

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

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## Abstract

Do coordination failures constrain financial technology adoption? Exploiting the Mexican government's rollout of one million debit cards to poor households from 2009–2012, 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% 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% of their supermarket consumption to small retailers, whose sales and profits increased.

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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 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.<sup>1</sup> 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 one 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.

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<sup>1</sup>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.

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%) in one week.<sup>2</sup> 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 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 pre-treatment 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 data sets on both consumers and retailers. The key data set on supply-side financial technology adoption is a confidential data set on the universe of POS terminal adoptions by retailers over a twelve-year period, accessed on-site at Mexico’s Central Bank. For spillovers on other consumers, the two key data sets 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 data sets: 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 barcode 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% during the two-month period in which the shock occurred.<sup>3</sup> Adoption continued to increase over time: two years after the shock, 18% 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.

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<sup>2</sup>In the median treated locality, 36% of households had a debit or credit card prior to the shock (based on household survey data), and the shock increased the proportion of households with a card to 54%. 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.

<sup>3</sup>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.

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 6 months after the shock occurred, the number of cards held by other consumers increased by 19%. Two years after the shock, 28% 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 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% to 63% 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.<sup>4</sup>

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% 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).<sup>5</sup>

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 five-year period between survey rounds, corner store sales increased by 6% more in earlier-treated localities. Corner stores increased the

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<sup>4</sup>In China, debit card adoption increased from 41% in 2011 to 67% in 2017 (Demirgüç-Kunt et al., 2018). In the US, adoption increased from 34% in 1998 to 59% 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 IV.B).

<sup>5</sup>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 data set in this paper I also find evidence of a reduction in overall consumption by Prospera beneficiaries as a result of receiving a debit card.

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% 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 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.<sup>6</sup>

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 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%—compared to 50% worldwide (Demirgüç-Kunt et al., 2018). The proportion of the population with a debit or credit card is also highly correlated with income, as shown in Figure 1a. In urban areas, 12% of households in the bottom income quintile had a debit or credit card prior to the Prospera debit card rollout, compared to 54% of households in the top income quintile. On the supply side of the market, 32% of retailers in urban areas had adopted POS

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<sup>6</sup>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, forthcoming). 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.

terminals prior to the rollout, including 26% of corner stores and nearly 100% of supermarkets.

Figure 1b shows the cross-sectional municipality-level correlation between adoption on each side of the market: the y-axis shows the proportion of retailers with POS terminals, and the x-axis shows the number of debit cards per person.<sup>7</sup> 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 any bank's ATM and to make purchases at POS terminals at any merchant accepting Visa.<sup>8</sup> 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 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

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<sup>7</sup>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).

<sup>8</sup>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% of all retail consumption.

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 check-ups. 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% 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.” 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% 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.<sup>9</sup> 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% of beneficiaries

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<sup>9</sup>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.



used their cards to make POS transactions, and that the proportion actively using the cards at POS terminals increased steadily over time, reaching 47% of beneficiaries after they had the card for 3 years (Figure A.1).

In addition to about 35% 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% 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 3 years to 72%, 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 terminals), beneficiaries initially spent 128 pesos per two-month period at corner store POS terminals, which can be compared to an average Prospera transfer amount of 1,636 pesos in the two-month period in which they received cards. Conditional on making a POS transaction at a corner store, 25% 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% of their total withdrawals after having the card for 3 years (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 towards 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% per year, the rollout was not accompanied by a differential change in the number of beneficiaries or transfer amounts (Appendix C.2 and Figure A.3a). 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.<sup>10</sup> The terminal has a low upfront cost of US\$23, but includes a monthly rental fee of US\$27 per month if the business does not transact

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<sup>10</sup>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.

at least US\$2000 per month in electronic sales through the terminal. This constraint would bind for about 95% of corner stores. In addition, there is a proportional transaction fee that varies by sector and bank; it was 1.75% for retailers at this large commercial bank during the period of the card rollout. For most corner stores, the monthly fee would swamp the transaction fee: as a percent of total (cash and non-cash) sales, the median corner store would pay 0.5% in transaction fees and 3.2% in monthly fees.<sup>11</sup>

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 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.<sup>12</sup>

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% of corner store owners who had adopted POS terminals reported higher sales after adoption. In addition, 51% of corner store owners reported attracting new customers once they began accepting card payments. The majority (65%) 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).

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<sup>11</sup>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% for corner stores. The estimate of fees as a fraction of sales are based on the same combination of data sources.

<sup>12</sup>In contrast, in the US, 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.

## II. Data

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

### *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. Examples of payment methods include cash, bank account without debit card, and bank account with debit card.<sup>13</sup> 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. In addition, I observe when each account holder receives a debit card. Across all transaction types (including cash withdrawals, card payments, deposits, interest payments, and fees), the data set includes 106 million transactions. I use this data set to measure whether the beneficiaries who directly received cards 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.

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<sup>13</sup>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.

### *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 data set includes all changes to POS contracts between retailers and banks from 2006–2017, where contract changes include adoptions of POS terminals, cancellations, and changes to the fee structure or other contract parameters. The data include the store type (more precisely, the merchant category code) and a geographic identifier (postal code).<sup>14</sup> In total, the data set includes over five million contract changes, 1.7 million of which are adoptions. I use both the adoptions and cancellations—combined with another data set that allows me to back out existing POS terminals prior to 2006 that had no contract changes over the period for which I have data—to construct a data set with the stock of POS terminals in each locality by store type by two-month period.

**Universe of card transactions at POS terminals.** These data were also accessed on-site at Mexico’s Central Bank, and include card transactions made at a POS terminal between July 2007 and March 2015. The data include an average of 1.7 million card transactions per day, for a total of 4.7 billion transactions. For each transaction, I know the date of the transaction, amount of pesos spent, the store type (merchant category code) of the business, and the name of the locality in which the business is located. The data only include the universe of card transactions through mid-2013, as some banks shifted to a different transaction clearing house 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 post-treatment 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.<sup>15</sup> 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

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<sup>14</sup>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. Appendix B explains how I map from postal codes, the geographic identifier in this data set, to localities, the relevant geographic area for the card rollout.

<sup>15</sup>Gender-disaggregated data on the number of debit cards is 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.

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.<sup>16</sup> 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.

**Google searches for supermarkets.** I use data on Google searches for large supermarket chains in Mexico to corroborate the findings from the consumption survey with higher-frequency data. While Google Trends data is not available at a geographic level below the state level in Mexico for the relevant time 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 data set 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 data set is provided in Appendix B.7. I also show in 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.

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<sup>16</sup>Table A.1 shows the distribution of when the localities of the 49,810 surveyed households were treated and when the households were surveyed.

#### *D. Retail Outcomes and Prices*

**Retail outcomes.** Every five years, INEGI conducts an Economic Census of the universe of firms in Mexico. This census includes all retailers, regardless of whether they are formally registered (with the exception of vendors who do not have a fixed business establishment, such as street vendors). Firm type and store type are determined in this data set using six-digit North American Industry Classification System (NAICS) codes.<sup>17</sup> 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.<sup>18</sup> 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 non-food items). Importantly, the goods are coded at the barcode-equivalent level (such as “600ml bottle of Coca-Cola”), which helps to disentangle price and quality differences between different types of store—for example, larger stores sell larger pack sizes or higher-quality varieties (Atkin, Faber and Gonzalez-Navarro, 2018). After averaging price quotes across two-month periods for consistency with Prospera’s payment periods, the data set includes 5.4 million observations from 2002–2014.

#### *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. The survey was conducted from June–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 sam-

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

<sup>18</sup>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.)

ple 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 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 non-selected localities to never receive Prospera debit cards; *ex post*, the non-selected localities never received debit cards.<sup>19</sup> 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 (Figure A.2a). 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 one million beneficiaries had received cards (Figure A.2b). Since—as I show below—the timing of the shock is not correlated with levels or trends in locality-level financial infrastructure or other observables (conditional on being included in the rollout), but the initial selection of which localities to include in the rollout *is* correlated with locality characteristics, I restrict all estimates to the set of 259 urban localities included in the rollout.

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

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

changing treatment effects over time:

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

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  $\lambda_j$  to capture arbitrary time-invariant heterogeneity across localities and time fixed effects  $\delta_t$  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 data set being used (i.e.,  $a = -18, b = 24$ ). Additional details about this specification are included in Appendix C.1.<sup>20</sup>

I conduct two sets of tests to determine whether the timing of the rollout is correlated with trends or levels of financial infrastructure or other locality-level observables. First, Figure 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 data sets, 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). 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 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

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<sup>20</sup>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 data sets.



using INEGI data. Of the 40 variables, including both levels and trends, only two are correlated with the timing of the rollout, as can be expected by chance: the coefficient on the proportion of households without plumbing is statistically significant at the 5% level and the coefficient on the percent of children not attending school is statistically significant at the 10% level (Table 1).

Third, since some of the most interesting results are those on changes to the sales and profits of corner stores and supermarkets which 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 1993–2008 (see Appendix C.4 for details). Figures A.4 and A.5 show the results. When comparing pre-trends across nine 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% level, as could be expected by chance.

## IV. Results

### A. POS Terminal Adoption by Retailers

Using the data set 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% and 26% of retail consumption, respectively.<sup>21</sup> In the Central Bank transactions data, card transactions at corner stores and supermarkets made up 54% of all card transactions at POS terminals.

I estimate (1) with the log number of POS terminals at corner stores, supermarkets, or all other businesses in locality  $j$  during two-month period  $t$  as the dependent variable. The estimation is restricted to urban localities included in the card rollout; 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% 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 adop-

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<sup>21</sup>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.

tion after the debit card shock of about 3%. This rose to about 18% two years after the shock; all coefficients after the shock are positive and statistically significant for corner stores (Figure 4a).<sup>22</sup> For supermarkets—which made up another 26% of retail consumption—all but one pre-treatment coefficient are statistically nonsignificant from 0, but there was no effect of the card shock (Figure 4b). This finding is not surprising as supermarkets already had high rates of adoption prior to the debit card shock: in the National Enterprise Financing Survey, 100% of supermarkets reported accepting card payments. Similarly, there is neither a pre-trend nor effect of the card shock for all other businesses, which made up the remaining 26% of retail consumption (Figure 4c).<sup>23</sup>

### *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 data set because the data are at the bank by municipality level. The estimation is restricted to urban municipalities included in the card rollout. Figure 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%. 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% increase in the following quarter corresponds to other consumers' debit card adoption nearly six months after the shock. Treated localities had 28% more non-Bansefi debit cards two years after the

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

<sup>23</sup>These results are also shown in table form in Table A.2. 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.

shock.<sup>24</sup>

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 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% 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% 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 3–6 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, 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.

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 0 and not statistically significant in the first two quarters (measured nearly 3 and 6 months after the shock occurred). In contrast, in municipalities with an above-median immediate POS adoption response by corner stores, nearly 3 months after the shock occurred the point estimate shows 19% higher adoption of debit cards by other consumers ( $p = 0.11$ ), and in the following quarter nearly 6 months after the shock occurred there is 33% higher debit card adoption by other consumers ( $p = 0.02$ ). When I test the difference in coefficients between municipalities with above vs. below-median immediate POS adoption response by interacting  $D_{jt}^k$  and  $\delta_t$  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% 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

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<sup>24</sup>Figure A.7a 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. Figure A.7b shows that results are robust to using the log number of credit and debit cards rather than the log number of just debit cards.

(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.<sup>25</sup> 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 2-month period after the card shock, there is a small 6% increase in transactions (statistically significant at the 10% 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% increase in other consumers' transactions at POS terminals that is statistically significant at the 5% 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% 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 municipality by month level with the Bansefi transactions-level data to subtract out ATM withdrawals by Prospera beneficiaries.<sup>26</sup> 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.<sup>27</sup> On the other hand, as corner stores adopted POS terminals, consumers

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<sup>25</sup>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 clearing house. Because the debit card shock ended in mid-2012, I am thus only able to show effects up to one year after the shock.

<sup>26</sup>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 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.

<sup>27</sup>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).

who already had cards prior to the shock may withdraw cash less frequently as they use their cards more for transactions at POS terminals. 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% two to six months after the shock occurred, as consumers shifted from cash to card transactions. After 6 months, the coefficients are no longer statistically significant in most periods but the point estimates remain between  $-8\%$  and  $-11\%$ .

### 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

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

where  $y_{it}$  is the outcome (such as log spending at corner stores or the number of trips per week to corner stores) for household  $i$  in survey wave  $t$ ,  $\lambda_{j(i)}$  is a set of locality fixed effects,  $\delta_t$  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  $t$ .<sup>28</sup>

Part of the card rollout overlaps with the ENIGH data collection. Specifically, 10% 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% 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

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<sup>28</sup>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% random sample from the universe of 80 million voter registrations in Mexico), Bachas et al. (2021) find that only 4.5% of residents migrated from one locality to another over a three-year period.

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.<sup>29</sup> 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. 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% 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% (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).<sup>30</sup>

**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

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

<sup>29</sup>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 5 households out of 49,810 that were surveyed in 2012 in localities treated in May–June 2012.

<sup>30</sup>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. 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% level rather than at the 5% 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.

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 *base-line* 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  $\omega$  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% larger increase in spending at corner stores and a 6% 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% 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

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

where  $\theta_{q(i)t}$  is a full set of income quintile by time fixed effects and  $\mathbb{I}(\text{quintile} = q)_{it}$  is a set of dummies that equal 1 if household  $i$  from survey wave  $t$  belongs to income quintile  $q$ , with  $q = 1$

as the omitted category.<sup>31</sup>

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% and increased their consumption at corner stores by 15% in response to the debit card shock and subsequent POS adoption by corner stores. The second-richest quintile also appears to have increased its consumption at corner stores (by 8%, significant at the 10% level), while the results for the poorest three quintiles are statistically nonsignificant from zero (Figure 6a and 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 6b and 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 consumed 24% of total consumption at corner stores and 17% at supermarkets. Thus, the magnitudes of the 15% increase in corner store consumption and 13% decrease in supermarket consumption come fairly close to lining up, each representing 2.2–3.5% of total consumption.

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. 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 Tables A.5 and A.6. The product categories where there was 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 Figure A.11 shows results for total consumption across all store types; all but one of the 16 coefficients are statistically nonsignificant, indicating that households in the richest quintile likely did not sub-

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<sup>31</sup>Income quintiles are estimated separately within each survey year (i.e.,  $q = 1$  corresponds to the poorest 20% of households in each survey wave). Since all localities included in (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.



stantially 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.

Figure A.12a 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.<sup>32</sup> 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% 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.<sup>33</sup>

**Alternative explanations.** In 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 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 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.

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

<sup>33</sup>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 Figure A.12a 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 Figure A.12a), I conduct a placebo test using Google searches for the common search term “weather” in Figure A.12b, 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.

#### *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 IV.C; furthermore, I conduct locality-level parallel trends tests using the 1993–2008 Economic Census waves in Figures A.4 and A.5.

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

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

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.<sup>34</sup>

Corner stores in localities treated 3–4.5 years before the second census wave experienced increases in sales of 8% relative to corner stores in the latest-treated localities (statistically significant at the 5% level), while those in localities treated 1.5–3 years before the second census wave have a statistically nonsignificant point estimate of a 5% sales increase (Table 4, panel A, column 1). The

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<sup>34</sup>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.

pooled estimate shows that corner stores in earlier-treated localities experienced a 6% increase in sales relative to those in later-treated localities (statistically significant at the 10% 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.<sup>35</sup> This increase in corner store sales came at the expense of supermarkets, which experienced a 12% decrease in sales (statistically significant at the 5% level; Table 4, panel B, column 1). While the sales of each supermarket are much higher on average than those of each corner store, there are also thirteen times as many corner stores as supermarkets. In aggregate, the 6% increase in sales at the average corner store and 12% decrease in sales at the average supermarket line up very closely, since aggregate corner store sales 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% in earliest-treated localities, significant at the 10% level), while the amount spent by supermarkets on purchasing inventory decreased (by 14% in earliest-treated localities, significant at the 5% 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).<sup>36</sup> As a result, corner store profits increased by 19% 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.<sup>37</sup>

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<sup>35</sup>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), 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.

<sup>36</sup>The coefficients are statistically nonsignificant from 0 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% and an increase in the number of employees greater than 0.9%. 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 0, 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%. 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 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%) and I can rule out a reduction in supermarket wages as a result of the card shock of more than 3% (Table A.7). 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 0.

<sup>37</sup>The standard errors in Table 4 are asymptotic cluster-robust standard errors, clustered at the locality level. I also

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 (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 towards 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 (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., 2015; Gupta, 2022), or (iii) pass-through of the costs of POS terminals to all of their customers.<sup>38</sup> 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:

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

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

Figure A.16 shows the results. All of the  $\phi_k$  coefficients are statistically nonsignificant from 0 for both corner stores and supermarkets, both before and after the card shock. Using each estimate’s 95% confidence interval, I can rule out price effects outside of the range  $[-1.7\%, 1.1\%]$  during the first ten months after the shock and outside of the range  $[-2.5\%, 2.4\%]$  during the first

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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 2000 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. 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% level in Table 4 are still significant at the 10% 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.

<sup>38</sup>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.

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% at corner stores and greater than 0.7% at supermarkets after the card shock (Table A.7). Consistent with this, in the survey of corner stores I conducted, only 3% 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 US.<sup>39</sup>

**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%. Increased formalization of small retailers could be an additional societal benefit of increased financial technology adoption.<sup>40</sup>

## 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 II.E and 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.

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

<sup>40</sup>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% 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.

Only 11–16% 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 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% of corner store owners predict that they would have a negative or zero change in profits upon adopting a point-of-sale terminal.<sup>41</sup>

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% of corner store owners without a POS terminal thought that the number of customers coming to their store would increase after adopting, whereas 51% 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% said it was because customers they already had wanted to pay by card, while only 15% said it was to attract new customers. Furthermore, among corner stores with a POS terminal, 93% reported that prior to adopting, customers had asked to pay by card, and 65% 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% reported that customers had asked to pay by card, and 18% reported that customers had left the store without purchasing anything when

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<sup>41</sup> 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.

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."<sup>42</sup>

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.

### *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 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.<sup>43</sup> 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 non-beneficiaries

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<sup>42</sup>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% responded yes, and when asked whether more customers would adopt debit cards if many corner stores adopted POS terminals, 73% responded yes.

<sup>43</sup>The finding that supermarkets charge *more* than corner stores for identical products comes from a regression with barcode-level product by locality by month fixed effects. Specifically, I use barcode 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  $\log Price_{gst} = \lambda_{gj(s)t} + \beta \mathbb{I}(\text{Corner})_s + \epsilon_{gst}$ . The results from this regression are in Table A.9, and Appendix F provides more detail.

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.

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). Specifically, for a given municipality's set of Facebook users, I use a measure of the 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 Figure A.18 show this heterogeneity test, where (1) is run sep-



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

Table 2, columns 4 and 5, and 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% increase in other consumers' debit card adoption. In municipalities with above-median baseline ATM density, on the other hand, there appears to be a smaller, immediate 10% increase in debit card adoption (statistically significant at the 10% level in the quarter of the shock) but no increase thereafter: coefficients for later periods remain around 10% but are no longer statistically significant. This smaller, immediate increase in other consumers' card adoption in low-ATM areas could be due to social learning. 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 6 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 large effect on other consumers' card adoption (Table 2, column 6, and Figure A.20a). 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% increase in cards (Table 2, column 7, and Figure A.20b).<sup>44</sup> 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.

### *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.

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<sup>44</sup>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%. Thus, we would not expect precise 0 point estimates.

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, any bank response driving the results would have to be a locally-targeted response that banks 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%; the share of POS terminals issued by these national banks is 91%. It is thus unlikely that smaller local banks, which would be more likely to respond to local shocks, could drive the response.<sup>45</sup>

**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. Figure A.21a 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 0 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, 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 does not differ by geographic area, and they post these prices online.<sup>46</sup> The third of these costs, the transaction fee, is also largely set 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 (Figure A.21b).

**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 Mex-

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<sup>45</sup>For the purposes of this calculation, “national banks” are defined as banks with branches in every state in Mexico.

<sup>46</sup>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.

ico’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.<sup>47</sup> Furthermore, to change the fees charged for non-basic 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% relative to not-yet-treated localities. Other consumers responded to the increase in retailers’ financial technology adoption in two ways. Some—who likely already shopped at the corner stores that were now adopting POS terminals—adopted debit cards. Richer consumers—who mostly already had cards—shifted 13% of their supermarket consumption to corner stores. Corner stores, in turn, benefited from the demand shock: their profits increased due to their ability to turn over more inventory, increasing both sales and inventory costs while keeping other input costs fixed.

Governments and non-governmental organizations (NGOs) around the world are increasingly fostering financial technology adoption by their poorest citizens, often by paying government welfare payments into bank accounts tied to debit cards or into mobile money accounts (e.g., Muralidharan, Niehaus and Sukhtankar, 2016). However, because many financial technologies have

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<sup>47</sup>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.”

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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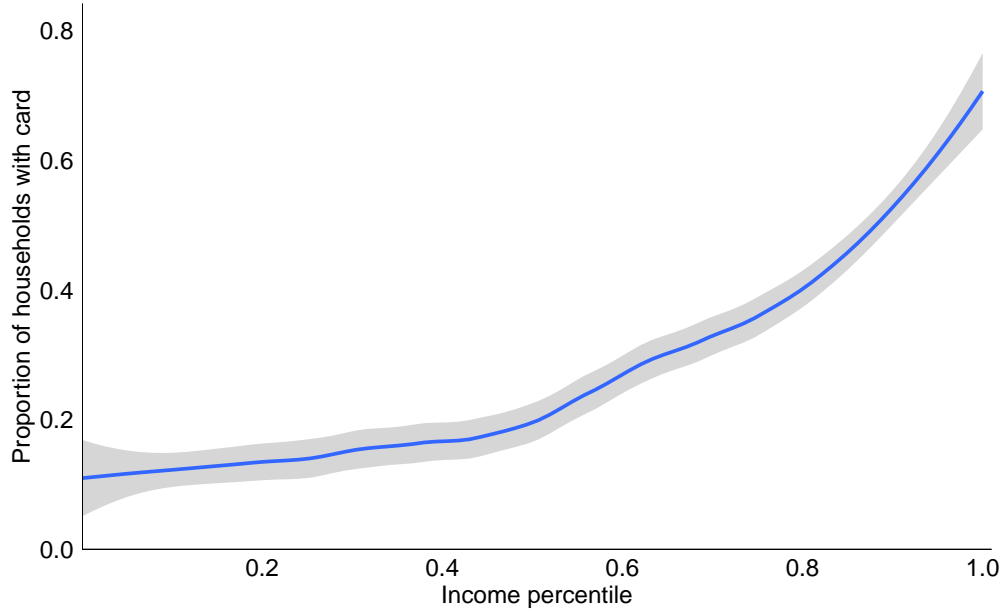
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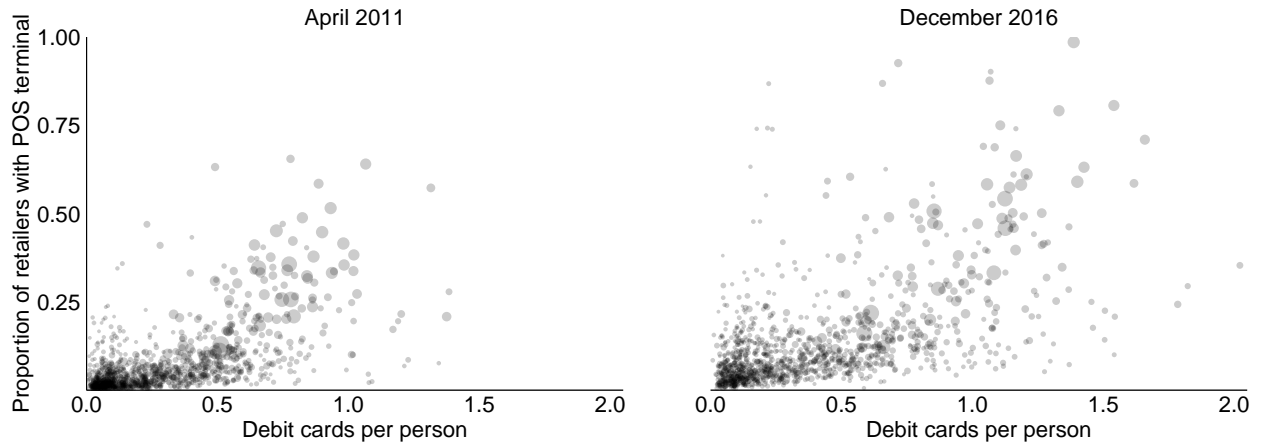
## Figures and Tables

Figure 1: Financial technology adoption in Mexico

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



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

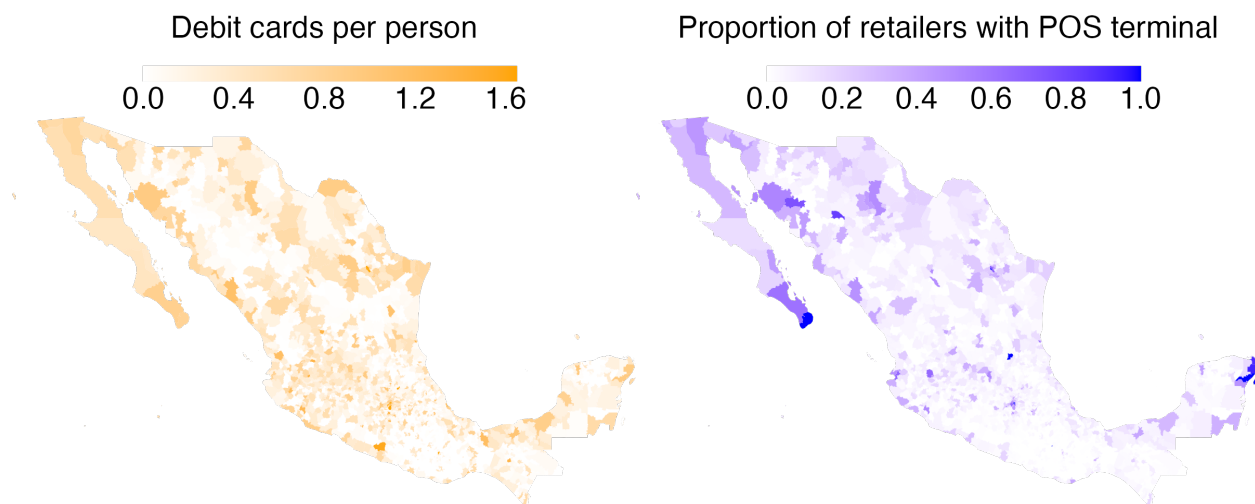


This figure shows that card adoption is highly correlated with income, and that adoption of POS terminals and cards within a municipality are highly correlated. Panel a shows the proportion of urban households with a debit or credit card across the income distribution using data from the 2009 Mexican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

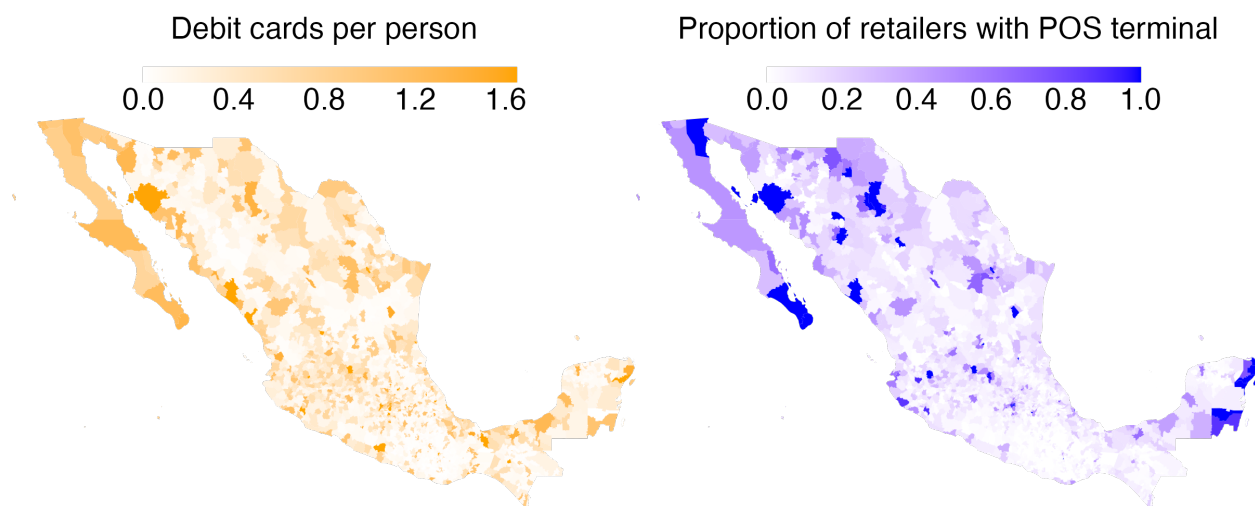


Figure 2: Concentration of cards and POS terminals over space and time

(a) April 2011

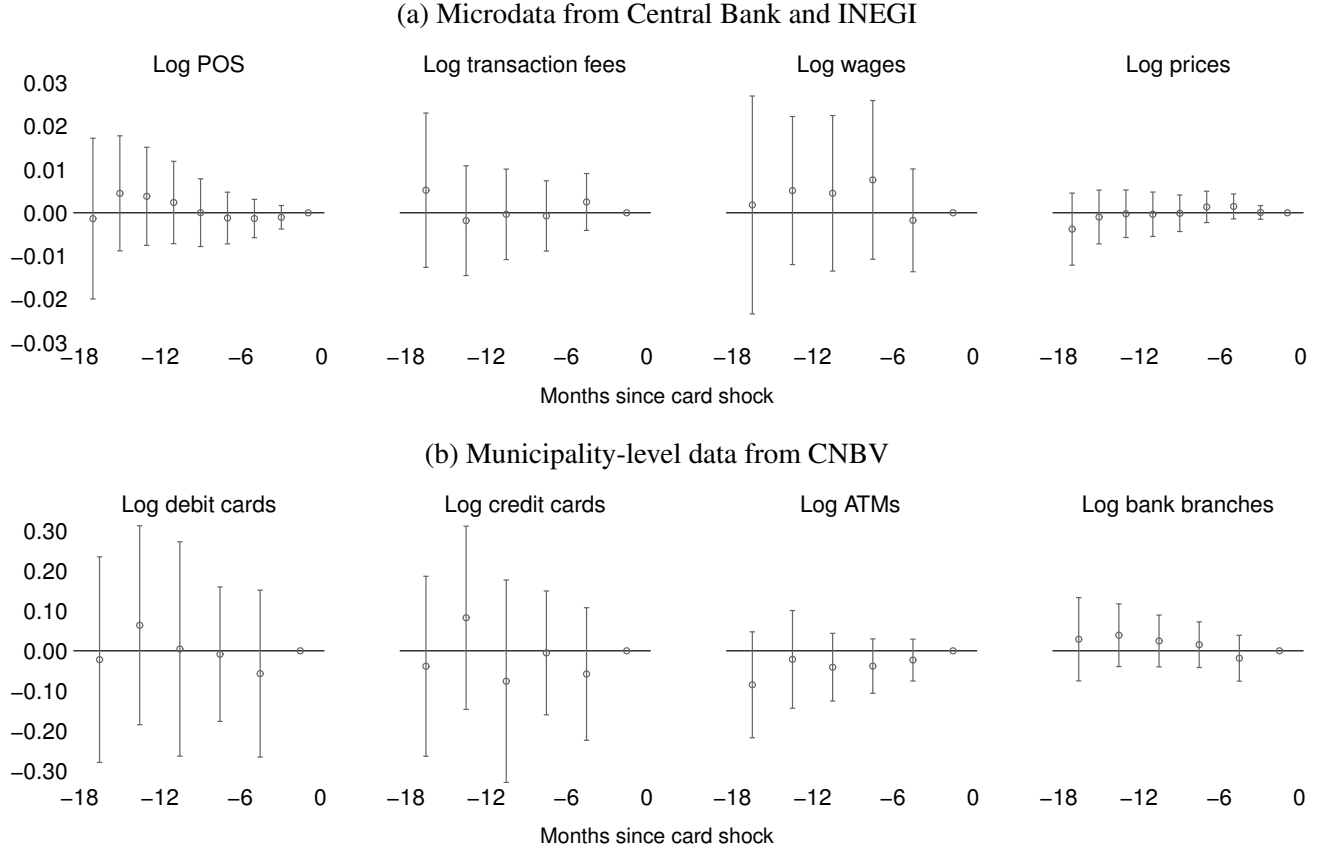


(b) December 2016



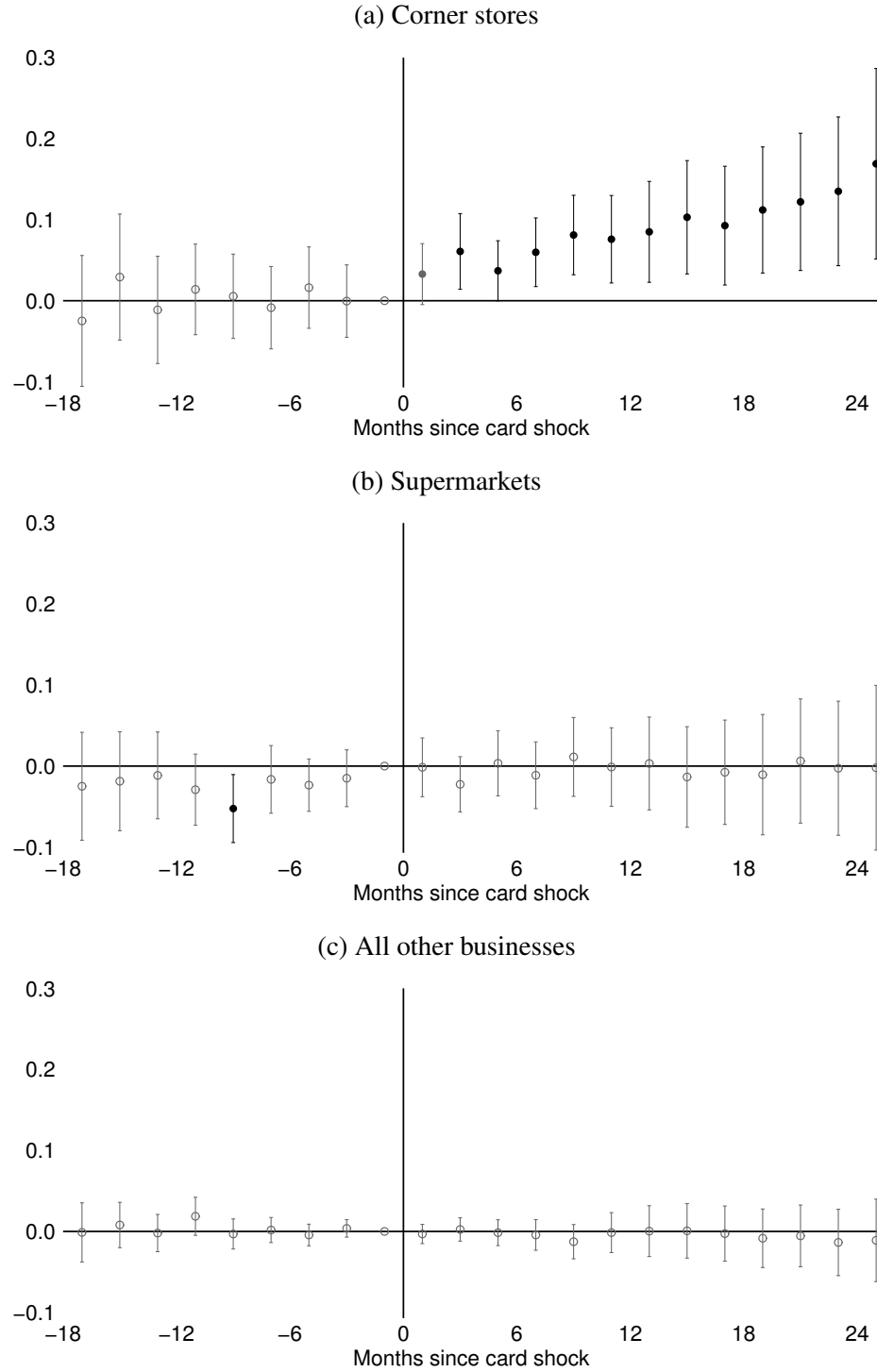
This figure shows the municipality-level number of debit cards per person (constructed as the number of debit cards using CNBV data divided by the population using INEGI data) and proportion of retailers accepting cards (constructed as the number of businesses with POS terminals using CNBV data divided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

Figure 3: Balanced pre-trends



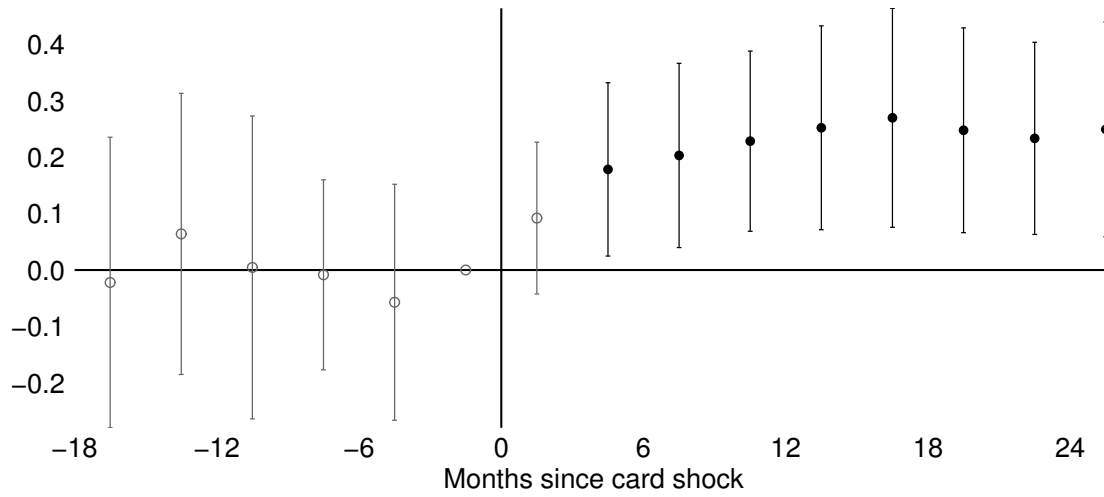
This figure shows parallel pre-trends in variables from data on POS terminal adoptions from Mexico's Central Bank, data on merchant fees charged by bank over time from Mexico's Central Bank, data on wages from INEGI's labor force survey, data on prices from INEGI, and municipality-level data on financial variables (debit cards, credit cards, ATMs, and bank branches) from CNBV. Point estimates are  $\phi_k$  for  $k < 0$  from (1), where  $k = -1$  is the omitted period. In the POS terminals regression the data are aggregated to the locality level and each observation is a locality by two-month period ( $N = 8,806$ ); in the transaction fees regression the data are aggregated to the municipality level and each observation is a municipality by quarter ( $N = 7,823$ ); in the wages regression each observation is a worker by quarter but since the panel only lasts five quarters for each worker, municipality but not worker fixed effects are included ( $N = 4,404,678$ ); 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 ( $N = 4,107,314$ ); in each regression in panel b, each observation is a municipality by quarter ( $N = 8,243$ ). The frequency of  $\phi_k$  coefficients depends on the frequency of each data set. Standard errors are clustered at the locality level, except when data are at the municipality level in which case they are clustered at the municipality level, and 95% confidence intervals are shown. Filled black circles indicate statistically significant at the 5% level, filled gray circles at the 10% level, and hollow gray circles indicate not statistically significant.

Figure 4: Effect of card shock on log POS terminals



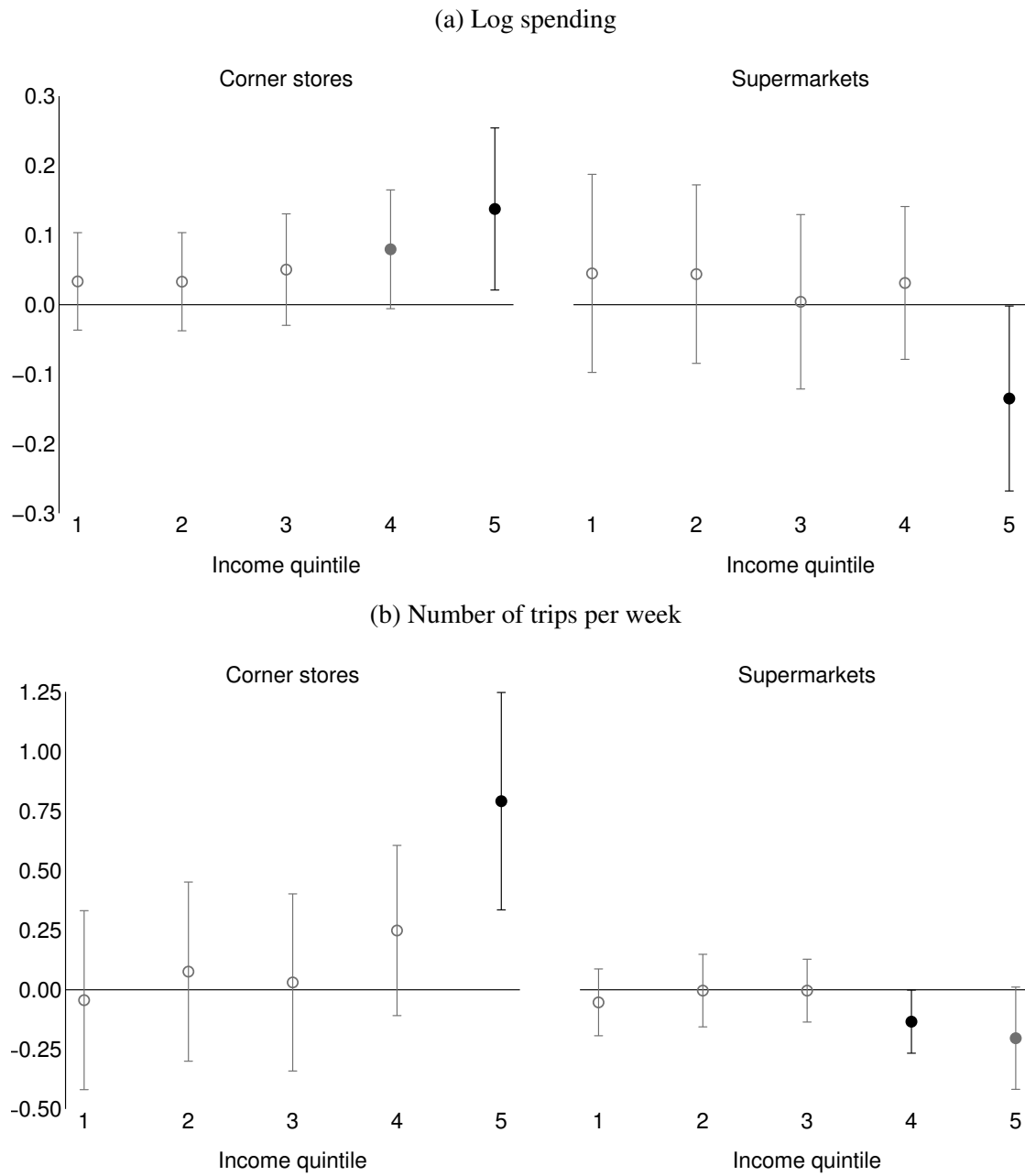
This figure shows the effect of the debit card shock on the stock of point-of-sale (POS) terminals at the two types of retailers that make up the majority of consumption: corner stores (panel a) and supermarkets (panel b), as well as all other businesses (panel c). It graphs the coefficients from (1), where the dependent variable is the log stock of point of sale terminals by type of merchant (corner store, supermarket, or other) in locality  $j$  at two-month period  $t$ , using data on the universe of POS terminal adoptions and cancellations from Mexico's Central Bank. Observations are at the locality by two-month period level.  $N = 8,806$  locality by time observations from 259 localities. Standard errors are clustered at the locality level, and 95% confidence intervals are shown. Filled black circles indicate statistically significant at the 5% level, filled gray circles at the 10% level, and hollow gray circles indicate not statistically significant. The same results can be found in Table A.2.

Figure 5: Spillovers on other consumers' card adoption



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.  $N = 8,243$  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% confidence intervals are shown. Filled black circles indicate statistically significant at the 5% level, filled gray circles at the 10% level, and hollow gray circles indicate not statistically significant. The same results can be found in Table 2.

Figure 6: Spillovers on consumer spending across store types by income quintile



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.  $N = 49,810$  households from 220 localities. Standard errors are clustered at the locality level, and 95% confidence intervals are shown. Filled black circles indicate statistically significant at the 5% level, filled gray circles at the 10% level, and hollow gray circles indicate not statistically significant. The same results can be found in Table A.4.

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

<i>Panel A: Banco de México, CNBV, population, Prospera, and electoral data</i>	(1)	(2)	(3)	<i>Panel B: INEGI measures used to track development</i>	(1)	(2)	(3)
Variable	Mean	Standard deviation	Discrete time hazard	Variable	Mean	Standard deviation	Discrete time hazard
Log point-of-sale terminals	5.82	1.84	0.006 (0.007)	% illiterate (age 15+)	6.13	3.94	0.007 (0.005)
$\Delta$ log point-of-sale terminals	0.68	0.17	-0.012 (0.026)	$\Delta$ % 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)
$\Delta$ log bank accounts	2.07	4.02	0.001 (0.004)	$\Delta$ % 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)
$\Delta$ log commercial bank branches	0.65	0.97	-0.009 (0.018)	$\Delta$ % 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)
$\Delta$ log government bank branches	0.18	0.41	0.001 (0.016)	$\Delta$ % 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)	$\Delta$ % 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)
$\Delta$ log population	0.10	0.18	-0.021 (0.031)	$\Delta$ % without toilet	-0.02	0.04	-0.024 (0.167)
Log Prospera beneficiaries	7.09	1.11	-0.003 (0.010)	% without water	6.23	9.00	0.000 (0.001)
$\Delta$ log Prospera beneficiaries	0.07	0.38	-0.000 (0.015)	$\Delta$ % without water	-0.04	0.05	0.088 (0.109)
% vote share PAN	29.01	15.00	0.000 (0.001)	% without plumbing	3.62	6.20	0.004 (0.002)
$\Delta$ % vote share PAN	-0.51	17.49	0.001 (0.001)	$\Delta$ % without plumbing	-0.06	0.06	0.111 (0.139)
Mayor = PAN ( $\times 100$ )	19.31	39.55	-0.000 (0.000)	% without electricity	4.32	2.19	0.006 (0.006)
$\Delta$ mayor = PAN ( $\times 100$ )	-11.97	58.17	0.000 (0.000)	$\Delta$ % without electricity	0.02	0.03	0.109 (0.629)
				% without washing machine	33.81	14.47	0.001 (0.001)
				$\Delta$ % without washing machine	-0.10	0.05	-0.017 (0.252)
				% without refrigerator	17.31	10.13	-0.002 (0.001)
				$\Delta$ % without refrigerator	-0.08	0.06	0.043 (0.268)

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

Table 2: Spillovers on other consumers' card adoption

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

This table shows spillovers within the demand side of the market onto other consumers' adoption of debit cards. It shows the coefficients from (1), where the dependent variable is the log stock of debit cards (excluding debit cards issued by Bansefi) in a municipality by quarter, using data from CNBV. Observations are at the municipality by quarter level since the CNBV data is at the issuing bank by municipality by quarter level. Column 1 shows the main estimates. Columns 2–3 show heterogeneity by the within-municipality Social Connectedness Index, which measures how connected the set of Facebook users in a municipality are to one another. Columns 4–5 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 6–7 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 6 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 SCI 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 6 months with the card, and hence the heterogeneity variable is missing for those municipalities. Standard errors are clustered at the municipality level.

Table 3: Spillovers on consumer spending across store types

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

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



Table 4: Retail firm outcomes

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

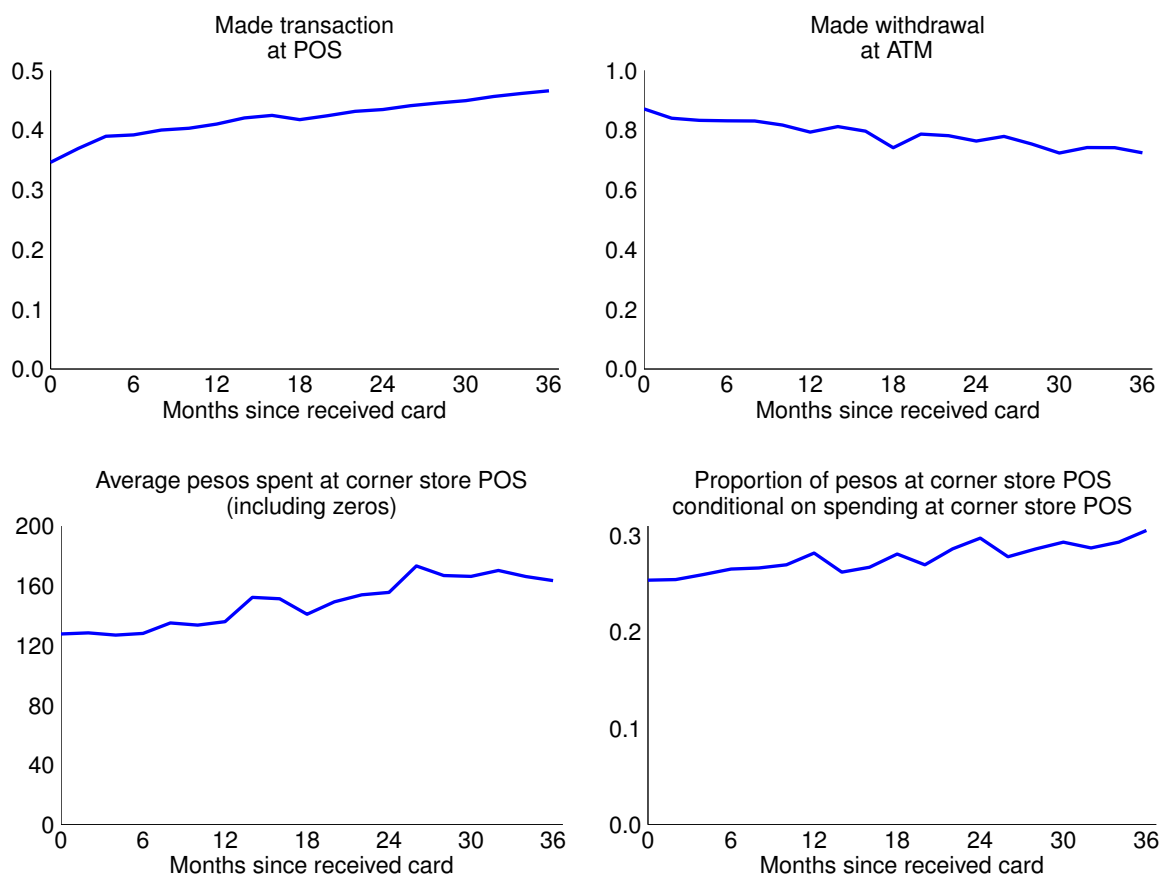
This table shows that the debit card shock led to an increase in corner store sales at the expense of supermarket sales. Corner stores also increase their inventory costs while keeping other input costs fixed, which leads to an increase in profits. The table shows intent-to-treat estimates of the effect of the card shock on various outcomes listed in the column headings for corner stores (panel A) and supermarkets (panel B), using firm panel data from the 2008 and 2013 Economic Census. It shows results from (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.

**Online Appendix**  
Financial Technology Adoption:  
Network Externalities of Cashless Payments in Mexico

Sean Higgins

## A Figures and Tables (For Online Publication)

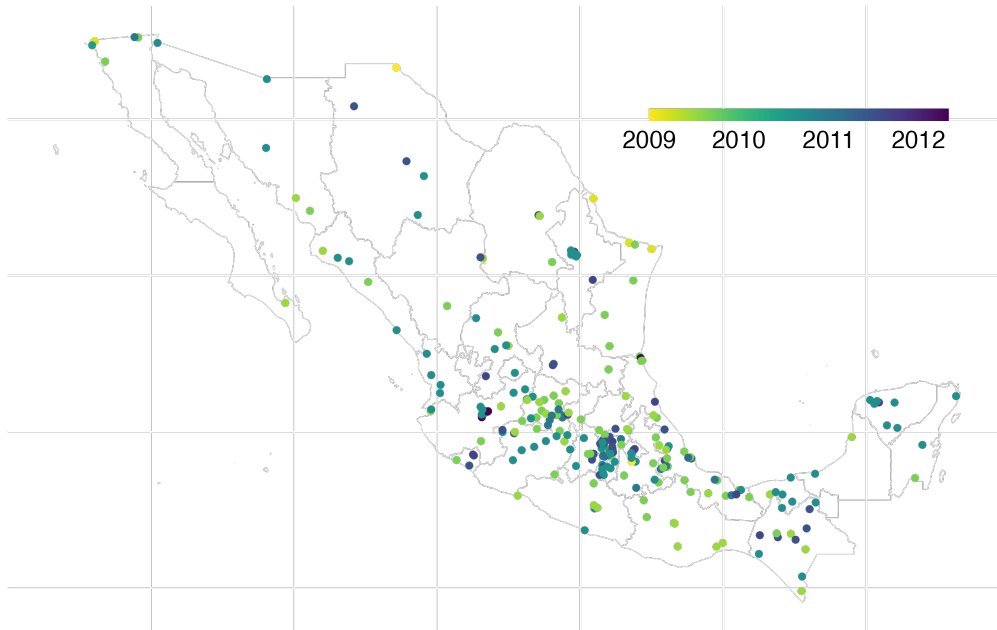
Figure A.1: Use of cards by Prospera beneficiaries



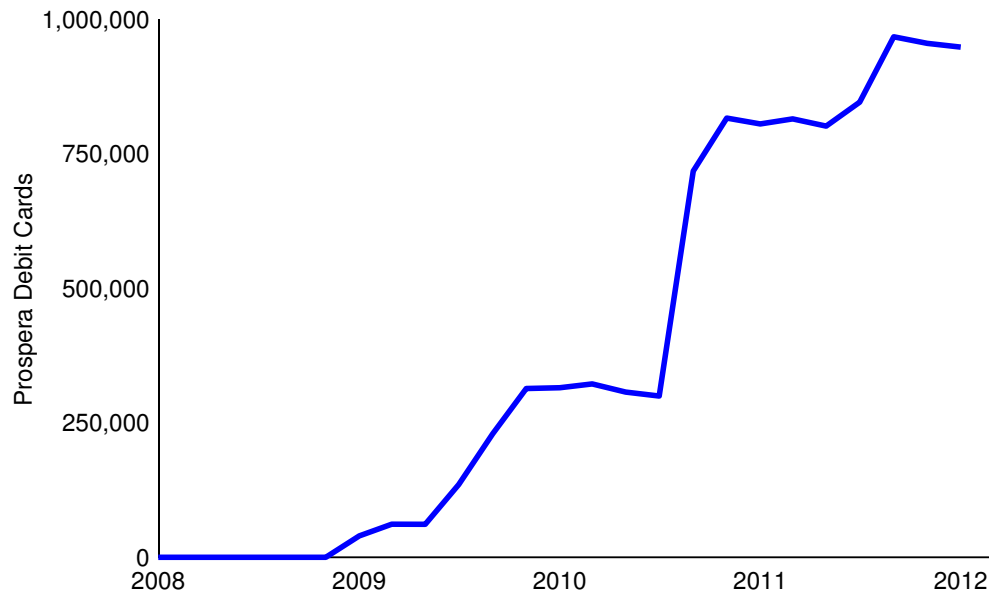
This figure shows the (i) proportion of Prospera card holders who make at least one transaction at a POS terminal using their card during each two-month period; (ii) proportion of Prospera card holders who make at least one ATM withdrawal during each two-month period; (iii) the average pesos transacted at corner store POS terminals (not conditional on making a transaction at a corner store POS terminal); (iv) spending at corner stores as a proportion of total withdrawals from the account (including all types of withdrawals and spending), conditional on making any transaction at a corner store POS terminal. Periods are binned in two-month intervals because the cash transfer is paid every two months. The figure uses Bansefi transactions data with  $N = 106,449,749$  transactions from 961,617 Prospera accounts.

Figure A.2: Debit card rollout over space and time

(a) Rollout over space and time

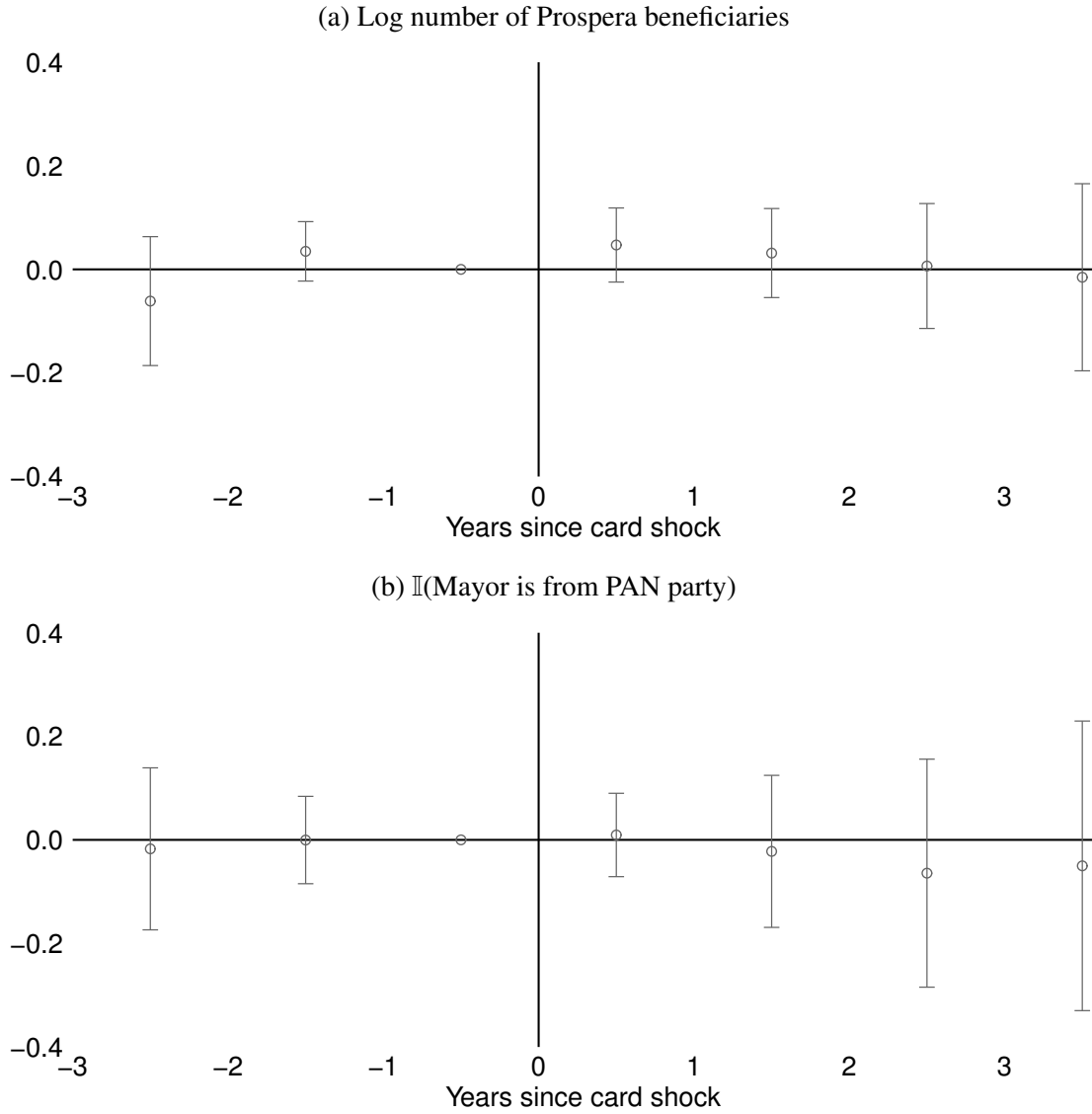


(b) Number of households treated over time



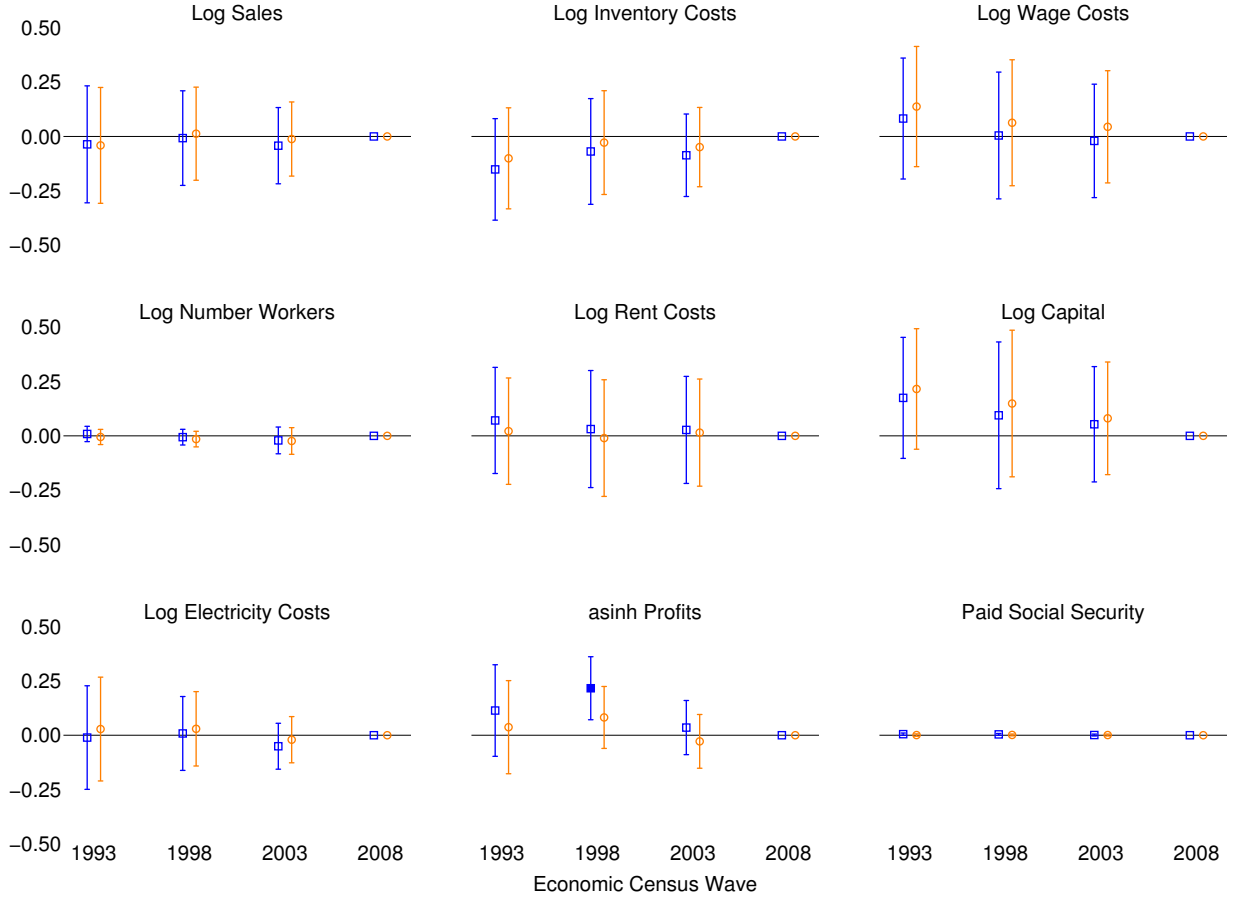
This figure shows when beneficiaries in each urban locality received debit cards from Prospera. It uses administrative data from Prospera on the number of beneficiaries and payment method in each locality during each payment period ( $N = 5,807,552$  locality by two-month period observations), which I used to determine which localities were included in the rollout and when the debit card shock occurred in each locality; it also uses locality and state shapefiles. The declines during some months in panel (b) reflect beneficiaries no longer being eligible for the program (e.g., because their children aged out of it).

Figure A.3: Rollout not correlated with number of beneficiaries or political party



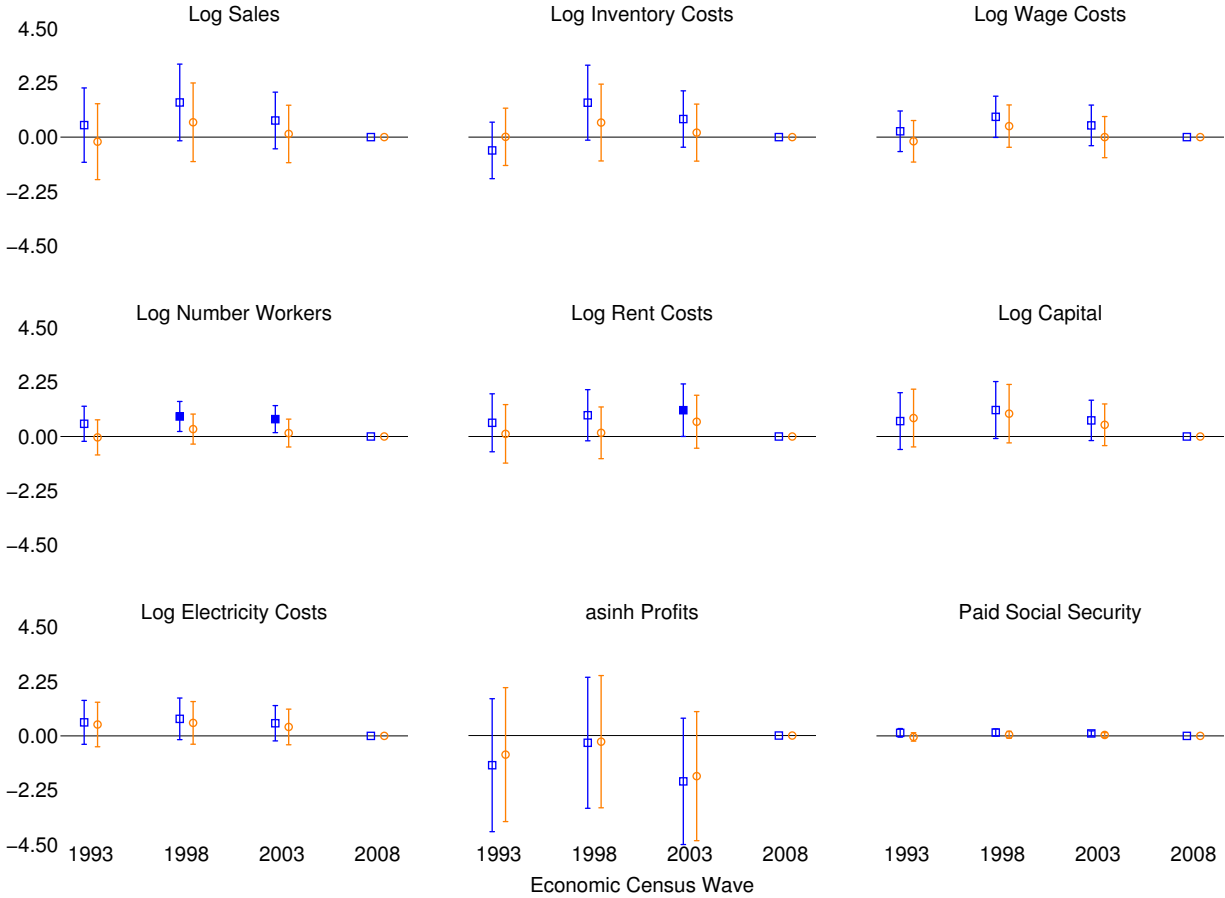
This figure shows that the rollout of debit cards is not correlated with changes in the number of beneficiaries or in the political party in power at the local level. Panel a shows the coefficients from (1), where the outcome is the log number of Prospera beneficiaries in locality  $j$  during the last two-month period of year  $t$ , using administrative data from Prospera on the number of beneficiaries in each locality over time (available by year prior to 2009 and by two-month period from 2009 on).  $N = 2,590$  locality by year observations in 259 treated localities. Standard errors are clustered at the locality level. Panel b shows the coefficients from (1), where the outcome is a dummy variable equal to one if the municipal mayor is from the PAN, the party of the country's president during the card rollout, in municipality  $m$  during year  $t$ . The estimation uses hand-digitized data on vote shares from municipal elections.  $N = 2,805$  municipality by year observations in 255 municipalities. Standard errors are clustered at the municipality level, and 95% confidence intervals are shown. Filled black circles indicate statistically significant at the 5% level, filled gray circles at the 10% level, and hollow gray circles indicate not statistically significant.

Figure A.4: Balanced Economic Census pre-trends for corner stores



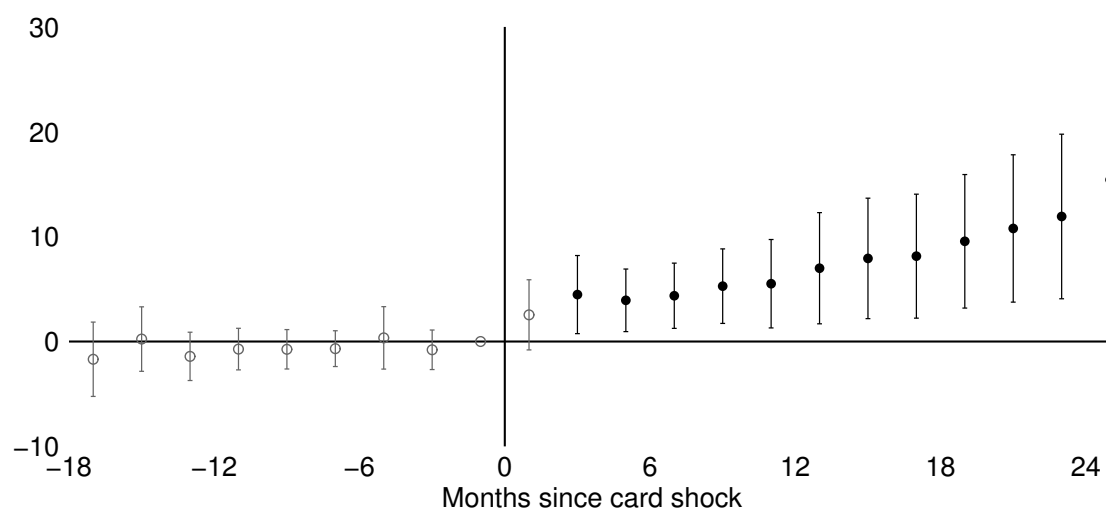
This figure shows parallel pre-trends in variables from the Economic Census, where data are restricted to corner stores then averaged across corner stores within a locality (see Appendix C.4 for details). Point estimates are  $\gamma_{k\tau}$  from (9).  $N = 1,016$  locality by time observations from 254 localities. The reason that there are 254 rather than 259 localities in these regressions is that 5 of the localities included in the debit card rollout did not exist yet as of the 1993 Economic Census. Blue squares indicate the coefficients for localities treated 3–4.5 years prior to the 2013 census wave, while orange circles indicate the coefficients for localities treated 1.5–3 prior to the 2013 census wave; the omitted group is localities treated 0–1.5 years prior to the 2013 census wave. The frequency of  $\gamma_{k\tau}$  coefficients is every five years (for each Economic Census wave). “Paid social security” is the proportion of firms that report any costs from paying social security for employees. Standard errors are clustered at the locality level, and 95% confidence intervals are shown. Filled squares or circles indicate significant at the 5% level, while hollow squares or circles indicate not statistically significant. Only 1 out of 54 coefficients is statistically significant at the 5% level, as could be expected by chance.

Figure A.5: Balanced Economic Census pre-trends for supermarkets



This figure shows parallel pre-trends in variables from the Economic Census, where data are restricted to supermarkets then averaged across supermarkets within a locality (see Appendix C.4 for details). Point estimates are  $\gamma_{k\tau}$  from (9).  $N = 1,016$  locality by time observations from 254 localities. The reason that there are 254 rather than 259 localities in these regressions is that 5 of the localities included in the debit card rollout did not exist yet as of the 1993 Economic Census. Blue squares indicate the coefficients for localities treated 3–4.5 years prior to the 2013 census wave, while orange circles indicate the coefficients for localities treated 1.5–3 years prior to the 2013 census wave; the omitted group is localities treated 0–1.5 years prior to the 2013 census wave. The frequency of  $\gamma_{k\tau}$  coefficients is every five years (for each Economic Census wave). “Paid social security” is the proportion of firms that report any costs from paying social security for employees. Standard errors are clustered at the locality level, and 95% confidence intervals are shown. Filled squares or circles indicate significant at the 5% level, while hollow squares or circles indicate not statistically significant. Only 3 out of 54 coefficients are statistically significant at the 5% level, as could be expected by chance.

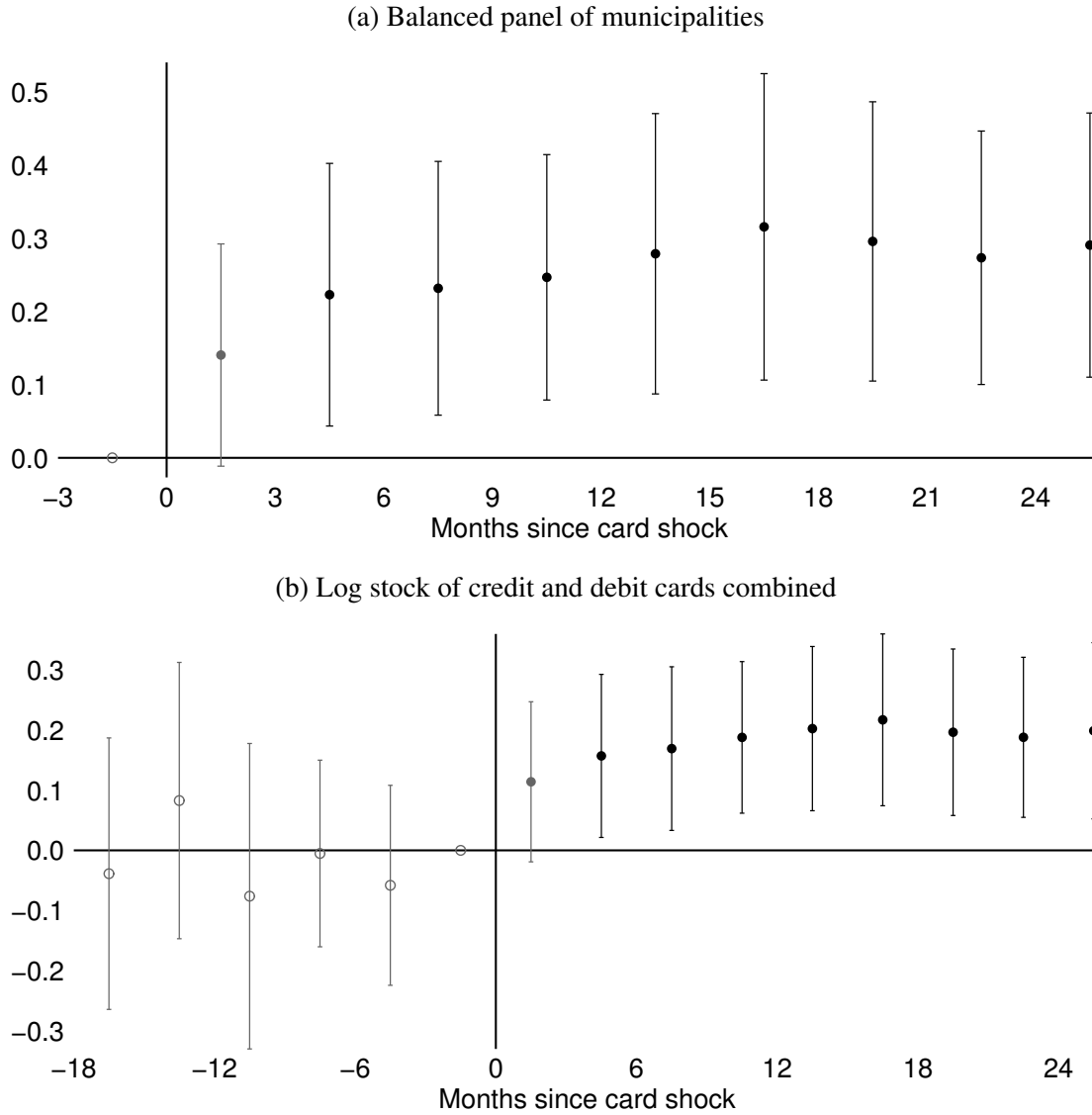
Figure A.6: Effect of card shock on corner store POS adoption **in levels**



This figure shows the effect of the debit card shock on the stock of point-of-sale (POS) terminals at corner stores, measured in levels. It graphs the coefficients from (1), where the dependent variable is the number of corner stores with POS terminals (measured in levels rather than logs). Observations are at the locality by two-month period level.  $N = 8,806$  locality by time observations from 259 localities. Standard errors are clustered at the locality level, and 95% confidence intervals are shown. Filled black circles indicate statistically significant at the 5% level, filled gray circles at the 10% level, and hollow gray circles indicate not statistically significant.

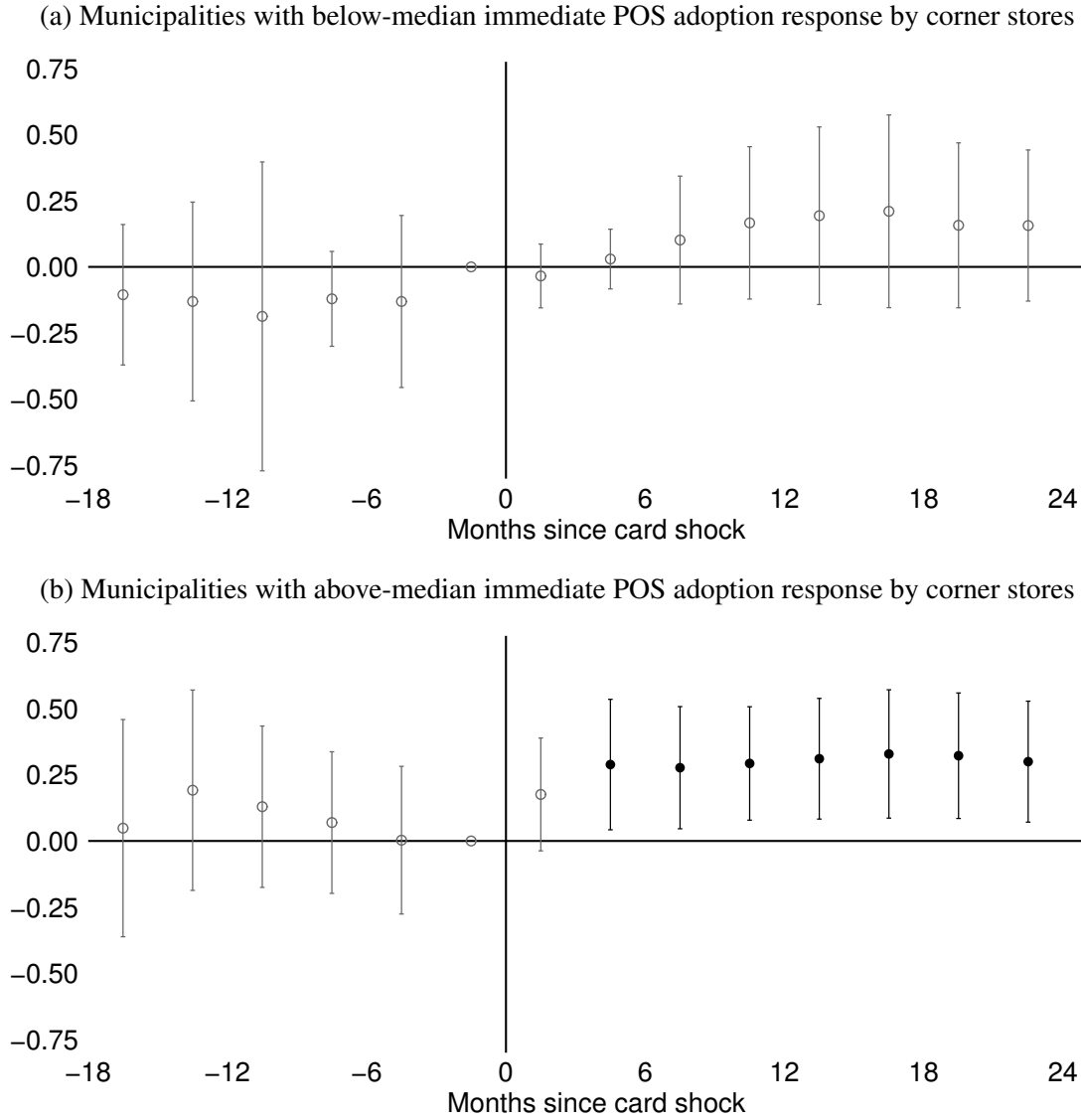


Figure A.7: Robustness of spillovers on other consumers' card adoption



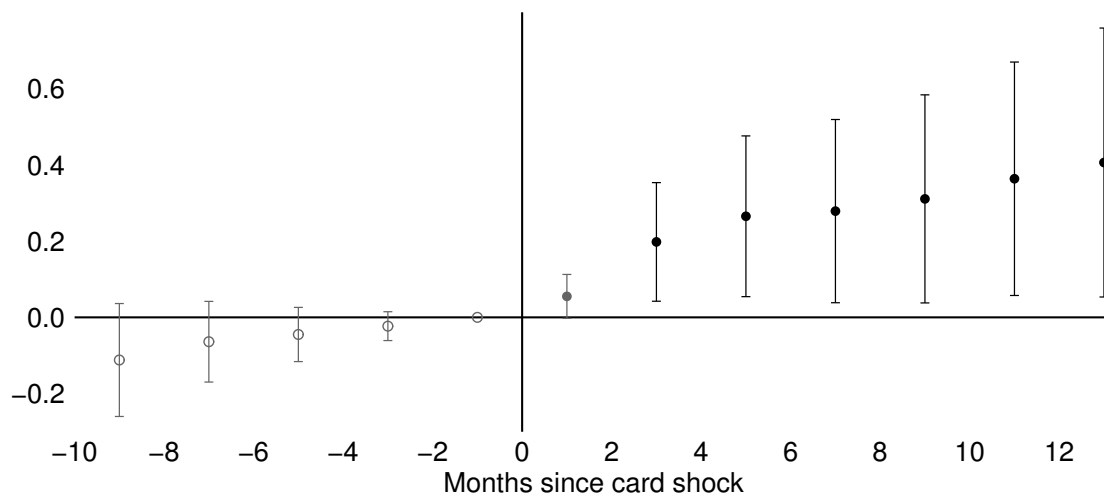
This figure shows robustness of the spillover effect on other consumers' card adoption, showing coefficients from (1) using the CNBV data. Panel a uses the same outcome variable as Figure 5—the log stock of non-Bansefi debit cards in municipality  $m$  in quarter  $t$ —but in the estimation uses only the relative periods for which the full sample of 255 municipalities is available. (Because the data begin in the last quarter of 2008 and the rollout begins in the first quarter of 2009, pre-trends cannot be shown in this figure beyond the omitted period. This is in contrast to the pre-trends for POS terminals in Figure 4, which are already based on a balanced panel since the POS data begin in 2006, i.e. three years before the rollout began.)  $N = 5,076$  municipality by quarter observations from 255 municipalities. Panel b shows the adoption of debit *and* credit cards, i.e. the outcome variable is the log stock of non-Bansefi debit and credit cards in municipality  $m$  in quarter  $t$ .  $N = 8,243$  municipality by quarter observations from 255 municipalities. Standard errors are clustered at the municipality level, and 95% confidence intervals are shown. Filled black circles indicate statistically significant at the 5% level, filled gray circles at the 10% level, and hollow gray circles indicate not statistically significant.

Figure A.8: Heterogeneous spillover effect on card adoption by immediate POS adoption response



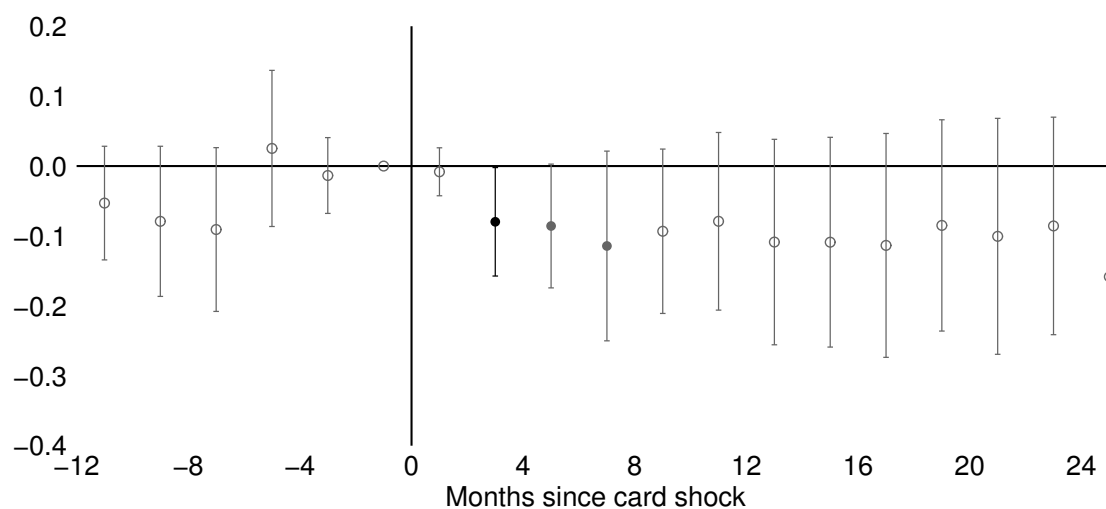
This figure shows that the short-run spillover on other consumers' debit card adoption is higher in municipalities with an immediate corner store POS adoption response. It graphs coefficients from (1), where the outcome variable is the log stock of non-Bansefi debit cards in municipality  $m$  in quarter  $t$ . Panel a restricts to municipalities with below-median immediate POS adoption response by corner stores and panel b to above-median immediate POS adoption response by corner stores. Immediate POS adoption response is measured as the month-over-month change in the number of corner stores with POS terminals in a municipality based on the Central Bank data in the period in which the debit card shock occurred, normalized by the month-over-month change in the number of corner store POS terminals in the same municipality in the period before the debit card shock occurred. (a)  $N = 3,369$  municipality by quarter observations from 104 municipalities. (b)  $N = 4,874$  municipality by quarter observations from 151 municipalities. Standard errors are clustered at the municipality level, and 95% confidence intervals are shown. Filled black circles indicate statistically significant at the 5% level, filled gray circles at the 10% level, and hollow gray circles indicate not statistically significant. The differences in post-shock coefficients between panels a and b are statistically significant in the first two quarters after the card shock, and nonsignificant in all other periods.

Figure A.9: Effect of card shock on other consumers' POS transactions



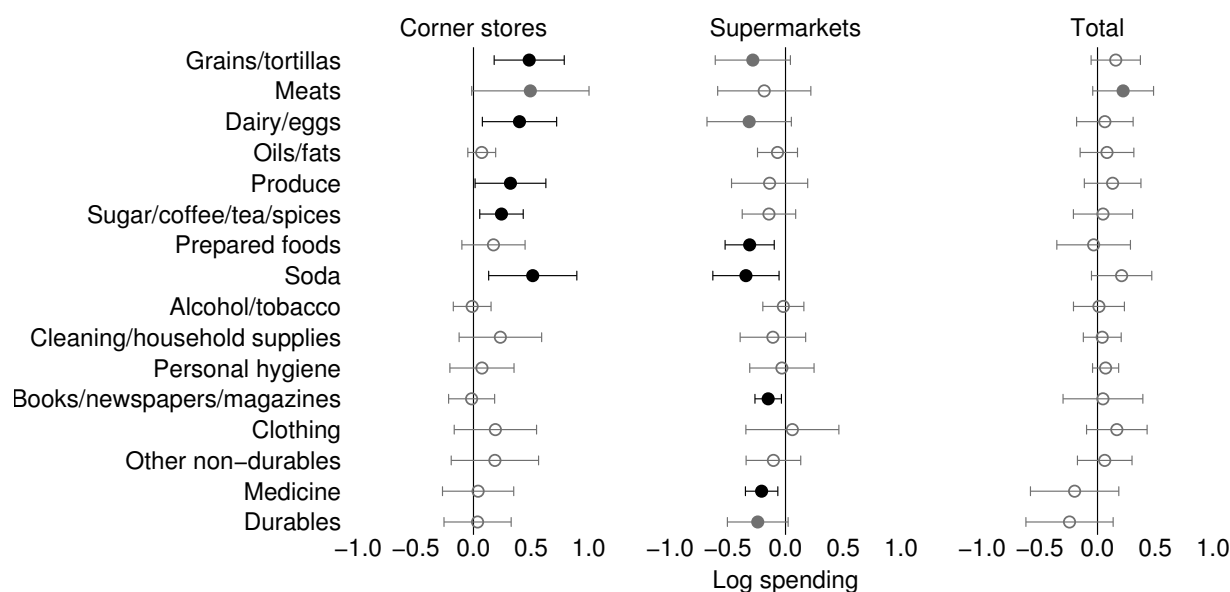
This figure shows that POS transactions excluding transactions made by the Prospera debit cards increase after the debit card shock. It graphs the coefficients from (1), where the outcome variable is the log number of transactions at POS terminals excluding transactions by Prospera beneficiaries in locality  $j$  in quarter  $t$ ; this variable comes from transactions-level data from Mexico's Central Bank.  $N = 11,655$  locality by two-month period observations from 259 localities. Standard errors are clustered at the locality level, and 95% confidence intervals are shown. Filled black circles indicate statistically significant at the 5% level, filled gray circles at the 10% level, and hollow gray circles indicate not statistically significant.

Figure A.10: Effect of card shock on number of ATM withdrawals



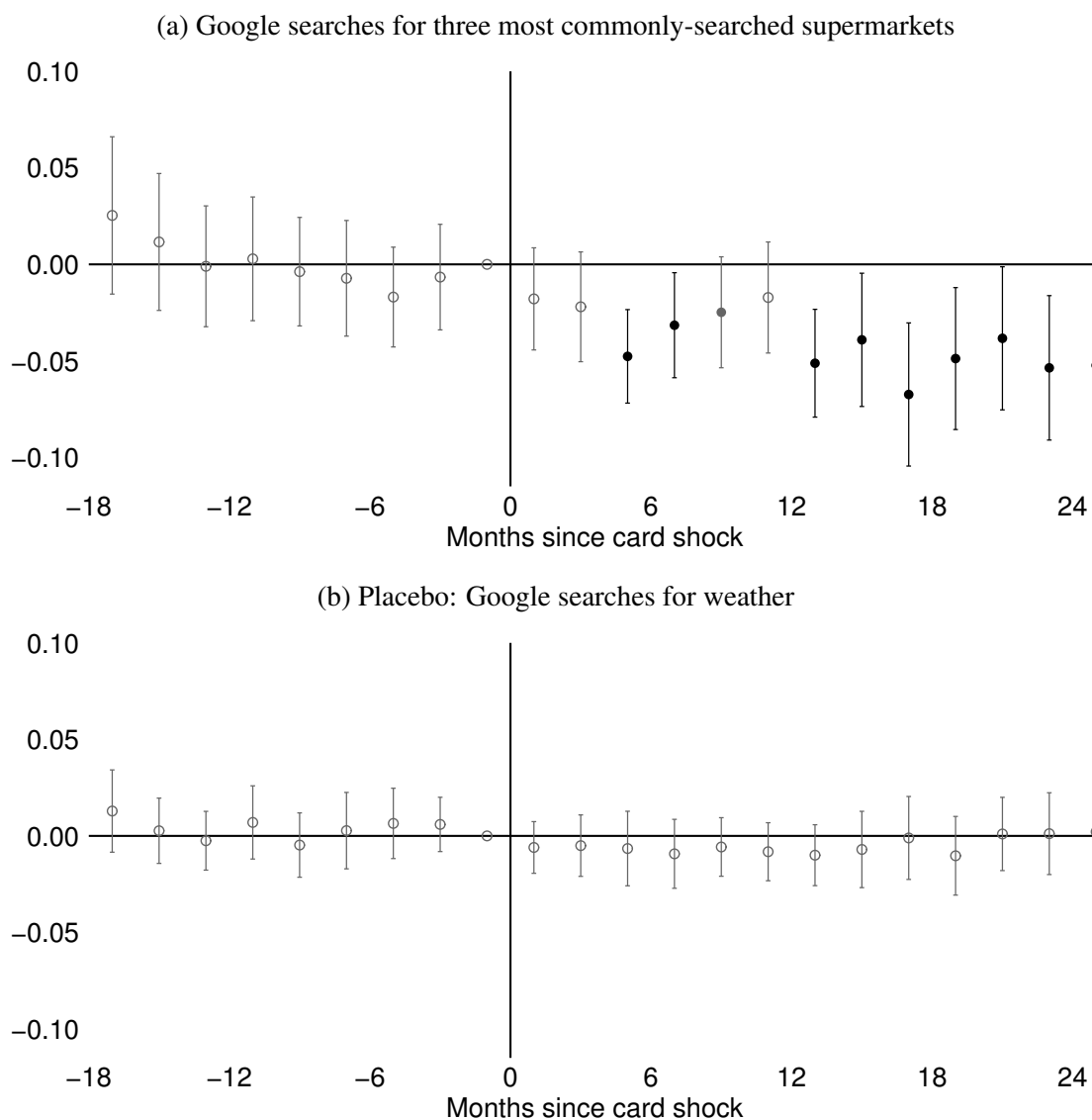
This figure shows that ATM withdrawals decrease after the debit card shock. It graphs the coefficients from (1), where the outcome variable is the log number of ATM withdrawals excluding withdrawals made by Prospera debit cards in municipality  $m$  in two-month period  $t$ ; this variable comes from the CNBV data merged with the Bansefi data.  $N = 8,925$  municipality by two-month period observations from 255 municipalities. Standard errors are clustered at the municipality level, and 95% confidence intervals are shown. Filled black circles indicate statistically significant at the 5% level, filled gray circles at the 10% level, and hollow gray circles indicate not statistically significant.

Figure A.11: Effect on fifth quintile's log spending by product category and store type



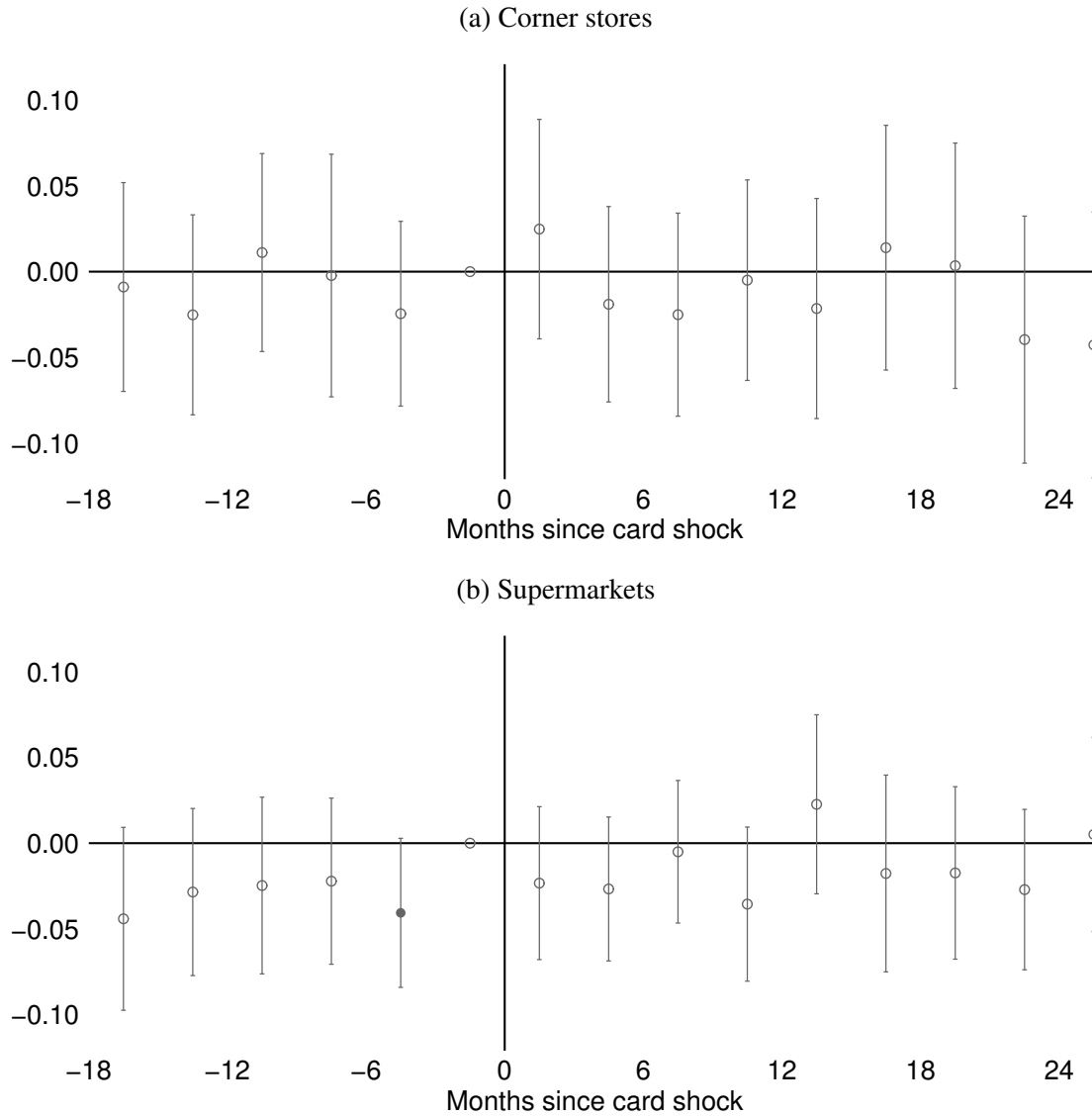
This figure shows a breakdown by product category of the partial shift in the fifth quintile's consumption from supermarkets to corner stores. Each coefficient is  $\gamma + \psi_5$  from a separate regression using specification (4), where the outcome is log spending on a particular product category (rows of the figure) at a particular store type (columns of the figure) from the consumption module of ENIGH. The "total" column includes spending not only at corner stores and supermarkets but also at other types of stores such as open-air markets. Each regression has  $N = 49,810$  households from 220 localities. Standard errors are clustered at the locality level, and 95% confidence intervals are shown. Filled black circles indicate statistically significant at the 5% level, filled gray circles at the 10% level, and hollow gray circles indicate not statistically significant. The same results and results for other quintiles can be found in Tables A.5 and A.6.

Figure A.12: Effect of card shock on Google searches for supermarkets and weather placebo



This figure shows that the the number of Google searches for supermarkets fell about 4–6 months after the debit card shock occurred, while placebo searches for weather did not change after the card shock. It shows coefficients from (1). In panel a, the outcome is log Google searches for one of the three most commonly-searched supermarket chains (Walmart, Soriana, and Comercial Mexicana) plus the locality name, for locality  $j$  in two-month period  $t$ .  $N = 4,318$  locality by time observations from 127 localities that returned non-zero numbers of Google searches for these supermarkets plus the locality name. In panel b, the outcome is log Google searches for “weather” (*clima*) plus the locality name, for locality  $j$  in two-month period  $t$ .  $N = 7,718$  locality by time observations from 227 localities that returned non-zero numbers of Google searches for “weather” plus the locality name. Zero Google searches can mean a bottom-coded but non-zero number of searches, which is why localities with zero Google searches over the entire time period are not included in the regression. Standard errors are clustered at the locality level, and 95% confidence intervals are shown. Filled black circles indicate statistically significant at the 5% level, filled gray circles at the 10% level, and hollow gray circles indicate not statistically significant.

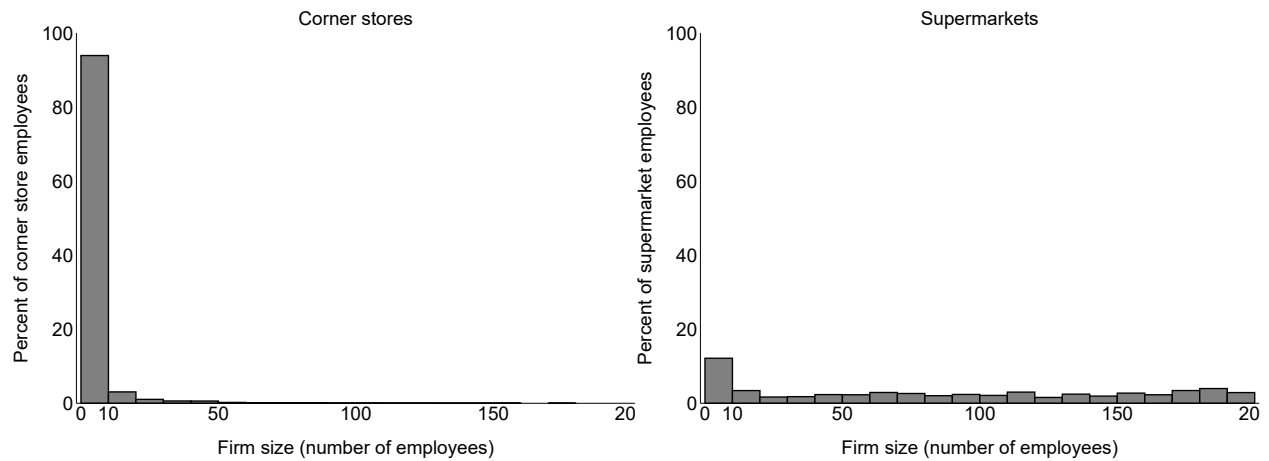
Figure A.13: Effect of card shock on log retail wages



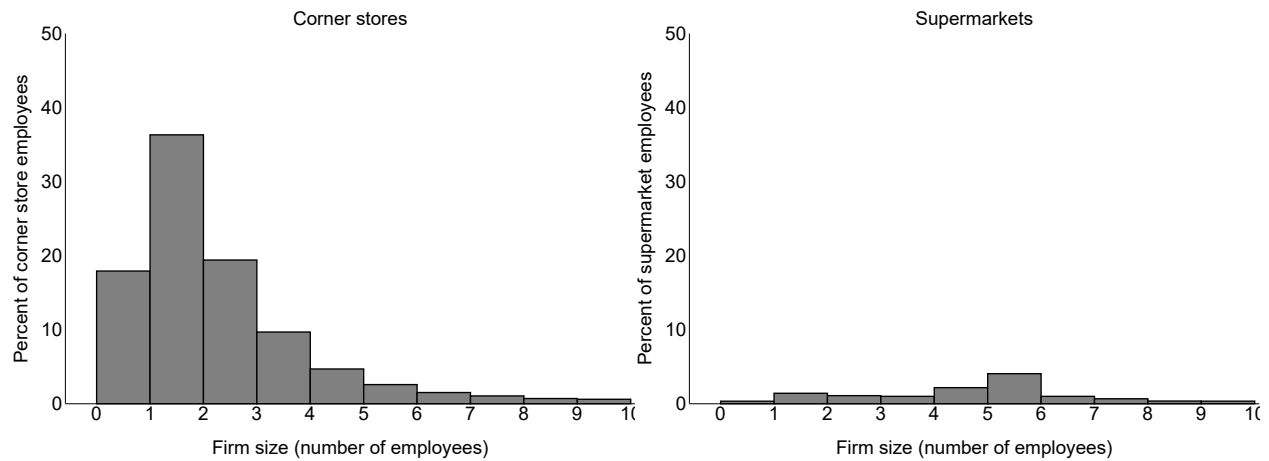
This figure shows that the rollout of debit cards did not have an effect on retail wages. It shows the coefficients from (1), where the outcome is log monthly wages of individual  $i$  in municipality  $m$  during quarter  $t$  and municipality and quarter fixed effects are included, using Mexico's quarterly labor force employment survey. (a)  $N = 83,222$  individual by quarter observations of individuals employed at corner stores (excluding store owners) in 250 treated municipalities; (b)  $N = 96,380$  individual by quarter observations of individuals employed at supermarkets (excluding store owners) in 244 treated municipalities. Standard errors are clustered at the municipality level, and 95% confidence intervals are shown. Filled black circles indicate statistically significant at the 5% level, filled gray circles at the 10% level, and hollow gray circles indicate not statistically significant.

Figure A.14: Distribution of retail employees across the firm size distribution

(a) Entire firm size distribution



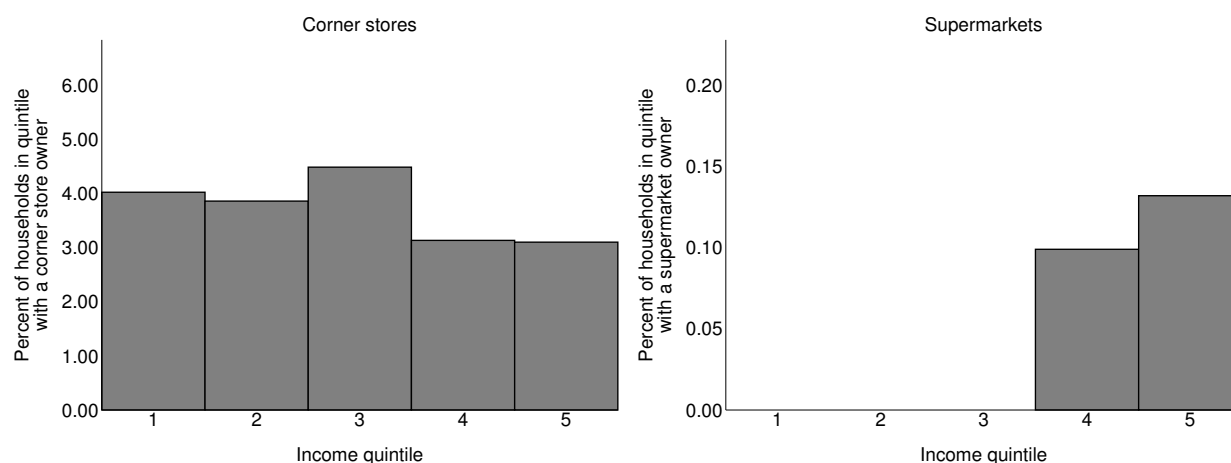
(b) Firms with less than 10 employees



This figure shows the percent of corner store and supermarket employees that work at each type of retail firm throughout the firm size distribution, using data from the 2008 Economic Census. Supermarkets are substantially larger than corner stores.

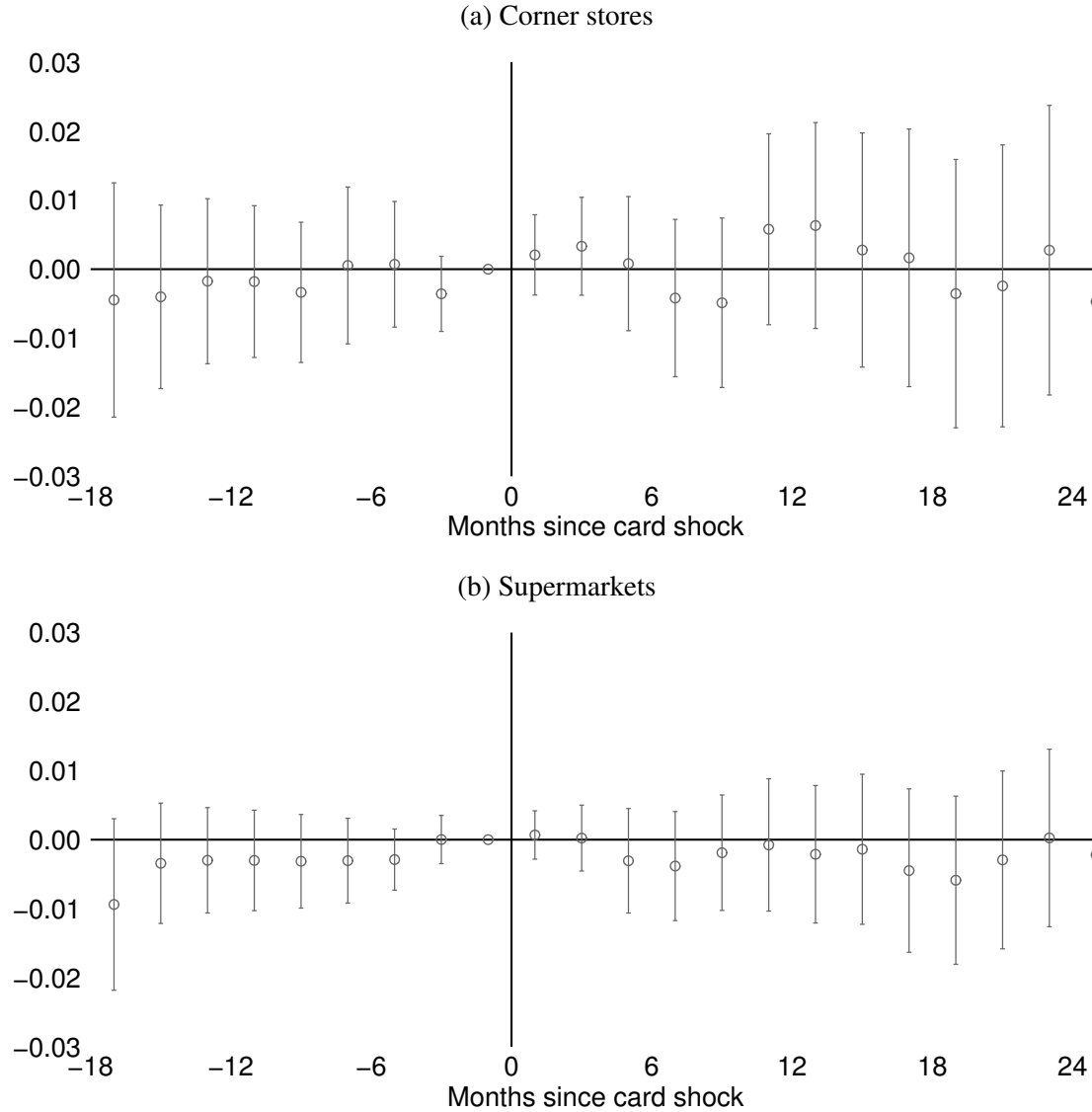


Figure A.15: Percent of households with retail firm owners by income quintile



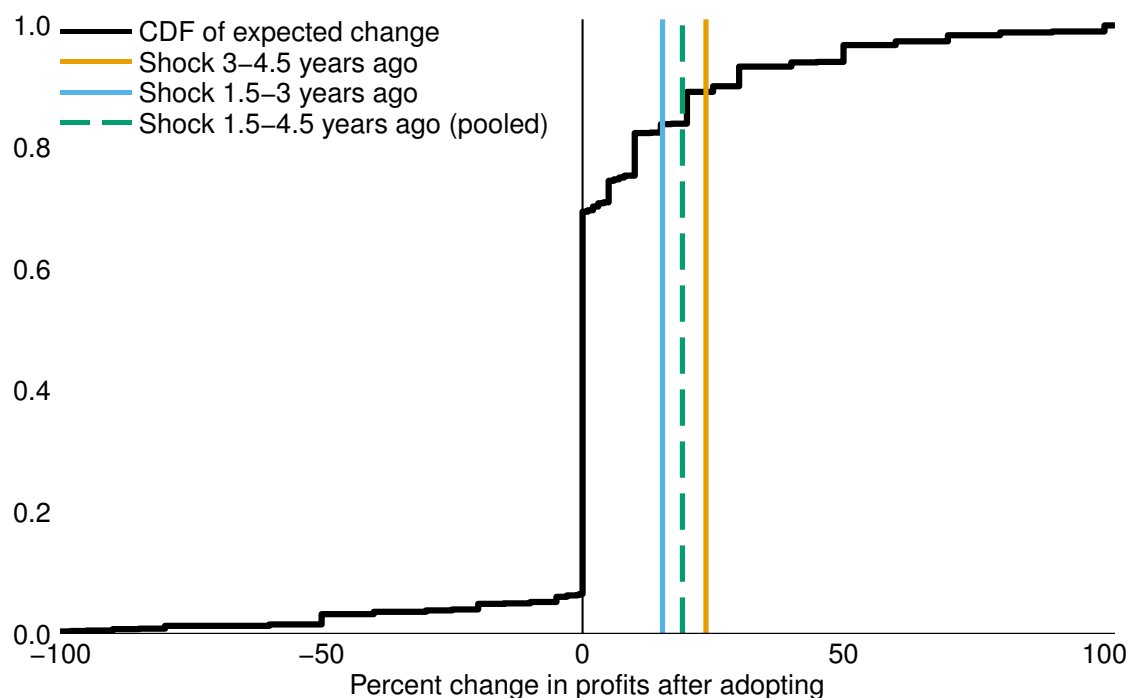
This figure shows the percent of households in each income quintile with a corner store owner or supermarket owner, using data on occupations and household income from the 2008 ENIGH. Corner store and supermarket ownership is identified using the four-digit NAICS code for each individual's occupation, combined with a variable asking whether the individual does not have a boss to determine whether they are the owner of the firm.  $N = 15,156$  households in treated localities in the 2008 ENIGH.

Figure A.16: Price effects



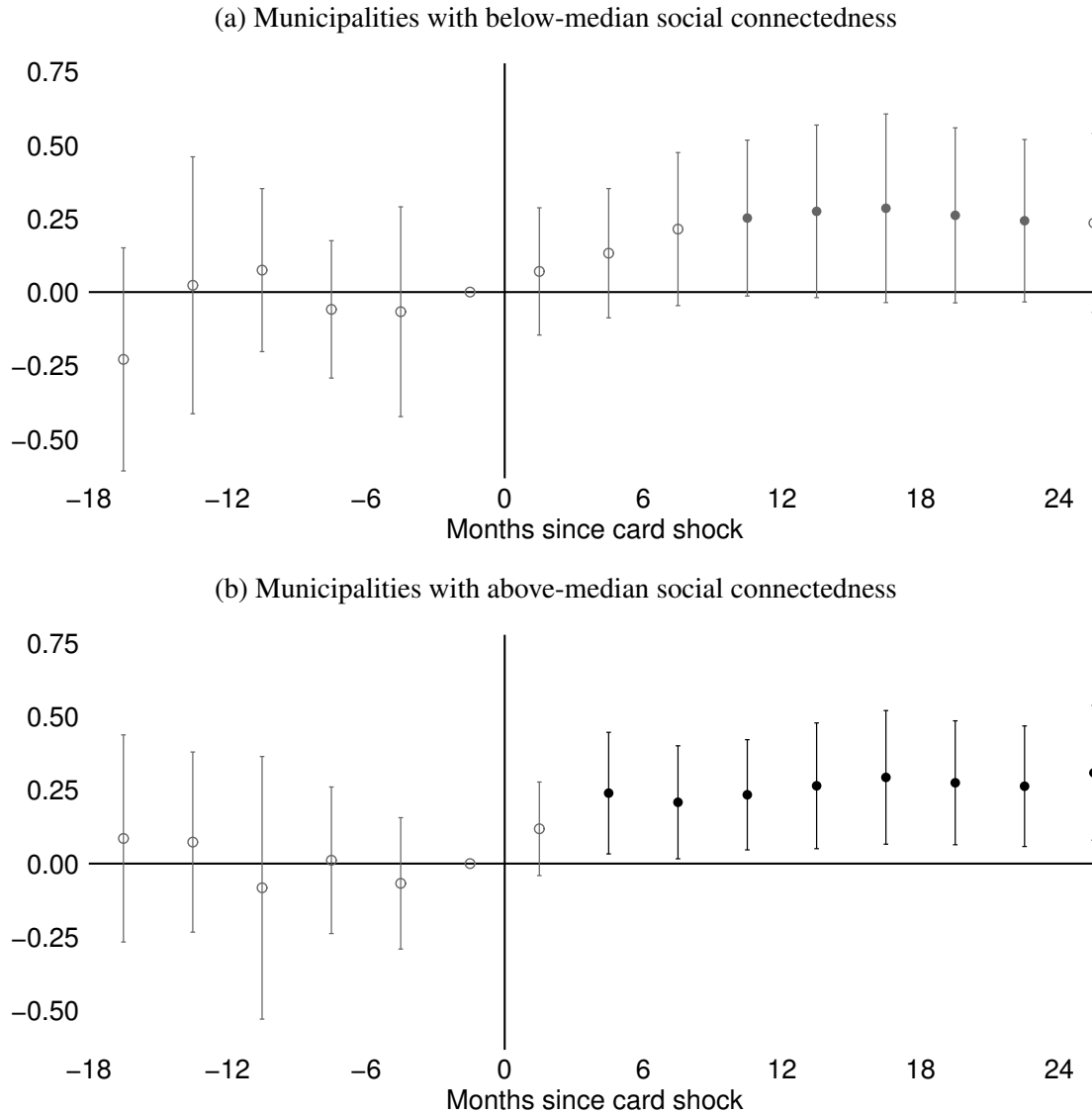
This figure shows that neither corner stores nor supermarkets change prices in response to the debit card shock. It shows the results from (6), where the outcome variable is the log price of barcode-level product  $g$  at store  $s$  at time  $t$ . It uses the microdata used to construct Mexico's Consumer Price Index; the data were collected by Mexico's Central Bank from 2002–2010 and by INEGI from 2010–2014. (a)  $N = 531,762$  product by store by two-month period observations from 72 municipalities; (b)  $N = 979,108$  product by store by two-month period observations from 64 municipalities. Standard errors are clustered at the municipality level, and 95% confidence intervals are shown. Filled black circles indicate statistically significant at the 5% level, filled gray circles at the 10% level, and hollow gray circles indicate not statistically significant.

Figure A.17: Comparing expected change in profits in survey to average treatment effects



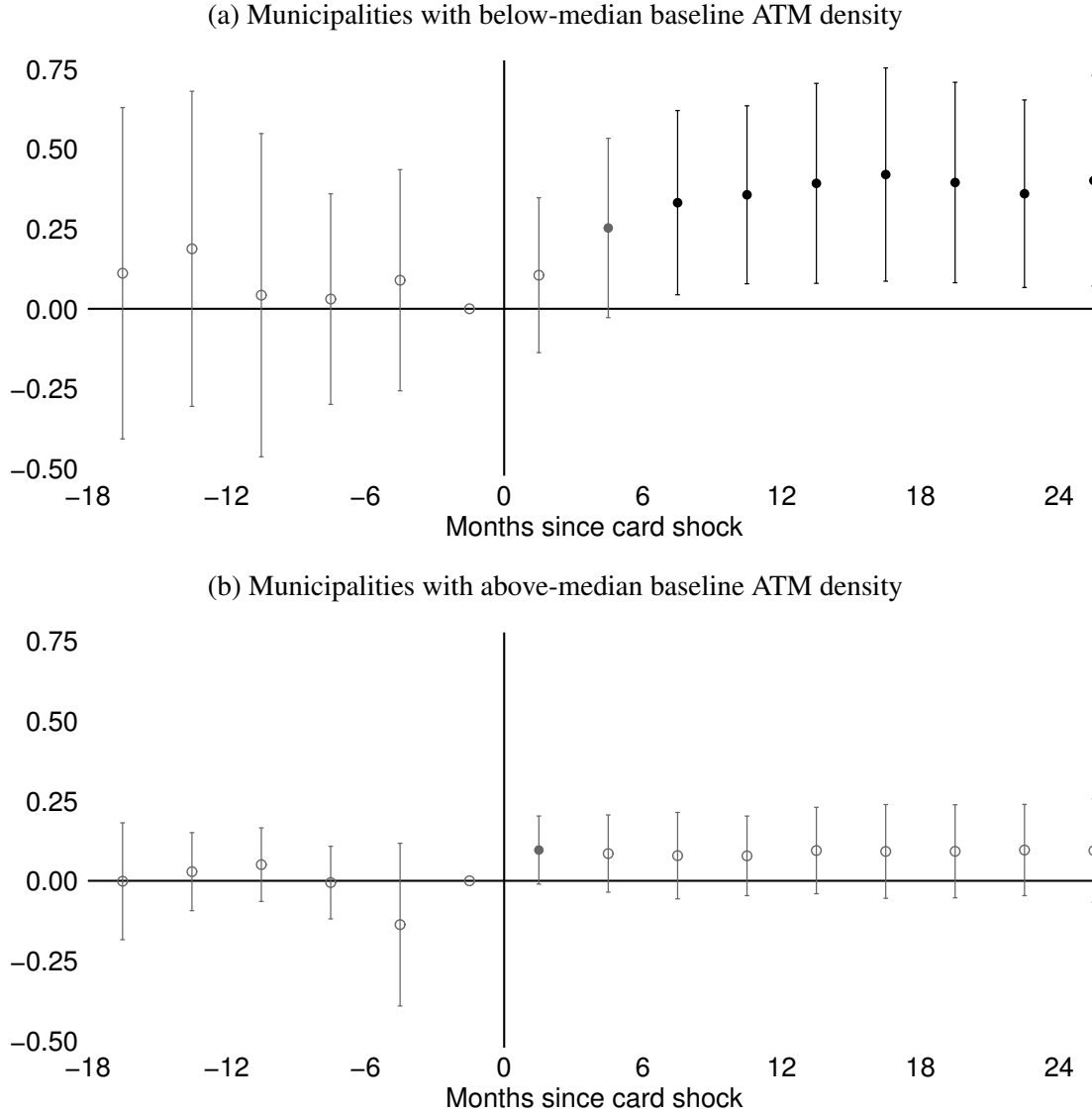
This figure shows the cumulative distribution of corner store owners' expected change in profits after adopting a POS terminal, based on survey responses. It also plots average treatment effects from the Economic Census. The solid gold line shows the coefficient on those treated 3–4.5 years ago, the solid blue line the coefficient on those treated 1.5–3 years ago, and the dashed green line the pooled coefficient (all converted to percent changes) from Table 4. The cumulative distribution uses data from the survey I conducted and is based on  $N = 1,300$  corner store owners without POS terminals and with non-missing responses to the question on expected change in profits after adopting a POS terminal. The treatment effects are from the regressions reported in Table 4 with  $N = 172,441$  corner stores from 259 localities.

Figure A.18: Heterogeneous spillover effect on card adoption by social connectedness



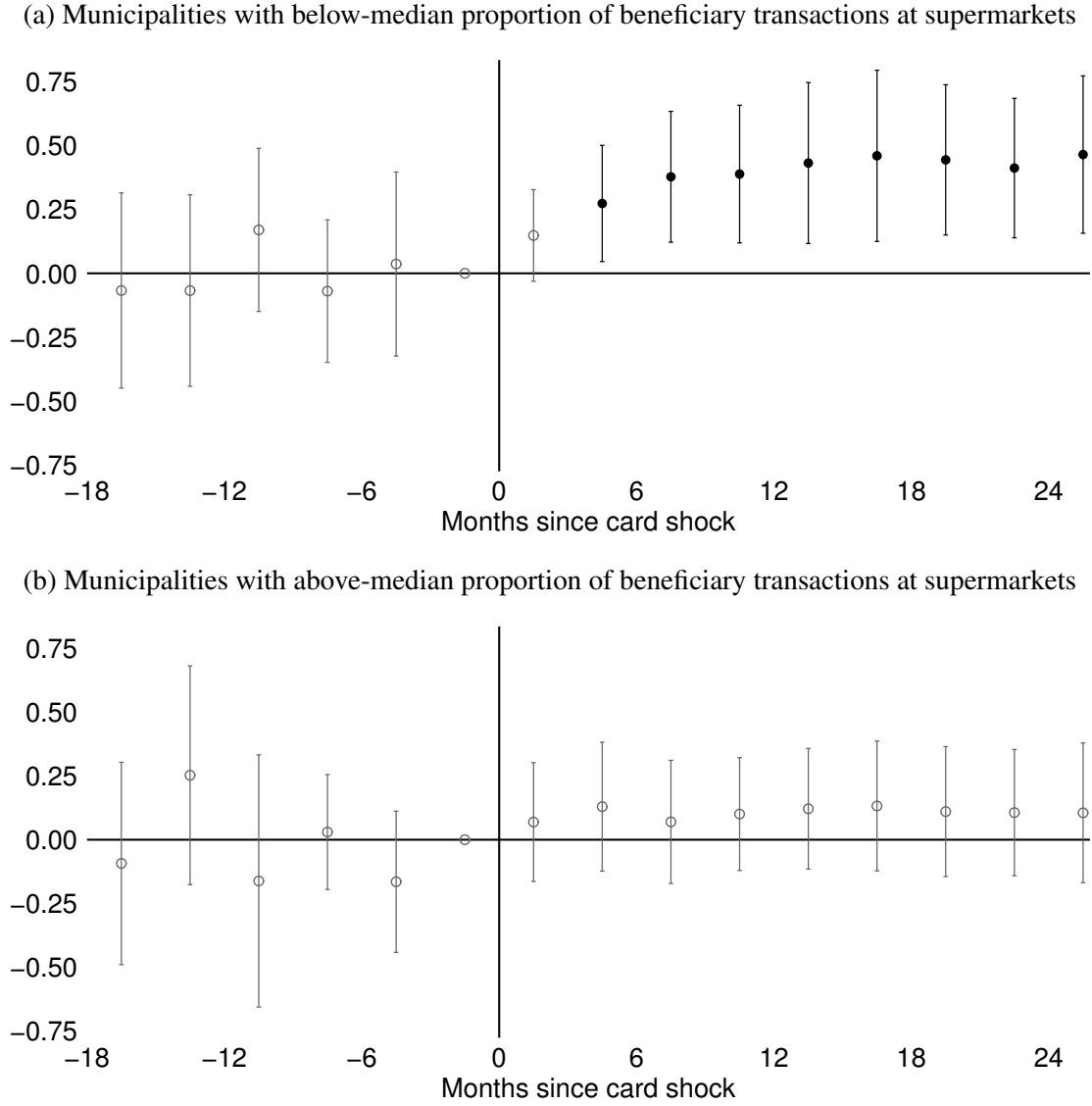
This figure shows that there are no statistically significant differences between the spillover effect on other consumers' card adoption based on a municipality's social connectedness. It graphs coefficients from (1), where the outcome variable is the log stock of non-Bansefi debit cards in municipality  $m$  in quarter  $t$ . Panel a restricts to municipalities with below-median within-municipality social connectedness index (SCI) and panel b to above-median within-municipality SCI, where SCI is measured using data provided by Facebook. (a)  $N = 4,157$  municipality by quarter observations from 127 municipalities. (b)  $N = 4,055$  municipality by quarter observations from 127 municipalities. Standard errors are clustered at the municipality level, and 95% confidence intervals are shown. Filled black circles indicate statistically significant at the 5% level, filled gray circles at the 10% level, and hollow gray circles indicate not statistically significant. The same results can be found in Table 2. The differences in post-shock coefficients between panels a and b are statistically nonsignificant in all periods.

Figure A.19: Heterogeneous spillover effect on card adoption by ATM density



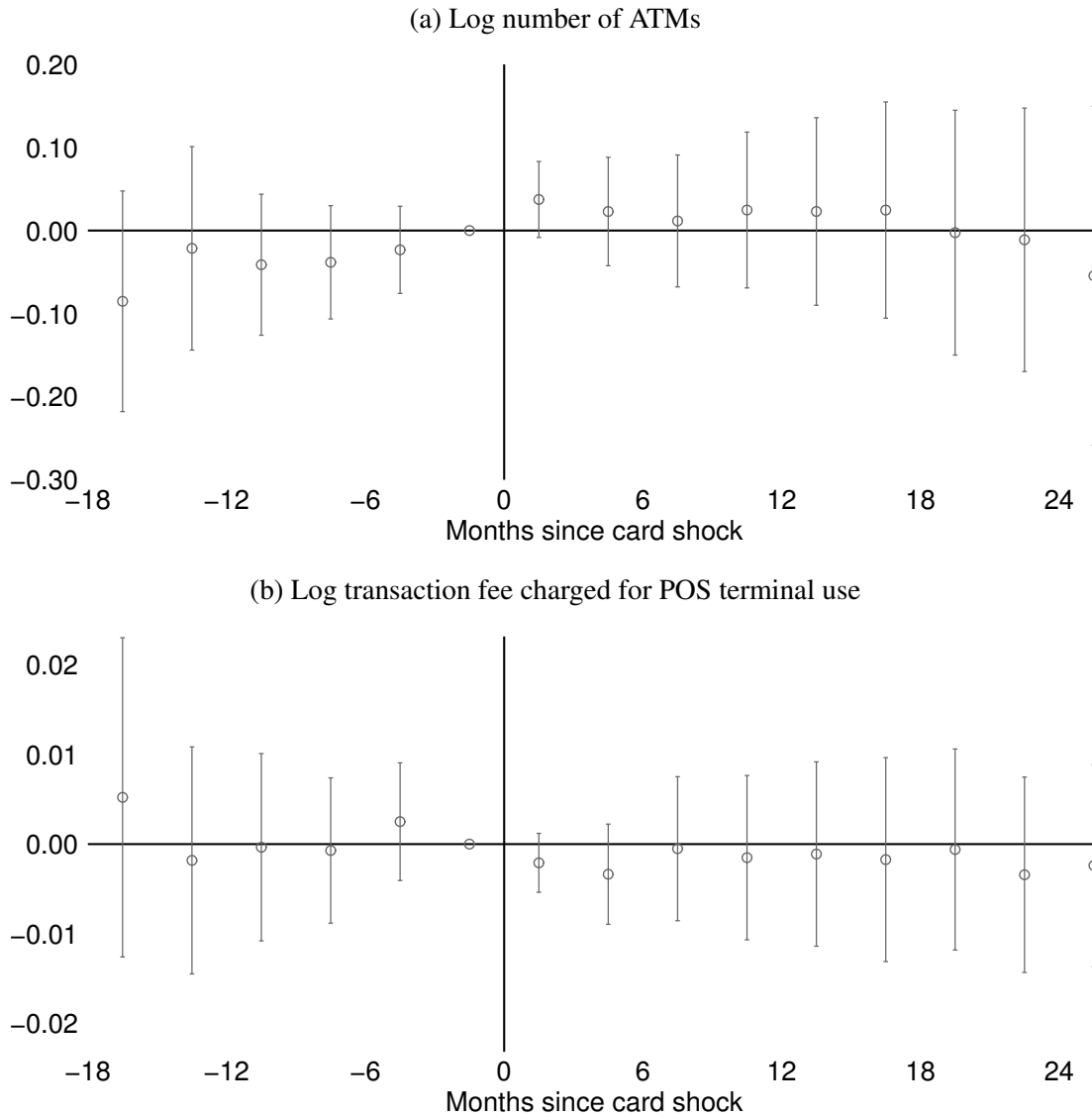
This figure shows that the spillovers on other consumers' card adoption appear to be concentrated in municipalities with low ATM density. It graphs coefficients from (1), where the outcome variable is the log stock of non-Bansefi debit cards in municipality  $m$  in quarter  $t$ . Panel a restricts to municipalities with below-median baseline ATM density and panel b to above-median baseline ATM density, where baseline ATM density is measured using the last quarter of 2008 in the CNBV data, divided by population in INEGI data. (a)  $N = 4,035$  municipality by quarter observations from 127 municipalities. (b)  $N = 4,208$  municipality by quarter observations from 128 municipalities. Standard errors are clustered at the municipality level, and 95% confidence intervals are shown. Filled black circles indicate statistically significant at the 5% level, filled gray circles at the 10% level, and hollow gray circles indicate not statistically significant. The same results can be found in Table 2. The differences in post-shock coefficients between panels a and b are statistically significant in 5 out of 9 periods.

Figure A.20: Heterogeneous spillover effect on card adoption by beneficiary shopping patterns



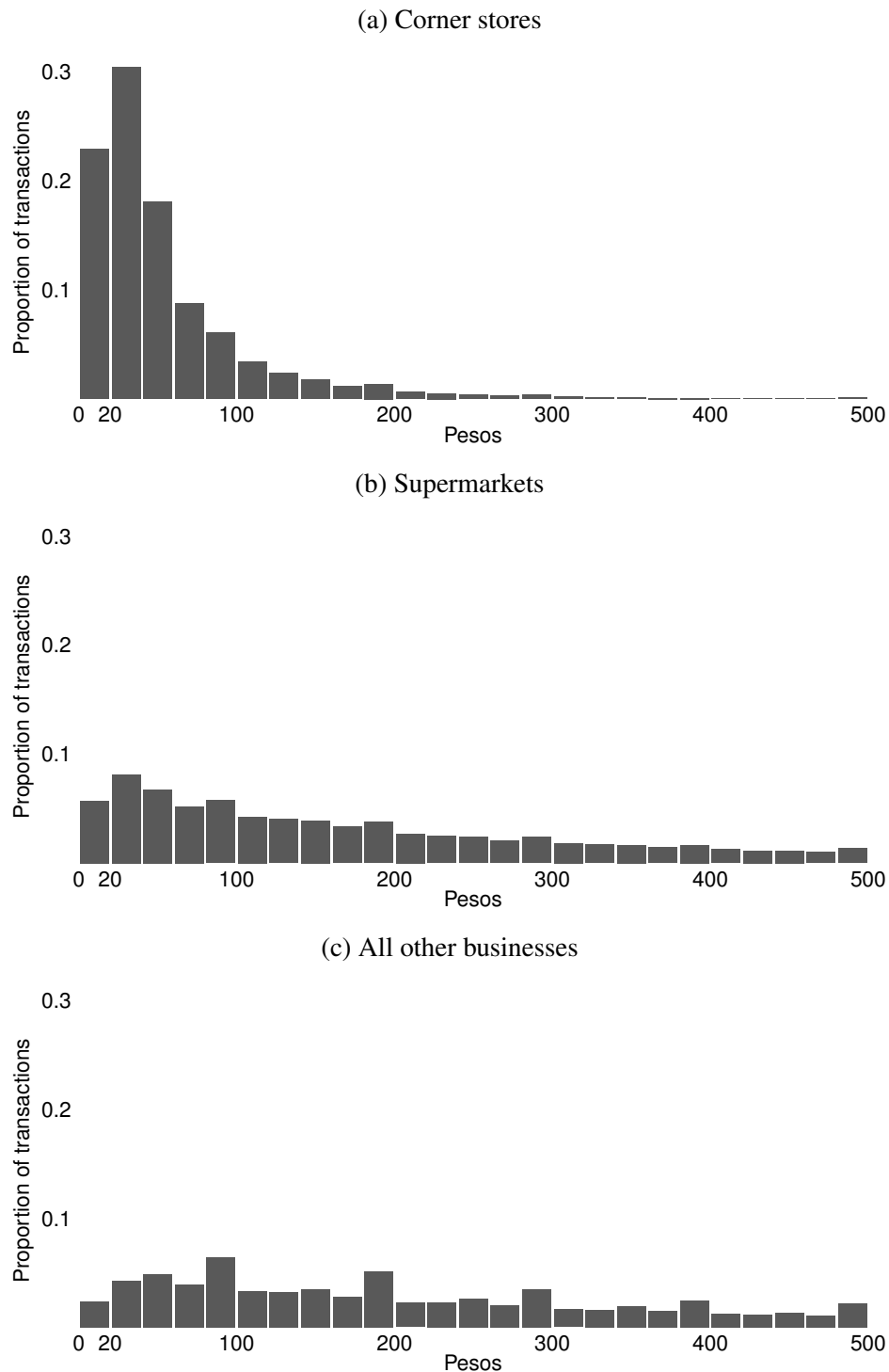
This figure shows that the spillovers on other consumers' card adoption appear to be concentrated in municipalities where beneficiaries use their cards relatively more at corner stores. It graphs coefficients from (1), where the outcome variable is the log stock of non-Bansefi debit cards in municipality  $m$  in quarter  $t$ . Panel a restricts to municipalities with below-median beneficiary card spending at supermarkets and panel b to above-median beneficiary card spending at supermarkets. The heterogeneity measure is constructed as the proportion of card transactions during their first 6 months with the card that Prospera beneficiaries make at supermarkets, using the Bansefi transactions data, while the outcome variable is from CNBV data. (a)  $N = 3,833$  municipality by quarter observations from 119 municipalities. (b)  $N = 3,852$  municipality by quarter observations from 118 municipalities. The sum of the number of municipalities in panels a and b is less than 255 because in 18 municipalities no Prospera beneficiaries use the card to make POS transactions during the first 6 months with the card, and hence the heterogeneity variable is missing for those municipalities. Standard errors are clustered at the municipality level, and 95% confidence intervals are shown. Filled black circles indicate statistically significant at the 5% level, filled gray circles at the 10% level, and hollow gray circles indicate not statistically significant. The same results can be found in Table 2. The differences in post-shock coefficients between panels a and b are statistically significant in 4 out of 9 periods.

Figure A.21: Lack of bank response to card shock



This figure tests for a response by commercial banks to the debit card shock. Panel a shows that the number of commercial bank branches does not change in response to the card shock. It shows coefficients from (1) where the outcome variable is the log number of commercial bank branches in the municipality, using quarterly data from CNBV.  $N = 4,832$  municipality by quarter observations from 255 municipalities. Panel b shows that the per-transaction merchant fee charged by banks for retailers' use of POS terminals does not change in response to the card shock. It shows coefficients from (1) where the outcome variable is the log of the per-transaction fee (e.g. a 2.75% fee would be coded in the data as  $\log(2.75)$ ), averaged over all banks with a presence within municipality  $m$  at time  $t$ , using data on these fees at each bank over time from Mexico's Central Bank.  $N = 7,823$  municipality by quarter observations from 250 municipalities. Standard errors are clustered at the municipality level, and 95% confidence intervals are shown. Filled black circles indicate statistically significant at the 5% level, filled gray circles at the 10% level, and hollow gray circles indicate not statistically significant.

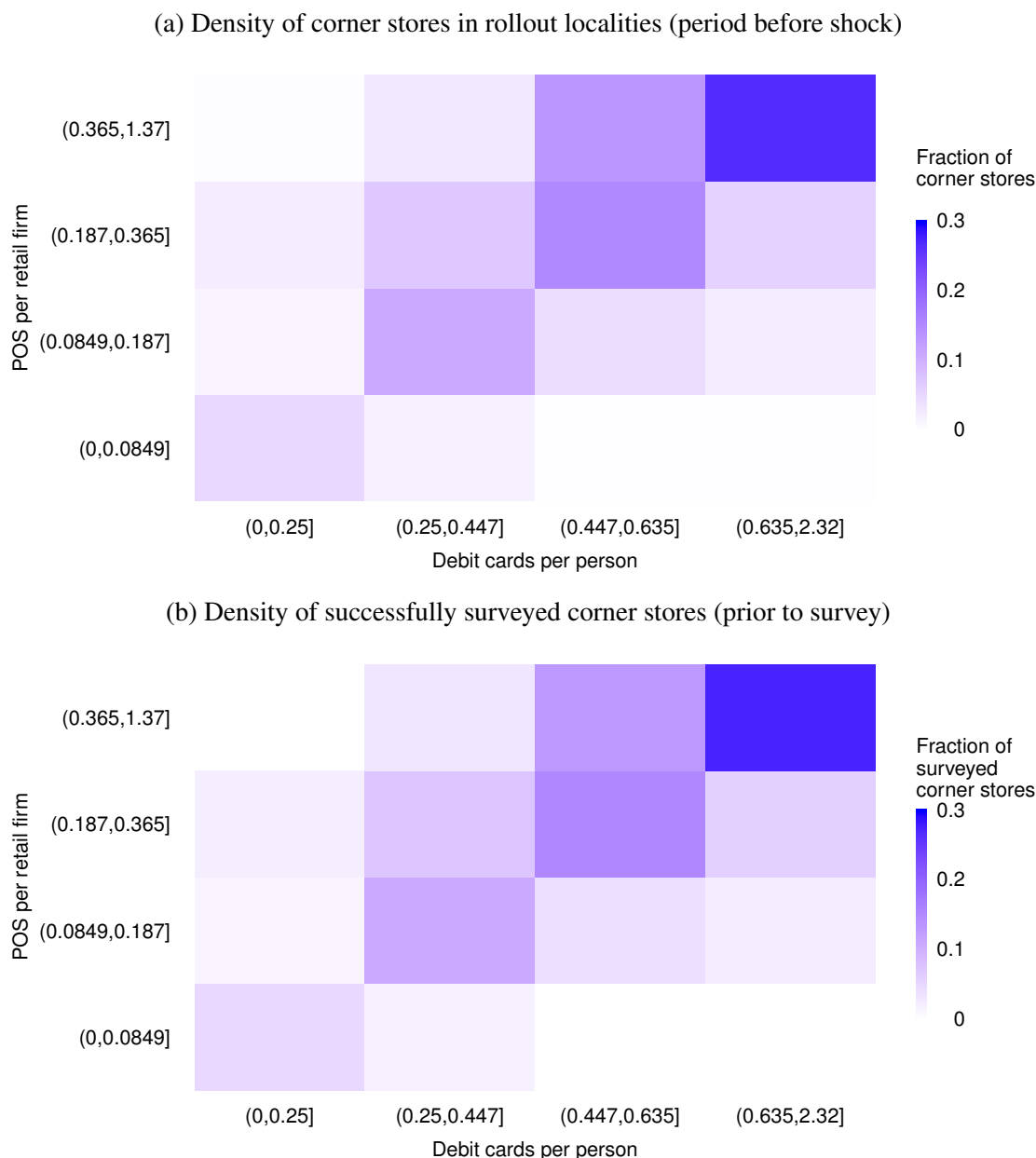
Figure A.22: Histograms of transaction amounts (transactions at POS terminals)



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

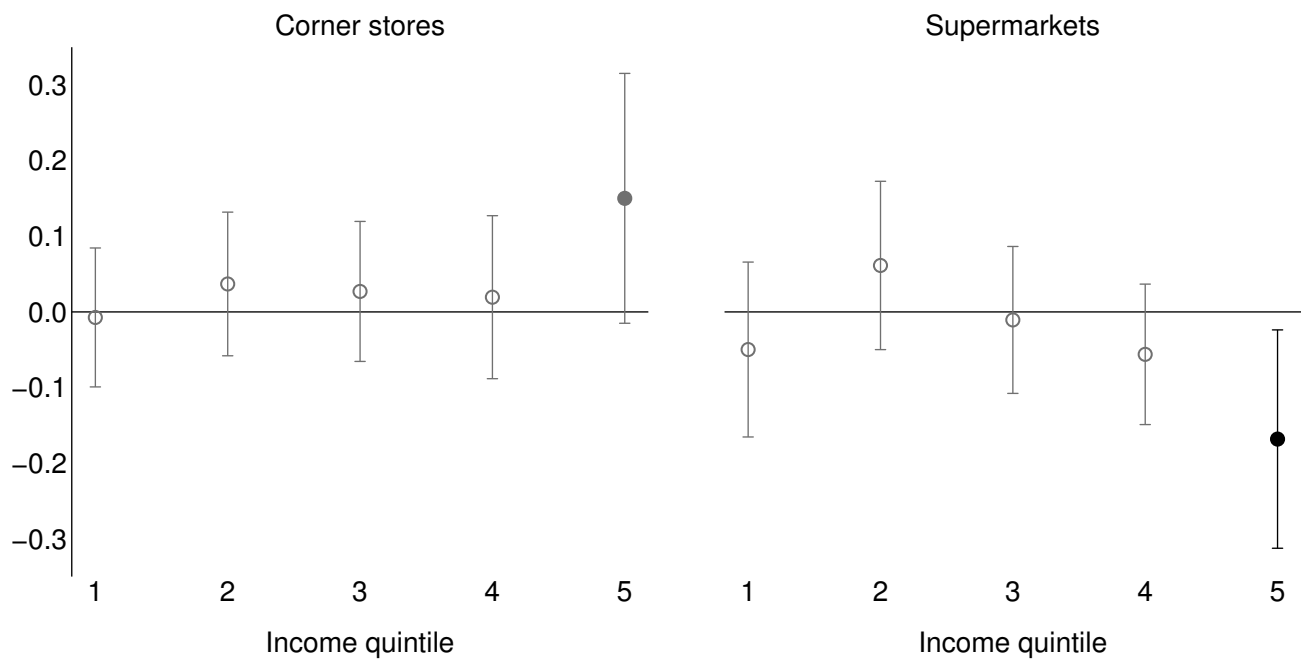


Figure A.23: Bivariate distribution of debit card and POS adoption



This figure shows the bivariate distribution of POS adoption and debit card adoption faced by corner stores in the debit card rollout (panel a) and successfully surveyed corner stores (panel b). It uses data on POS terminal adoption from Mexico's Central Bank, data on debit card adoption from CNBV, data on the number of retail firms and population from INEGI, and data from the survey I conducted. Each axis is divided into quartiles, where the quartiles are calculated at the municipality level (not weighted by number of corner stores in each municipality). "Prior to survey" in panel b refers to data as of the end of 2021, which was the last quarter for which data was available prior to conducting the survey.  $N = 255$  treated municipalities in panel a, where the density shown in the heatmap is weighted by the number of corner stores in each municipality.  $N = 29$  surveyed municipalities in panel b, where the density shown in the heatmap is weighted by the number of successfully completed surveys in each municipality.

Figure A.24: Effect of corner store POS adoption on quantity consumed



This figure shows that richer consumers substitute some of the quantity (measured in kilograms and liters) that they purchase from supermarkets to corner stores. This suggests that the results in Figure 6a are not explained by prices. The figure graphs coefficients from (4) where the outcome variable is  $\log(\text{kilograms} + \text{liters purchased})$  at the particular store type (corner stores or supermarkets).  $N = 49,810$  households from 220 localities. Standard errors are clustered at the locality level, and 95% confidence intervals are shown. Filled black circles indicate statistically significant at the 5% level, filled gray circles at the 10% level, and hollow gray circles indicate not statistically significant.

Table A.1: Distribution of treatment timing and ENIGH survey timing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Number of households						Percent
	Timing of survey						
Two-month period in which locality received debit card shock	Aug 21–Nov 27 2006	Aug 21–Nov 17 2008	Aug 21–Nov 18 2010	Aug 27–Nov 24 2012	Aug 11–Nov 28 2014	Total	Total (%)
Jan–Feb 2009	424	501	544	153	461	2,083	4.2
Mar–Apr 2009	374	352	370	186	425	1,707	3.4
Jul–Aug 2009	1,415	2,313	1,726	416	1,259	7,129	14.3
Sep–Oct 2009	2,364	2,647	3,402	724	1,660	10,797	21.7
Jul–Aug 2010	19	0	0	0	19	38	0.1
Sep–Oct 2010	3,786	6,879	4,765	1,247	3,520	20,197	40.5
Nov–Dec 2010	37	83	87	18	48	273	0.5
Jan–Feb 2011	107	94	130	39	23	393	0.8
Mar–Apr 2011	136	264	305	46	81	832	1.7
Jul–Aug 2011	806	949	1,042	303	621	3,721	7.5
Sep–Oct 2011	505	692	612	195	518	2,522	5.1
May–Jun 2012	23	29	24	5	37	118	0.2
Total	9,996	14,803	13,007	3,332	8,672	49,810	100.0
Total (%)	20.1	29.7	26.1	6.7	17.4	100.0	

This table includes the 49,810 households surveyed by the ENIGH 2006–2014 in localities included in the debit card rollout, and shows the distribution of when the households were surveyed and when their localities received the debit card shock.

Table A.2: Effect of card shock on log POS terminals

Months since card shock	Corner stores	Supermarkets	All other businesses
–18 to –16	–0.025 (0.041)	–0.025 (0.034)	–0.001 (0.019)
–16 to –14	0.029 (0.040)	–0.019 (0.031)	0.008 (0.014)
–14 to –12	–0.011 (0.034)	–0.012 (0.027)	–0.002 (0.012)
–12 to –10	0.014 (0.028)	–0.029 (0.022)	0.019 (0.012)
–10 to –8	0.005 (0.026)	–0.052 (0.021)	–0.003 (0.009)
–8 to –6	–0.009 (0.026)	–0.016 (0.021)	0.002 (0.008)
–6 to –4	0.016 (0.025)	–0.023 (0.016)	–0.004 (0.007)
–4 to –2	–0.000 (0.023)	–0.015 (0.018)	0.004 (0.005)
–2 to 0 (omitted)	0	0	0
0 to 2	0.033 (0.019)	–0.001 (0.018)	–0.003 (0.006)
2 to 4	0.061 (0.024)	–0.023 (0.017)	0.002 (0.007)
4 to 6	0.037 (0.019)	0.003 (0.020)	–0.002 (0.008)
6 to 8	0.060 (0.022)	–0.011 (0.021)	–0.004 (0.010)
8 to 10	0.081 (0.025)	0.011 (0.025)	–0.013 (0.011)
10 to 12	0.076 (0.027)	–0.001 (0.025)	–0.001 (0.013)
12 to 14	0.085 (0.032)	0.003 (0.029)	0.000 (0.016)
14 to 16	0.103 (0.036)	–0.013 (0.032)	0.001 (0.017)
16 to 18	0.093 (0.037)	–0.008 (0.033)	–0.003 (0.017)
18 to 20	0.112 (0.040)	–0.011 (0.038)	–0.008 (0.018)
20 to 22	0.122 (0.043)	0.006 (0.039)	–0.005 (0.019)
22 to 24	0.135 (0.047)	–0.003 (0.042)	–0.014 (0.021)
24 to 26	0.169 (0.060)	–0.002 (0.052)	–0.011 (0.026)
<i>N</i> (locality $\times$ 2-month period)	8,806	8,806	8,806
Number of localities	259	259	259
Locality fixed effects	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes

This table shows the point estimates and standard errors from Figure 4. It shows the coefficients from (1), where the dependent variable is the log number of POS terminals by type of merchant (corner store, supermarket, or other). Observations are at the locality by two-month period level. Standard errors are clustered at the locality level.

Table A.3: Spillovers on consumption using de Chaisemartin and D’Haultfœuille (2020) estimator

	(1)	(2)	(3)
	Dependent variable: log spending at. . .		
	Corner stores	Supermarkets	Total
Standard diff-in-diff (Table 3)	0.067 (0.032)	−0.018 (0.043)	0.029 (0.030)
de Chaisemartin and D’Haultfœuille (2020) estimator	0.082 (0.048)	−0.025 (0.049)	0.012 (0.029)
Number of households	49,810	49,810	49,810
Number of localities	220	220	220
Locality fixed effects	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes

This table shows the robustness of the effect of the debit card shock on consumption at corner stores, supermarkets, and total to using the de Chaisemartin and D’Haultfœuille (2020) estimator for two-way fixed effects difference-in-differences. The upper rows reproduce the results from columns 1, 4, and 7 of Table 3, while the lower rows show the results using the de Chaisemartin and D’Haultfœuille (2020) estimator. Standard errors are clustered at the locality level; standard errors for the de Chaisemartin and D’Haultfœuille (2020) estimator are estimated using a cluster bootstrap (clustered at the locality level) with 2000 replications.

Table A.4: Changes in consumption and number of trips by quintile

	(1)	(2)	(3)	(4)	(5)	(6)
	Log spending		Log quantity		Number of trips	
	Corner store	Supermarket	Corner store	Supermarket	Corner store	Supermarket
Quintile 1	0.034	0.045	−0.007	−0.050	−0.044	−0.053
	(0.036)	(0.072)	(0.047)	(0.059)	(0.190)	(0.071)
Quintile 2	0.033	0.044	0.037	0.061	0.076	−0.003
	(0.036)	(0.065)	(0.048)	(0.056)	(0.191)	(0.077)
Quintile 3	0.050	0.004	0.027	−0.011	0.031	−0.004
	(0.041)	(0.064)	(0.047)	(0.049)	(0.189)	(0.067)
Quintile 4	0.080	0.031	0.019	−0.056	0.249	−0.134
	(0.043)	(0.056)	(0.055)	(0.047)	(0.181)	(0.067)
Quintile 5	0.138	−0.135	0.150	−0.168	0.791	−0.203
	(0.059)	(0.068)	(0.084)	(0.073)	(0.231)	(0.109)
P-values comparing quintiles						
1 vs. 5	[0.060]	[0.090]	[0.084]	[0.234]	[0.008]	[0.246]
2 vs. 5	[0.070]	[0.081]	[0.223]	[0.017]	[0.034]	[0.117]
3 vs. 5	[0.135]	[0.140]	[0.179]	[0.084]	[0.018]	[0.118]
4 vs. 5	[0.337]	[0.018]	[0.115]	[0.161]	[0.014]	[0.589]
Baseline mean	8.626	7.786	2.533	0.870	7.432	0.886
Number of households	49,810	49,810	49,810	49,810	49,810	49,810
Number of localities	220	220	220	220	220	220
Locality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Quintile × time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

This table shows the point estimates and standard errors from Figure 6 and Figure A.24. Each column is from (4) where the outcome variable is log spending in pesos at the particular store type (corner stores or supermarkets) in columns 1–2, log(kilograms + liters purchased) at the particular store type (restricted to goods with quantities purchased recorded in the consumption data) in columns 3–4, and number of trips over the course of one week to the particular store type in columns 5–6. Standard errors are clustered at the locality level.

Table A.5: Changes in log spending by category and store type: food

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

This table shows a breakdown by product category of the shifts in consumption by income quintile. It includes *food* product categories; non-food product categories are in Table A.6. Each column and panel shows coefficients from a separate regression using specification (4), where the outcome is log spending on a particular product category from the consumption module of ENIGH. The “total” in panel C includes spending not only at corner stores and supermarkets but also at other types of stores such as open-air markets. Standard errors are clustered at the locality level.

Table A.6: Changes in log spending by category and store type: non-food

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

This table shows a breakdown by product category of the shifts in consumption by income quintile. It includes *non-food* product categories; food product categories are in Table A.5. Each column and panel shows coefficients from a separate regression using specification (4), where the outcome is log spending on a particular product category from the consumption module of ENIGH. The “total” in panel C includes spending not only at corner stores and supermarkets but also at other types of stores such as open-air markets. Standard errors are clustered at the locality level.



Table A.7: Effect of card shock on wages and prices

	(1)	(2)	(3)	(4)
	Log wage		Log price	
	Corner store	Supermarket	Corner store	Supermarket
Diff-in-diff	0.006 (0.018)	0.002 (0.016)	0.002 (0.004)	−0.000 (0.003)
Baseline mean	9.450	9.133	3.162	3.278
Number of observations	83,222	96,380	531,762	979,108
Number of localities	250	244	72	64
Municipality fixed effects	Yes	Yes		
Quarter fixed effects	Yes	Yes		
Good-by-store fixed effects			Yes	Yes
Two-month period fixed effects			Yes	Yes

This table shows difference-in-differences estimates of the debit card shock on wages (using the quarterly labor force survey) and prices (using the INEGI microdata used to construct Mexico's CPI), for increased precision relative to the event study estimates in Figures A.13 and A.16. In columns 1 and 2, the observation is at the employee by quarter level; in columns 3 and 4, the observation is at the barcode-level product by store by 2-month period level. Standard errors are clustered at the municipality level.

Table A.8: Clustered randomization inference p-values for retail firm outcomes

	(1) Log Sales	(2) Log Inventory Costs	(3) Log Wage Costs	(4) Log Number Workers	(5) Log Rent Costs	(6) Log Capital	(7) Log Electricity Costs	(8) asinh Profits	(9) Charged VAT or Paid Social Security
<i>Panel A: Corner stores</i>									
Shock 3–4.5 years ago	[0.080]	[0.156]	[0.362]	[0.982]	[0.370]	[0.615]	[0.468]	[0.091]	[0.210]
Shock 1.5–3 years ago	[0.338]	[0.625]	[0.310]	[0.947]	[0.436]	[0.801]	[0.901]	[0.261]	[0.084]
Number of firms	172,441	172,441	172,441	172,441	172,441	172,441	172,441	172,441	172,441
<i>Pooled coefficient</i>									
Shock 1.5–4.5 years ago	[0.164]	[0.335]	[0.294]	[0.957]	[0.967]	[0.692]	[0.782]	[0.151]	[0.099]
Number of firms	172,441	172,441	172,441	172,441	172,441	172,441	172,441	172,441	172,441
<i>Panel B: Supermarkets</i>									
Shock 3–4.5 years ago	[0.117]	[0.070]	[0.158]	[0.540]	[0.463]	[0.545]	[0.256]	[0.949]	[0.300]
Shock 1.5–3 years ago	[0.176]	[0.147]	[0.124]	[0.350]	[0.715]	[0.332]	[0.102]	[0.956]	[0.380]
Number of firms	13,782	13,782	13,782	13,782	13,782	13,782	13,782	13,782	13,782
<i>Pooled coefficient</i>									
Shock 1.5–4.5 years ago	[0.128]	[0.086]	[0.141]	[0.438]	[0.521]	[0.761]	[0.154]	[0.990]	[0.348]
Number of firms	13,782	13,782	13,782	13,782	13,782	13,782	13,782	13,782	13,782
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

This table shows clustered randomization-inference p-values for (5) where the omitted dummy corresponds to localities treated less than 1.5 years before the second census wave. 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 2000 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. The randomized permutations are clustered at the locality level.

Table A.9: Prices for identical goods in corner stores relative to supermarkets

	(1)	(2)
	Log price	
Corner store	−0.054 (0.049)	−0.098 (0.014)
<i>N</i> (barcode-level product × store × month)	1,256,221	1,685,223
Barcode-level product by locality by month fixed effect	Yes	Yes
Definition of corner store and supermarket	6-digit NAICS	4-digit NAICS

This table shows that supermarkets charge at least as much as corner stores for identical products. It shows estimates from (21) using price quotes at the barcode-level product by store by month level and including barcode-level product by locality by month fixed effects to compare identical products across corner stores and supermarkets in the same locality at the same point in time. Using six-digit NAICS codes, corner stores are defined by code 461110 and supermarkets by code 462111; using four-digit NAICS codes, corner stores are defined by code 4611 and supermarkets by code 4621. Standard errors are clustered at the product by store level.

Table A.10: Consumer value of supply-side response

Dependent variable: log share of expenditures at store type $s$ minus log share at outside option (1)	
Prospera beneficiaries	
Log price difference ( $-\alpha_k$ )	-3.33 (1.92)
Share of stores with POS ( $\theta_k$ )	0.24 (0.31)
New card adopters	
Log price difference ( $-\alpha_k$ )	-2.92 (1.26)
Share of stores with POS ( $\theta_k$ )	0.55 (0.21)
Existing card holders	
Log price difference ( $-\alpha_k$ )	-2.02 (1.29)
Share of stores with POS ( $\theta_k$ )	0.57 (0.22)
First-stage joint F-test	46.56
Number of observations	21,775
Locality by consumer type by store type fixed effects	Yes
Store type by consumer type by time fixed effects	Yes

This table shows results from (16) in Appendix E, which estimates the price elasticity of consumption across store types and the value of shopping at a store that has adopted a POS terminal. Observations are at the census tract by consumer type by store type by time level. There are two store types, corner stores and super markets (since the third store type, open-air markets, is treated as the outside option). Prices are instrumented by a within-region leave-one-tract-out price average. The share of stores with POS terminals is instrumented with the debit card shock. More detail about these instruments and the derivation of the estimating equation are in Appendix E. Standard errors are clustered at the locality level.

## **B Data (For Online Publication)**

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

### **B.1 Administrative data on debit card rollout**

My source of information on the timing of the card rollout is a locality by two-month period level administrative data set from Prospera that includes the total number of families receiving government transfers in each locality at each point in time, as well as the payment method by which they receive their transfers. The data span 2009–2016 and include 5,807,552 locality by two-month period observations because all 133,932 localities included in the Prospera program are included in the data set; I restrict it to the 630 urban localities eligible to be included in the rollout, and after using these data to determine which urban localities were included in the rollout I further restrict these data (and all other data sets I use in the analysis) to those 259 localities.<sup>48</sup> In addition, I have data at the locality by year level for the years 2007 and 2008, which I combine with the data for 2009–2016 when testing whether the rollout was accompanied by an overall expansion of the program to new beneficiaries (Prospera, 2007–2016).

### **B.2 Transactions of Prospera beneficiaries**

These data include the universe of transactions made by cash transfer beneficiaries (Bansefi, 2007–2015). The data set includes 106,449,749 transactions from 961,617 accounts. The data include type of transaction (including cash withdrawals, card payments, deposits, interest payments, and fees), amount in pesos, a timestamp, and other details about each transaction. I use this data set to measure whether the beneficiaries who directly received cards as part of the exogenous shock I use for identification are indeed using the cards to make purchases at POS terminals. Further-

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<sup>48</sup>In addition, I validate the rollout information provided by Prospera using data from the government bank Bansefi that administers the accounts. In these data, described in Appendix B.2, I observe when the beneficiary is switched to a debit card account.

more, the data contain a string variable with the name of the business at which each debit card purchase was made, which allows me to manually classify whether the purchase was made at a supermarket, corner store, or other type of business. I use these classifications to create a variable of the proportion of transactions made by Prospera beneficiaries at supermarkets, which I use for a heterogeneity test.

### **B.3 Universe of POS terminal adoptions**

Data on POS terminal adoption comes from Banco de México (Mexico’s Central Bank). The data are reported to the Central Bank by the Asociación de Bancos de México (Mexican Bank Association), which is made up of representatives from each bank in Mexico and which collects the data from the individual banks. I use two underlying data sets to construct a data set with the number of businesses with POS terminals during each two-month period since 2006 (aggregated to the two-month period for consistency with the administrative rollout data): (1) a data set of all changes to a POS terminal contract since 2006, which contains five million contract changes including 1.7 million POS adoptions, as well as cancellations and changes to contract terms; (2) a data set with all currently active POS terminals, which I use to back out the number of existing POS terminals at the beginning of 2006 that did not have any contract changes from 2006 to 2017 (Banco de México, 2006–2017).

These data sets include the store type (e.g., corner store, supermarket)—which is determined by the merchant category code (MCC). A catalog of MCC codes is provided by Knaddison (2012). They also include an anonymized firm ID and the postal code in which the firm is located. Because the card rollout occurred at the locality level and my demand estimation is at the AGEb (census tract) level, and because neither an official mapping between localities or AGEbs and postal codes nor complete shapefiles for postal codes exist, I create a crosswalk between postal codes and localities using a census of firm geocoordinates in Mexico which includes both the postal code of each firm and its geocoordinates to determine its AGEb and locality.<sup>49</sup> This data set on the geocoordinates of the universe of firms is described in more detail in Appendix B.11.

### **B.4 Universe of card transactions at POS terminals**

These data include debit and credit card transactions at POS terminals from July 2007 to March 2015 (Banco de México, 2007–2015). The data include an anonymized indicator of the acquiring and issuing bank for each transaction, type of business (MCC code), type of card (credit or debit),

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<sup>49</sup>Shapefiles for a partial set of postal codes are available at <https://datos.gob.mx/busca/dataset/poligonos-de-codigo-postal>, but a substantial fraction of postal codes are not included in the data set. I contacted Mexico’s Postal Service, which produced the data set, and they reported that they have not yet completed the project of constructing shapefiles for all postal codes in Mexico.

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

A 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 clearing house. Because the debit card shock ended in mid-2012, I am thus only able to show event study effects up to one year after the shock.

### **B.5 Quarterly data on debit cards by issuing bank by municipality**

Mexico's National Banking and Securities Commission (CNBV) publishes quarterly—and, since April 2011, monthly—data on a number of measures related to banks' operating activities (CNBV, 2008–2016). These numbers are reported at the bank by municipality level, and include the number of ATMs, number of bank branches, number of employees, number of checking and savings accounts, number of debit cards, and number of credit cards. The data also include the number of POS terminals, but not by type of business and only since April 2011. I use these data to (i) present descriptive statistics on financial technology adoption on the two sides of the market, (ii) test whether the card rollout is correlated with pre-treatment levels and trends of financial infrastructure, and (iii) test for spillovers of retailer POS adoption on other consumers' card adoption. Because the data are at the bank level, I can exclude cards issued by Bansefi—the bank that administers Prospera beneficiaries' accounts and debit cards—for the spillover test.

These data are at the municipality level, which is larger than a locality (the level of the card rollout). Nevertheless, most urban municipalities only include one urban locality; because my analysis focuses only on urban localities, using municipality rather than locality for these results should merely create noise that attenuates any observed effect. I restrict to municipalities with at least one urban locality, and consider a municipality as treated at a particular time if it contains an urban locality that has been treated by that time. Of Mexico's 2,458 municipalities in 2010, 521 contain at least one urban locality, and 255 of these are included in the debit card rollout.

The number of debit and credit cards are first included in the data in the last quarter of 2008, as are the number of ATMs; in total, the data include 139,420 municipality by quarter (or month starting in April 2011) observations from the last quarter of 2008 to the last month of 2016. For consistency over time, I use the last month of each quarter from 2011–2016 so that the data are at the municipality by quarter level throughout. The number of debit card and number of credit card variables measure the stock of cards as of the last day of the quarter. These data also include the

number of POS terminals (not differentiated by type of store) and the number of ATM transactions, but only since April 2011; this is why the descriptive figures comparing card and POS adoption across all municipalities in Mexico (Figures 1b and 2) begin in April 2011, and why the event study of the number of ATM transactions (Figure A.10) only includes 12 months of pre-period observations rather than 18. Other variables, such as the number of bank accounts and number of bank branches, extend back to 1995.

## **B.6 Consumption data by store type from household survey**

I use the Encuesta Nacional de Ingresos y Gastos de los Hogares (ENIGH), Mexico’s household income and expenditure survey (INEGI, 2006–2014). The survey is a repeated cross-section conducted every two years by INEGI. Because the card rollout occurred between 2009 and 2012, I use the 2006–2014 waves of the ENIGH. In the survey’s consumption module, each household is asked to record all purchases over the course of a one-week period in a consumption diary format. For each item purchased, they record the product, total expenditure, quantity purchased (for food items only), and type of store such as open-air market, corner store, supermarket, etc. I use these data to construct a measure of total spending at each of the different types of store. The survey also includes a detailed income module, which allows me to measure household income per capita, which I use to test for heterogeneity throughout the income distribution. In addition, I use data in the survey about whether a household is a Prospera beneficiary (based on questions about income, scholarships, or health services received through Prospera) and whether a household has a credit card—as the survey does not ask about debit card or bank account ownership.

Across all survey years, there were 106,351 households included in the survey. Of these, I restrict the analysis to the 49,810 households living in localities included in the rollout (220 of the 259 treated urban localities are included in ENIGH). The ENIGH is used extensively both by the government—for example, to construct its official poverty statistics—and by researchers (e.g., Atkin, Faber and Gonzalez-Navarro, 2018). The data are publicly available, with exception of below-municipality level geographic identifiers which are confidential in the survey waves prior to 2012. To determine which households live in treated localities, I obtained the locality identifier corresponding to each household from INEGI. I also obtained a finer-grained geographic variable, the “basic geographic area” (AGEB), which I use in the demand estimation in Section V.B. Within Mexico’s 630 urban localities, there are 61,424 AGEBS, making them roughly analogous to census tracts in the US.

## **B.7 Google Trends data on Google searches for supermarkets**

**Supermarkets.** I query Google Trends for data on searches for supermarkets at the locality by month level (Higgins, 2024). Over the relevant time period, Google Trends data are only available



down to the state level; however, I am able to construct a locality by month level data set by querying Google searches for “[supermarket name] [locality name]”; I also restrict the query to searches that occurred in the state in which the locality is located. The idea of this query is that when people search for the location or hours of the nearby branch of a supermarket chain, their search will often include both the name of the supermarket and the name of the locality in which they live. The overall time window for the queries is January 2008 to February 2017 to match the timing of the POS terminal adoption data from Mexico’s Central Bank; each query returns data for each month of this time window.

I first describe how data from Google Trends are measured, following Oster (2018). For a given “[supermarket name] [locality name]” query, first define a search rate as:

$$\theta_{jt} = \frac{\text{Number of searches for “[supermarket name] [locality } j\text{]” in state } s(j) \text{ at time } t}{\text{Total number of searches in state } s(j) \text{ at time } t}.$$

The Google Trends API does not return the search rate  $\theta_{jt}$ , but it instead returns a relative search intensity given by

$$(7) \quad \tau_{jt} = \frac{\theta_{jt}}{\max(\theta_{jt})}.$$

I first determine which supermarkets are the most-searched in Mexico over the pre-rollout period 2006–2008 by conducting queries for “[supermarket name]” (without “[locality name]” in the query); I search for all supermarket chains included here: [https://en.wikipedia.org/wiki/List\\_of\\_supermarket\\_chains\\_in\\_Mexico](https://en.wikipedia.org/wiki/List_of_supermarket_chains_in_Mexico), with the exception of supermarket chains that have names that correspond to commonly-used words in Spanish (MEGA, Nena’s, Blanco, and Gigante) as those search terms would include searches unrelated to supermarkets. The reason for restricting the locality-level queries to only the three most-searched supermarkets is that multiple supermarket names can be included in the same query separated by “+”, but the overall query is constrained by a character limit. In practice, given the length of locality names, I could query up to three supermarkets at once. The results from a query for three supermarkets would not be comparable to a query for three different supermarkets as each query would have a different  $\max(\theta_{jt})$  in (7). The three most-searched supermarket chains in Mexico over 2006–2008 were Walmart, Soriana, and Comercial Mexicana. Thus, the query for locality Apodaca would be “Walmart Apodaca + Soriana Apodaca + Comercial Mexicana Apodaca” (where “+” means *or* in Google Trends queries), and the query would be restricted to searches from the state of Nuevo León, which is where Apodaca is located.

Each time Google Trends is queried, the results are selected from a sample of searches for that query; Google’s documentation does not provide additional detail about the size of the sample that

is used. Because of this sampling, the results returned by Google Trends vary each time Google Trends is queried. I thus downloaded the same query for 18 days and took the average across each day's data to average out noise. In addition, Google Trends bottom-codes results at 0 when there are too few searches, but the documentation does not specify the threshold for bottom-coding.

Using this method, the Google Trends supermarket queries return non-zero results for 127 of the 259 treated localities, which is the sample of localities that I include in the event study regressions with log Google search intensity for supermarkets as the outcome variable. I do not include the localities with zero search intensity for all periods as these localities may have had non-zero but bottom-coded search intensity.

**Corner stores.** It was not possible to obtain high-quality data on Google searches for corner stores for a number of reasons. First, since corner stores are independent stores rather than chains, I could not search for the three main stores as I did for supermarkets. Second, most of these stores did not have an online presence during the time period of this study, whereas supermarket chains did, so people would not have been Googling specific corner stores. Third, I tried generic corner store search terms (searching “*tiendita* [locality name] + *tienda de abarrotes* [locality name]”, where “+” means *or* in Google Trends queries) but—likely for the reasons explained above—there were far fewer Google searches for corner stores using these generic search terms than there were for supermarket chains. Specifically, only 7 out of 259 localities have any Google trends data for searches for corner store search terms over 2006–2017.

**Weather.** Finally, I query Google Trends for data on searches for “weather” as a placebo test to ensure that the trends observed in searches for supermarkets are not driven by differential changes in overall search patterns across localities. The specific search terms that I query are “*clima* [locality name]”. The Google Trends weather queries return non-zero results for 227 of the 259 treated localities.

## **B.8 Economic Census on the universe of retailers**

Every five years, Mexico's National Statistical Institute, the Instituto Nacional de Estadística y Geografía (INEGI), conducts an Economic Census of the universe of firms in Mexico (INEGI, 1993–2013). This census includes all retailers, regardless of whether they are formally registered (with the exception of street vendors who do not have a fixed business establishment). I use the 2008 and 2013 census waves since these years bracket the rollout of cards; I cannot include additional pre-periods in the main regressions because the business identifier that allows businesses to be linked across waves was introduced in 2008, and my main specification includes firm fixed effects. I do use prior waves of the Economic Census to test for locality by store type parallel trends

of the outcome variables from 1993–2008.

The 2008 census includes about 4 million firms, about 2 million of which are retailers. Of the 2 million retailers, about 1 million are also observed in the 2013 census, indicating that they survived over the five year period between census waves. This rate of firm survival is consistent with estimates of firm survival in developing countries (McKenzie and Paffhausen, 2019). Of the retailers observed in both census waves, 354,820 are corner stores and 172,441 of those are in the urban localities included in the Prospera card rollout; 20,879 are supermarkets, department stores, and chain convenience stores included in both survey waves, of which 13,782 are in the urban localities included in the card rollout.

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

## **B.9 High-frequency product-by-store price panel**

Mexico collects weekly price estimates for food products and biweekly price estimates for other products to construct its consumer price index (CPI). I use the store by product by week price microdata to test whether the debit card shock had a general equilibrium price effect. Until 2010 the data were collected by Mexico’s Central Bank, and from 2010 on they were collected by INEGI. I have data from 2002–2014, with monthly price averages for each store by product observation through 2010 and weekly price quotes from 2010–2014 (?); as with the other data sets, I average across two-month periods for consistency with Prospera’s payment periods. After making this aggregation, the data set includes 5.4 million product by store by two-month period observations; over the twelve-year period, price quotes are collected from 122,789 unique stores for 313,915 barcode-equivalent goods (such as “600ml bottle of Coca-Cola”).

I again restrict the data to municipalities included in the card rollout.<sup>50</sup> Because the Mexican government focuses on the largest urban areas when collecting price data for its CPI, most stores are in urban municipalities included in the card rollout: after removing stores in other municipalities, there are still 4.9 million product by store by two-month period observations. For the event study regressions, I further restrict the analysis to the category of goods encompassing food, beverages, alcohol, and tobacco, as this is likely the main type of product for which consumers are deciding between purchasing at the supermarket and corner store.<sup>51</sup> Finally, for the event study re-

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<sup>50</sup>For each store, I have a string variable identifying the municipality name, but do not have a locality identifier. As a result, after using a catalog to convert municipality names to INEGI municipality codes (INEGI, 2017), I follow the same approach as with the CNBV data described in Appendix B.5.

<sup>51</sup>The other product categories are clothing, shoes and accessories; housing; furniture, appliances, and domestic

gressions I restrict to a balanced sample of products so that results are not affected by a change in the composition of products included in the price list. This leaves 531,762 price quotes from corner stores and 979,108 price quotes from supermarkets in the final data set used for the event study regressions.

## **B.10 Survey of corner store owners**

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 20–August 12, 2022.

**Sampling of municipalities.** First, a set of 29 urban municipalities in which surveys would be conducted was drawn from the set of 217 potential urban municipalities. The set of 217 potential municipalities are urban municipalities that were not included in the Prospera debit card rollout. Urban municipalities are defined as municipalities with at least one urban locality (which is the geographic level below municipality), where localities are defined as urban if they have at least 15,000 inhabitants. Municipalities were drawn using a weighted random sampling method, where the weight for each municipality was chosen to make the sample included in the survey match the bivariate distribution of POS terminal adoption and debit card adoption in the municipalities that were included in the Prospera debit card rollout. Priority was given to municipalities in states that are closer to Mexico City in order to reduce travel costs. This was accomplished by initially restricting the sampling to municipalities in eight states within driving distance of Mexico City: Guanajuato, Guerrero, Hidalgo, Estado de México, Morelos, Puebla, Querétaro, and Tlaxcala. However, for some bins from the bivariate distribution of POS terminal and debit card adoption there were not sufficient municipalities in these states to match the bivariate distribution in the municipalities included in the debit card rollout. Thus, after drawing from these states, the restriction was removed and an unrestricted weighted random sampling of municipalities was conducted to complete the sample of municipalities.

**Sampling of corner stores.** Within each sampled municipality, I first restricted to the urban locality within the municipality (as municipalities are one geographic level above locality; the CNBV data used for the sampling is at the municipality level but the debit card rollout was at the locality level). I then created a sampling frame of all corner stores in the municipality. The sampling frame was obtained from the 2021 National Statistical Directory of Economic Units (DENUE), compiled by the National Institute of Statistics and Geography (INEGI) in Mexico. This publicly available directory contains information of active businesses in the country and includes a business

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products; health and personal hygiene; transport; education and recreation; and other.

type identifier using the North American Industry Classification System (NAICS). The DENU is publicly available and includes name of the business, business type (6-digit NAICS code), address, geographic coordinates, size in terms of number of employees, and telephone number. The 6-digit NAICS code was used to filter DENU and only include corner stores (6-digit NAICS code 461110) in the sampling frame. The number of surveys to be conducted in each locality was also weighted to replicate the desired bivariate distribution of debit card and POS adoption faced by firms in localities included in the debit card rollout. Finally, to ensure a proportional geographic distribution of surveyed corner stores within each sampled locality, I set the number of successful surveys to complete within each of the 212 postal codes within the 29 sampled localities.

Figure A.23 shows that the bivariate distribution of debit card adoption and POS terminal adoption in municipalities of successfully surveyed corner stores (as of the end of 2021) closely matches that of corner stores in localities that were included in the debit card rollout (as of the period prior to the debit card shock occurring in their locality).

**Survey respondent.** I sought to survey the owner of the corner store. Thus, after a short introduction the surveyor asked whether the owner of the store was there. If not, the surveyor explained “We are trying to survey one of the people that makes important decisions about the business. Do you make important decisions about this business?” If the owner was present or the respondent answered “yes” to the second question, the survey continued. Otherwise, the surveyor asked when the owner would be present to try to reschedule the survey.

**Survey questions.** The survey first asks whether the corner store has a POS terminal. For corner stores with a POS terminal, the survey includes questions on the main reason they adopted a POS terminal; whether customers asked to pay by card before adopting a POS terminal; whether customers left the store without purchasing anything when they were told the store didn’t accept card payments (before adopting a POS terminal); how the number of customers, sales, and profits changed after adopting; costs of the POS terminal; whether the merchant passes through the costs either by raising overall prices or surcharging customers paying by card; whether the merchant imposes a minimum payment amount for card payments; spillovers of their POS adoption onto consumers’ card adoption; and what percent of customers pay by card. For corner stores without a POS terminal, it includes questions on whether they have considered adopting; the main reason they have not adopted or have not considered adopting; whether customers have asked to pay by card; whether customers have left the store without purchasing anything when they were told the store doesn’t accept card payments; their expectations about how the number of customers, sales, and profits would change if they adopted a POS terminal; the costs of adopting a POS terminal; and their expectations about spillovers of POS adoption on customers’

card adoption. The full survey questionnaire is available in English at [https://seankhiggins.com/assets/pdf/Higgins\\_FinancialTechnologyAdoption\\_survey.pdf](https://seankhiggins.com/assets/pdf/Higgins_FinancialTechnologyAdoption_survey.pdf) and in Spanish at [https://seankhiggins.com/assets/pdf/Higgins\\_FinancialTechnologyAdoption\\_survey\\_Spanish.pdf](https://seankhiggins.com/assets/pdf/Higgins_FinancialTechnologyAdoption_survey_Spanish.pdf).

**Response rates.** The survey team attempted surveys at 6,065 corner stores and successfully completed 1,760 surveys, for an overall response rate of 29%. The attempted surveys included corner stores that existed in the DENU data but had since closed permanently, as well as stores that were closed at the time the surveyor visited and stores that were open but in which the owner or decision-maker was not present. The response rate conditional on the store being open and the owner being present was 70%.

### **B.11 Auxiliary administrative data**

**Locality-level measures from population census.** INEGI conducts a comprehensive population census every ten years and an intermediate population census—which still includes a number of sociodemographic variables from all households in the country—every five years between full census rounds (INEGI, 2000–2020). I use locality-level summary statistics constructed from the 2005 intermediate population census (since this is the most recent census prior to the beginning of the debit card rollout) to test whether the card rollout is correlated with locality characteristics. I also measure changes in these variables relative to the same variables from the 2000 population census. I use the same characteristics that are used to measure locality-level development by INEGI and Mexico’s National Council for the Evaluation of Social Development (CONEVAL). These locality-level measures based on the population census are publicly available from <https://www.inegi.org.mx/programas/ccpv/2005/default.html#Microdatos> and <https://www.inegi.org.mx/programas/ccpv/2000/default.html#Microdatos>.

**Shapefiles.** I use polygons corresponding to the border of each state, municipality, locality, and AGEB (census tract) for several figures in the paper (INEGI, 2016). These shapefiles are publicly available from INEGI.

**Geocoordinates of the universe of retail firms in Mexico.** These data are a directory of all firms in Mexico, including the name and six-digit NAICS code of the firm (which allow me to identify the type of store), its postal code, and exact geocoordinates (INEGI, 2014–2021). This directory is publicly available and is thoroughly updated after each Economic Census. I combine these data with AGEB shapefiles (INEGI, 2016) to (i) create a mapping between AGEBs, localities, and postal codes since some of the Central Bank data are at the postal code level and (ii) determine

the number of each type of store by locality and municipality, which I use to construct the measure of the proportion of all retailers and proportion of each type of retailer that accepts cards at the postal code level, after merging the data with the number of retailers with POS terminals from the Central Bank data.

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

**Postal code to municipality mapping.** While a postal code to locality mapping is not available, a postal code to municipality mapping is available from Mexico’s postal service (SEPOMEX, 2018). I use this mapping when I need a mapping between municipalities and postal codes.

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

**Social Connectedness Index (SCI).** The Facebook SCI data report social connectedness between and within municipalities in Mexico (Facebook, 2020). Specifically, for the set of Facebook users in municipalities  $i$  and  $j$ , the data report the total number of friendship links from users in  $i$  to users in  $j$  as a fraction of all potential friendship links (number of users in  $i$  times number of users in  $j$ ). I use within-municipality social connectedness, i.e. the diagonal entries of a matrix of social connectedness between municipalities. These data can be obtained by submitting a proposal to `sci_data@fb.com`.

## **B.12 Auxiliary survey data**

**Quarterly labor force survey.** Mexico’s quarterly labor force survey, the Encuesta Nacional de Ocupación y Empleo (ENOE), conducted by INEGI, includes about 400,000 individuals in each survey wave. It is a rotating panel where individuals are included for five consecutive quarters (INEGI, 2005–2016). The data set includes questions about wages, current and former jobs, reason for termination of a previous job, municipality, and includes four-digit NAICS codes that I use to determine the type of store at which retail employees work. I use data spanning 2005–2016, which

include over 20 million individual by quarter observations. After restricting to urban localities included in the debit card rollout, there are 83,222 employees employed at corner stores at the time they are surveyed and 96,380 employees employed at supermarkets. These samples exclude owners of corner stores and supermarkets, whom I identify in the data using a question about whether the worker has a boss.

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

**Mexican Family Life Survey.** This survey has more detailed information about debit and credit card ownership than other household surveys in Mexico. The most recent wave of the Mexican Family Life Survey was conducted in 2009, prior to the debit card rollout in nearly all localities included in the rollout (Rubalcava and Teruel, 2013). The survey also includes detailed questions about income, as well as numerous other survey modules. I use the survey for summary statistics prior to the card rollout, such as the proportion of households with a debit or credit card across the income distribution. The 2009 wave includes 9,205 households; because the survey oversampled rural areas, just 4,234 of these households live in urban areas, which is the sample I use for the summary statistics presented in the paper. These data are publicly available from <http://www.ennvih-mxfls.org/>, with the exception of the questions about whether a household is a Prospera beneficiary and the income they receive from the program. To include that income in the household income aggregate, I requested and received these additional variables from the data provider.

**National Enterprise Financing Survey.** This survey of 3,469 firms was conducted jointly by CNBV and INEGI, and I accessed the data on-site at INEGI (INEGI, 2015). The data set includes a number of questions about the banking, financing, and payment methods used by small businesses. I use it for descriptive statistics on the fraction of firms of each type that accept card payments and the fraction of transactions that are paid by card conditional on a store accepting card payments.

**Payment Methods Survey of Prospera beneficiaries.** This publicly-available survey was conducted by Prospera after the card rollout was completed. Because it was conducted in mid-2012, most beneficiaries had already accumulated at least one year with the card at the time they were surveyed (Prospera, 2012). The data set includes 5,381 Prospera beneficiaries, 1,641 of whom live in localities included in the rollout and hence received cards. Restricting the analysis to these 1,641 who received cards, I use this data set to investigate whether Prospera beneficiaries open



other bank accounts after receiving a debit card, which could explain the increase in cards adopted at other banks. The survey includes questions about beneficiaries' use of financial services and their satisfaction with the debit cards.

## C Identification (For Online Publication)

### C.1 Identification Strategy

As described in Section III, the paper's main identification strategy is the following event study difference-in-differences specification:

$$(8) \quad y_{jt} = \lambda_j + \delta_t + \sum_{k=a}^b \phi_k D_{jt}^k + \varepsilon_{jt}.$$

In most cases, the outcome  $y_{jt}$  is for locality  $j$  and two-month period  $t$ .

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

I include 18 months prior to the shock and 24 months after the shock regardless of the data set being used (i.e.,  $a = -18, b = 24$ ). When this involves changes in the sample of localities underlying each coefficient (e.g., if a data set begins at the end of 2008, a locality treated in 2009 does not enter into the estimate for  $k = -18$  because that locality has no observations in the data set 18 months before it is treated), I also show results for the balanced sample of localities over the more restricted time span for which I can include all localities in the rollout in the estimate of each coefficient.<sup>52</sup> In the data sets in which the time dimension is already aggregated at a level higher than two-month periods, I use these periods as  $t$ ; for example, the CNBV data described in Section II.C are at the quarterly level. For data sets at the annual level, which are used in the tests for confounding factors below, I set  $a = -3$  years and  $b = 3$  years since there would be few coefficients if I used the standard limits of 1.5 years before and 2 years after the shock.

As in most event study specifications (e.g., McCrary, 2007; Atkin, Faber and Gonzalez-Navarro, 2018), I do not drop observations that are further than 18 months prior to or 24 months after the shock, but rather “bin” these by setting  $D_{jt}^{-18} = 1$  if  $k \leq -18$  and  $D_{jt}^{24} = 1$  if  $k \geq 24$ . Because I

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<sup>52</sup>To facilitate discussion I described  $k$  as the number of months even though time periods are aggregated to the two-month level; hence, the term  $\sum_{k=a}^b \phi_k D_{jt}^k$  in (8) is a slight abuse of notation, as it will actually include every other integer between  $a$  and  $b$ , rather than every integer. Each of these integers would represent a two-month period.

only include localities that were included in the debit card rollout in all event study results—since localities excluded from the debit card rollout are observably different and thus could differ from treated localities in ways that could have a time-varying effect on the outcomes of interest—there is no “pure control” group that has  $D_{jt}^k = 0$  for all  $k$ . When there is no pure control group, “binning” in this way is required in order to identify the calendar time fixed effects (McCrary, 2007; Borusyak and Jaravel, 2016).

## C.2 Potential Confounds

An important potential confound would be if the rollout of debit cards was accompanied by a *differential* expansion of the Prospera program to additional beneficiaries—which would confound my results as any effect of the card rollout could then merely be an effect of increased transfer income in the locality. I estimate (1) with  $y_{jt}$  as the log number of Prospera beneficiaries in locality  $j$  (regardless of the method of transfer payment in locality  $j$ ) in the last payment period of year  $t$ . I use years rather than two-month periods since the administrative data on the number of Prospera beneficiaries is available only at the annual level in 2007 and 2008.<sup>53</sup> Figure A.3a shows the results: there is no differential change in the number of beneficiaries that occurs at the same time as the card rollout. None of the point estimates either before or after the shock is statistically significant from zero. It is also worth noting that the overall number of beneficiaries in the program was largely static by the time of the debit card rollout: the number of beneficiaries was growing at a rate of only 2% per year (and, as tested above, was not growing differentially in areas that received the card shock earlier). While I do not have data on the total benefits disbursed in each locality, because benefits are based on a strictly-followed formula, the absence of a differential trend in the number of beneficiary households suggests that there was no differential trend in total transfer payments correlated with the card rollout either.

Another potential confound would be if the rollout of debit cards was correlated with local politics, e.g. if the program decided to first distribute cards in areas where the party in power at the local level was the same party as the one in power at the national level. This does not appear to be the case, however. I use hand-digitized data from municipal elections, which contain vote shares for each party at the municipal level, to construct a variable that equals 1 if the municipal mayor belongs to the PAN party, which was the party of Mexico’s president during the debit card rollout. I include this variable in the discrete time hazard estimation in Table 1 and also show that it neither exhibits differential pre-trends nor is impacted by the debit card shock in Figure A.3b. I also include a variable for the PAN vote share in Table 1 (but cannot conduct an event study for this variable given the infrequency of elections).

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<sup>53</sup>The data correspond to the last payment period of those years; for 2009–2016 I thus use data only from the last payment period of the year to make it consistent with the earlier data.

### C.3 Discrete Time Hazard

To test whether the timing of the rollout is correlated with levels or trends in locality-level observables in a way that accounts for the staggered timing of the card shock in different localities, I model the probability of receiving cards in period  $t$  among accounts that have not yet received cards by period  $t - 1$  as a function of baseline levels and trends using a discrete-time hazard model (Jenkins, 1995). As in Galiani, Gertler and Schargrotsky (2005), I include a fifth-order polynomial in time. Changes are measured from 2006–2008 whenever possible, and from 2000–2005 (the two most recent pre-rollout population census waves) for the INEGI variables. I use number of bank accounts rather than number of debit cards because debit cards were only included in the CNBV data beginning in the last quarter of 2008 so their pre-trend cannot be measured for early-treated localities. Nevertheless, exploiting the staggered rollout timing (i.e., that for later-treated localities pre-trends can be measured), Figure 3 shows that there is no differential pre-trend in debit card adoption. ATMs were also not included in the CNBV data until the last quarter of 2008, which is why commercial bank ATMs and government bank ATMs are the only variables in Table 1 that do not include changes in addition to levels.

The results are shown in Table 1 and discussed in Section III.

### C.4 Parallel Trends in Economic Census

I use the Economic Census data, which is collected once every five years, from 1993–2008 to test for pre-trends at the locality level for the all of the variables in Table 4. I am not able to include these pre-periods in the firm-level regression in (5) because the firm-level identifiers are only available starting in 2008. Nevertheless, I use the earlier waves of the Economic Census to test for parallel pre-trends at the locality level going back to 1993. I calculate locality-level averages separately for corner stores and supermarkets for the outcomes in Table 4 in levels, then transform these locality-level averages using the same transformation as in Table 4 (i.e., logs for most variables, inverse hyperbolic sine for profits, and no transformation for “Charged VAT or Paid Social Security” since that variable is a dummy variable at the firm level and a proportion at the locality level).

I then estimate

$$(9) \quad y_{jt} = \lambda_j + \delta_t + \sum_k \sum_{\tau} \gamma_{k\tau} [\mathbb{I}(\text{received cards at } k)_j \times \mathbb{I}(t = \tau)_t] + \varepsilon_{jt}$$

where  $y_{jt}$  is the log (or inverse hyperbolic sine) average outcome across corner stores or supermarkets in locality  $j$  in survey wave  $t$ ,  $\lambda_j$  are locality fixed effects, and  $\delta_t$  are time fixed effects. As in (5), 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. The omitted value of  $\tau$  is 2008.

Figures A.4 and A.5 show the results. In total there are 54 coefficients in each figure, which correspond to 9 variables  $\times$  (3 – 1) groups  $\times$  (4 – 1) years. For corner stores, only 1 out of 54 coefficients is statistically significant at the 5% level, and for supermarkets only 3 out of 54 coefficients are statistically significant at the 5% level, as could be expected by chance.

## **D Additional Alternative Explanations (For Online Publication)**

### **D.1 Increase in Corner Store POS Adoption**

One possibility is that it is easier to adopt a POS terminal after receiving a debit card, and that many corner store owners were Prospera beneficiaries who had wanted to adopt a POS terminal but only found it feasible to do so once they received a debit card. However, there are three main reasons that this is not the case. First, the government bank Bansefi that issued debit cards to Prospera beneficiaries does not offer POS terminals since it is a government bank founded to increase the financial inclusion of low-income households; this rules out that receiving a debit card from Bansefi would make it easier to obtain a POS terminal from the same bank. Second, while adopting a POS terminal from a bank does require setting up a bank account at the bank that issues the POS terminal, beneficiaries already had a bank account with Bansefi; even if Bansefi did issue POS terminals, there would be no additional benefit from having a debit card beyond the benefits of already having a bank account. Third, using the ENIGH survey, only 5% of households that include a corner store owner also include someone that receives Prospera benefits in the household; thus, even if it were easier for them to adopt POS terminals after receiving cards, the group of corner store owner Prospera beneficiaries is too small to explain the increase in corner store POS adoption.

### **D.2 Increase in Consumption at Corner Stores**

**Prices.** In Section IV.D, I test for a price effect using high-frequency product by store by week price data, and find point estimates close to 0 for the change in prices at both corner stores and supermarkets in response to the shock. Furthermore, I can rule out price effects large enough to explain the increase in consumption observed in Section IV.C. Nevertheless, here I use an additional test to see if the increase in consumption at corner stores can be explained by an increase in prices at those corner stores. For food items purchased in the ENIGH, the quantity purchased is also recorded, and follow-up questions are included so that this quantity can be converted into kilograms or liters. Thus, I construct a measure of the total quantity of food purchased, where quantity

is measured as the sum of kilograms and liters (depending on which unit a particular food item is measured in). Figure A.24 and Table A.4, columns 3 and 4, show that it is not just the amount spent (price  $\times$  quantity) at corner stores that increases for the richest quintile, but also the quantity purchased. Specifically, the richest quintile increases quantity purchased from corner stores by 16% and decreases quantity purchased from supermarkets by 15%.

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

It is nevertheless an empirical question whether many Mexican retailers impose minimum card payment amounts in practice. Figure A.22 shows histograms of debit card transaction amounts for transactions made at POS terminals by all card holders in Mexico, using the Central Bank data on the universe of card transactions at POS terminals described in Section II.B. Over 20% of all transactions at corner stores are between 0 and 20 pesos, which is less than \$2, and over 50% of all transactions at corner stores are for less than 40 pesos. The high prevalence of these small transaction sizes suggests that most corner stores do not impose a minimum payment, or that if they do, the minimums are quite low. This is corroborated by the survey of corner stores that I conducted (described in Section II.E and Appendix B.10). In the survey, 77% of corner stores with a POS terminal do not impose a minimum payment amount for card payments. Those that do impose a minimum impose a very low one: conditional on imposing a minimum payment amount to pay by card, the median of this minimum payment amount is 20 pesos (about \$1 at the time the survey was conducted). The vast majority of corner stores have not changed whether they charge a minimum over time: 87% still do the same thing they have done since they adopted a POS terminal (i.e., if they currently impose a minimum payment, they always did, and if they currently do not impose a minimum payment, they never did).

**Supermarket data breaches.** The richest quintile’s 13% decrease in consumption at supermarkets and substitution to corner stores could be driven by a “push” (i.e., a reason to shop less at supermarkets) rather than a “pull” (a reason to shop more at corner stores, namely that more of them have now adopted POS terminals). One potential “push” is data breaches at supermarket chains: Agarwal et al. (2022) show that a data breach in India led to a temporary reduction in the use of digital payments, but that this effect was short-lived and disappeared by the third month after the breach. It is unlikely that data breaches caused the richest quintile’s shift in consumption to corner stores, as this 13% reduction is a difference-in-differences result, comparing the change in the richest quintile’s consumption between treated and not-yet-treated localities. Thus, data breaches or reporting on these breaches would need to occur *differentially* across localities over time in a way that is correlated with the card rollout—otherwise the effects of these data breaches on supermarket consumption over time would be absorbed by the time fixed effects. It is unlikely that data breaches were correlated with the card rollout given the tests in Section 4 and Table 1 showing that the timing of the rollout is not correlated with levels or trends in locality-level observables.

### D.3 Increase in Corner Store Sales

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

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

## E Discrete–Continuous Choice Model (For Online Publication)

### E.1 Summary of Method and Results

I use a simple theoretical framework to quantify the consumer gains accruing from retail firms’ technology adoption in response to the debit card shock. I then quantify what fraction of the total consumer gains are spillovers to consumers who did not directly receive cards from the Mexican government, which provides a quantitative estimate of how large the indirect network externalities are in this two-sided market.

Specifically, I estimate consumer gains for three types of consumers who had cards after the policy shock, and thus benefited from the increase in supply-side POS terminal adoption: (i) Prospera beneficiaries; (ii) existing card holders; and (iii) non-beneficiaries who adopted cards in response to the shock. I impose structural assumptions on consumer utility and combine data on consumption and local product prices across store types with data on point-of-sale terminal adoptions and store geocoordinates to estimate a discrete–continuous choice model. My empirical strategy is related to the discrete–continuous choice literature that began with Hanemann (1984); it combines features of the demand models in Atkin, Faber and Gonzalez-Navarro (2018), Björnerstedt and Verboven (2016), and Dolfen et al. (2023).

**Model.** The model requires several assumptions, and the results should thus be interpreted with the appropriate caveats. First, I assume that for each trip that an individual makes, the individual has a set budget and decides where to make the shopping trip. Empirically, supermarkets are on average farther than corner stores and charge more for identical products (based on a regression using price data with barcode by locality by week fixed effects), but supermarkets also accept card payments and offer other amenities.<sup>54</sup> These other amenities—which could include, for example, greater product variety (as in Atkin, Faber and Gonzalez-Navarro, 2018; Li, 2021)—are included in the model as unobservables. Corner stores, on the other hand, may or may not accept card payments.

I assume that consumers have Cobb–Douglas preferences over the goods they consume and also get some utility from store-specific characteristics, possibly including whether the store accepts

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<sup>54</sup>Specifically, to compare the prices charged for identical goods at corner stores and supermarkets, I use product by store by week level price quotes, restrict to price quotes from corner stores and supermarkets using four- or six-digit NAICS codes, and estimate  $\log Price_{gst} = \lambda_{gj(s)t} + \beta \mathbb{I}(\text{Corner})_s + \varepsilon_{gst}$ . Appendix F goes into more detail on the finding that corner stores charge lower prices than supermarkets for identical products.

card payments.<sup>55</sup> Specifically, consumer  $i$ 's utility from trip  $t$  to store  $s$  is

$$(10) \quad u_{ist} = \left( \prod_g x_{igst}^{\phi_{a(i)gst}} \right)^{\alpha_{k(i)}} \cdot \exp(\theta_{k(i)} POS_{ist} + \xi_{a(i)k(i)st} + \varepsilon_{ist}),$$

where  $a(i)$  denotes the census tract in which household  $i$  lives,  $k(i)$  denotes consumer groups over which the parameters  $\alpha$  and  $\theta$  are allowed to vary,  $x_{igst}$  is the quantity of product  $g$  purchased by household  $i$  during trip  $t$  to store  $s$ ,  $\sum_g \phi_{a(i)gst} = 1 \forall a, s, t$ ,  $POS_{ist} = 1$  if store  $s$  at which household  $i$  makes trip  $t$  has adopted a POS terminal,  $\xi_{a(i)k(i)st}$  capture preferences over other (potentially unobserved and time-varying) store characteristics and taste shifters that are common within census tract by consumer group by store by time, and  $\varepsilon_{ist}$  are unobserved individual by store by time shocks.

The key parameters of interest from (10) are  $\alpha_k$ , which measures consumer group  $k$ 's price elasticity, and  $\theta_k$ , which measures the value consumer group  $k$  attaches to a store having a POS terminal.<sup>56</sup> Appendix E.2 shows how  $-\theta_k/\alpha_k$  can be interpreted as the price-index-equivalent willingness to pay of consumers to shop at a store with a POS terminal. Thus,  $-\theta_k/\alpha_k$  multiplied by the observed change in the fraction of retailers with a POS terminal as a result of the debit card shock can be plugged into a first-order approximation of the proportional change in consumer surplus induced by a price change. Doing so quantifies the consumer gains from the increase in POS terminal adoption induced by the government's disbursement of debit cards, again with the caveat that estimating these consumer gains requires several assumptions.

Appendix E.2 also shows how the discrete–continuous choice problem with the indirect utility in (10) can be used to derive an estimating equation for log expenditure shares at the census tract by consumer group by store type by time level, which I estimate using a combination of data on expenditure shares and prices from ENIGH, number of retailers with POS terminals from the Central Bank, and total number of retailers by census tract from a data set with the geocoordinates of the universe of retail firms in Mexico.

**Estimation results.** I estimate  $\alpha_k$  and  $\theta_k$  for the three consumer groups described above; the results are shown in Table A.10. First, I find  $\alpha_k$  of 3.33 for Prospera beneficiaries. Noting that  $\alpha_k + 1$  gives the elasticity of substitution across store types if utility exhibits constant elasticity of substitution (CES) across store types (as shown in Appendix E.2), this estimate of  $\alpha_k$  is at the

<sup>55</sup>Atkin, Faber and Gonzalez-Navarro (2018) assume Cobb-Douglas preferences over product categories, while Björnerstedt and Verboven (2016) show how assuming “constant expenditures demand” (or, equivalently, Cobb-Douglas preferences) affects the estimating equation relative to the unit demand assumption in Berry (1994).

<sup>56</sup>Note that if corner stores surcharge customers who pay by card (as many do; see footnote 39), this is accounted for in the model as it will lead to a lower estimate of  $\theta_k$  because we will observe fewer people switching consumption to stores with POS terminals.



upper end of the range of estimates from Atkin, Faber and Gonzalez-Navarro (2018) and Dolfen et al. (2023), which makes it conservative for estimates of consumer surplus. The estimates of  $\alpha_k$  for other consumers are lower, at 2.02 for existing card holders and 2.92 for new card adopters. These magnitudes are consistent with richer consumers being less price elastic (although I do not have enough power to reject no difference between the estimates for each group), and they are also in the range of price elasticity estimates from Atkin, Faber and Gonzalez-Navarro (2018) and Dolfen et al. (2023).

Existing card holders and new card adopters put a higher value on the store having a POS terminal than Prospera beneficiaries. Specifically, the estimates of  $\theta_k$  are 0.57 and 0.55 for existing card holders and new card adopters (each significant at the 1% level), while the value for Prospera beneficiaries is 0.24 (not statistically significant). Under the assumptions of the model, we can interpret  $-\theta_k/\alpha_k$  as the price index equivalent value of *all* stores adopting POS relative to a scenario in which *no* stores have adopted POS: this extreme change in technology adoption would be equivalent, from a consumer surplus perspective, to a 28% price reduction for existing card holders and to a 7% price reduction for Prospera beneficiaries. Given that nearly half of Prospera beneficiaries use their cards for POS transactions (Figure A.1), the price-index-equivalent value they derive from a store having a POS terminal *conditional on being a beneficiary who uses the card* is thus about half of the value derived by existing credit card holders.

**Consumer gains.** Under the assumptions of the model, the consumer gains from the increase in POS terminal adoption, measured as a percentage of household expenditure, can be written as

$$(11) \quad \left[ \sum_s \phi_{ks}^1 \frac{\theta_k}{\alpha_k} \Delta POS_{ks} \right] - \frac{A_k}{y_k}$$

for each consumer group  $k$ , where  $\phi_{ks}^1$  is the share of  $k$ 's expenditure spent at store type  $s$  after the shock,  $\Delta POS_{ks}$  is the change in the fraction of stores of type  $s$  that have POS terminals that can be used by consumer group  $k$ ,  $A_k$  is the cost of card adoption paid by consumer group  $k$ , and  $y_k$  is total expenditures by consumer group  $k$ . Appendix E.2 derives (11), following Atkin, Faber and Gonzalez-Navarro (2018), and provides more details on its estimation for each consumer group.

With the caveat that these results require many assumptions as outlined above, I estimate that existing card holders experienced a 0.5% increase in consumer surplus as a result of retail POS adoption and that new card adopters experienced between a 0% (if they are just on the margin of adopting after the shock) and 0.4% (if they were just on the margin of adopting before the shock) increase in consumer surplus. Prospera beneficiaries have a larger  $\Delta POS_{ks}$  than existing card holders since they went from being able to use a card at no stores to being able to use a card at all retailers who had adopted ex post; they experience a 1.9% increase in consumer surplus.

Summing absolute gains across consumers, I estimate that between 52 and 55% of the increase in consumer gains caused by retailers' response to the policy of distributing debit cards to cash transfer beneficiaries accrued as spillovers to non-beneficiaries.<sup>57</sup>

**Cost–benefit.** These estimates of consumer gains can be used in a cost–benefit analysis. The cost of producing and distributing debit cards, documented by Bansefi and Prospera in a 2010 agreement between the two entities, was 27.5 pesos per card (\$2.18 per card in 2010 US dollars). Thus, the total cost of the debit card rollout was approximately 29 million pesos (2.3 million US dollars). This can be compared to the aggregate consumer surplus benefits. Even focusing exclusively on the spillovers accruing to non-beneficiaries from the value they place on being able to use a debit card at retail stores, the aggregate consumer value of spillovers in the first two years after the card shock was 37 to 41 times as large as the aggregate costs incurred by the Mexican government to provide debit cards. Because these spillover benefits accrue to richer consumers, this also speaks to the political economy of government policy to subsidize financial inclusion. Such spending may be politically popular even among richer households that pay a larger share of the taxes used to fund fiscal spending, thanks to its effect on retailers' POS terminal adoption and the resulting spillover benefits for richer households.<sup>58</sup>

## E.2 Further Details

In this subsection I provide more technical detail on the discrete–continuous choice model estimated above to quantify indirect network externalities and consumer gains.

**Estimating equation.** Starting with the indirect utility function in (10), the first order condition for good  $g$  from utility maximization with a linear budget constraint gives  $x_{igst} = \phi_{a(i)gst} y_{it} / p_{a(i)gst}$ . Plugging this into (10) and taking logs:

$$(12) \quad \log u_{ist} = \alpha_{k(i)} \log y_{it} - \underbrace{\alpha_{k(i)} \sum_g \phi_{a(i)gst} \log p_{a(i)gst}}_{\equiv v_{ist}} + \theta_{k(i)} POS_{ist} + \tilde{\xi}_{a(i)k(i)st} + \varepsilon_{ist},$$

<sup>57</sup>This percentage is large despite Prospera beneficiaries experiencing a larger proportional change in consumer gains because (i) there are 2.4 times as many existing card holders and new card adopters as beneficiaries and (ii) absolute gains are the relevant metric for calculating this percentage, and absolute gains are a function of expenditures, which are larger for existing card holders and new card adopters than for beneficiaries.

<sup>58</sup>As above, the range of estimates reflects the unknown cost of card adoption for new card adopters. Bansefi also incurred an average cost of 172 pesos per account per year to maintain beneficiaries' bank accounts, but since beneficiaries were already paid in bank accounts prior to receiving cards, this cost was already being incurred prior to the debit card rollout. Thus, I do not include it in the cost–benefit calculation. For a government considering both opening accounts for unbanked households and providing them with debit cards, my estimates suggest that spillover benefits would still be 2.7 to 3.0 times greater than costs.

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

Assuming overall utility for trip  $t$  is additively separable in the potential  $u_{ist}$  across stores (Domencich and McFadden, 1975), for a particular trip the consumer will choose the store that gives the most utility. Thus, if we define  $v_{ist} \equiv \alpha_{k(i)} \log y_{it} - \alpha_{k(i)} \sum_g \phi_{a(i)gst} \log p_{a(i)gst} + \theta_{k(i)} POS_{ist} + \tilde{\xi}_{a(i)k(i)st}$ , then the probability of choosing store  $s$  over all other stores  $r \neq s$  is  $\pi_{ist} = \text{Prob}(u_{ist} > u_{irt} \forall r \neq s) = \text{Prob}(\varepsilon_{irt} < \varepsilon_{ist} + v_{irt} - v_{ist} \forall r \neq s)$ . Integrating over the probability distribution that the store  $i$  chooses has adopted POS and integrating over the stochastic error term,

$$(13) \quad \begin{aligned} \pi_{ist} &= \int_{\varepsilon} \int_{POS} \mathbb{I}(\varepsilon_{ikt} < \varepsilon_{ist} + v_{ist} - v_{ikt}) f(POS) f(\varepsilon) dPOS d\varepsilon \\ &= \int_{\varepsilon} \mathbb{I}(\varepsilon_{ikt} < \varepsilon_{ist} + \gamma_{a(i)k(i)st} - \gamma_{a(i)k(i)rt}) f(\varepsilon) d\varepsilon, \end{aligned}$$

where  $\gamma_{a(i)k(i)st} \equiv -\alpha_{k(i)} \log p_{a(i)st} + \theta_{k(i)} POS_{z(a(i))k(i)st} + \tilde{\xi}_{a(i)k(i)st}$  and  $POS_{z(a(i))st}$  denotes the fraction of stores of type  $s$  that have adopted POS terminals at time  $t$  in postal code  $z(a(i))$  in which individual  $i$  lives.<sup>59</sup>

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

$$(14) \quad \pi_{ist} = \frac{\exp(\gamma_{a(i)k(i)st})}{\sum_r \exp(\gamma_{a(i)k(i)rt})}$$

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

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

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

where we can pull the  $\pi_{akst}$  out of the summation because it does not depend on  $i$ , and the last equality arises from plugging in the Marshallian demand  $x_{igst} = \phi_{a(i)gst} y_{it} / p_{a(i)gst}$  and recalling  $\sum_g \phi_{a(i)gst} = 1$ , or by noting that the first order condition for the budget constraint gives  $y_{it} = \sum_g p_{agst} x_{igst}$ .

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<sup>59</sup>I observe POS adoption at the level of the postal code. Postal codes are larger than census tracts but smaller than localities.

Substituting  $\phi_{akst}$  into (14) and taking logs gives the following expression for the share of expenditures at store type  $s$  by consumer group  $k$  in census tract  $a$  and survey wave  $t$ , denoted  $\phi_{akst}$ :

$$(15) \quad \log \phi_{akst} = -\alpha_k \log P_{ast} + \theta_k POS_{z(a)st} + \tilde{\xi}_{akst} - \log \sum_r \exp \gamma_{akrt}.$$

$P_{ast}$  is a Stone price index implicitly defined by  $\log P_{ast} = \sum_g \phi_{a(i)gst} \log p_{a(i)gst}$  (i.e. a consumption share-weighted average of log prices across goods),  $POS_{z(a)st}$  is the fraction of stores of type  $s$  in postal code  $z(a)$  that have POS terminals at time  $t$ , and  $\gamma_{akst} \equiv -\alpha_k \log P_{ast} + \theta_k POS_{z(a)st} + \tilde{\xi}_{akst}$ . Finally, to remove the  $\log \sum_r \exp \gamma_{akrt}$  term, I subtract the log share of spending on the outside option of open-air markets, denoted  $\phi_{ak0t}$ , which I assume do not accept card payments (i.e.,  $POS_{z(a)0t} = 0 \forall z(a), t$ ).<sup>60</sup>

This leads to the following estimating equation:

$$(16) \quad \log \phi_{akst} - \log \phi_{ak0t} = -\alpha_k (\log P_{ast} - \log P_{a0t}) + \theta_k POS_{z(a)st} + \eta_{j(a)ks} + \delta_{kst} + v_{akst}.$$

In this estimating equation I have rewritten  $\tilde{\xi}_{akst} - \tilde{\xi}_{ak0t} = \eta_{j(a)ks} + \delta_{kst} + v_{akst}$  so that the estimation will include locality by consumer group by store type and consumer group by store type by time fixed effects, where  $j(a)$  denotes the locality of census tract  $a$ .<sup>61</sup>

The left-hand side of (16) is a difference in log expenditure shares between the share of expenditures at store type  $s$  by consumer group  $k$  in census tract  $a$  at time  $t$ , denoted  $\phi_{akst}$ , and the expenditure share at the outside option. The right-hand side includes a difference in log price indices between store type  $s$  and the outside option (open-air markets), where the log price index is a weighted average of the log prices of each good, weighted by expenditure shares. It also includes the fraction of stores in postal code  $z(a)$  in which census tract  $a$  is located that have adopted point of sales terminals,  $POS_{z(a)st}$ , as well as locality by consumer group by store type fixed effects  $\eta_{j(a)ks}$  and consumer group by store type by time fixed effects  $\delta_{kst}$ .

**Interpretation of  $\alpha_k$ .** First,  $\alpha_k$  is a price elasticity. Second,  $\alpha_k + 1$  gives the elasticity of substitution across store types under constant elasticity of substitution (CES) utility. To see this, consider the simplified model with a composite good  $x_s$  available from each store type  $s$  and CES utility function  $U(\mathbf{x}) = \left( \sum_s x_s^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}}$ , where  $\sigma$  is the elasticity of substitution. The first order condi-

<sup>60</sup>There is no merchant category code for merchants at open air markets. Over the time period studied (up to 2014), it is reasonable to assume that no merchants at open air markets had adopted POS terminals to accept card payments. Today, now that non-bank e-payment companies (analogous to Square and Clover in the US) have entered the market, some open-air merchants have adopted technology to accept card payments.

<sup>61</sup>Because the household survey is not a panel of individuals, it is also not a panel at the census tract level. Including census tract by consumer group by store type fixed effects would entail the loss of many observations.

tions from maximizing utility subject to a linear budget constraint lead to the following expression for quantities consumed at store types  $s$  and 0:  $(x_s/x_0)^{-1/\sigma} = p_s/p_0$ . Multiplying both sides by  $(p_s/p_0)^{-1/\sigma}$ , taking logs, and simplifying gives  $\log(p_s x_s/p_0 x_0) = (1 - \sigma) \log(p_s/p_0)$ . Finally, dividing the numerator and denominator in the left-hand side by total expenditures,

$$(17) \quad \log \phi_s - \log \phi_0 = (1 - \sigma)(\log p_s - \log p_0),$$

where  $\phi_s$  is the share of expenditures at store type  $s$ . Comparing (17) to (16), we see that  $1 - \sigma = -\alpha_k$ , or  $\sigma = \alpha_k + 1$ .

**Interpretation of  $-\theta_k/\alpha_k$ .** From (15),

$$(18) \quad -\frac{\theta_k}{\alpha_k} = \frac{d \log \phi_{akst} / d POS_{ast}}{d \log \phi_{akst} / d \log P_{ast}} = \frac{d \log P_{ast}}{d POS_{ast}}.$$

Thus  $-(\theta_k/\alpha_k)$  gives the price index equivalent of a change from a world in which no stores have adopted POS terminals to one in which all stores have adopted POS terminals. Multiplying  $-(\theta_k/\alpha_k)$  by  $\Delta POS_{ks}$ , i.e. by the change in the fraction of stores of type  $s$  at which consumer group  $k$  can use cards, gives the price index equivalent value to consumers of the observed adoption of POS terminals by retailers in response to the debit card shock.

**Endogeneity.** There are two endogenous variables on the right-hand side of (16):  $(\log P_{ast} - \log P_{a0t})$  and  $POS_{z(a)st}$ , as both prices and POS adoption likely respond to demand shocks and are thus correlated with  $v_{akst}$ . I instrument for prices using a Hausman (1996) price index, which is based on prices in different areas. Specifically, following Atkin, Faber and Gonzalez-Navarro (2018), the instrument is the leave-one-out average price difference in *other* census tracts within the same region,  $\frac{1}{|R(a)|-1} \sum_{b \neq a \in R(a)} (\log P_{bst} - \log P_{b0t})$ , where  $|R(a)|$  is the number of census tracts in region  $R(a)$  in which census tract  $a$  is located.<sup>62</sup> The intuition behind this instrument, also used by Nevo (2000), is that prices have marginal cost and mark-up components; the mark-up charged in a particular census tract is endogenously affected by local demand in that census tract that is uncorrelated with demand in other areas, while the marginal costs are correlated across census tracts within a region due to common supply chains and distribution networks. A supply shock such as a disruption to the supply chain will affect marginal cost of the good throughout the region; the exclusion restriction is that the increase in marginal cost (and hence price) in other areas in the region caused by such a supply shock will affect demand in census tract  $a$  only through its correlation with the increase in marginal cost (and hence price) in census tract  $a$ .

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<sup>62</sup>There are five official regions in Mexico, defined by the Instituto Nacional Electoral (?).

I instrument for adoption of POS terminals,  $POS_{z(a)st}$ , with the exogenous shock to debit card adoption  $D_{j(a)t} = 1$  if locality  $j$  has received the card shock yet at time  $t$ . As shown in earlier sections, this instrument is plausibly exogenous and has a strong first stage on POS terminal adoption. Identification of  $\theta_k$  then depends on the debit card shock leading to a change in corner stores' POS terminal adoption, which leads some consumers to shift some of their shopping trips from supermarkets to corner stores, as shown in Section IV. The exclusion restriction requires that the debit card shock affects expenditure *shares* only through its effect on POS terminal adoption by retailers (and through any spillover effects directly caused by this increase in POS terminal adoption, such as its effect on other consumers' card adoption and on other consumers' decisions of where to shop).

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

The store types encompass all retail consumption except consumption at restaurants, purchases from ambulatory vendors, international purchases, and online consumption, as these categories do not involve making trips to the store or market. Specifically, the store types  $s$  are defined as (i) corner stores, which include both corner stores and other small shops such as bakeries and butcher shops; (ii) supermarkets, which include supermarkets, department stores, “membership stores” such as Costco, and chain convenience stores; and (iii) the outside option, spending at open-air markets.

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

I use an imperfect mapping, limited by the available data, between these three consumer groups and the three groups I am interested in. While Prospera beneficiaries are easily identified in ENIGH, I use estimates of  $\alpha_k$  and  $\theta_k$  for the group of credit card holders to estimate benefits for “existing card holders” and  $\alpha_k$  and  $\theta_k$  for the group of non-credit card holders to estimate benefits for “new card adopters.” In reality, some existing card holders will be in the non-credit card

holders group (since they could have had debit cards already, which I do not observe) and some of the non-credit card holders group will be made up of households that had no card before or after the shock.

Prices are unit values (i.e., total spent on a good divided by quantity purchased) from ENIGH, where the unit value of good  $g$  in store type  $s$  at time  $t$  is averaged across the unit value reported by each household that consumed that good within each census tract  $a$ . The alternative of using prices from the micro-CPI data adds additional noise since the geographic identifier in those data is the municipality and only 96 urban municipalities are included, so over half of the sample would be lost. Furthermore, unit values have been used in many studies to estimate price elasticities (e.g., Deaton, 1988). The weights  $\phi_{a(i)gst}$  used to construct the price indices are expenditure shares calculated within each census tract by good by store type by survey wave in ENIGH. Each good is one of the 242 food and beverage product categories included in the survey's consumption module; the data are restricted to food and beverages for this estimation because other consumption categories do not include quantities to calculate unit values. Goods that are not available in a particular area or store type are accounted for in the estimation since these will have zero expenditures and thus zero weight in the price index. The impacts of differences in available variety across store types are captured in the locality by consumer group by store type and consumer group by store type by time fixed effects  $\eta_{j(a)ks}$  and  $\delta_{kst}$  (as long as these differences are not time-varying within a locality by store type).

The share of stores of type  $s$  that have adopted POS terminals in postal code  $z(a)$  at time  $t$  is constructed by combining two data sets. The number of stores with POS terminals comes from the data from Mexico's Central Bank described in Section II.B, where store type is identified using merchant category codes. The total number of stores in each postal code is constructed from a data set on the geocoordinates of the universe of firms in Mexico, described in Appendix B.11, where store type is identified using four-digit NAICS codes.

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

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

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

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

Using Shephard's lemma and duality,

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

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

$$(20) \quad \frac{CV_k}{e(P^1, U^0)} \approx - \sum_s \phi_{ks}^1 \left( \frac{P_s^1 - P_s^0}{P_s^1} \right),$$

where  $CV_k$  denotes the compensating variation for consumer group  $k$ ,  $e$  is the expenditure function, and  $\phi_{ks}^1$  is the expenditure share of consumer group  $k$  at store type  $s$  after the change.

To obtain the proportional change in consumer surplus from POS terminal adoption, using (18), replace  $(P_s^1 - P_s^0)/P_s^1 \approx d \log P_s \approx -(\theta_k/\alpha_k) \Delta POS_{ks}$  in (20). This leads to the term in square brackets in (11), from which I subtract the cost of card adoption relative to total expenditures,  $A_k/y_k$ . If consumer group  $k$  already had cards,  $\Delta POS_{ks}$  is the change in the concentration of POS terminals and  $A_k = 0$  since the adoption cost was already paid in a previous period. If consumer group  $k$  previously did not have cards,  $\Delta POS_{ks}$  is the fraction of stores with POS after the shock, given that before the shock these consumers did not have cards and hence experienced  $POS_{ks} = 0$ . For consumers who receive cards from the program I assume  $A_k = 0$ , while I use a revealed preference approach to impose upper and lower bounds on  $A_k$  for consumers who did not receive cards from the program but adopt now that they can use a card at more corner stores. The revealed preference approach simply assumes that since new card adopters did not adopt before the shock and did adopt after the shock, they were somewhere in the range between being just on the margin of adopting prior to the shock and being just on the margin of adopting after the shock.

## F Price Differences Across Store Types (For Online Publication)

Using encrypted store identifiers, I merge the microdata on product-by-store level price quotes used by the Mexican government to construct the consumer price index with firm-level data from



the Economic Census, which allows me to precisely identify store type using six-digit NAICS codes. I use the price data from 2010–2014, as the store identifiers (and thus the fine-grained definitions of store type based on NAICS codes) are only available once the price data began to be collected by INEGI—rather than Mexico’s Central Bank—in 2010.

As described in Section II.D and Appendix B.9, these data are at the barcode-level product (such as “600ml bottle of Coca-Cola”) by store by week level. I average weekly price quotes within a month and restrict to corner stores and supermarkets using four- or six-digit NAICS codes; I also restrict to localities included in the Prospera debit card rollout. After these restrictions, the data have 1,256,221 observations when using six-digit NAICS codes and 1,685,223 observations when using four-digit NAICS codes. I estimate

$$(21) \quad \log Price_{gst} = \lambda_{gj(s)t} + \beta \mathbb{I}(\text{Corner})_s + \varepsilon_{gst}$$

where  $g$  is a barcode-level product,  $s$  is a store,  $t$  is a month, and  $j(s)$  is the locality  $j$  in which store  $s$  is located.  $\mathbb{I}(\text{Corner})_s$  is a dummy equal to one if the store is a corner store. The regression includes product by locality by month fixed effects to compare identical products within the same locality in the same month. Standard errors are clustered at the product by store level.

The results are shown in Table A.9. Using six-digit NAICS codes, the difference in prices is not statistically significant but the point estimate is that corner stores charge 5% less than supermarkets for identical products ( $p = 0.27$ ); using four-digit NAICS codes (which broadens the definition of corner stores to include other small retail shops and broadens the definition of supermarkets to include other chains such as chain convenience stores like Oxxo and 7-Eleven), corner stores charge 9% less for identical products ( $p < 0.01$ ).

There are numerous potential reasons that corner stores have cheaper prices than supermarkets in Mexico. First, 87.6% of corner stores are informal and report neither charging value-added tax (VAT) to their customers (which would be included in the price quotes collected by INEGI), nor paying social security benefits to their workers (which would lower their labor costs relative to supermarkets, most of which are formal). Second, because corner stores offer less of other amenities such as variety, cleanliness, and parking lots, they may need to offer a compensating differential in the form of lower prices. Some of these amenities offered more often by supermarkets, such as keeping the store clean, also increase marginal cost. Third, a larger share of supermarket expenditures is done by richer consumers who are likely more price-inelastic, allowing the supermarkets to increase prices. Fourth, there are many corner stores in close proximity, making competition for corner stores closer to perfect competition, whereas supermarkets may be able to charge higher mark-ups given that—while they compete with corner stores and other supermarkets to some extent—the different product set offered by supermarkets compared to corner stores and

the lower density of supermarkets makes competition across supermarkets more imperfect.

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