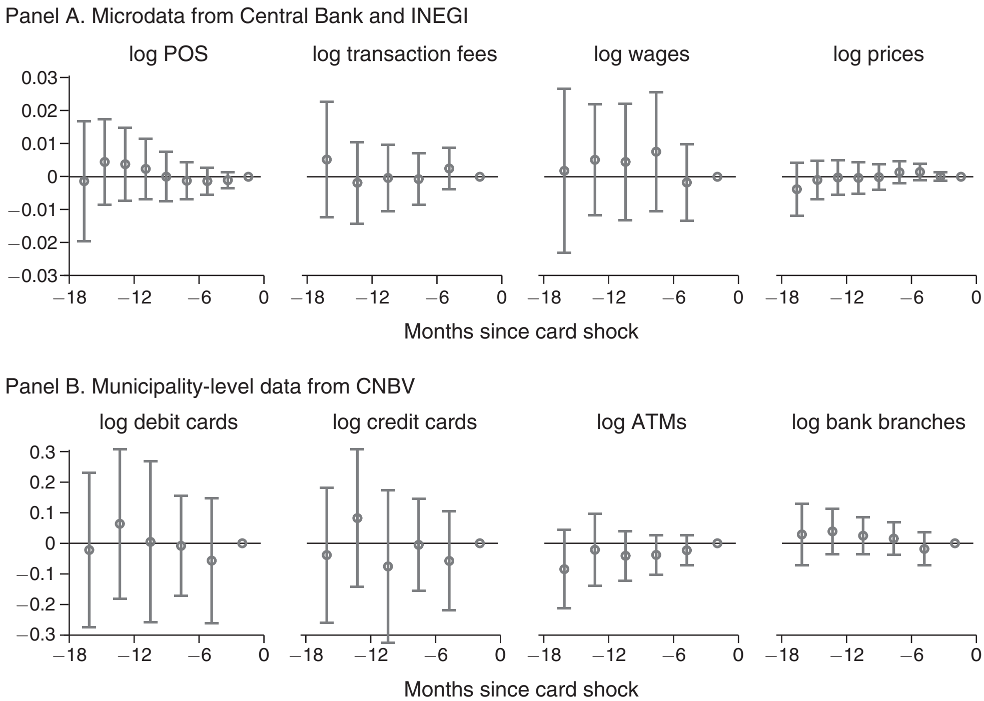

Figure 3. Balanced Pre-trends

Notes: 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 ($Observations = 8{,}806$). In the transaction fees regression, the data are aggregated to the municipality level, and each observation is a municipality by quarter ($Observations = 7{,}823$). 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 ($Observations = 4{,}404{,}678$). 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 ($Observations = 4{,}107{,}314$). In each regression in panel B, each observation is a municipality by quarter ($Observations = 8{,}243$). The frequency of ϕ_k coefficients depends on the frequency of each dataset. 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, and 95 percent confidence intervals are shown. Filled black circles indicate statistically significant at the 5 percent level, filled gray circles at the 10 percent level, and hollow gray circles indicate not statistically significant.

statistically significant at the 5 percent level, and the coefficient on the percent of children not attending school is statistically significant at the 10 percent level (Table 1).

Third, since some of the most interesting results are those on changes to the sales and profits of corner stores and supermarkets that come from the Economic Census, and since the Economic Census is only conducted every five years, I test for parallel trends for locality-level averages of the Economic Census outcomes from 1993 to 2008 (see online Appendix C.4 for details). Online Appendix Figures A.4 and A.5 show the results. When comparing pre-trends across 9 variables for localities treated 0–1.5, 1.5–3, and 3–4.5 years prior to the 2013 wave of the Economic Census, only 1 out of 54 coefficients for corner stores and only 3 out of 54 coefficients for supermarkets are statistically significant at the 5 percent level, as could be expected by chance.

Table 1—Pre-rollout Levels and Trends of Locality Characteristics Not Correlated with Rollout

Variable	Mean (1)	Standard deviation (2)	Discrete time hazard (3)	Variable	Mean (1)	Standard deviation (2)	Discrete time hazard (3)
Panel A. Banco de México, CNBV, population, Prospera, and electoral data				*Panel B. INEGI measures used to track development*			
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)

(*continued*)

IV. Results

A. *POS Terminal Adoption by Retailers*

Using the dataset I constructed at Mexico's Central Bank 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 made up 48 percent and 26 percent of retail consumption, respectively.[21] In the Central Bank transactions data, card transactions at corner stores and supermarkets made up 54 percent of all card transactions at POS terminals.

[21] Retail 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 calculations are restricted to households in urban localities. Throughout my analysis, I use "corner stores" and "supermarkets" as shorthand; "corner stores" refers to both corner stores and other small stores (e.g., bakeries and butcher shops), while "supermarkets" refers to supermarkets; department stores; "membership stores," such as Costco; and chain convenience stores, such as Oxxo and 7-Eleven.

Table 1—Pre-rollout Levels and Trends of Locality Characteristics Not Correlated with Rollout (*continued*)

Variable	Mean (1)	Standard deviation (2)	Discrete time hazard (3)	Variable	Mean (1)	Standard deviation (2)	Discrete time hazard (3)
Panel A. Banco de México, CNBV, population, Prospera, and electoral data				*Panel B. INEGI measures used to track development*			
log Prospera beneficiaries	7.09	1.11	−0.003	% without water	6.23	9.00	0.000
			(0.010)				(0.001)
Δ log Prospera beneficiaries	0.07	0.38	−0.000	Δ % without water	−0.04	0.05	0.088
			(0.015)				(0.109)
% vote share PAN	29.01	15.00	0.000	% without plumbing	3.62	6.20	0.004
			(0.001)				(0.002)
Δ % vote share PAN	−0.51	17.49	0.001	Δ % without plumbing	−0.06	0.06	0.111
			(0.001)				(0.139)
Mayor = PAN (× 100)	19.31	39.55	−0.000	% without electricity	4.32	2.19	0.006
			(0.000)				(0.006)
Δ mayor = PAN (× 100)	−11.97	58.17	0.000	Δ % without electricity	0.02	0.03	0.109
			(0.000)				(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)

Notes: 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 fifth-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 five 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. *Observations* = 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.

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; all coefficients are based on a balanced sample of localities, given that the data span 2006–2017, while the rollout spanned 2009–2012.

For corner stores, which made up 48 percent of all retail consumption independent of payment method, the coefficients prior to the debit card shock are all statistically nonsignificant from 0. Within the first two-month period after cards were disbursed, there was an increase in POS adoption after the debit card shock of about 3 percent. This rose to about 18 percent two years after the shock; all coefficients after the shock are positive and statistically significant for corner stores (Figure 4, panel A).[22] For supermarkets, which made up another 26 percent of retail consumption, all but one pretreatment coefficient are statistically nonsignificant from 0, but there was no effect of the card shock (Figure 4, panel B). 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 percent of supermarkets reported accepting card payments. Similarly, there is neither a pre-trend nor effect of the card shock for all other businesses, which made up the remaining 26 percent of retail consumption (Figure 4, panel C).[23]

B. *Spillovers on Other Consumers' Card Adoption and Use*

Do other consumers adopt and use 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 or a combination of indirect network externalities and social learning. In Section VI, I include a number of tests to distinguish between these mechanisms underlying the spillover effect on card adoption.

Card Adoption by Other Consumers: 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 dataset because the data are at the bank-by-municipality level. The estimation is restricted to urban municipalities included in the card rollout. Figure 5 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 occurred, in the following quarter the stock of non-Bansefi cards increased by 19 percent. Because the CNBV data are measured as stocks as of the last day of the quarter, and because Bansefi data show that the card rollout generally occurred in the first week of each period, the positive but statistically nonsignificant coefficient in the period in which the shock occurred corresponds to other consumers' debit card adoption nearly three months after the shock, while the 19 percent increase in the following quarter corresponds to other consumers' debit

[22] For all regressions with coefficients that are changes in logs, if we denote those coefficients as ϕ, the percent changes I report are $100 \times \left(\exp(\phi) - 1\right)\%$. Online Appendix Figure A.6 shows the results from the same specification using levels rather than logs of the number of POS terminals.

[23] These results are also shown in table form in online Appendix Table A.2. Online Appendix D.1 discusses why the increase in POS terminal adoption by corner stores is unlikely to be driven by corner store owners themselves being Prospera beneficiaries and finding it easier to adopt a POS terminal once they received a debit card.

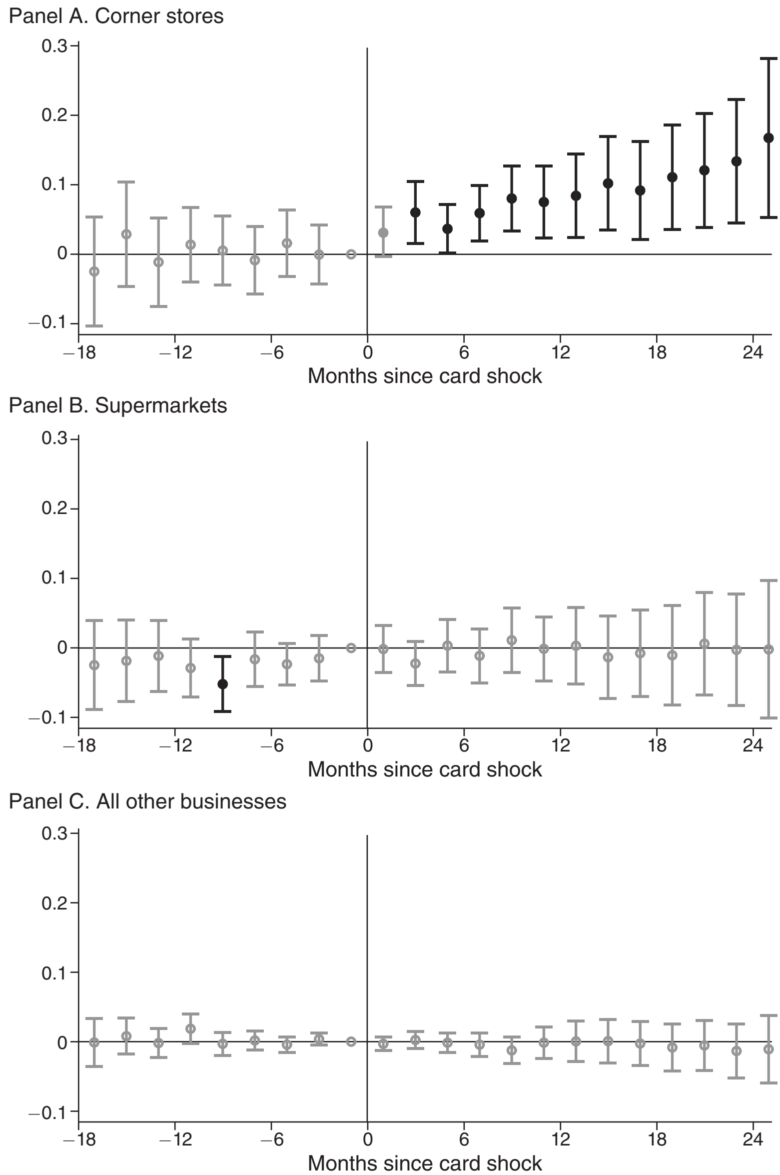

FIGURE 4. EFFECT OF CARD SHOCK ON LOG POS TERMINALS

Notes: This figure shows the effect of the debit card shock on the stock of point-of-sale 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. *Observations* = 8,806 locality-by-time observations from 259 localities. Standard errors are clustered at the locality level, and 95 percent confidence intervals are shown. Filled black circles indicate statistically significant at the 5 percent level, filled gray circles at the 10 percent level, and hollow gray circles indicate not statistically significant. The same results can be found in online Appendix Table A.2.

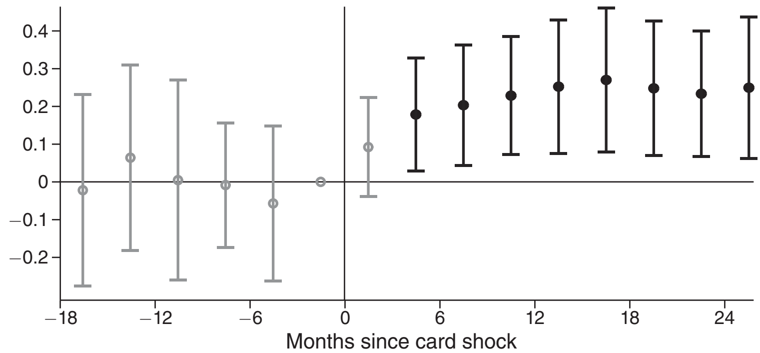

Figure 5. Spillovers on Other Consumers' Card Adoption

Notes: This figure 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. *Observations* $=$ 8,234 municipality-by-quarter observations from 255 municipalities. Pooled difference-in-differences coefficient $= 0.189$ (standard error $= 0.065$), 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, and 95 percent confidence intervals are shown. Filled black circles indicate statistically significant at the 5 percent level, filled gray circles at the 10 percent level, and hollow gray circles indicate not statistically significant. The same results can be found in Table 2.

card adoption nearly six months after the shock. Treated localities had 28 percent more non-Bansefi debit cards two years after the shock.[24]

One possibility is that new non-Bansefi cards were not spillovers to other consumers but were instead adopted by Prospera beneficiaries or other members of their household (e.g., after they discovered the benefits of having a card and thus decided to open a debit card account at a different bank). To explore this, I use data from the Payment Methods Survey described in online Appendix B.12, where Prospera beneficiaries were asked in mid-2012 (after the rollout) if they had a bank account at another bank, which is a prerequisite to having a debit card from another bank. Just 6 percent of beneficiaries who were receiving their Prospera benefits by debit card reported having an additional bank account at another bank. Because the base of beneficiaries who received cards was less than half the size of the existing number of households with cards, even if all beneficiary households with accounts at other banks had a card attached to that account and had adopted that other card after receiving a Prospera card, beneficiary adoption could explain at most a 3 percent increase in the number of non-Bansefi cards.

Timing of Spillover on Card Adoption: The short-run increase in other consumers' debit card adoption around three to six months after the debit card shock shown in Figure 5 should be larger in areas that had a faster corner store POS adoption response to the card shock. To test for this, I measure a municipality's "immediate" POS adoption response to the debit card shock as its month-over-month change in the number of corner store POS terminals in the period in which the shock occurred,

[24] Online Appendix Figure A.7 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. Online Appendix Figure A.7, panel B shows that results are robust to using the log number of credit and debit cards rather than the log number of just debit cards.

Table 2—Spillovers on Other Consumers' Card Adoption

	Dependent variable: log debit cards (excluding Bansefi cards)						
		Heterogeneity					
		Social connectedness		ATM density		Proportion of Prospera transactions at supermarkets	
	Main	< median	> median	< median	> median	< median	> median
Months since card shock	(1)	(2)	(3)	(4)	(5)	(6)	(7)
–18 to –15	−0.022	−0.229	0.086	0.111	−0.001	−0.067	−0.093
	(0.131)	(0.192)	(0.178)	(0.262)	(0.092)	(0.193)	(0.200)
–15 to –12	0.064	0.023	0.073	0.188	0.029	−0.068	0.252
	(0.127)	(0.221)	(0.155)	(0.249)	(0.062)	(0.189)	(0.216)
–12 to –9	0.005	0.075	−0.082	0.043	0.051	0.170	−0.162
	(0.136)	(0.140)	(0.226)	(0.256)	(0.058)	(0.161)	(0.249)
–9 to –6	−0.008	−0.059	0.011	0.030	−0.006	−0.070	0.030
	(0.086)	(0.118)	(0.126)	(0.167)	(0.057)	(0.141)	(0.114)
–6 to –3	−0.057	−0.067	−0.067	0.089	−0.137	0.036	−0.165
	(0.106)	(0.180)	(0.113)	(0.175)	(0.129)	(0.182)	(0.140)
–3 to 0 (omitted)	0	0	0	0	0	0	0
0 to 3	0.092	0.070	0.118	0.105	0.096	0.148	0.069
	(0.068)	(0.109)	(0.080)	(0.122)	(0.054)	(0.091)	(0.117)
3 to 6	0.178	0.132	0.240	0.253	0.085	0.274	0.130
	(0.078)	(0.111)	(0.105)	(0.142)	(0.061)	(0.115)	(0.128)
6 to 9	0.203	0.214	0.209	0.332	0.079	0.378	0.070
	(0.083)	(0.132)	(0.097)	(0.146)	(0.068)	(0.129)	(0.122)
9 to 12	0.229	0.252	0.234	0.357	0.078	0.389	0.101
	(0.081)	(0.134)	(0.095)	(0.141)	(0.063)	(0.136)	(0.112)
12 to 15	0.252	0.275	0.265	0.393	0.095	0.432	0.121
	(0.092)	(0.148)	(0.108)	(0.158)	(0.068)	(0.159)	(0.119)
15 to 18	0.270	0.285	0.293	0.420	0.092	0.460	0.132
	(0.099)	(0.162)	(0.115)	(0.169)	(0.074)	(0.169)	(0.128)
18 to 21	0.248	0.261	0.275	0.395	0.092	0.444	0.110
	(0.092)	(0.151)	(0.107)	(0.159)	(0.074)	(0.149)	(0.128)
21 to 24	0.234	0.243	0.263	0.360	0.096	0.412	0.106
	(0.087)	(0.140)	(0.104)	(0.148)	(0.072)	(0.138)	(0.125)
24 to 27	0.250	0.235	0.309	0.401	0.095	0.465	0.105
	(0.097)	(0.154)	(0.116)	(0.166)	(0.082)	(0.156)	(0.138)
N (municipality × quarter)	8,243	4,157	4,055	4,035	4,208	3,833	3,852
Number of municipalities	255	127	127	127	128	119	118
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time (quarter) fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: 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 are at the issuing-bank-by-municipality-by-quarter level. Column 1 shows the main estimates. Columns 2 and 3 show heterogeneity by the within-municipality Social Connectedness Index, which measures how connected the set of Facebook users in a municipality are to one another. Columns 4 and 5 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 6 and 7 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 six months they have the debit card and split the municipalities at the median. The sum of the number of municipalities in columns 1 and 2 is one less than in column 1 because one municipality is missing in the Social Connectedness Index data; the sum of the number in columns 6 and 7 is less than in column 1 because in 18 municipalities no Prospera beneficiaries used the card to make POS transactions during the first six months with the card, and hence, the heterogeneity variable is missing for those municipalities. Standard errors are clustered at the municipality level.

relative to (i.e., divided by) the month-over-month change in the number of corner store POS terminals in the period before the shock occurred.

Online Appendix Figure A.8 shows that in municipalities with a below-median immediate POS adoption response by corner stores, the coefficients on other consumers' card adoption are very close to zero and not statistically significant in the first two quarters (measured nearly three and six months after the shock occurred). In contrast, in municipalities with an above-median immediate POS adoption response by corner stores, nearly three months after the shock occurred, the point estimate shows 19 percent higher adoption of debit cards by other consumers ($p = 0.11$), and in the following quarter nearly six months after the shock occurred, there is 33 percent higher debit card adoption by other consumers ($p = 0.02$). When I test the difference in coefficients between municipalities with above- versus below-median immediate POS adoption response by interacting D_{jt}^{k} and δ_t in (1) with the above-median immediate POS adoption response dummy (rather than running separate regressions for above- and below-median municipalities), the difference in coefficients is statistically significant at the 10 percent level for the first six months after the shock occurred. After that, the difference in coefficients is no longer statistically significant, and the point estimates in below-median municipalities increase (consistent with a slower POS adoption response in those localities leading to a delay before the spillover on other consumers' debit card adoption is observed).

POS Transactions by Other Consumers: Do consumers use debit cards more after the shock? I use transactions-level data from Mexico's Central Bank on POS transactions, merged at the locality-by-two-month-period level with Prospera transactions-level data to subtract out POS transactions by beneficiaries.[25] Online Appendix Figure A.9 suggests that consumers who were indirectly shocked (i.e., did not receive a debit card from the government) indeed increased the number of transactions they made at POS terminals. Specifically, in the first two-month period after the card shock, there is a small 6 percent increase in transactions (statistically significant at the 10 percent level). This lack of a substantial effect on other consumers' POS transactions is consistent both with the fact that the immediate increase in corner store POS adoption after the debit card shock was small and with the positive but statistically nonsignificant increase in other consumers' debit card adoption in the quarter in which the debit card shock occurred. In the second two-month period after the shock, there is a 22 percent increase in other consumers' transactions at POS terminals that is statistically significant at the 5 percent level, which also coincides with when we see a positive and statistically significant spillover effect on other households' debit card adoption in Figure 5. In the subsequent periods, the effects are also positive and significant at the 5 percent level.

ATM Withdrawals by Other Consumers: I also assess what happened to other consumers' ATM use by merging the CNBV data on all ATM withdrawals at the

[25] A caveat about the POS transactions data is that after mid-2013, there is a significant drop in POS transactions in the data because some banks switched to a different clearinghouse. Because the debit card shock ended in mid-2012, I am thus only able to show effects up to one year after the shock.

municipality-by-month level with the Bansefi transactions-level data to subtract out ATM withdrawals by Prospera beneficiaries.[26] The overall effect on the number of ATM withdrawals could go in either direction. On the one hand, one of the spillover effects of the debit card shock was that other consumers adopted debit cards (Figure 5), and since debit cards are necessary to make ATM transactions, this could push ATM transactions up.[27] On the other hand, as corner stores adopted POS terminals, consumers who already had cards prior to the shock may withdraw cash less frequently as they use their cards more for transactions at POS terminals. Online Appendix Figure A.10 shows that the number of ATM withdrawals by other consumers (i.e., excluding those by Prospera beneficiaries) did not change in the first two-month period in which the shock occurred but then fell by 8 percent two to six months after the shock occurred, as consumers shifted from cash to card transactions. After six months, the coefficients are no longer statistically significant in most periods, but the point estimates remain between -8 percent and -11 percent.

C. *Spillovers on Consumption across Stores*

Do some consumers shift a portion of their consumption from supermarkets to corner stores now that more corner stores accept card payments? To estimate changes in consumption as a result of the card shock across *all consumers* (i.e., not restricted to Prospera beneficiaries), 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. Thus, it is not possible to subject the outcomes in ENIGH to the same parallel trends and robustness tests as is possible in the higher-frequency administrative data; the results should therefore be treated with additional caution. Nevertheless, it is reassuring that the consumption results are corroborated by higher-frequency data on Google searches.

Continuing to restrict the sample to urban localities included in the rollout (but including all households in those localities, not just those that received debit cards), I estimate

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

where y_{it} is the outcome (such as log spending at corner stores or the number of trips per week to corner stores) for household i in survey wave t, $\lambda_{j(i)}$ is a set of locality

[26] The CNBV data on ATM transactions only begin in April 2011, which is after most of the debit card rollout had already occurred and about one year before the latest-treated localities received the card shock. Thus, I am only able to include 12 rather than 18 months of pre-period data when estimating (1). As explained in online Appendix B.5, the CNBV data also shift from quarterly to monthly frequency in April 2011, so I am able to aggregate the monthly flows of ATM transactions in the CNBV and Bansefi data to the two-month period rather than quarter. Two-month periods correspond to the administrative data on the debit card rollout, and this is also the aggregation I use in the regressions using data from Mexico's Central Bank.

[27] I subtract out ATM withdrawals made by Prospera beneficiaries, which are observed in the Bansefi data. Because Prospera beneficiaries could not make ATM withdrawals from their Bansefi accounts prior to having a debit card and because nearly all of them do use their debit cards for ATM withdrawals, their number of ATM transactions mechanically increases after the debit card shock (Bachas et al. 2021, Figure 4).

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.[28]

Part of the card rollout overlaps with the ENIGH data collection. Specifically, 10 percent of households in the sample were surveyed in the same year as their locality was treated. Furthermore, I do not observe the exact timing of each survey, so for these 10 percent of ENIGH observations where the year of the card shock is equal to the year of the survey, I do not know if that locality had been treated yet at the time a particular household was surveyed. To be conservative, I set $D_{j(i)t} = 0$ if the year t of the survey is less than *or equal to* the year that locality $j(i)$ received the debit card rollout.[29] By counting a locality as not yet treated if the year of survey is equal to the year of the debit card shock in that locality, any observed treatment effects occurred at least 11 months after the card rollout took place (given the timing of the card rollout and survey). This minimum of 11 months corresponds to households surveyed in August–September 2010 from localities treated in September–October 2009 and households surveyed in August–September 2012 from localities treated in September–October 2011. Online Appendix Table A.1 shows the full distribution of the timing of the survey and the card rollout for households included in the ENIGH data.

Table 3 shows how consumers changed their consumption in response to the shock, with results from (2) where the dependent variable is log spending at a particular store type. Overall, there was a 7 percent increase in consumption at corner stores, which, from the earlier results, were more likely to accept card payments after the shock. The point estimate for spending at supermarkets is −2 percent (not statistically significant). Column 7 shows 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), I cannot reject no change in overall spending ($p = 0.33$). These changes in spending are across all consumers (i.e., the sample is not restricted to Prospera beneficiaries).[30]

[28] I include locality rather than household fixed effects since the survey is a repeated cross section rather than a panel at the household level. The underlying assumption is that households did not move to a particular locality *in response* to the debit card shock, which seems reasonable given that the costs of moving were likely high relative to the benefits of having a debit card. Households that moved for reasons uncorrelated with the debit card shock would not bias my estimates; nevertheless, migration in these localities was relatively low: using data from a panel of over 12 million voter registrations (a 15 percent random sample from the universe of 80 million voter registrations in Mexico), Bachas et al. (2021) find that only 4.5 percent of residents migrated from one locality to another over a 3-year period.

[29] Note that for nearly all of the observations where the year of the survey equals the year of the debit card shock, the timing of the card rollout overlaps with the timing of the survey. These include the households surveyed in 2010 in localities that received the card rollout in September–October or November–December 2010. For the households surveyed in 2012 in localities that received the card rollout in May–June 2012, I do know they were surveyed prior to the card rollout, but I still set $D_{j(i)t} = 0$ to be conservative, so that $D_{j(i)t} = 1$ always corresponds to being surveyed at least 11 months after the card rollout occurred in their locality. This choice is inconsequential, as there are only five households out of 49,810 that were surveyed in 2012 in localities treated in May–June 2012.

[30] The issues with two-way fixed effects estimators highlighted by Goodman-Bacon (2021) do apply to (2). Hence, I test for the robustness of these results to using the estimator proposed by de Chaisemartin and D'Haultfœuille (2020), which is not susceptible to these issues. Online Appendix Table A.3 shows that the results are robust: point estimates are similar, while the coefficient on log corner stores spending is significant at the 10 percent level rather than at the 5 percent level. Because (2) uses data from a repeated cross section at the household level and thus includes locality rather than household fixed effects, it is not possible to conduct a Goodman-Bacon (2021) decomposition of the estimates.

TABLE 3—SPILLOVERS ON CONSUMER SPENDING ACROSS STORE TYPES

	Dependent variable: log spending at...								
	Corner stores			Supermarkets			Total		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Diff-in-diff	0.067	0.051	0.076	−0.018	0.003	−0.016	0.029	0.029	0.041
	(0.032)	(0.033)	(0.033)	(0.043)	(0.050)	(0.045)	(0.030)	(0.033)	(0.030)
Diff-in-diff × has credit card		0.061			−0.058			−0.012	
		(0.040)			(0.062)			(0.040)	
Diff-in-diff × Prospera beneficiary			−0.127			−0.030			−0.161
			(0.060)			(0.133)			(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 FE		Yes	Yes		Yes	Yes		Yes	Yes
Card/beneficiary by time FE		Yes	Yes		Yes	Yes		Yes	Yes

Notes: 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.

Heterogeneity in Spillovers on Consumption across Stores: 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 had a credit card with all of the terms on the right-hand side of (2). Specifically, I estimate

$$y_{it} = \xi_{h(i)j(i)} + \eta_{h(i)t} + \gamma D_{j(i)t} + \omega D_{j(i)t} \times h_{it} + \varepsilon_{it}, \tag{3}$$

where h_{it} is the heterogeneity dummy and the $h(i)$ subscript denotes interacting fixed effects with the heterogeneity dummy (in this case, whether the household has a credit card): $\xi_{h(i)j(i)}$ are a set of heterogeneity-dummy-by-locality fixed effects, while $\eta_{h(i)t}$ are a set of heterogeneity-dummy-by-time fixed effects. (Even though the data are not a panel and I thus cannot measure *baseline* credit card ownership by each household, the spillover on card adoption was concentrated on debit and not credit card adoption, so the heterogeneity dummy was not differentially impacted by treatment.)

If the change in consumption at corner stores was indeed driven by an influx of new customers who already had cards and shopped at retailers with POS terminals, 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 percent larger increase in spending at corner stores and a 6 percent 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 was a Prospera beneficiary (meaning the household would have directly received a card when the shock occurred). As shown in Bachas et al. (2021), beneficiaries responded to receiving a debit card by *decreasing* total consumption to finance an *increase* in overall savings because the debit card made saving in the account more attractive and since saving informally was difficult. I estimate (3) where the heterogeneity dummy equals 1 if the household was a Prospera beneficiary. While Bachas et al. (2021) 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 decreased their overall consumption in response to the card shock ($\gamma + \omega$ is statistically significant at the 10 percent level; column 9 of Table 3).

Spillovers on Consumption across Stores by Income Quintile: 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

$$(4) \qquad y_{it} = \lambda_{j(i)} + \theta_{q(i)t} + \gamma D_{j(i)t} + \sum_{q=2}^{5} \psi_q \mathbf{1}\{quintile = q\}_{it} \times D_{j(i)t} + \varepsilon_{it},$$

where $\theta_{q(i)t}$ is a full set of income quintile by time fixed effects and $\mathbf{1}\{quintile = q\}_{it}$ 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.[31]

Figure 6 shows how consumers in each quintile of the income distribution changed their consumption in response to the shock, plotting $\gamma + \psi_q$ for each quintile. The richest quintile of consumers reduced their consumption at supermarkets by 13 percent and increased their consumption at corner stores by 15 percent in response to the debit card shock and subsequent POS adoption by corner stores. The second-richest quintile also appears to have increased its consumption at corner stores (by 8 percent, significant at the 10 percent level), while the results for the poorest three quintiles are statistically nonsignificant from 0 (Figure 6, panel A and online Appendix Table A.4, columns 1 and 2). This shift in spending appears to be driven (at least partially) by a change in the number of trips; the richest quintile increased trips to corner stores by 0.8 trips per week and decreased trips to the supermarket by 0.2 trips per week on average (Figure 6, panel B and online Appendix Table A.4, columns 5 and 6). There is again no effect of the card shock on the number of trips made to corner stores or supermarkets by 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

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

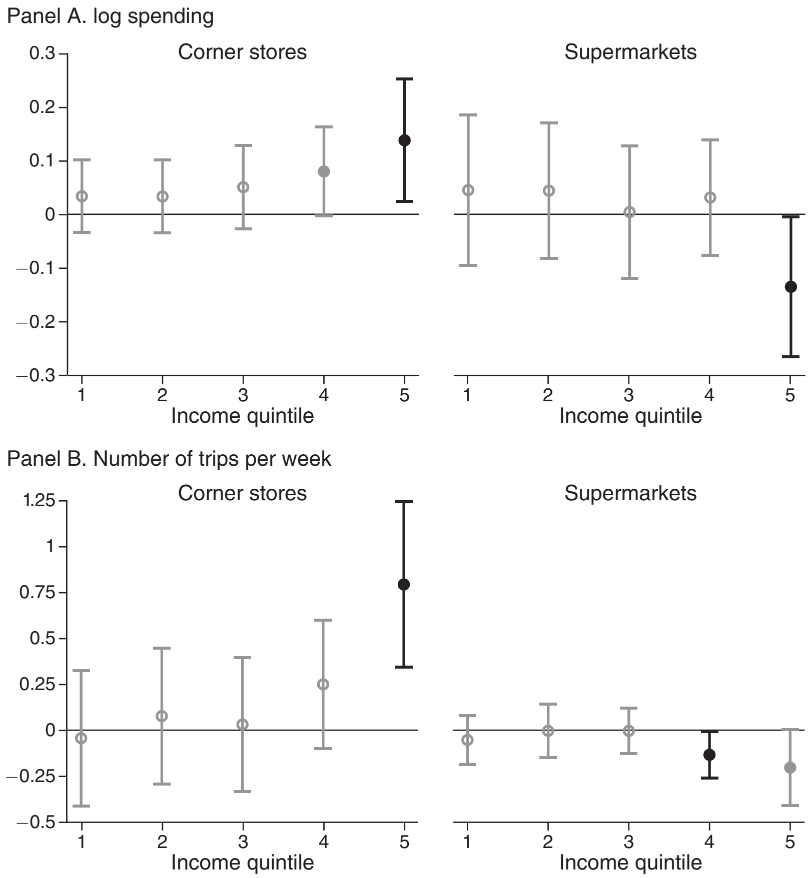

Figure 6. Spillovers on Consumer Spending across Store Types by Income Quintile

Notes: 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 (4), 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. *Observations* = 49,810 households from 220 localities. Standard errors are clustered at the locality level, and 95 percent confidence intervals are shown. Filled black circles indicate statistically significant at the 5 percent level, filled gray circles at the 10 percent level, and hollow gray circles indicate not statistically significant. The same results can be found in online Appendix Table A.4.

consumed 24 percent of total consumption at corner stores and 17 percent at supermarkets. Thus, the magnitudes of the 15 percent increase in corner store consumption and 13 percent decrease in supermarket consumption come fairly close to lining up, each representing 2.2–3.5 percent of total consumption.

Given the shift in consumption from supermarkets to corner stores by richer consumers, which goods that they previously consumed at supermarkets did they shift to consuming at corner stores? Did the shift in consumption across stores also involve a change in the type of goods they consumed? To answer these questions, I reestimate (4) with log spending on a particular category of goods at a particular store type as the outcome. Online Appendix Figure A.11 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 shifted from supermarkets to corner stores; results for all quintiles are in online Appendix Tables A.5 and A.6. The product categories where there were both a statistically significant 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 nonsignificant. The right column of online Appendix Figure A.11 shows results for total consumption across all store types; all but 1 of the 16 coefficients are statistically nonsignificant, 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).

Timing of Consumption Shift: One concern is whether the shift in richer households' consumption from supermarkets to corner stores truly occurred after corner stores began adopting POS terminals. As discussed above, based on the timing of the debit card rollout and the surveys, as well as the definition of $D_{j(i)t}$ in (2), the observed treatment effects occurred for households in localities that had received the debit card shock at least 11 months prior. An additional piece of evidence comes from searches for supermarkets on Google.

Online Appendix Figure A.12, panel A shows the effect of the card rollout on the log frequency of Google searches for supermarkets, using data I collected through Google Trends on searches for "[store name] [locality name]" for the three most-searched supermarket chains in Mexico.[32] There is no statistically significant effect of the debit card shock in the period in which the shock occurred or the subsequent period, but there is a statistically significant 4 percent average decrease between four months and two years after the shock. As Google searches for stores are likely correlated with shopping at those stores (Choi and Varian 2012), these results provide further and higher-frequency evidence that the timing of the shift in consumption from supermarkets to corner stores occurred after corner stores had begun adopting POS terminals in response to the debit card shock.[33]

Alternative Explanations: In online Appendix D.2, 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

[32] The three most searched supermarket chains prior to the debit card shock were Walmart, Soriana, and Comercial Mexicana. As explained in online Appendix B.7, Google searches for corner stores are much less common, which is why I only query data on searches for supermarkets.

[33] The point estimates in the first two periods are negative but not statistically significant; a small negative effect in these first couple of periods would not be inconsistent with the shift to supermarkets happening after corner store POS adoption, as there was a small and statistically significant effect on POS adoption even in the first two-month period in which the debit card shock occurred. To ensure that the trends in online Appendix Figure A.12, panel A are not driven by overall changes in internet use (although those changes would need to be correlated with the timing of the card rollout across localities to create the trends seen in online Appendix Figure A.12, panel A), I conduct a placebo test using Google searches for the common search term "weather" in online Appendix Figure A.12, panel B and find that the point estimates are statistically nonsignificant, are close to zero, and have tight confidence intervals both before and after the debit card shock.

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. I also discuss in online Appendix D.2 that other mechanisms such as supermarket data breaches (Agarwal et al. 2022) would need to be correlated with the card rollout to explain richer consumers' shift in consumption from supermarkets to corner stores, which is unlikely.

D. *Retail Firm Outcomes*

Sales, Costs, and Profits: Given that corner stores adopted POS terminals in response to the shock and that richer consumers shifted part of their consumption in response to corner store POS adoption, I now investigate how retail firm outcomes were affected using the 2008 and 2013 Economic Census waves. Because these census waves bracket the rollout of cards, I exploit 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 retail firm outcomes in localities that received the shock earlier. These results should be treated with additional caution since the Economic Census waves are five years apart and all treated localities have received the shock by the 2013 census wave. I cannot conduct high-frequency parallel trends tests or observe when effects occur at high frequency relative to the timing of the debit card shock. Nevertheless, it is reassuring that results on sales from the Economic Census are consistent with results from the consumption survey (conducted every two years) and Google searches for supermarkets (aggregated to two-month periods) from Section IVC; furthermore, I conduct locality-level parallel trends tests using the 1993–2008 Economic Census waves in online Appendix Figures A.4 and A.5.

I restrict the Economic Census to corner stores or, in a separate regression, to supermarkets, and I estimate

$$y_{it} = \gamma_i + \delta_t + \sum_k \gamma_k \mathbf{1}\{\text{received cards at } k\}_{j(i)} \times D_{j(i)t} + \varepsilon_{it} \tag{5}$$

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.[34]

Corner stores in localities treated 3–4.5 years before the second census wave experienced increases in sales of 8 percent relative to corner stores in the latest-treated localities (statistically significant at the 5 percent level), while those in localities

[34] The issues with two-way fixed effects estimators do not apply to (5) since there are only two time periods in the Economic Census data.

Table 4—Retail Firm Outcomes

	log sales (1)	log inventory costs (2)	log wage costs (3)	log number workers (4)	log rent costs (5)	log capital (6)	log electricity costs (7)	asinh profits (8)	Charged VAT or paid social security (9)
Panel A. Corner stores									
Shock 3–4.5 years ago	0.081	0.059	−0.022	0.000	−0.028	0.047	−0.029	0.212	0.014
	(0.036)	(0.034)	(0.020)	(0.005)	(0.025)	(0.083)	(0.034)	(0.099)	(0.009)
Shock 1.5–3 years ago	0.045	0.022	−0.022	0.000	0.022	0.024	0.005	0.143	0.031
	(0.037)	(0.035)	(0.017)	(0.004)	(0.023)	(0.089)	(0.034)	(0.104)	(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
Pooled coefficient									
Shock 1.5–4.5 years ago	0.062	0.039	−0.022	0.000	−0.002	0.035	−0.011	0.175	0.023
	(0.034)	(0.032)	(0.017)	(0.004)	(0.022)	(0.082)	(0.032)	(0.096)	(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
Panel B. Supermarkets									
Shock 3–4.5 years ago	−0.143	−0.155	−0.151	−0.014	0.314	−0.064	0.180	−0.228	−0.054
	(0.063)	(0.062)	(0.316)	(0.019)	(0.300)	(0.085)	(0.254)	(2.353)	(0.082)
Shock 1.5–3 years ago	−0.119	−0.124	−0.346	−0.022	0.135	0.144	0.153	0.149	−0.013
	(0.062)	(0.063)	(0.348)	(0.019)	(0.256)	(0.116)	(0.259)	(2.341)	(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
Pooled coefficient									
Shock 1.5–4.5 years ago	−0.131	−0.140	−0.246	−0.018	0.227	0.037	0.167	−0.045	−0.034
	(0.058)	(0.057)	(0.308)	(0.019)	(0.242)	(0.086)	(0.253)	(2.326)	(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

Notes: 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 (5), 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.

treated 1.5–3 years before the second census wave have a statistically nonsignificant point estimate of a 5 percent sales increase (Table 4, panel A, column 1). The pooled estimate shows that corner stores in earlier-treated localities experienced a 6 percent increase in sales relative to those in later-treated localities (statistically significant at the 10 percent level). For all treatment effect coefficients on corner store sales reported here, the effects are measured after the locality had been treated for at least 1.5 years.[35] This increase in corner store sales came at the

[35] While it is possible that firms could have changed their reporting in the Economic Census even if their sales did not actually change (e.g., if firms misreported sales less after adopting a POS terminal), online Appendix D.3 explains why this is unlikely to explain the effect. Most critically, the increase in corner store sales estimated here lines up very closely with the increase in spending at corner stores *reported by consumers* in Table 3.

expense of supermarkets, which experienced a 12 percent decrease in sales (statistically significant at the 5 percent 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 13 times as many corner stores as supermarkets. In aggregate, the 6 percent increase in sales at the average corner store and 12 percent decrease in sales at the average supermarket line up very closely since aggregate corner store sales were 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 increased (by 6 percent in earliest-treated localities, significant at the 10 percent level), while the amount spent by supermarkets on purchasing inventory decreased (by 14 percent in earliest-treated localities, significant at the 5 percent level). Corner stores were 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).[36] As a result, corner store profits increased by 19 percent in earlier-treated localities (panel A, column 8, pooled coefficient). The story that emerges is that corner stores increased their profits by buying and selling more inventory while keeping other input costs fixed. It is possible that a portion of the profits increase was due to other factors related to the demand shock they experienced; for example, richer customers likely bought 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.[37]

The shift in sales from supermarkets to corner stores has important distributional implications. First, it represents a shift in consumption across the firm size distribution, as corner stores are much smaller than supermarkets (online Appendix Figure A.14). Second, assuming the benefits of the increase in corner store profits accrued at least partly to corner store owners, it represents redistribution toward

[36] The coefficients are statistically nonsignificant from zero for corner stores' log wage costs, log number of workers, log rent costs, log capital, and log electricity costs. I can rule out an increase in corner store spending on wages greater than 1.1 percent and an increase in the number of employees greater than 0.9 percent. The standard errors on log capital expenditures and log electricity costs are larger, making those tests less informative. There are fewer supermarkets than corner stores, and although coefficients for supermarkets on wages, number of workers, rent, capital, and electricity are statistically nonsignificant from zero, standard errors are quite large for all of these outcomes except number of workers. For number of workers, I can rule out a decrease at supermarkets of more than 5.3 percent. Because it is valuable to know whether supermarkets responded to their reduction in sales by reducing wages, I turn to Mexico's publicly available quarterly labor force described in online Appendix B.12. Estimating the simple difference-in-differences in (2) for increased power, using log wages of supermarket employees as the outcome variable, the point estimate is very close to 0 (+0.2 percent), and I can rule out a reduction in supermarket wages as a result of the card shock of more than 3 percent (online Appendix Table A.7). Online Appendix Figure A.13 shows the full event study estimates using (1) for log wages in the quarterly labor force survey separately for corner store and supermarket employees: all point estimates after the card shock are statistically nonsignificant from zero.

[37] The standard errors in Table 4 are asymptotic cluster-robust standard errors, clustered at the locality level. I also conduct clustered randomization inference, where I continue to restrict to localities that were included in the debit card rollout and randomly block-permute the vector of treatment timing; I conduct 2,000 permutations and calculate randomization inference p-values as the proportion of permutations for which the absolute value of the permutation's t-statistic is greater than the absolute value of the t-statistic from the true treatment assignment. Online Appendix Table A.8 shows the clustered randomization inference p-values. While the randomization inference p-values are higher than asymptotic cluster-robust p-values, all of the results for corner stores that are statistically significant at the 5 percent level in Table 4 are still significant at the 10 percent level using the randomization inference p-values: these include the increase in sales and profits for corner stores treated 3–4.5 years ago and the increase in formality for corner stores treated 1.5–3 years ago, both relative to corner stores treated 0–1.5 years ago.

lower- and middle-income households: corner store owners are spread throughout the income distribution—and more concentrated in the bottom three income quintiles—while supermarket owners are concentrated in the top two income quintiles (online Appendix Figure A.15).

Prices: Corner stores might have increased prices for a number of reasons, including (i) the overall demand shock documented above; (ii) a shift in the composition of demand to include more demand from richer, less price-elastic consumers (Atkin et al. 2017; Gupta 2022); or (iii) pass-through of the costs of POS terminals to all of their customers.[38] 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. Because the data are at the product-by-store level rather than the locality level, I use the same specification as Atkin, Faber, and Gonzalez-Navarro (2018) use with the same data:

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

where $Price_{gst}$ is the price of bar code–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. Importantly, the specification includes bar code–level product (e.g., "600ml bottle of Coca-Cola") by store fixed effects, so any shift in demand to higher-priced products will not be picked up by ϕ_k.

Online Appendix Figure A.16 shows the results. All of the ϕ_k coefficients are statistically nonsignificant from zero for both corner stores and supermarkets, both before and after the card shock. Using each estimate's 95 percent confidence interval, I can rule out price effects outside of the range $[-1.7\%, 1.1\%]$ during the first 10 months after the shock and outside of the range $[-2.5\%, 2.4\%]$ during the first two years after the shock. For increased precision, I estimate the simple difference-in-differences from (2) and can rule out an average change in prices greater than 1.0 percent at corner stores and greater than 0.7 percent at supermarkets after the card shock (online Appendix Table A.7). Consistent with this, in the survey of corner stores I conducted, only 3 percent of corner store owners with POS terminals reported increasing prices after adopting, and the most common reason given for not increasing prices was that doing so would drive away some of their customers in a competitive market. Gomes and Tirole (2018) derive the theoretical conditions under which a retailer is better-off absorbing the costs of a POS terminal rather than passing them through to prices, and Mukharlyamov and Sarin (2022) find little to no pass-through of the reduced debit card interchange fees resulting from the Durbin Amendment of the 2010 Dodd-Frank Act in the United States.[39]

[38] An alternative way to pass through the costs is to surcharge only customers who pay by card rather than pass through the costs to product prices for all customers. There is no law in Mexico that prohibits surcharging, although the consumer protection agencies argue that it is not allowed based on the terms of use that retail firms sign with the bank that issues them the POS terminal. This type of surcharging would not be captured in the price data used here.

[39] Nevertheless, passing through costs by surcharging customers paying by card is relatively common. In the survey of corner stores I conducted, 63 percent of corner store owners with POS terminals reported surcharging consumers paying by card, and 80 percent have not changed whether or not they surcharge at any point since adopting a POS terminal.

Formality: There is also evidence that the card shock led firms to increasingly formalize: Table 4, column 9 shows the results from (5), 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 (VAT) to any of its customers or paid social security benefits for its employees. Using the pooled coefficient, the probability of formalization increased by 2.3 percentage points on a low base of 12.4 percent. Increased formalization of small retailers could be an additional societal benefit of increased financial technology adoption.[40]

V. Evidence of a Coordination Failure

A. *Survey Evidence*

To explore whether coordination failures constrain financial technology adoption, I conducted a survey of 1,760 corner store owners in 29 urban localities that were not included in the debit card rollout but that have similar levels of debit card and POS adoption as the localities included in the rollout had just before the shock. More detail about the survey is provided in Section IIE and online Appendix B.10. In the survey, I asked corner store owners who did not have POS terminals how much they thought their profits would change if they adopted a POS terminal. I then compare the cumulative distribution function of these responses to treatment effect estimates of the effect of the debit card shock on profits from the Economic Census.

Only 11–16 percent of corner store owners thought that their profits would increase by as much as the treatment effect I find (depending on whether I use the coefficient comparing localities treated 0–1.5 years ago to those treated 1.5–3 years ago or 3–4.5 years ago in Table 4). I show these results in online Appendix Figure A.17, which compares the cumulative distribution function of their expected change in profits after adopting a POS terminal to the treatment effects of the debit card shock on profits. As in Table 4, these are intent-to-treat estimates, whereas it would be more appropriate to compare treatment-on-the-treated estimates of corner stores' increase in profits to beliefs about how much profits would increase after adopting. However, the assumptions required to calculate treatment-on-the-treated effects may be violated due to potential competition and spillovers across corner stores; the intent-to-treat estimates can be thought of as a lower bound of the true treatment effect of adopting a POS terminal. Furthermore, 69 percent of corner store owners predict that they would have a negative or 0 change in profits upon adopting a point-of-sale terminal.[41]

[40] Higher formality can also lead to higher costs from tax payments, but these costs are already subtracted out of the profits measure I use here. Indeed, the debit card shock leads to a 13 percent increase in VAT payments by the firm ($p < 0.01$). To disentangle whether this increase in VAT paid by retailers is due to higher rates of formality or higher profits, I estimate (5) with VAT collected from customers divided by sales as the outcome variable. The debit card shock leads to a 0.3 percentage point increase in VAT collected as a proportion of sales ($p < 0.01$), which suggests that at least part of the increase in VAT collected is due to the increase in formality.

[41] Among those who predicted that their profits would decrease after adopting a POS terminal, in an open-ended survey question asking why their profits would decrease, the majority responded with a combination of their customers not having debit cards and the costs of the POS terminal as the most common reasons.

These results are evidence of a coordination failure: in the absence of a shock to debit card adoption, the vast majority of corner store owners in the survey thought that their change in profits would be lower than the treatment effect of the debit card shock on profits. This coordination failure could arise due to a combination of a classical coordination failure—where the benefits of adopting a POS terminal are only sufficiently large after a high enough fraction of consumers have adopted debit cards—and due to biased expectations about the benefits of adopting a POS terminal. Biased expectations would exacerbate the coordination failure by making fewer corner stores adopt than is optimal in the absence of a shock. The survey provides suggestive evidence that corner store owners do underestimate how many new customers would come to the store if they adopted; only 28 percent of corner store owners without a POS terminal thought that the number of customers coming to their store would increase after adopting, whereas 51 percent of corner store owners with POS terminals reported that their number of customers increased after adopting.

Most corner store owners who have adopted POS terminals only did so once their current customers began asking to pay by card rather than to attract new customers; when asked the main reasons they adopted a POS terminal, 59 percent said it was because customers they already had wanted to pay by card, while only 15 percent said it was to attract new customers. Furthermore, among corner stores with a POS terminal, 93 percent reported that prior to adopting, customers had asked to pay by card, and 65 percent reported that they had lost sales to customers who had asked to pay by card and left the store without purchasing anything when told they didn't accept card payments. In contrast, among corner stores without a POS terminal, only 35 percent reported that customers had asked to pay by card, and 18 percent reported that customers had left the store without purchasing anything when told they didn't accept card payments. This is consistent with responses from the focus groups I conducted, where for example, one participant answered the question about why he adopted a POS terminal with "customers would come in and tell me, 'I need to pay by card.' We started to lose sales."[42]

Taken together, this evidence suggests that a coordination failure exists and is exacerbated by corner store owners underestimating the benefits of POS adoption in terms of how many new customers would come to their store. It further suggests that corner store owners only determine it is optimal to adopt once enough of their current customers begin asking to pay by card and leaving to shop somewhere else if the store does not accept card payments. For some stores, in the absence of coordinated debit card adoption, the fraction of their customers asking to pay by card was not large enough, but after Prospera's debit card rollout led to a coordinated shock to their customers' debit card adoption, it was.

[42] On the other hand, many corner store owners do appear to understand the spillover effects of their adoption decision: when asked whether more customers would adopt debit cards if they adopted a POS terminal, 51 percent responded yes, and when asked whether more customers would adopt debit cards if many corner stores adopted POS terminals, 73 percent responded yes.

B. *Quantifying Indirect Network Externalities*

To quantify the magnitude of the indirect network externalities, I use a simple theoretical framework to estimate the fraction of consumer gains from the shock-induced supply-side POS adoption that accrued to consumers who did not directly receive debit cards from the government. The estimation of consumer gains from POS adoption requires several assumptions, and many caveats must be kept in mind when interpreting the results. These assumptions and caveats, as well as the method and results, are described in more detail in online Appendix E.

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.[43] Corner stores, on the other hand, may or may not accept card payments. Using the coefficients from this demand model, I estimate the price index–equivalent consumer gains resulting from the shock-induced change in the proportion of corner stores accepting cards. Over half of the consumer gains were spillovers to existing card holders and to nonbeneficiaries who adopted cards as a result of the shock, which implies that indirect network externalities were large. Furthermore, the aggregate value of the spillovers in the first two years was 37 times as large as the aggregate costs incurred by the Mexican government to provide debit cards.

VI. Mechanisms

A. *Indirect Network Externalities and Social Learning*

The spillover effects on other consumers' card adoption were likely driven by a combination of indirect network externalities (i.e., that the benefits of card adoption increased as more corner stores adopted POS terminals) and social or word-of-mouth learning about the benefits of debit cards as POS terminal adoption increased. An alternative is that this spillover was driven *solely* by social learning, meaning that it would have occurred independently of whether corner stores adopted POS terminals in response to the shock. Directly testing whether the spillovers on other consumers' card adoption were driven solely by social learning is difficult since many of the pathways through which social learning would occur—for example, among people with close geographic proximity—are also the channels through which the network externality would occur (since these individuals shop at the same retail stores). Nevertheless, in this section I present a number of tests that, taken together, suggest that the spillovers on other consumers' card adoption did not occur solely through social learning.

[43] The finding that supermarkets charge *more* than corner stores for identical products comes from a regression with bar code–level product-by-locality-by-month fixed effects. Specifically, I use bar-code-by-store-by-week-level price quotes, average over weeks in a month, restrict to price quotes from corner stores and supermarkets using four- or six-digit NAICS codes, and estimate $\log Price_{gst} = \lambda_{gj(s)t} + \beta \mathbf{1}\{Corner\}_s + \varepsilon_{gst}$. The results from this regression are in online Appendix Table A.9, and online Appendix F provides more detail.

Before turning to those tests, it is worth noting that debit cards were 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 social learning effect would likely need to be learning about the *benefits* of using cards, not their existence as a technology.

Heterogeneity by Social Connectedness: A measure of the extent of social connections within each municipality is available from the Social Connectedness Index (SCI), which measures connections between Facebook users (Bailey et al. 2018; Facebook 2020). Specifically, for a given municipality's set of Facebook users, I use a measure of the number of friendship connections between two users both in that municipality divided by the total number of possible friendship connections between Facebook users within that municipality. If the spillover effects were driven by social learning, we might see a larger spillover effect in more socially connected municipalities (not because the social learning would happen on Facebook, but because the SCI captures underlying social connections between people who generally know each other in the real world). I create a dummy variable for above-median within-municipality social connectedness using the SCI.

Table 2, columns 2 and 3, and online Appendix Figure A.18 show this heterogeneity test, where (1) is run separately for municipalities with below- or above-median within-municipality social connectedness. The point estimates are similar for both types of municipality, and when I test the difference in coefficients by interacting D_{jt}^k and δ_t with the above-median connectedness dummy in (1) rather than running separate regressions for above- and below-median municipalities, the coefficients on the interactions between D_{jt}^k and the heterogeneity dummy are statistically nonsignificant in all periods. (The statistical nonsignificance of these interaction coefficients is not merely due to being under-powered for heterogeneity tests, as seen in the other heterogeneity tests below.)

Heterogeneity by ATM Density: Nearly no Prospera beneficiaries had debit cards prior to receiving one from the program, and Bachas et al. (2021) document the benefits Prospera beneficiaries experience from using the debit cards at ATMs to access their transfers. Thus, if the effect were due *solely* 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, indirect network externalities are a mechanism, the relative benefit to a nonbeneficiary 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 4 and 5, and online Appendix Figure A.19 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 particular social learning channel described above, 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 percent 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 percent increase in debit card adoption (statistically significant at the 10 percent level in the quarter of the shock) but no increase thereafter; coefficients for later periods remain around 10 percent 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. When I test the difference in coefficients as above, the coefficients on the interactions between D_{jt}^{k} and the heterogeneity dummy are statistically significant in five of the nine post-shock periods.

Heterogeneity by Where Beneficiaries Shop: In some localities, the majority of beneficiaries lived close to supermarkets and thus had 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 shopped 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 shopped relatively more at supermarkets. The effect would instead be concentrated in areas where beneficiaries shopped relatively more 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 shopped at supermarkets or corner stores. I use the shopping patterns of beneficiaries within the first six months they have the card, using the Bansefi transaction-level data, to split the municipalities into two equal-sized groups: those in which the proportion of Prospera debit card transactions made at supermarkets was above median and those in which it was below median.

In municipalities where beneficiaries shopped relatively more at corner stores, where the network externality could occur, there was a large effect on other consumers' card adoption (Table 2, column 6, and online Appendix Figure A.20, panel A). The effect in these municipalities is statistically significant in all quarters after the initial quarter in which the shock occurred, and the point estimate reaches 0.47 two years after the shock. In contrast, in municipalities where beneficiaries shopped relatively more at supermarkets (which already accepted cards), there is no statistically significant effect on other consumers' card adoption. Furthermore, the (statistically nonsignificant) point estimates never exceed 0.13, which would indicate a 14 percent increase in cards (Table 2, column 7, and online Appendix Figure A.20, panel B).[44] When I test the difference in coefficients as above, the coefficients on the interactions between D_{jt}^{k} and the heterogeneity dummy are statistically significant in four of the nine post-shock periods.

[44]Note that many beneficiaries still shopped at corner stores in these municipalities, as the median municipality-level proportion of Prospera card transactions at supermarkets was 22 percent. Thus, we would not expect precise zero point estimates.

B. *Lack of Bank Response to Debit Card Rollout*

An alternative mechanism for the spillover on other consumers' card adoption would be if banks observed the debit card shock itself or the increase in POS terminal adoption in response to the debit card shock and responded by encouraging other consumers to adopt debit cards. From the banks' perspective, processing card transactions is more profitable than handling cash deposits and withdrawals, and a large fraction of the transaction fee for debit card transactions is paid to the card-issuing bank. Thus, after observing the debit card shock and increase in POS terminal adoption, banks would have an incentive to encourage further debit card adoption.

It is worth noting that the shock occurred in different localities over time, and the debit card and POS adoption responses in Figures 4 and 5 compare localities that have received the shock to those that have not yet received but will receive the card shock. Thus, to drive the results, any bank response would have to be locally targeted and restricted to the areas that had already received the debit card shock. This would likely need to be a locally targeted response by national banks, as the share of debit cards issued by the 9 largest banks in Mexico, which are all national, is 98 percent; the share of POS terminals issued by these national banks is 91 percent. It is thus unlikely that smaller local banks, which would be more likely to respond to local shocks, could drive the response.[45]

ATMs: One way to encourage debit card adoption would be to shut down ATMs, which is how banks in Singapore responded to the introduction of a new cashless payments technology (Agarwal et al. 2020). I test whether banks closed ATMs in response to the debit card shock, estimating (1) with the log number of ATMs in a municipality by quarter as the dependent variable, using CNBV data. Online Appendix Figure A.21, panel A shows that there were no statistically significant changes to the number of ATMs after the card shock throughout the entire period; furthermore, the point estimates are quite close to zero for the first year and a half after the card shock, whereas there was a substantial effect on POS adoption and other consumers' debit card adoption over the first year and a half after the card shock.

Transaction Fees: Banks could also respond by changing the fees merchants are charged for processing payments through POS terminals. There are three components to this cost: the fixed cost of adopting the POS terminal, a monthly rental cost that is waived if the volume of POS transactions exceeds a threshold, and the transaction fee. While I only have data on the third of these costs (Banco de México 2006–2018), the fixed adoption cost and monthly rental fee are unlikely to have changed in response to the shock: most Mexican banks charge a uniform adoption cost and monthly rental cost that do not differ by geographic area, and they post these prices online.[46] The third of these costs, the transaction fee, is also largely set

[45] For the purposes of this calculation, "national banks" are defined as banks with branches in every state in Mexico.

[46] For Mexico's largest bank, BBVA, the fixed adoption cost and monthly rental fee posted online have not changed over the past four years, further highlighting that it is unlikely banks are changing these fees in response to shocks.

nationally by banks; I nevertheless test whether transaction fees responded to the debit card shock by estimating (1) using log transaction fees for retail firms, constructed using data from Mexico's Central Bank, as the dependent variable. I find no evidence of changes in transaction fees in response to the debit card shock (online Appendix Figure A.21, panel B).

Debit Card Account Fees: Banks could potentially respond by lowering the fees they charge for issuing debit cards (or other fees related to debit card accounts). However, this is unlikely, as Mexico's Central Bank regulates that all banks must offer a no-fee "basic account" that includes a debit card and charges no fees and has no minimum initial deposit or minimum balance.[47] Furthermore, to change the fees charged for nonbasic accounts (as well as to change the fees for other financial products, such as credit cards), by law, banks must submit the fee change to the regulatory arm of Mexico's Central Bank and justify the fee change based on a change in the costs faced by the bank. As a result of these factors, it is unlikely that banks would respond by changing fees charged to the consumer for obtaining or using a debit card.

VII. Conclusion

Due to the network externalities of financial technologies—which arise from the interactions between consumers' and retail firms' financial technology adoption in a two-sided market—the spillovers of consumer financial technology adoption could be large. As a result, assessing the overall effects of financial technologies requires quantifying not only the direct effect on consumers who adopt these technologies but also how the supply side of the market responds to their adoption and how this response feeds back to the demand side. Because two-sided markets can generate coordination failures, the increase in financial technology adoption likely needs to be large and coordinated within local markets, requiring large-scale natural experiments or randomized controlled trials (as advocated by Muralidharan and Niehaus 2017) to study their effects.

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 percent 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 percent of their supermarket consumption to corner stores. Corner stores, in turn, benefited from the demand shock: their profits increased due to their ability

[47] The regulation is available at https://www.banxico.org.mx/CuentasBasicas/; in Section I "Minimum services that banks must offer without charging fees," the first two items are "opening and maintaining the account" and "issuing a debit card and replacing it due to wear or renewal."

to turn over more inventory, increasing both sales and inventory costs while keeping other input costs fixed.

Governments and nongovernmental 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 or into mobile money accounts (e.g., Muralidharan, Niehaus, and Sukhtankar 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 coordination 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 financial technology adoption on both sides of the market that benefits both consumers and small retail firms.

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