Online Appendix

Financial Technology Adoption:
Network Externalities of Cashless Payments in Mexico

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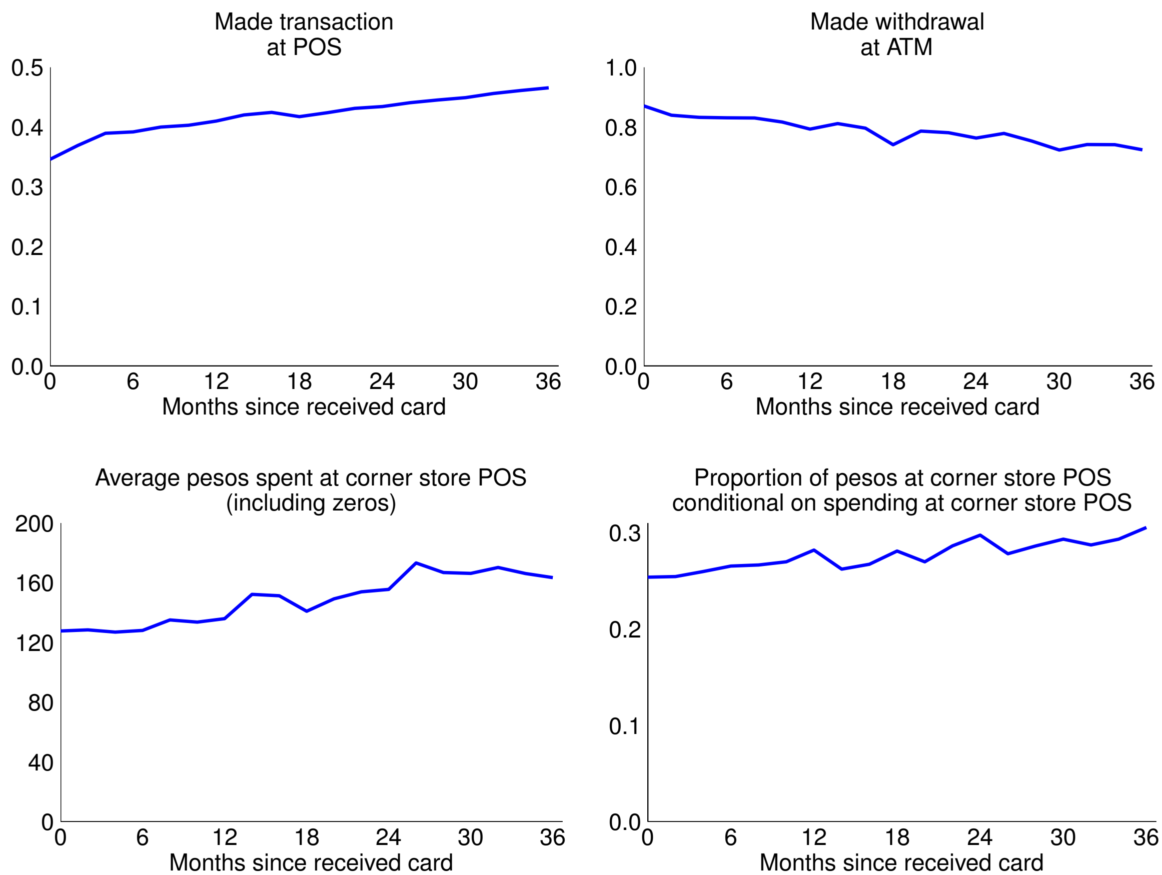
A Figures and Tables (For Online Publication)

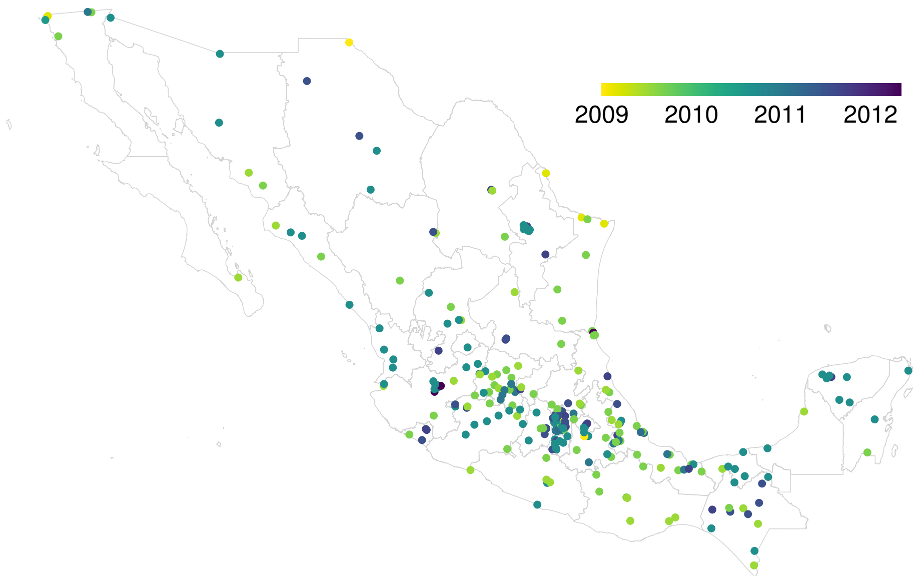
Figure A.1: Use of cards by Prospera beneficiaries



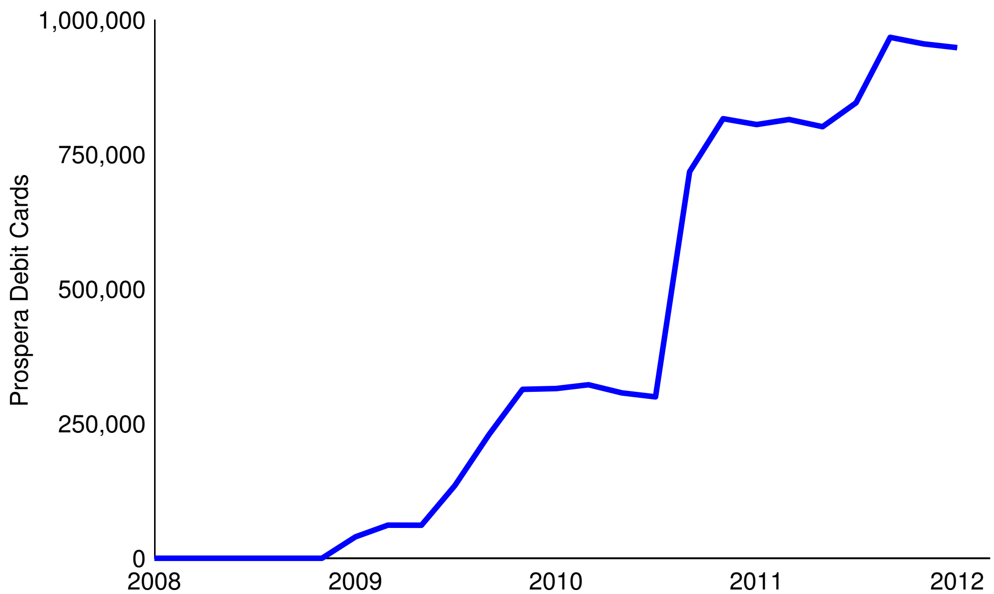
This figure shows the (i) proportion of Prospera card holders who make at least one transaction at a POS terminal using their card during each two-month period; (ii) proportion of Prospera card holders who make at least one ATM withdrawal during each two-month period; (iii) the average pesos transacted 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



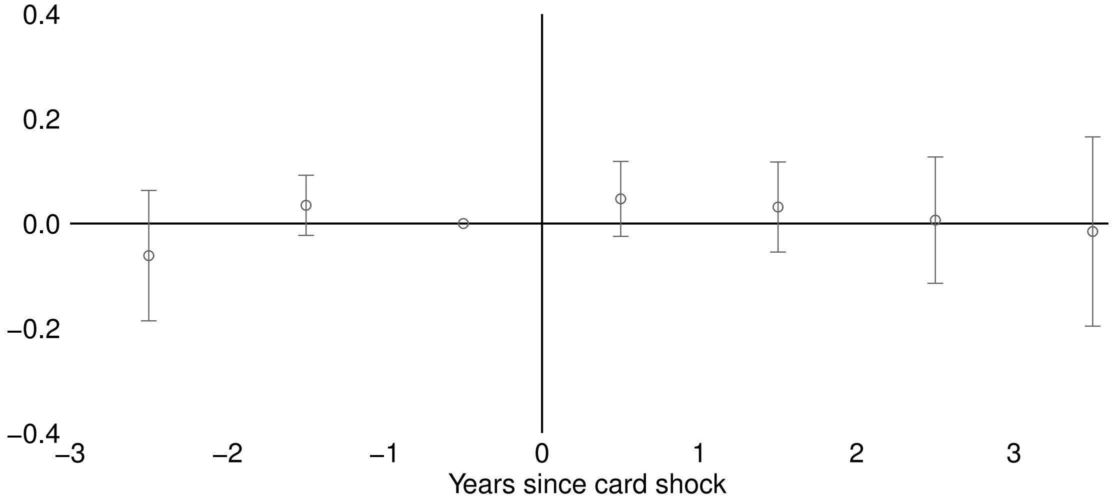
(b) Number of households treated over time



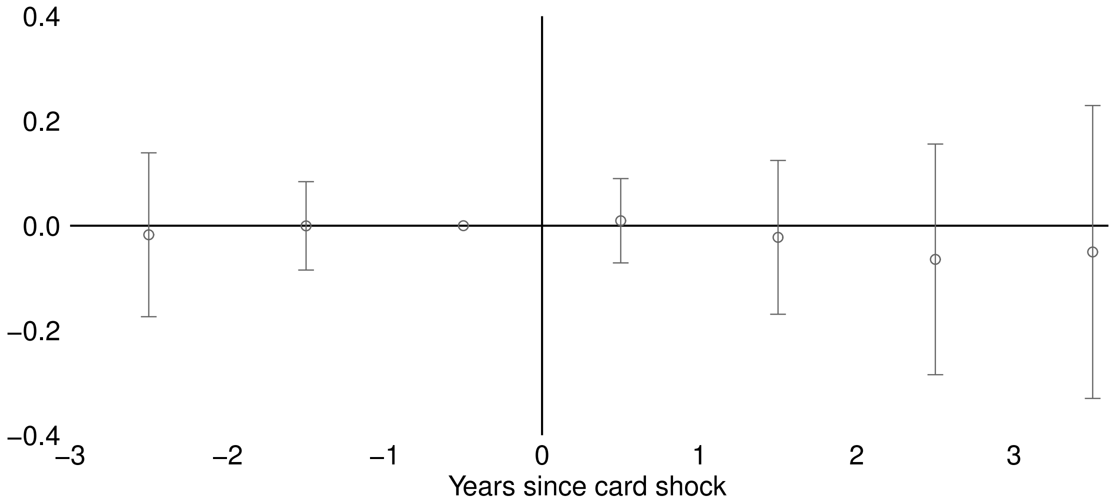
This figure shows when beneficiaries in each urban locality received debit cards from Prospera. It uses administrative data from Prospera on the number of beneficiaries and payment method in each locality during each payment period ($N = 5{,}807{,}552$ locality by two-month period observations), which 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

(a) Log number of Prospera beneficiaries

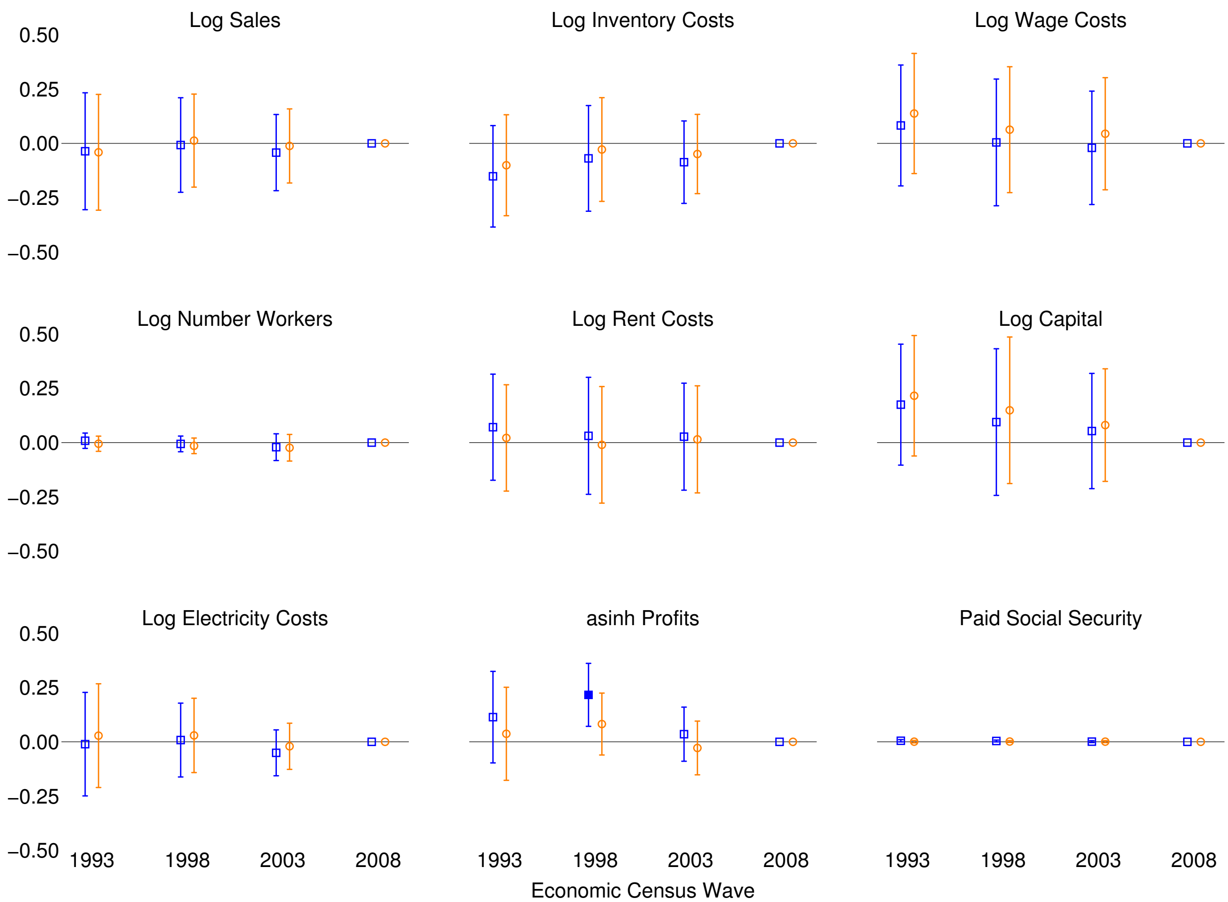


(b) $\mathbb{I}$(Mayor is from PAN party)



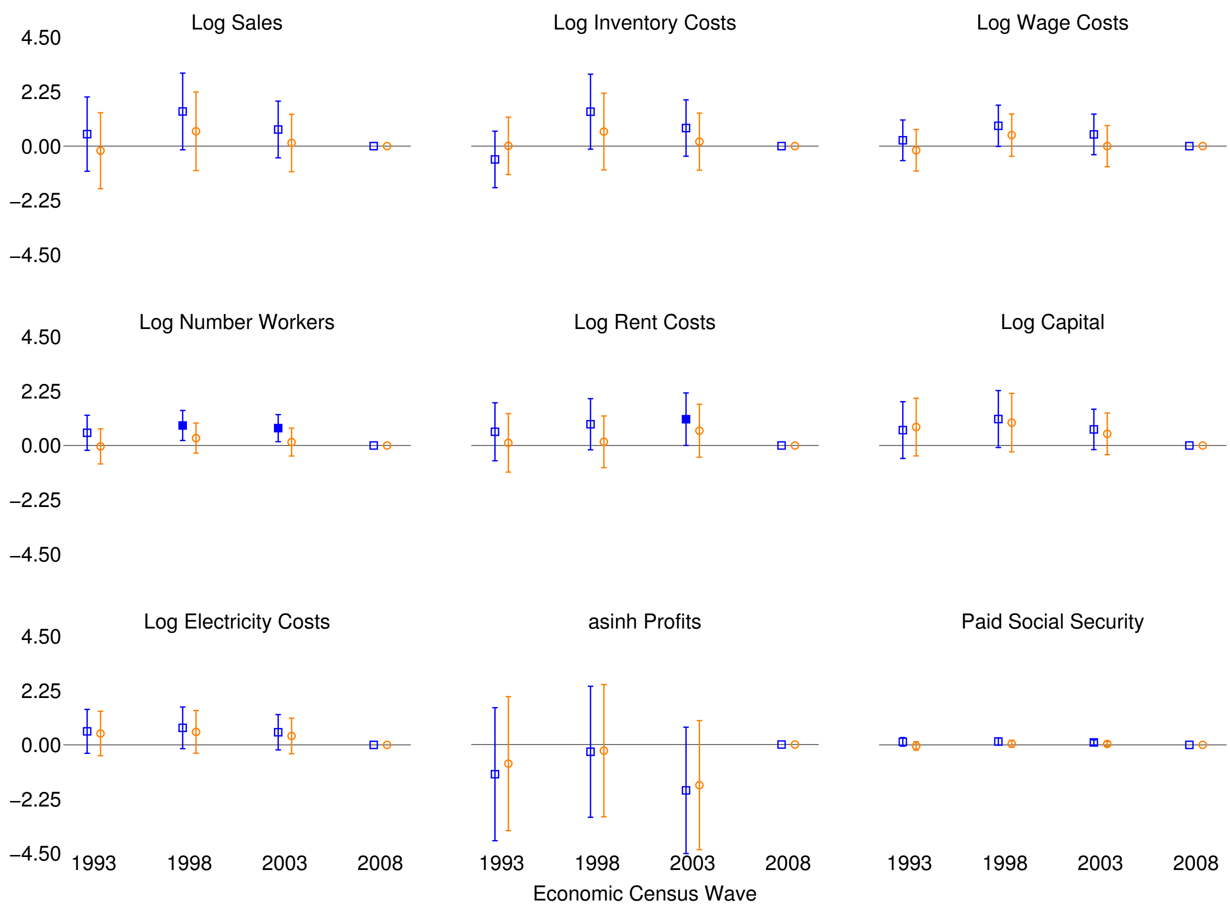
This figure shows that the rollout of debit cards is not correlated with changes in the number of beneficiaries or in the political party in power at the local level. Panel a shows the coefficients from (1), where the outcome is the log number of Prospera beneficiaries in locality j during the last two-month 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



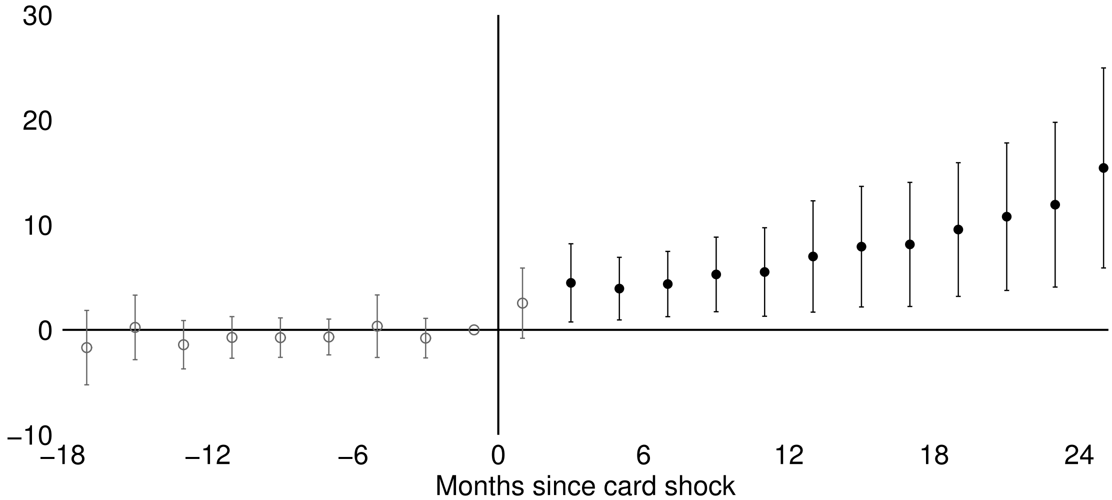
This figure shows parallel pre-trends in variables from the Economic Census, where data are restricted to corner stores then averaged across corner stores within a locality (see Appendix C.4 for details). Point estimates are $\gamma_{k\tau}$ from (9). $N = 1{,}016$ locality by time observations from 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 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.

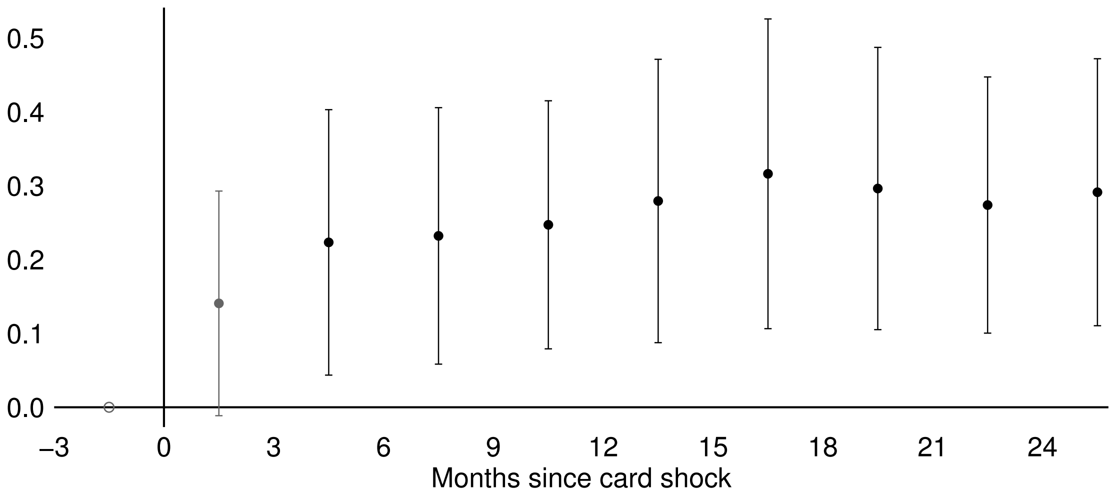
Figure A.6: Effect of card shock on corner store POS adoption **in levels**



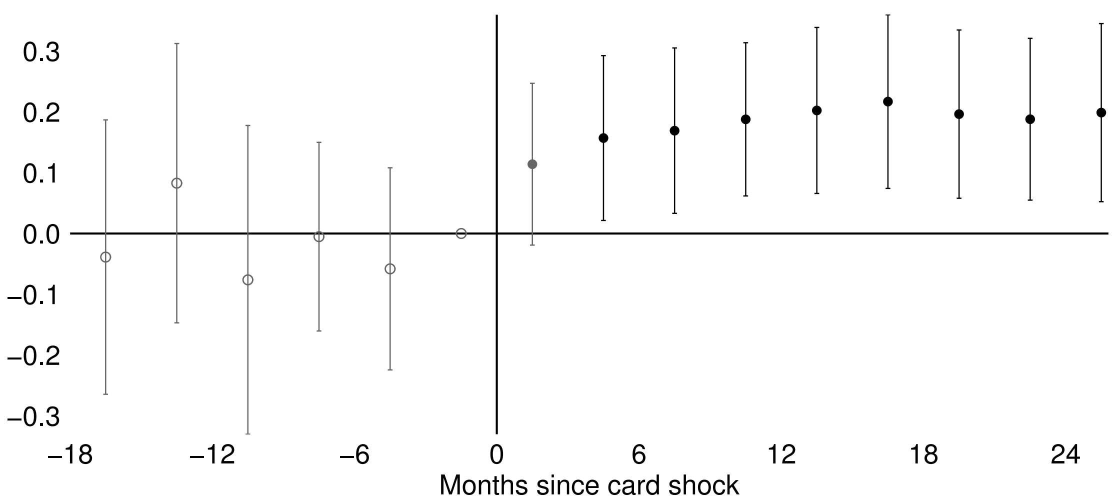
This figure shows the effect of the debit card shock on the stock of point-of-sale (POS) terminals at corner stores, measured in levels. It graphs the coefficients from (1), where the dependent variable is the number of corner stores with POS terminals (measured in levels rather than logs). Observations 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

(a) Balanced panel of municipalities



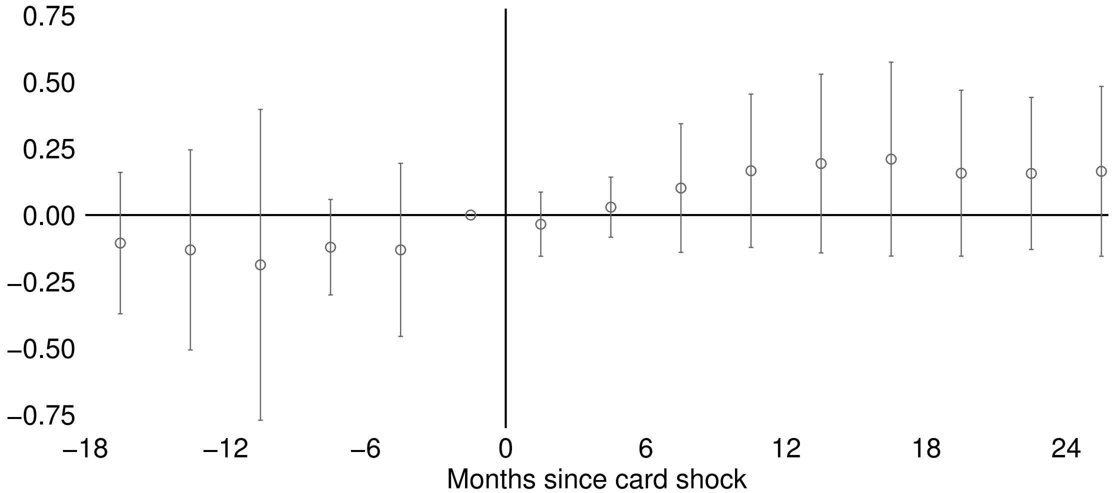
(b) Log stock of credit and debit cards combined



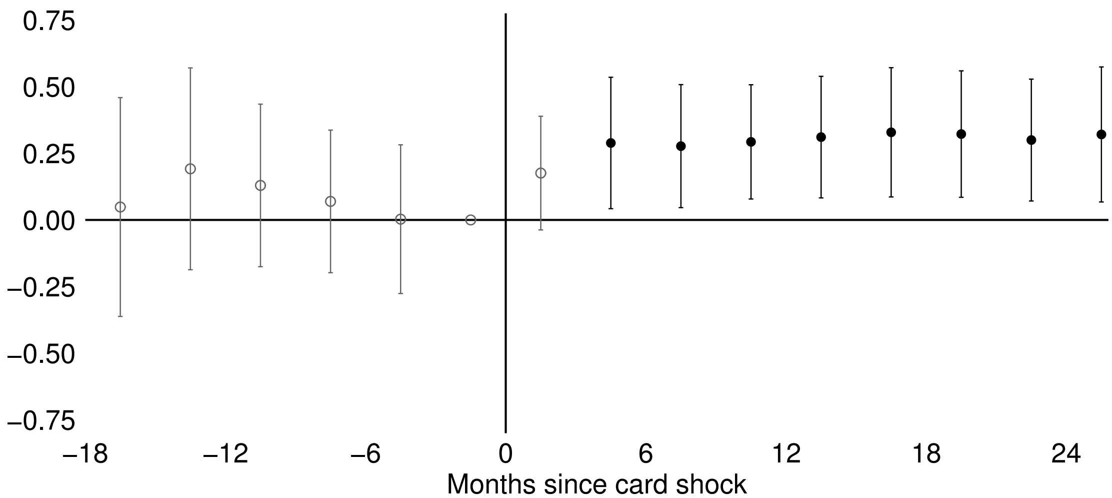
This figure shows robustness of the spillover effect on other consumers' card adoption, showing coefficients from (1) using the CNBV data. Panel a uses the same outcome variable as Figure 5—the log stock of non-Bansefi debit cards in municipality m in quarter t—but in the estimation uses only the relative 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

(a) Municipalities with below-median immediate POS adoption response by corner stores

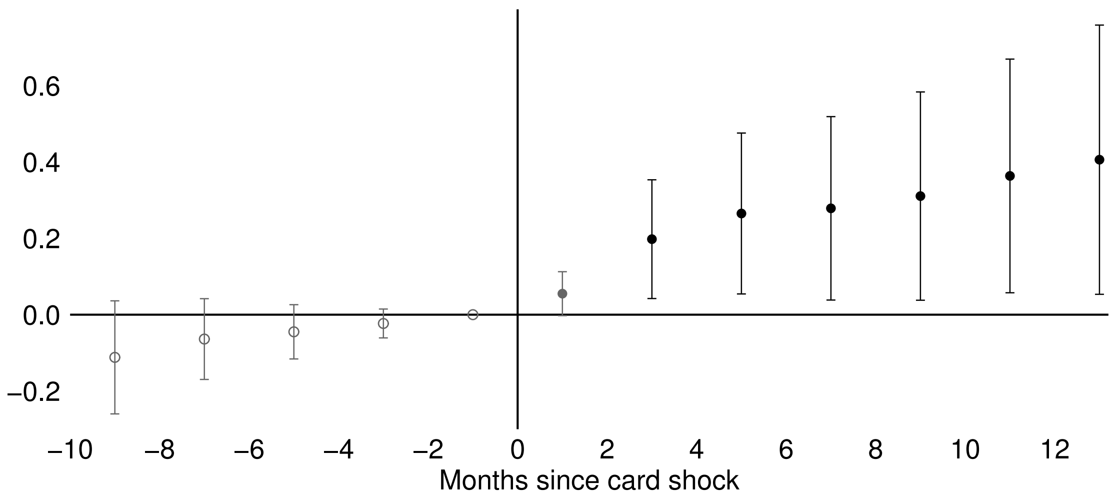


(b) Municipalities with above-median immediate POS adoption response by corner stores



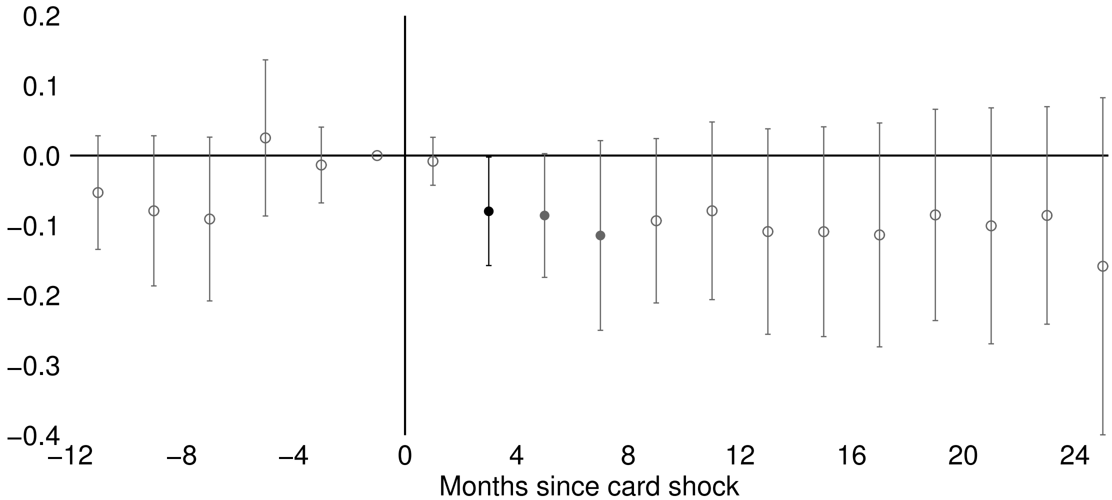
This figure shows that the short-run spillover on other consumers' debit card adoption is higher in municipalities with an immediate corner store POS adoption response. It graphs coefficients from (1), where the outcome variable is the log stock of non-Bansefi debit cards in municipality m in quarter 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



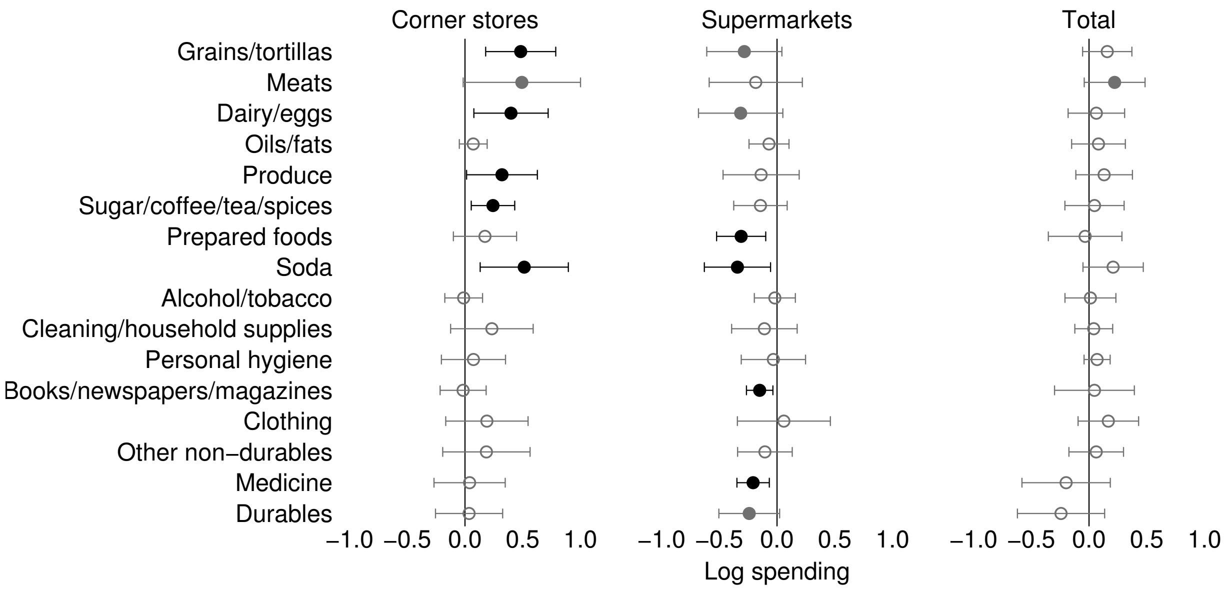
This figure shows that POS transactions excluding transactions made by the Prospera debit cards increase after the debit card shock. It graphs the coefficients from (1), where the outcome variable is the log number of transactions at POS terminals excluding transactions by Prospera beneficiaries in locality 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.

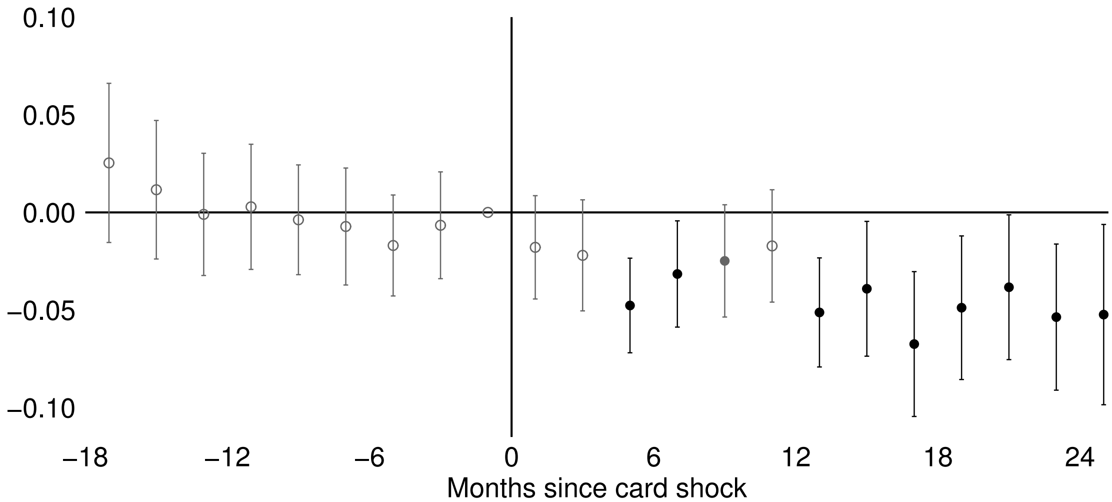
Figure A.11: Effect on fifth quintile's log spending by product category and store type



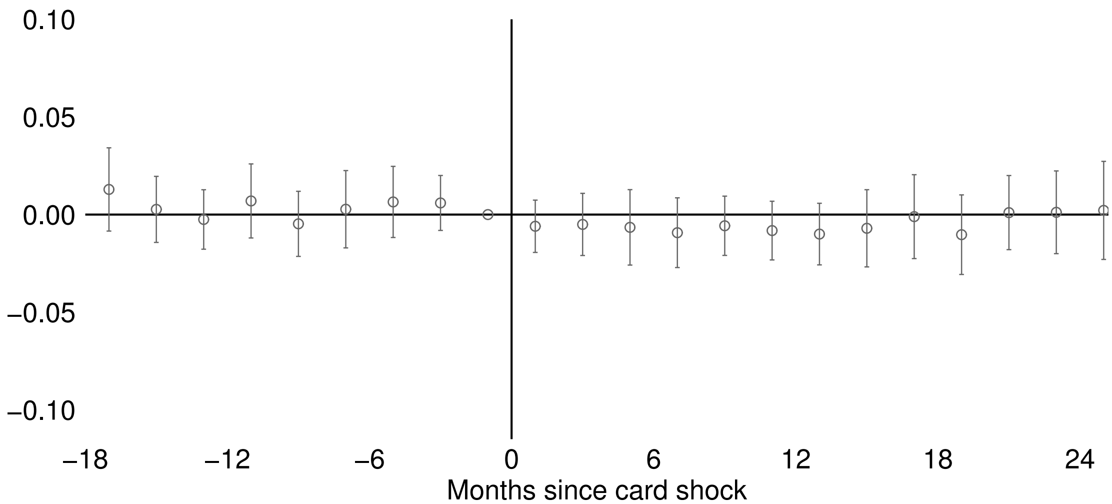
This figure shows a breakdown by product category of the partial shift in the fifth quintile's consumption from supermarkets to corner stores. Each coefficient is $\gamma + \psi_5$ from a separate regression using specification (4), where the outcome is log spending on a particular product category (rows 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

(a) Google searches for three most commonly-searched supermarkets



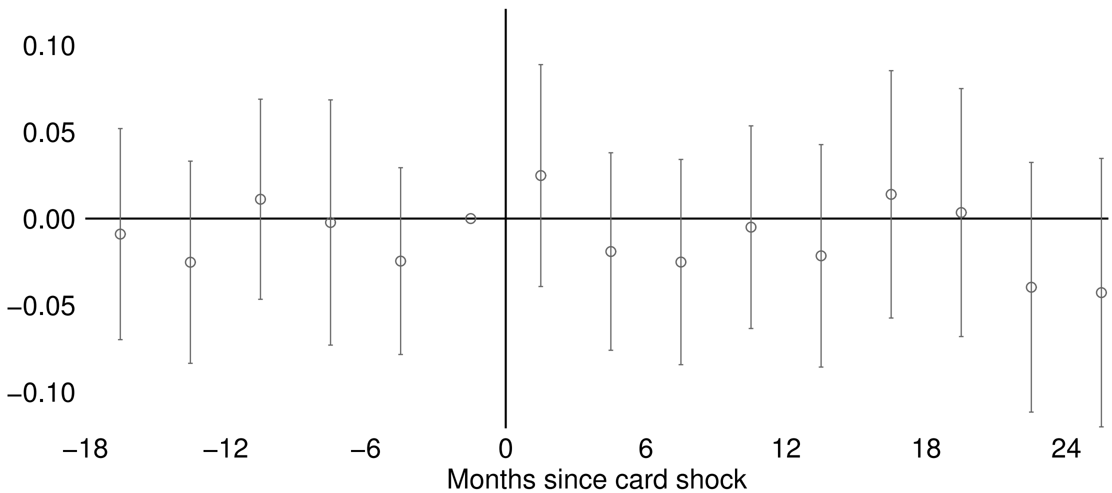
(b) Placebo: Google searches for weather



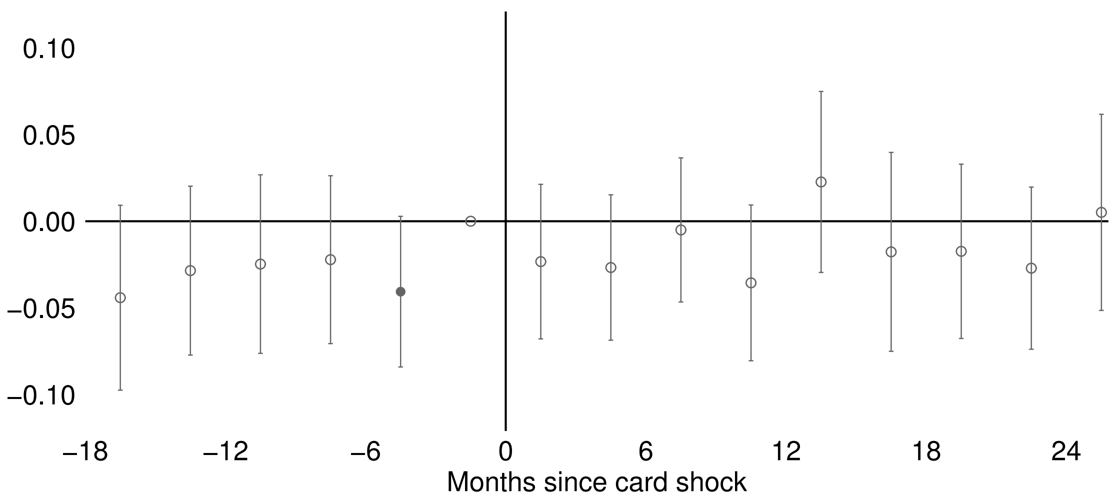
This figure shows that the the number of Google searches for supermarkets fell about 4–6 months after the debit card shock occurred, while placebo searches for weather did not change after the card shock. It shows coefficients from (1). In panel a, the outcome is log Google searches for one of the three 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

(a) Corner stores



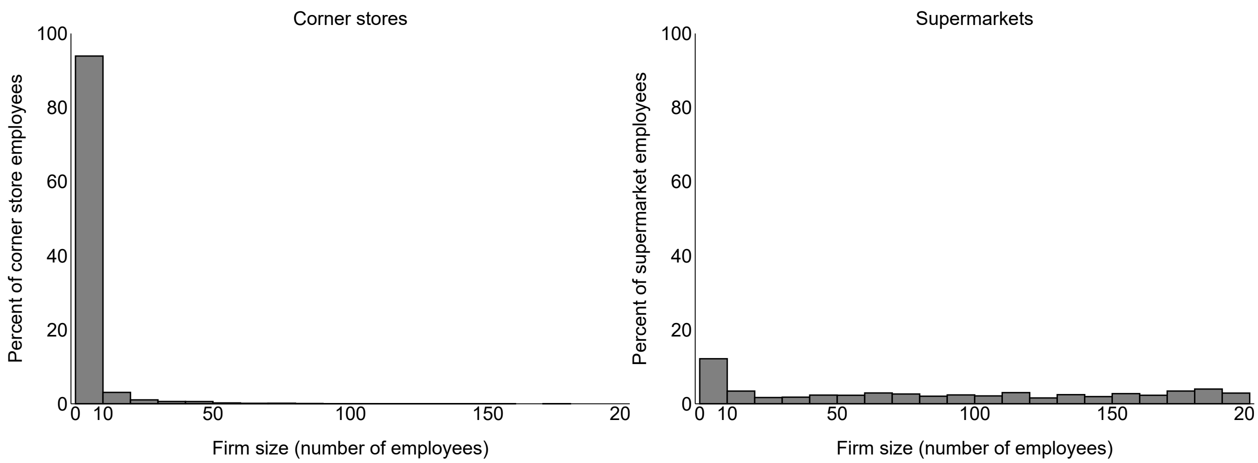
(b) Supermarkets



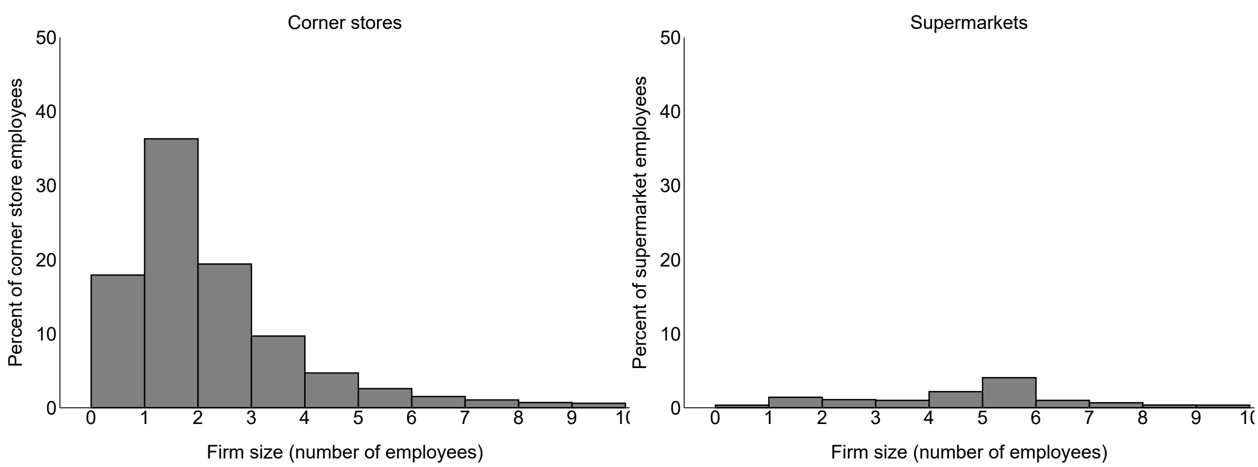
This figure shows that the rollout of debit cards did not have an effect on retail wages. It shows the coefficients from (1), where the outcome is log monthly wages of individual i in municipality m during quarter t and municipality and quarter fixed effects are included, using Mexico's quarterly labor 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

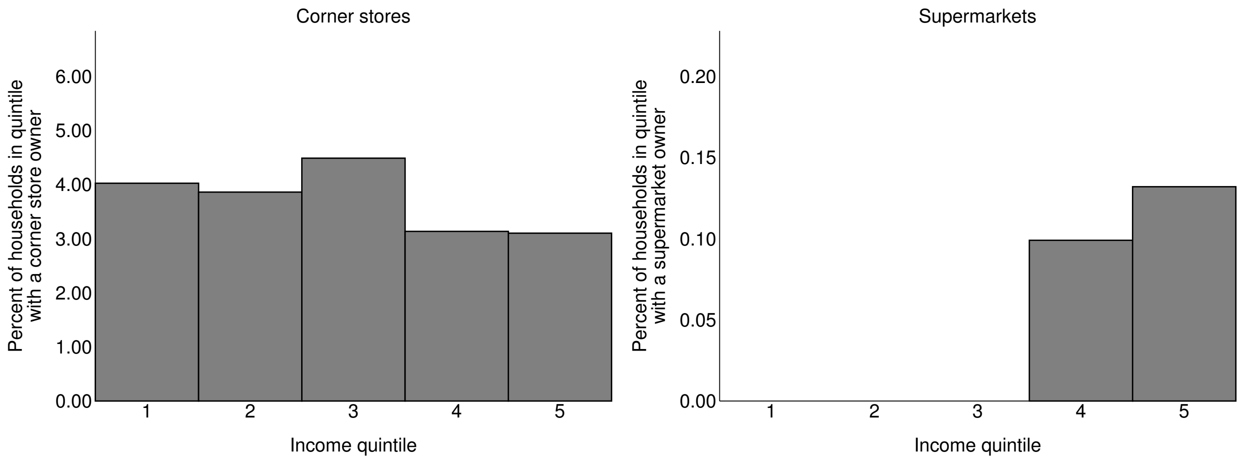


(b) Firms with less than 10 employees



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.

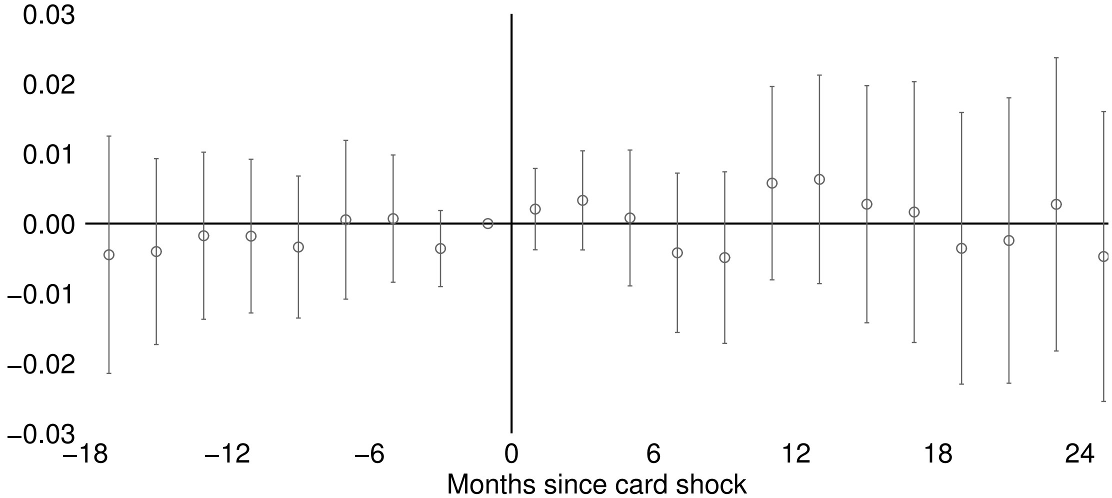
Figure A.15: Percent of households with retail firm owners by income quintile



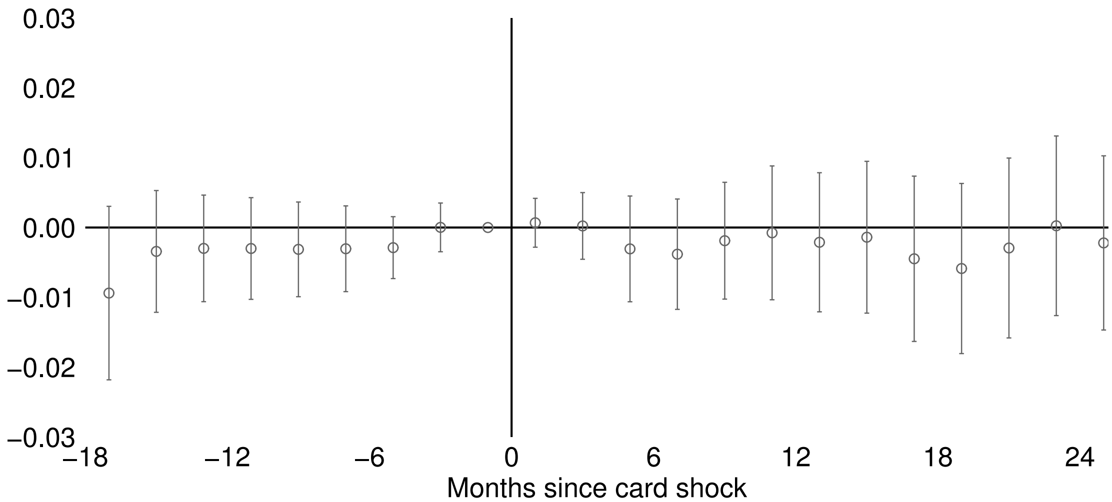
This figure shows the percent of households in each income quintile with a corner store owner or supermarket owner, using data on occupations and household income from the 2008 ENIGH. Corner store and supermarket ownership is identified using the four-digit NAICS code for each individual's occupation, 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

(a) Corner stores

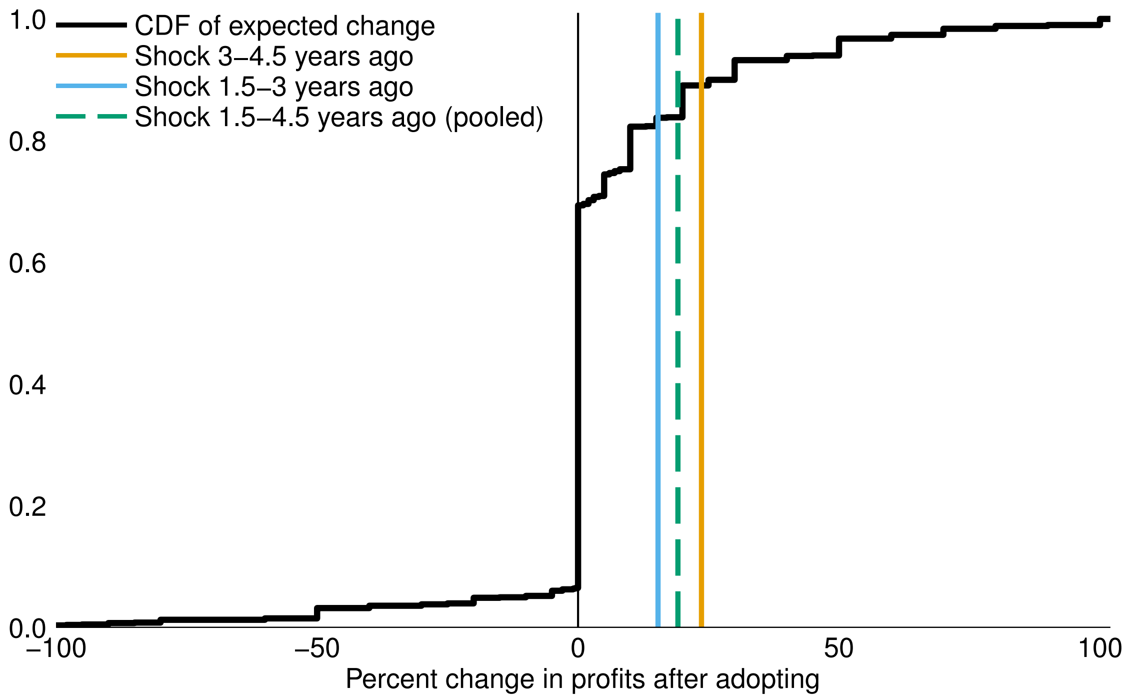


(b) Supermarkets



This figure shows that neither corner stores nor supermarkets change prices in response to the debit card shock. It shows the results from (6), where the outcome variable is the log price of barcode-level product g at store s at time t. 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.

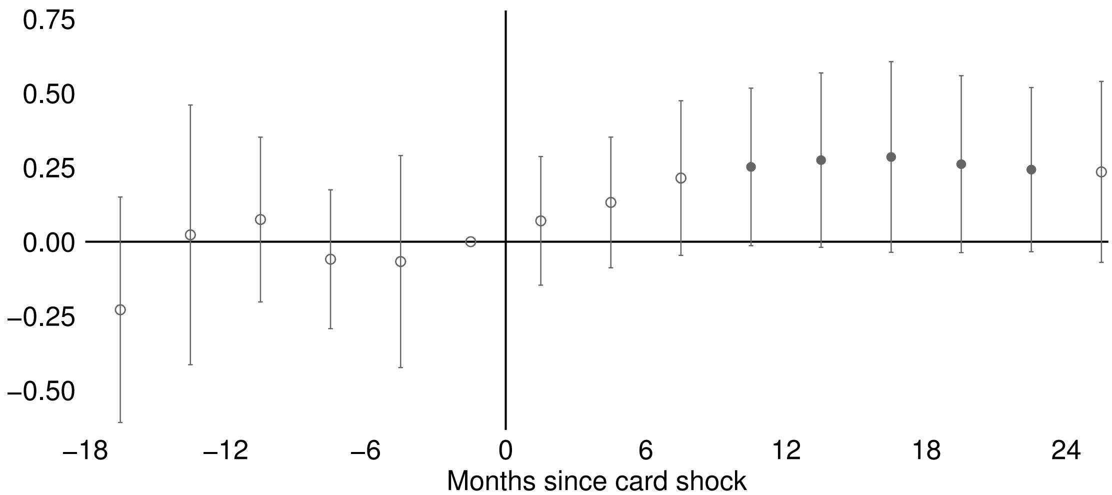
Figure A.17: Comparing expected change in profits in survey to average treatment effects



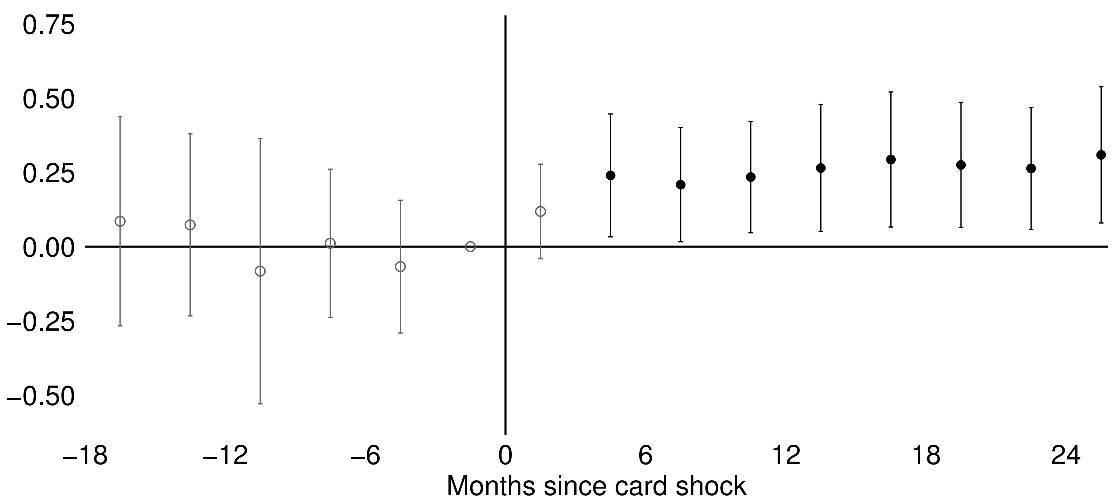
This figure shows the cumulative distribution of corner store owners' expected change in profits after adopting a POS terminal, based on survey responses. It also plots average treatment effects from the Economic Census. The solid gold line shows the coefficient on those treated 3–4.5 years ago, the 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

(a) Municipalities with below-median social connectedness



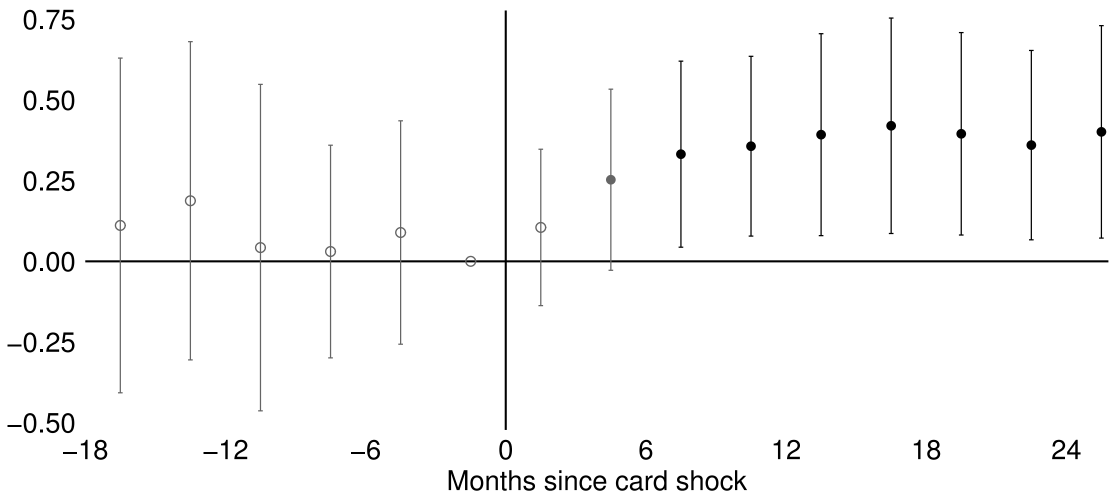
(b) Municipalities with above-median social connectedness



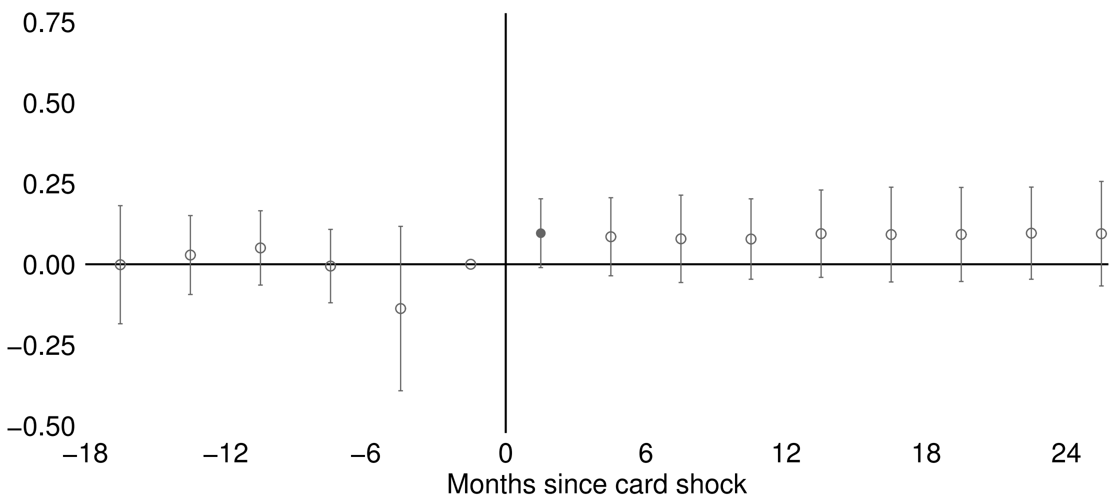
This figure shows that there are no statistically significant differences between the spillover effect on other consumers' card adoption based on a municipality's social connectedness. It graphs coefficients from (1), where the outcome variable is the log stock of non-Bansefi debit cards in municipality 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

(a) Municipalities with below-median baseline ATM density



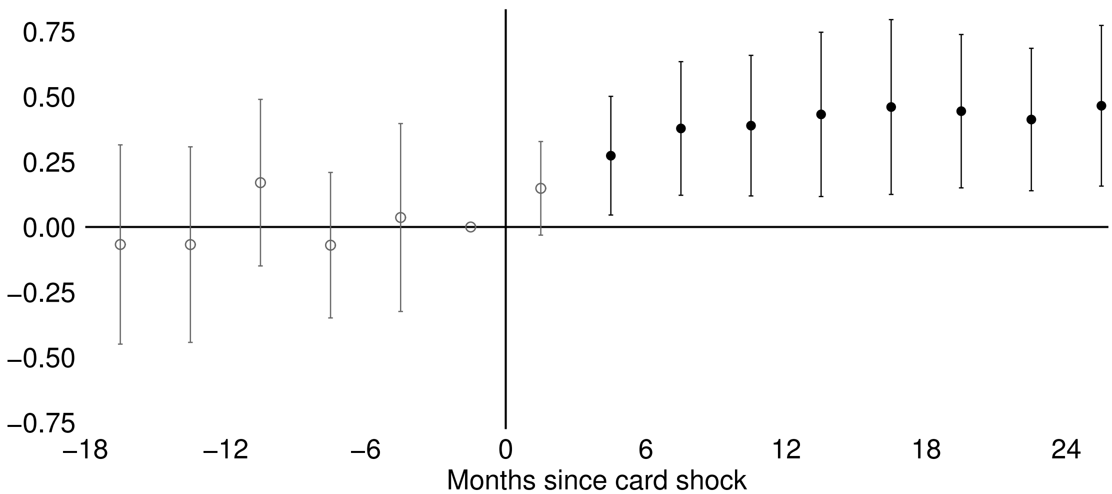
(b) Municipalities with above-median baseline ATM density



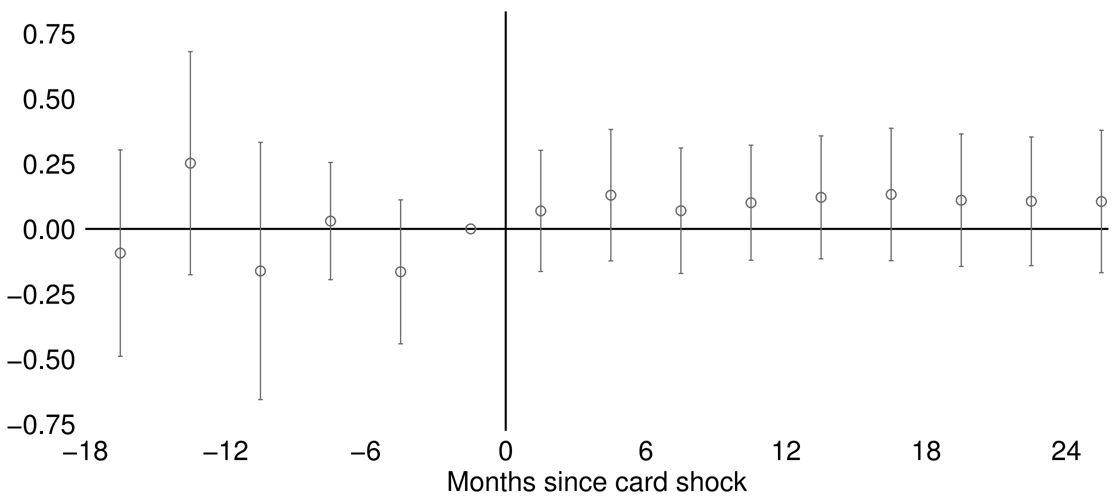
This figure shows that the spillovers on other consumers' card adoption appear to be concentrated in municipalities with low ATM density. It graphs coefficients from (1), where the outcome variable is the log stock of non-Bansefi debit cards in municipality m in quarter t. Panel a restricts to municipalities 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

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



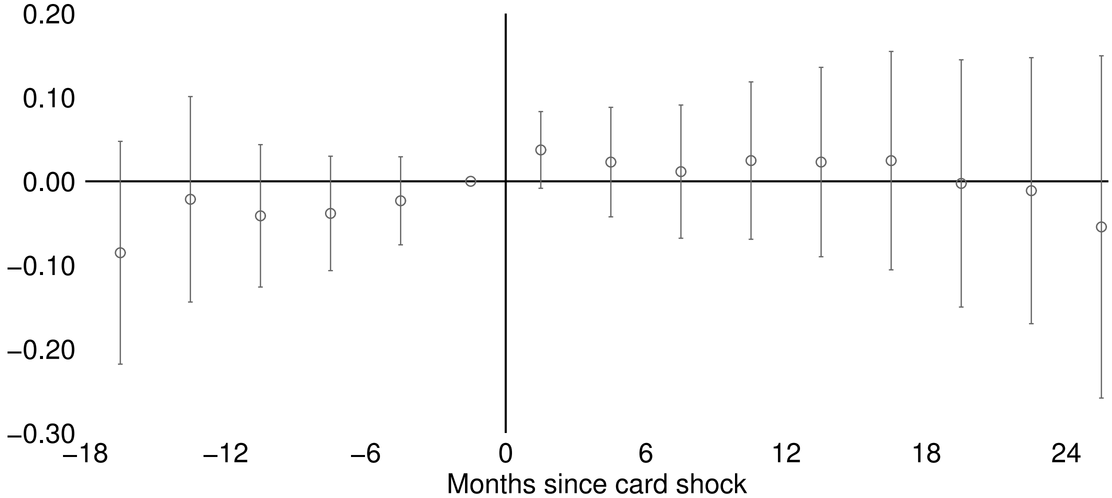
(b) Municipalities with above-median proportion of beneficiary transactions at supermarkets



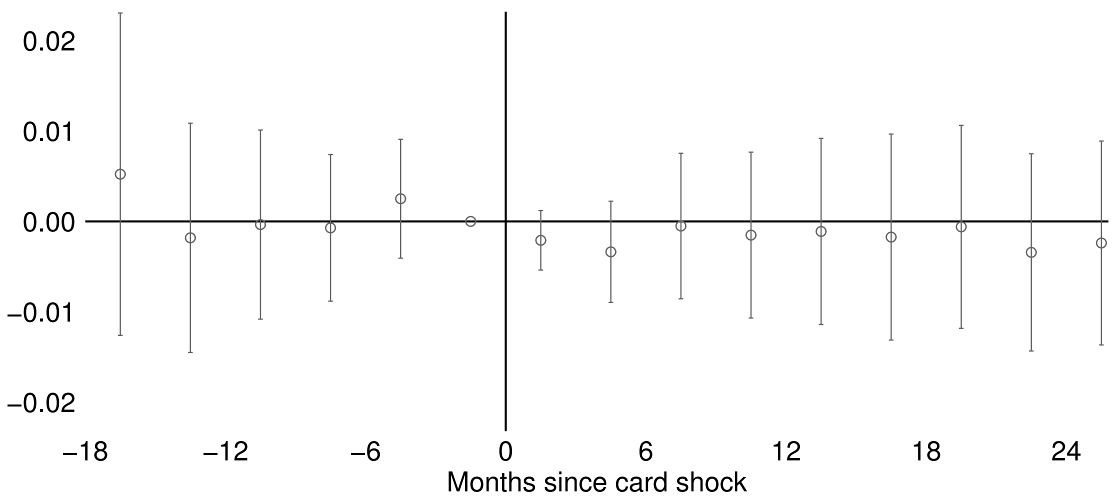
This figure shows that the spillovers on other consumers' card adoption appear to be concentrated in municipalities where beneficiaries use their cards relatively more at corner stores. It graphs coefficients from (1), where the outcome variable is the log stock of non-Bansefi debit cards in municipality 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

(a) Log number of ATMs

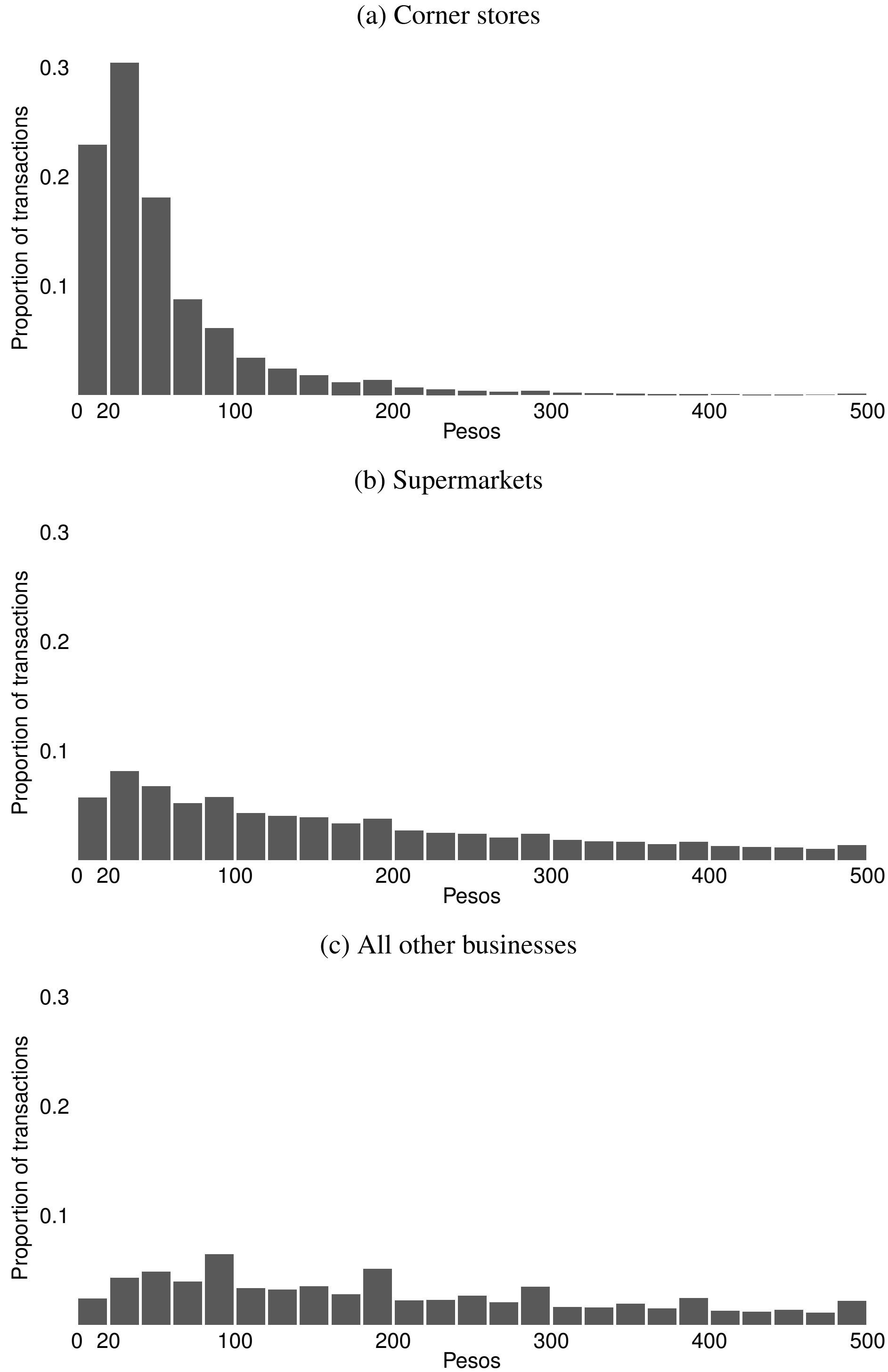


(b) Log transaction fee charged for POS terminal use



This figure tests for a response by commercial banks to the debit card shock. Panel a shows that that the number of commercial bank branches does not change in response to the card shock. It shows coefficients from (1) where the outcome variable is the log number of commercial bank branches in the municipality, using quarterly data from CNBV. $N = 4{,}832$ 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.

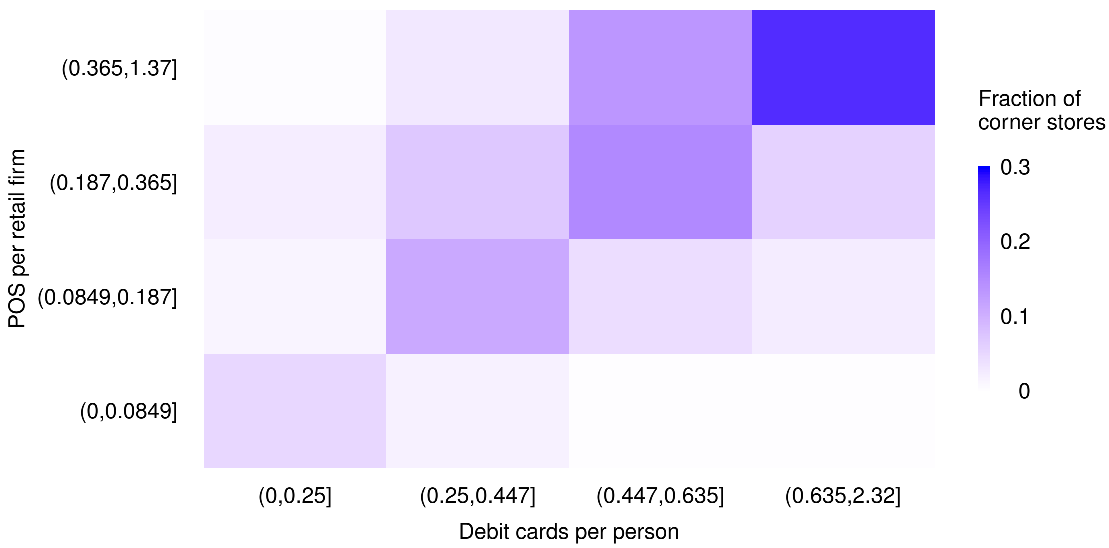
Figure A.22: Histograms of transaction amounts (transactions at POS terminals)



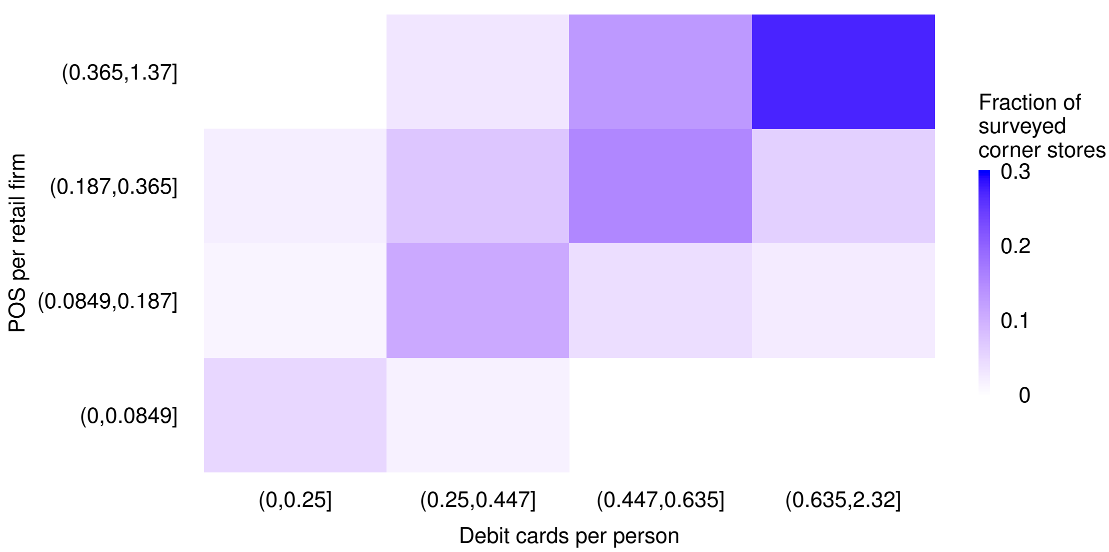
This figure shows that retailers likely do not impose minimum transaction amounts because a substantial proportion of transactions are made for very small amounts, especially at corner stores (20 pesos is less than $2). It graphs the histogram of transaction amount sizes using the universe of card transactions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

(a) Density of corner stores in rollout localities (period before shock)

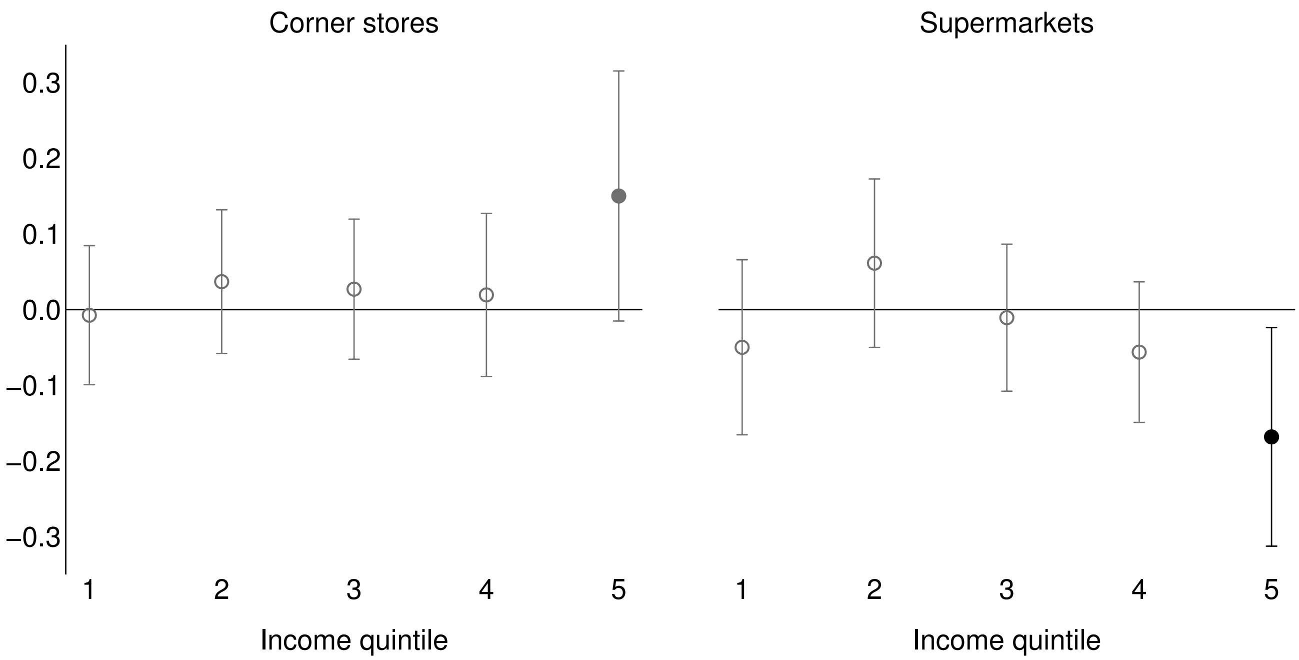


(b) Density of successfully surveyed corner stores (prior to survey)



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

This table shows the point estimates and standard errors from Figure 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	–0.018	0.029
	(0.032)	(0.043)	(0.030)
de Chaisemartin and D'Haultfœuille (2020) estimator	0.082	–0.025	0.012
	(0.048)	(0.049)	(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
Panel A: Corner stores									
Quintile 1	–0.051	0.145	0.292	–0.015	0.307	0.329	–0.031	–0.060	0.089
	(0.096)	(0.175)	(0.154)	(0.101)	(0.137)	(0.145)	(0.149)	(0.141)	(0.057)
Quintile 2	0.033	0.080	0.068	0.159	0.052	0.055	–0.243	0.180	0.070
	(0.099)	(0.167)	(0.130)	(0.078)	(0.120)	(0.115)	(0.145)	(0.136)	(0.071)
Quintile 3	–0.025	0.003	0.140	0.097	0.221	0.135	–0.056	0.191	0.128
	(0.109)	(0.160)	(0.132)	(0.073)	(0.133)	(0.124)	(0.129)	(0.129)	(0.079)
Quintile 4	0.144	0.167	0.162	0.013	0.130	0.019	–0.060	0.234	–0.053
	(0.103)	(0.145)	(0.124)	(0.074)	(0.145)	(0.102)	(0.150)	(0.131)	(0.079)
Quintile 5	0.483	0.493	0.399	0.072	0.321	0.243	0.173	0.514	–0.011
	(0.154)	(0.258)	(0.163)	(0.061)	(0.156)	(0.096)	(0.139)	(0.194)	(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
Panel B: Supermarkets									
Quintile 1	–0.024	–0.013	–0.092	0.018	–0.069	0.011	0.004	–0.096	0.039
	(0.142)	(0.122)	(0.121)	(0.079)	(0.138)	(0.099)	(0.069)	(0.089)	(0.036)
Quintile 2	0.210	0.151	0.161	0.121	0.086	0.250	–0.024	0.009	–0.007
	(0.151)	(0.128)	(0.132)	(0.068)	(0.143)	(0.120)	(0.073)	(0.093)	(0.038)
Quintile 3	–0.034	0.121	–0.070	0.076	–0.004	0.207	–0.022	–0.004	0.095
	(0.125)	(0.123)	(0.121)	(0.073)	(0.111)	(0.113)	(0.071)	(0.087)	(0.054)
Quintile 4	–0.030	0.057	–0.167	–0.087	–0.049	–0.048	0.013	–0.125	–0.092
	(0.113)	(0.141)	(0.108)	(0.071)	(0.124)	(0.092)	(0.088)	(0.095)	(0.054)
Quintile 5	–0.283	–0.184	–0.315	–0.069	–0.138	–0.144	–0.311	–0.343	–0.019
	(0.165)	(0.205)	(0.185)	(0.088)	(0.167)	(0.117)	(0.108)	(0.145)	(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
Panel C: Total									
Quintile 1	0.080	0.066	0.181	–0.018	0.106	0.296	–0.149	–0.083	0.122
	(0.054)	(0.115)	(0.102)	(0.108)	(0.091)	(0.131)	(0.170)	(0.135)	(0.071)
Quintile 2	0.054	0.200	0.108	0.341	0.122	0.263	–0.388	0.195	0.067
	(0.043)	(0.126)	(0.080)	(0.096)	(0.086)	(0.127)	(0.169)	(0.104)	(0.084)
Quintile 3	0.070	0.084	0.085	0.155	0.128	0.364	–0.105	0.235	0.194
	(0.071)	(0.125)	(0.098)	(0.105)	(0.118)	(0.158)	(0.145)	(0.109)	(0.097)
Quintile 4	0.114	0.270	0.098	–0.072	0.235	0.003	–0.173	0.144	–0.094
	(0.064)	(0.126)	(0.102)	(0.099)	(0.105)	(0.128)	(0.154)	(0.111)	(0.090)
Quintile 5	0.158	0.222	0.064	0.082	0.131	0.047	–0.034	0.209	0.012
	(0.108)	(0.134)	(0.124)	(0.118)	(0.124)	(0.130)	(0.162)	(0.132)	(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
Panel A: Corner stores								
Quintile 1	–0.093	–0.167	0.062	–0.148	0.368	–0.076	–0.090	0.033
	(0.141)	(0.164)	(0.054)	(0.268)	(0.190)	(0.273)	(0.138)	(0.111)
Quintile 2	–0.096	–0.162	0.055	0.073	0.157	0.110	–0.024	–0.023
	(0.151)	(0.168)	(0.051)	(0.225)	(0.166)	(0.227)	(0.151)	(0.108)
Quintile 3	–0.153	0.060	0.054	0.146	0.083	0.280	0.195	0.085
	(0.152)	(0.154)	(0.062)	(0.241)	(0.175)	(0.232)	(0.149)	(0.147)
Quintile 4	–0.114	–0.139	0.044	0.262	0.184	0.298	–0.036	–0.003
	(0.134)	(0.156)	(0.063)	(0.257)	(0.149)	(0.228)	(0.169)	(0.130)
Quintile 5	0.233	0.074	–0.016	0.178	0.190	0.186	0.040	0.036
	(0.181)	(0.141)	(0.101)	(0.225)	(0.181)	(0.192)	(0.157)	(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
Panel B: Supermarkets								
Quintile 1	0.128	0.240	0.012	–0.002	0.089	–0.023	0.038	–0.066
	(0.168)	(0.150)	(0.021)	(0.021)	(0.106)	(0.059)	(0.023)	(0.083)
Quintile 2	0.214	0.139	–0.016	0.028	0.078	0.056	0.018	–0.084
	(0.130)	(0.146)	(0.027)	(0.025)	(0.131)	(0.078)	(0.034)	(0.088)
Quintile 3	0.309	0.293	–0.012	0.040	0.031	0.119	–0.013	0.060
	(0.160)	(0.151)	(0.030)	(0.031)	(0.112)	(0.073)	(0.032)	(0.106)
Quintile 4	0.217	0.130	–0.047	–0.040	0.390	0.069	–0.033	–0.148
	(0.109)	(0.108)	(0.040)	(0.035)	(0.167)	(0.083)	(0.033)	(0.114)
Quintile 5	–0.109	–0.031	–0.150	–0.017	0.059	–0.105	–0.207	–0.241
	(0.144)	(0.141)	(0.058)	(0.042)	(0.204)	(0.120)	(0.071)	(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
Panel C: Total								
Quintile 1	0.047	0.033	–0.267	–0.281	0.096	–0.145	–0.109	–0.169
	(0.054)	(0.057)	(0.203)	(0.287)	(0.172)	(0.209)	(0.179)	(0.143)
Quintile 2	0.014	–0.029	–0.298	–0.128	0.036	–0.057	–0.006	–0.182
	(0.051)	(0.050)	(0.207)	(0.218)	(0.129)	(0.123)	(0.162)	(0.150)
Quintile 3	0.070	0.028	0.002	–0.080	–0.078	–0.014	0.227	–0.016
	(0.052)	(0.059)	(0.194)	(0.226)	(0.154)	(0.136)	(0.174)	(0.172)
Quintile 4	0.074	0.035	–0.012	–0.123	0.215	0.020	–0.040	–0.255
	(0.053)	(0.055)	(0.182)	(0.251)	(0.134)	(0.121)	(0.192)	(0.190)
Quintile 5	0.041	0.070	0.047	–0.338	0.167	0.062	–0.198	–0.242
	(0.083)	(0.057)	(0.175)	(0.285)	(0.133)	(0.120)	(0.194)	(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	0.002	0.002	–0.000
	(0.018)	(0.016)	(0.004)	(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
Panel A: Corner stores									
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
Pooled coefficient									
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
Panel B: Supermarkets									
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
Pooled coefficient									
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	–0.098
	(0.049)	(0.014)
N (barcode-level product $\times$ store $\times$ 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 (θ_k)	0.24
	(0.31)
New card adopters	
Log price difference ($-\alpha_k$)	–2.92
	(1.26)
Share of stores with POS (θ_k)	0.55
	(0.21)
Existing card holders	
Log price difference ($-\alpha_k$)	–2.02
	(1.29)
Share of stores with POS (θ_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.[48] 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-

[48] 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.[49] 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),

[49] 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 θ_{jt}, but it instead returns a relative search intensity given by

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

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`, 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 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 (Banco de México and INEGI, 2002–2014); 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.[50] 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

[50]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.

deciding between purchasing at the supermarket and corner store.[51] Finally, for the event study regressions 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),

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

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 type identifier using the North American Industry Classification System (NAICS). The DENUE 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 DENUE 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 and in Spanish at 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 DENUE 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:

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

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.[52] 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

[52] 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.

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 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.[53] 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

[53] 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.

this variable given the infrequency of elections).

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 Schargrodsky (2005), I include a fifth-order polynomial in time. Changes are measured from 2006–2008 whenever possible, and from 2000–2005 (the two most recent pre-rollout population census waves) for the INEGI variables. I use number of bank accounts rather than number of debit cards because debit cards were only included in the CNBV data beginning in the last quarter of 2008 so their pre-trend cannot be measured for early-treated localities. Nevertheless, exploiting the staggered rollout timing (i.e., that for later-treated localities pre-trends can be measured), Figure 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

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

where y_{jt} is the log (or inverse hyperbolic sine) average outcome across corner stores or supermarkets in locality j in survey wave t, λ_j are locality fixed effects, and δ_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 τ 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.[54] 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

[54] 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.[55] Specifically, consumer i's utility from trip t to store s is

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

where $a(i)$ denotes the census tract in which household i lives, $k(i)$ denotes consumer groups over which the parameters α and θ 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 ε_{ist} are unobserved individual by store by time shocks.

The key parameters of interest from (10) are α_k, which measures consumer group k's price elasticity, and θ_k, which measures the value consumer group k attaches to a store having a POS terminal.[56] 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 disbursal 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 α_k and θ_k for the three consumer groups described above; the results are shown in Table A.10. First, I find α_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 α_k is at the

[55] 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).

[56] 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 θ_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 α_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 θ_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

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

for each consumer group k, where ϕ_{ks}^1 is the share of k's expenditure spent at store type s after the shock, ΔPOS_{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 ΔPOS_{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.[57]

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.[58]

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:

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

[57] 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.

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

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 $v_{ist} \equiv \alpha_{k(i)} \log y_{it} - \alpha_{k(i)} \sum_g \phi_{a(i)gst} \log p_{a(i)gst} + \theta_{k(i)} POS_{ist} + \tilde{\tilde{\xi}}_{a(i)k(i)st}$, then the probability of choosing store s over all other stores $r \neq s$ is $\pi_{ist} = Prob(u_{ist} > u_{irt} \forall r \neq s) = Prob(\varepsilon_{irt} < \varepsilon_{ist} + v_{irt} - v_{ist} \forall r \neq s)$. Integrating over the probability distribution that the store i chooses has adopted POS and integrating over the stochastic error term,

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

where $\gamma_{a(i)k(i)st} \equiv -\alpha_{k(i)} \log P_{a(i)st} + \theta_{k(i)} POS_{z(a(i))k(i)st} + \tilde{\tilde{\xi}}_{a(i)k(i)st}$ 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.[59]

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

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

(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 ϕ_{akst}, are

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

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

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

Substituting ϕ_{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 ϕ_{akst}:

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

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 ϕ_{ak0t}, which I assume do not accept card payments (i.e., $POS_{z(a)0t} = 0\ \forall z(a), t$).[60]

This leads to the following estimating equation:

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

In this estimating equation I have rewritten $\tilde{\xi}_{akst} - \tilde{\xi}_{ak0t} = \eta_{j(a)ks} + \delta_{kst} + \nu_{akst}$ 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.[61]

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 ϕ_{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 δ_{kst}.

Interpretation of α_k. First, α_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 $U(\boldsymbol{x}) = \left(\sum_s x_s^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}}$, where σ is the elasticity of substitution. The first order condi-

[60] 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.

[61] 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,

$$\log\phi_s - \log\phi_0 = (1-\sigma)(\log p_s - \log p_0), \tag{17}$$

where ϕ_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),

$$-\frac{\theta_k}{\alpha_k} = \frac{d\log\phi_{akst}/dPOS_{ast}}{d\log\phi_{akst}/d\log P_{ast}} = \frac{d\log P_{ast}}{dPOS_{ast}}. \tag{18}$$

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 ΔPOS_{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 ν_{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.[62] 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.

[62] There are five official regions in Mexico, defined by the Instituto Nacional Electoral (INE, 2017).

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 θ_k then depends on the debit card shock leading to a change in corner stores' POS terminal adoption, which leads some consumers to shift some of their shopping trips from supermarkets to corner stores, 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 α_k and θ_k for the group of credit card holders to estimate benefits for "existing card holders" and α_k and θ_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 δ_{kst} (as long as these differences are not time-varying within a locality by store type).

The share of stores of type s that have adopted POS terminals in postal code $z(a)$ at time t is constructed by combining two data sets. The number of stores with POS terminals comes from the data from Mexico's Central Bank described in Section 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,

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

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

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

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

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

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

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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