Appendix. Online Supplement

Value Realization Intervention Details

To inform intervention design, we first identified costs and benefits of adopting digital payment instruments for consumers. We then designed the intervention to highlight benefits and underplay costs to consumers.

Table 1 Digital Payment Instruments: Adoption Benefits vs. Costs for a Consumer

Benefits	Costs
Safety:	Upfront costs:
Avoid theft or misplacement	Hassle cost of opening bank account
Obtain purchase protection	Minimum deposit: MXN $0 to $2000
Convenience:	Ongoing costs: habit changes
Avoid not having cash-on-hand	Regularly deposit cash earnings in bank account
Avoid ATM visits	Remember to carry card
Easy expense tracking	Remember PIN number
Expanded purchasing power	Emotional costs: common fears
Payment card rewards	Fear that card will be “cloned"
	Fear that merchants will overcharge card
	Fear that banks have hidden fees

Value Realization Kit: Advertising to Consumers. The purpose of the advertising to consumers is to inform them about the merchant’s acceptance of digital payment instruments.

(a) In Training Material. (b) In Practice.

Figure 1 Installation of Value Realization Kit

Customer Pitch. First, CSMs train all merchants and customer-facing staff to provide a quick pitch to every customer at checkout. We reproduce the English translation of the pitch below:

Would you like to pay card? It is a more convenient and safe way to pay, with the opportunity to earn rewards from your bank.

Next, CSMs train merchants and customer-facing staff to provide the following detailed pitches for each benefit, in case the customer is interested in learning more. These reflect the benefits in Table 1, above. The detailed pitches are reproduced below, with English translation:

1. Convenience:

• Avoid not having enough cash-on-hand and ATM visits. *"Card payments give you purchasing power without the hassle of cash availability, like having to go to the ATM several times a week to withdraw cash and not having enough cash on hand to buy whatever you want at the time. With cash, your spending will be limited to what you have in your wallet. On the other hand, paying with a card gives you additional purchasing power. You can access your balance in your bank account, and possibly more, as is the case with credit cards."*

• Easy Expense Tracking. *"With cash, it would take a lot of time and effort to keep track of expenses – and you run the risk of forgetting exactly how much you spent. But if you pay with a card, your spending is recorded automatically, all you would have to do is check your bank statement, plus you receive immediate messages directly from your bank about the amount spent. Which takes one or two minutes, compared to all the time you would have to spend doing the math."*

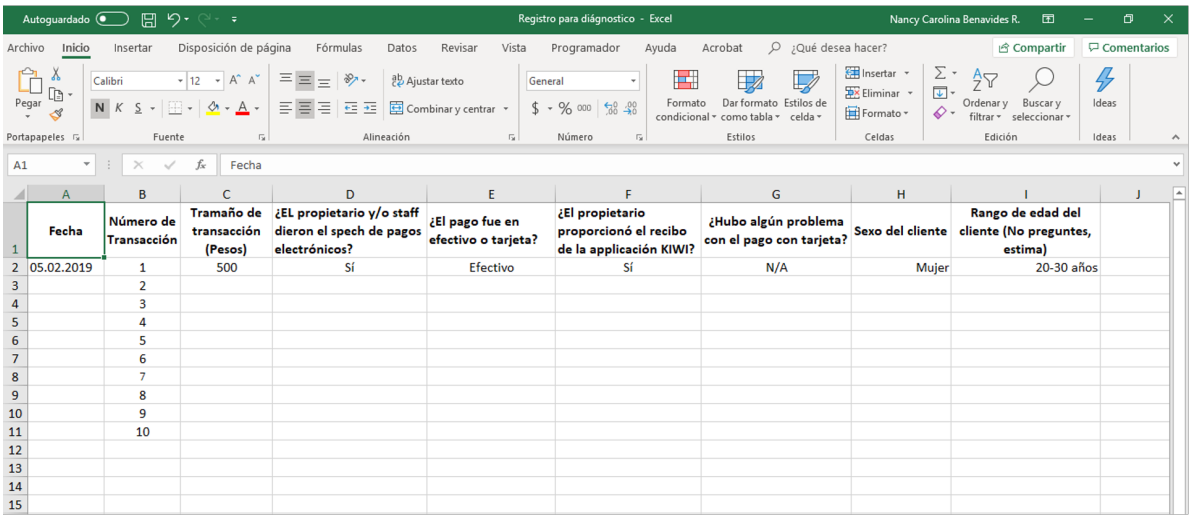
2. Safety. *"As a consumer, security is the most important thing. If your cash is stolen there is no way to get it back, whereas with cards you can report it to the bank and have a low or no loss. Banks make sure to prevent fraudulent charges, and your banking institution will issue another card usually at no additional cost. Not only do you get fraud protection when you make card payments, but you also get protection from items you purchased but never received or were damaged, or were not delivered as promised. With cash, you only have one option to resolve these issues directly with the merchant. With card payments, your provider can help you resolve these situations or do it for you, getting you a refund with minimal effort on your part."*

3. Rewards. *"Many banks offer rewards for using cards for purchases. You can earn points for purchases and those points can be redeemed as cashback, travel rewards, shopping vouchers, and more."*

Market Research. CSMs train all merchants and customer-facing staff to fill out the following spreadsheet (printed out) shown in Figure 2 for every single transaction over the course of the intervention. This spreadsheet tracks which customers pay using digital payments along with transaction size, gender, age, and any other notes. This data forms the basis of understanding the preferences of their customer base towards cash versus digital payment usage. The CSM then helps them analyze the data to identify patterns (e.g., young professionals like to use cards the most) and target their pitch (above) based on the customer type.

Value Realization Strategies. CSMs suggest the following examples of strategies to nudge customers to use digital payments.

1. A small gift (e.g., candy) for customers who pay using cards.
2. Labels on t-shirts worn by employees that say "card payments welcome."

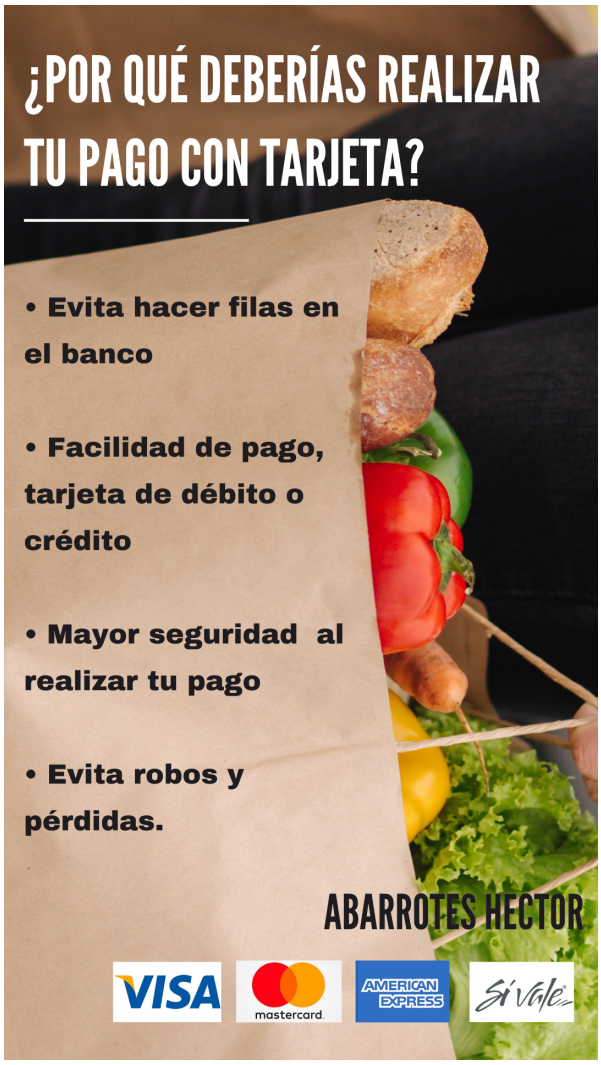


	A	B	C	D	E	F	G	H	I	J
1	**Fecha**	**Número de Transacción**	**Tramaño de transacción (Pesos)**	**¿EL propietario y/o staff dieron el spech de pagos electrónicos?**	**¿El pago fue en efectivo o tarjeta?**	**¿El propietario proporcionó el recibo de la applicación KIWI?**	**¿Hubo algún problema con el pago con tarjeta?**	**Sexo del cliente**	**Rango de edad del cliente (No preguntes, estima)**	
2	05.02.2019	1	500	Sí	Efectivo	Sí	N/A	Mujer	20-30 años	
3		2								
4		3								
5		4								
6		5								
7		6								
8		7								
9		8								
10		9								
11		10								
12										
13										
14										
15										

Figure 2 Market Research on Consumer Attitudes to Digital Payments

3. Post or write on WhatsApp to your most frequent customers how they can benefit from the card payment service.
4. Post on your social networks announcing that “this business now has card payment services.”
5. Change your Google Maps listing to indicate your business as one that accepts card payments.
6. Create and distribute flyers that say “This business now has card payment service,” to start making yourself known in the neighborhood.

Examples of strategies implemented in the field are shown below:



(a) Flyers with benefits of card payments.

(b) Menu nudges.

(c) Flat discount for digital payments.

(d) Gift for digital payments.

Figure 3 Examples of Value Realization Strategies

Qualitative Methodology for Government “Tech-Drop" Program Audit

Table A2 was constructed using the qualitative methodology outlined in Venugopal and Viswanathan (2019). Specifically, we complied with the following steps to collect and categorize the open-ended text responses:

- Enumerators conducted the interview in the local language of the respondents (Spanish) and established trust with small business owners (analogous to the trusted informants in Venugopal and Viswanathan (2019)) by disclosing their status as local university students and explaining that the purpose of the research was to support the economic development of small businesses like theirs.
- Enumerators knew the contextual history of the business owners as they were residents of the municipality and shopped frequently at the types of businesses interviewed. Authors obtained contextual history knowledge by spending 4 weeks in the field in Guadalajara in Summer to Fall 2018 (prior to the study beginning in 2019) talking to government partners, NGOs, university students, and business owners.
- A semi-structured interview process was followed: business owners were asked if they received the stipulated program components (binary coded 0/1); if they were accepting digital payments at present (binary coded 0/1); and if not, what was main reason why they were not accepting digital payments at present (open text response).
- Enumerators’ field notes (typed on a tablet device) on the last question constitute the text to be analyzed. Unfortunately, due to time limitations and privacy limitations in that business owners were unwilling to be recorded, responses were not transcribed verbatim, which is a limitation that underscores the need for our field experiment.
- We hired an RA to read text responses and independently conduct a thematic analysis per Venugopal and Viswanathan (2019): propose first-order concepts (i.e., distinct reasons for non-adoption); second-order themes (i.e., groupings of distinct reasons which followed similar themes); and then aggregated dimensions where we asked the RA to identify two to three major dimensions to categorize the responses.
- The RA then met with the authors to converge on these aggregated dimensions. This motivated the new labels for the frictions: installation complexity and value uncertainty.
- The RA then returned to the original dataset to classify each row response (corresponding to a business owner) by first-order concept, second-order theme, and aggregated dimension, coding reasons that didn’t fit into installation complexity and value uncertainty as “other".

Recruitment: SUTVA Violation Checks

Each retailer is in a census tract with an average of 221.59 retailers, yet only 1.18 (0.5%) of those retailers are also in the study sample. Moreover, the average distance to the *nearest* in-sample retailer in the same business line is 1.65km, making it unlikely that the dataset includes competitor pairs given the density of urban retail zones. This setup minimizes interference among retailers and thus reduces the likelihood of SUTVA violations.

Table 2 SUTVA CHECKS—SPACING OF FINAL SAMPLE

Variable	Mean	Median	75th Percentile	25th Percentile
Total retailers in same census tract	221.59	179.5	261.5	130
In-sample retailers in same census tract	1.18	1	2	0
In-sample retailers of same type in same census tract	0.14	0	0	0
Distance to nearest in-sample retailer (m)	384.59	256.25	400.52	151.53
Distance to nearest in-sample retailer of same type (m)	1654.18	1000.84	1932.73	509.69

Notes: This table presents summary statistics on N=479 retailers in the sample. Data on total retailers in the same census tract was obtained from confidential 2019 Census microdata. The remaining data was collected for N = 479 retailers (prior to randomization) at the business location.

Baseline Audit: Sample Descriptives and Representativeness Checks

Table 3 SUMMARY STATISTICS ON RETAILER CHARACTERISTICS (FINAL SAMPLE)

Variable	Mean	St. Dev.	Min	Max
Owner Characteristics				
Age	44.30	12.89	18	78
Age under 26	0.07	—	0	1
Age 26 to 45	0.47	—	0	1
Age over 45	0.46	—	0	1
Highest Education Level (1 to 13)	5.81	1.78	2	10
No Schooling	0.002	—	0	1
Primary Schooling Only	0.06	—	0	1
Secondary Schooling Only	0.49	—	0	1
Post-Secondary Education	0.45	—	0	1
Gender (Male=1)	0.54	—	0	1
Married	0.59	—	0	1
Number of Children	0.94	1.22	0	8
Business Characteristics				
Number of Employees: Unpaid and Paid	1.32	1.62	0	10
Total Assets (Pesos)	304,948	485,937	15,000	2,354,200
Weekly Customers (1 to 12)	4.82	3.35	1	12
Monthly Sales Estimate (Pesos)	46,902	54,167	3,000	250,000
Monthly Profits Estimate (Pesos)	9,504	8,880	0	38,000

Notes: This table presents summary statistics on all retailers in the sample. The data was collected for N = 479 retailers (prior to randomization) at the business location

We note a few differences between the variables included in Table 5 where sample means are compared to the 2019 census and Table 3 which describes our sample. First, we report monthly costs instead of monthly profits in Table 5 as the census makes sales and costs data available, but not profits. The census also makes gross margin data available, and

Table 4 PRODUCT TYPES AT BASELINE (FINAL SAMPLE)

	Number	Percentage
Goods	**289**	**60.33**
Food and Beverage (e.g., groceries)	105	21.92
Stationary, Crafts, and Gifts	53	11.07
Autoparts	33	6.89
Pharmacy, Beauty, and Health	26	5.43
Hardware	24	5.01
Clothes, Footwear, and Accessories	20	4.18
Electronics	13	2.71
Home Goods	11	2.30
Plants, Flower Shop	4	0.84
Services	**190**	**39.67**
Prepared Food (e.g. café, fast food)	111	23.17
Beauty Salon	46	9.60
Clinics	13	2.71
Laundromat or Tailor	9	1.88
Classes: Dance, Tutoring etc.	6	1.25
Event Spaces	2	0.42
Travel Agent	2	0.42
Gym	1	0.21

Notes: This table presents statistics summarizing the product types sold at retailers in our sample of N=479, collected at baseline at the business location.

Table 5 SUMMARY STATISTICS ON RETAILER CHARACTERISTICS: COMPARISON TO 2019 CENSUS

Variable	Mean	St. Dev.	Min	Max	Census 2019 Mean
Business Characteristics					
Number of Employees: Unpaid and Paid	1.32	1.62	0	10	1.02
Monthly Sales Estimate (Pesos)	46,902	54,167	3,000	250,000	51,133
Monthly Costs Estimate (Pesos)	37,750	47,935	1,350	222,500	35,534
Gross Margin (%)	27.78	29.66	−375	125	26.78
Assets: Buildings (Pesos)	112,302	339,266	0	1,500,000	88,236
Assets: IT Equipment (Pesos)	7,231	9,373	0	40,000	5,636
Assets: Machinery (Pesos)	40,407	62,636	0	300,000	17,423
Assets: Furniture (Pesos)	16,431	17,711	0	80,000	19,417

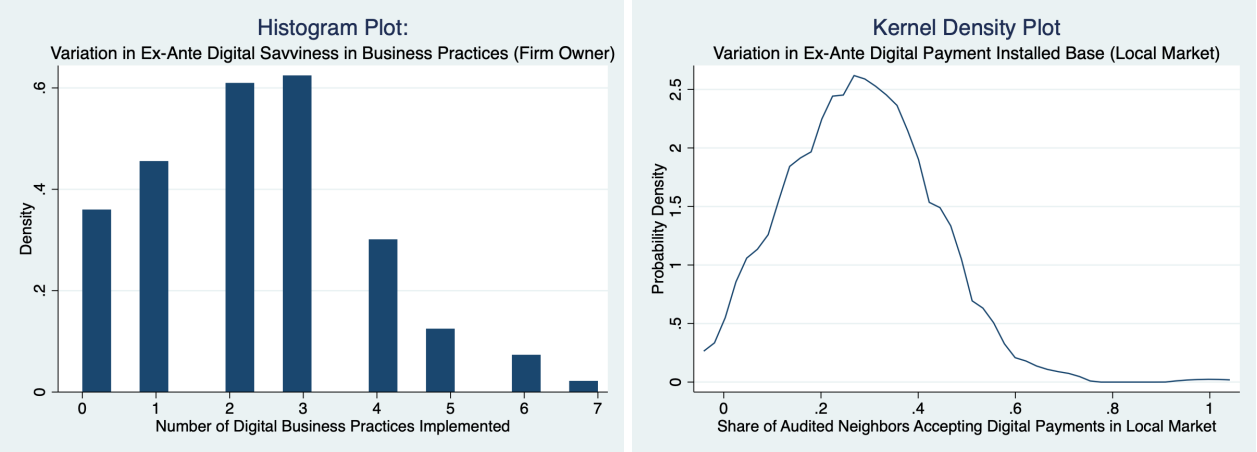
Notes: This table compares sample characteristics to means from the 2019 census. Establishments included in census means calculations are those with NAICS codes of 46 (retail), 722 (prepared food services), 812 (personal services, e.g., beauty salons and laundromats) in the strata of 0 to 10 employees. Costs are reported as opposed to profits due to census availability.

we note similar gross margins in our sample. Finally, we compare several major physical asset categories—buildings, IT, machinery, and furniture. We are unable to compare total assets, as our baseline measure includes cash-on-hand, which is excluded in the census calculation. We are also unable to compare inventories, as our baseline measure bundles inventory and input materials. Nevertheless, monthly output (sales) in our sample is very close to the census mean.

Finally we illustrate the sample heterogeneity in digital capacity at baseline.

Figure 4 Sample Heterogeneity in Digital Capacity at Baseline

(a) Owner Digital Savviness (b) Market Installed Base



Note. The seven digital practices measured (Yes/No) are: using the internet for product research, using the internet for supplier research, using digital technology for marketing, using a digital system for recordkeeping, using a digital system for customer analysis, using a digital system for stock ordering, and using a digital system for stock monitoring. The installed base is measured through an audit of N=1551 neighbors of the study sample, grouped into respective 4-digit Zipcode areas to compute acceptance shares. They include traditional stand-alone retailers as well as modern chains (e.g. 7-eleven, Starbucks) to accurately capture the digital payment installed base in the local market.

Randomization: Balance Checks

Table 6 Columns 1–3 show the means of each variable for the control group, T1, and T2, respectively. In Column 4, we report the p-value from the analysis of variance F-tests of equality of the three means. We find that across the 10 tests (for equality of three means), we fail to reject all null hypotheses of mean equality at the 10% level. In addition, the F-test for joint equality of balance variables is not significant for the relevant three-group comparisons.

Table 6 BALANCE CHECKS FOR RETAILERS RANDOMIZED AT BASELINE (FINAL SAMPLE)

	Control Mean	T1 Mean	T2 Mean	P-Value (F-Test)
Number of Employees: Unpaid and Paid	1.28	1.47	1.22	0.379
Total Assets (Pesos)	266,077	308,540	243,777	0.277
Weekly Customers (1 to 12)	4.73	5.05	4.69	0.569
Monthly Sales Estimate (Pesos)	49,397	45,787	45,564	0.779
Monthly Profits Estimate (Pesos)	10,119	8,987	9,412	0.520
Age	43.47	45.31	44.10	0.432
Highest Education Level (1 to 13)	5.81	5.81	5.82	0.997
Gender (Male=1)	0.56	0.57	0.49	0.369
Married	0.59	0.60	0.57	0.808
Number of Children	0.97	0.91	0.94	0.900
Joint Equality F-Stat (Control v T1)				0.796
Joint Equality F-Stat (Control v T2)				0.205
Joint Equality F-Stat (T1 v T2)				0.870

Notes: This table presents balance checks for the full sample of retailers based on pre-intervention data on business and owner characteristics. The first three columns present average values by experimental group. The fourth column presents the equality of means F-test. Statistically significant p-values are highlighted by: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Attrition and Non-Survival

From the sample of N = 479 retailers randomized at baseline, we were able to reach 94.2% of the owners during the audit twelve months post-intervention. Attrition from our sample was not systematically related to treatment assignment. Table 7 presents linear regression analysis to check for differential attrition between our three experimental groups. The results in Column (1) indicate that we do not find any evidence for differential attrition occurring in any of the treatment groups relative to the control group. In Table 7, we also examine whether business closure rates were systematically related to treatment assignment. We do so to check that business closure does not threaten the validity of our experimental inferences. For example, if small retailers in the control group were less likely to survive than those in the treatment groups, we might not observe relevant counterfactual adoption for treated retailers across the full distribution of retailer size. The results in Column (2) indicate no evidence for differential retailer closure in any of the treatment groups relative to the control group. Finally, we show that a retailer's "attrition" from the dataset shared by our partner Fintech company was not systematically related to treatment assignment in Table 7 Column (3).

Next, to demonstrate that the post-attrition sample is not unbalanced, we present in Table 8 balance checks for the sample of N = 451 retailers that responded during the audit. Across the ten tests on balance variables (for equality of three means), we do not reject any of the null hypotheses of mean equality at the 5% level. Thus, we conclude that attrition is not a significant concern in our sample.

To demonstrate balance in the operational sample of retailers upon which our treatment effects are based, in Table 9, we present balance checks for the operational sample of retailers. Across the ten tests on balance variables (for equality

Table 7 Attrition and Non-Survival by Treatment Assignment

	Audit		Admin Data
	(1) Attrition (Yes = 1)	(2) Non-Operational (Yes = 1)	(3) Data Unavailable (Yes = 1)
T1	-0.0186	-0.0348	
	(0.0266)	(0.0446)	
T2	-0.0228	-0.0565	-0.0484
	(0.0257)	(0.0433)	(0.0460)
Biz/Owner Controls	Yes	Yes	Yes
Strata FE	Yes	Yes	Yes
Mean of DV: Control	0.0759	0.253	—
Mean of DV: T1	0.0566	0.240	0.233
Obs.	479	451	321

Notes: This table analyzes attrition and non-survival status by treatment assignment. The DV in Column (1) is a binary indicator coded '0' if the retailer responded in the audit (data obtained or non-operational status confirmed) and '1' for attriter if the retailer did not respond in the audit (no data obtained and could not reach in any way to confirm operating status). The DV in Column (2) is a binary indicator coded '0' if the retailer was operational at the time the audit was conducted and '1' if the retailer had closed by the time the audit was conducted. The DV in Column (3) is a binary indicator coded '0' if the retailer was part of the administrative data sample and '1' if the retailer was not part of the administrative data sample (did not comply with System Integration or did not provide consent to share data). The indicated regressions include: baseline controls for owner and retailer characteristics (gender; age; marital status; children status; education; average customer basket size; monthly sales; total employees; 5 subsector fixed effects) and 9 strata dummies indicating which randomization/implementation batch the retailer was part of. Robust standard errors are in parentheses. P-values are highlighted as: $p < 0.1^{*}$ $p < 0.05^{**}$ $p < 0.01^{***}$

Table 8 BALANCE CHECKS FOR RETAILERS RESPONDING TO AUDIT

	Control Mean	T1 Mean	T2 Mean	P-Value (F-Test)
Number of Employees: Unpaid and Paid	1.29	1.45	1.21	0.405
Total Assets (Pesos)	271,939	312,020	247,874	0.322
Weekly Customers (1 to 12)	4.81	5.08	4.71	0.613
Monthly Sales Estimate (Pesos)	48,646	44,889	45,921	0.825
Monthly Profits Estimate (Pesos)	10,152	8,991	9,436	0.519
Birth Year	43.62	45.45	44.43	0.471
Highest Education Level (1 to 13)	5.73	5.81	5.79	0.930
Gender (Male=1)	0.56	0.55	0.49	0.424
Married	0.60	0.61	0.57	0.698
Number of Children	0.96	0.90	0.95	0.895
Joint Equality F-Stat (Control v T1)				0.704
Joint Equality F-Stat (Control v T2)				0.295
Joint Equality F-Stat (T1 v T2)				0.866

Notes: This table presents balance checks for the full sample of retailers based on pre-intervention data on business and owner characteristics. The first three columns present average values by experimental group. The fourth column presents the equality of means F-test. Statistically significant p-values are highlighted by: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

of three means), we do not reject any of the null hypotheses of mean equality at the 5% level. Thus, we conclude that retailer closure is not a significant issue in our sample either.

Table 9 BALANCE CHECKS FOR RETAILERS OPERATIONAL AT AUDIT

	Control Mean	T1 Mean	T2 Mean	P-Value (F-Test)
Number of Employees: Unpaid and Paid	1.24	1.57	1.30	0.276
Total Assets (Pesos)	286,230	340,697	255,602	0.227
Weekly Customers (1 to 12)	4.98	5.29	4.74	0.457
Monthly Sales Estimate (Pesos)	54,557	48,335	50,727	0.718
Monthly Profits Estimate (Pesos)	11,176	9,586	10,010	0.403
Birth Year	44.37	45.32	44.64	0.851
Highest Education Level (1 to 13)	5.67	5.84	5.80	0.743
Gender (Male=1)	0.59	0.55	0.50	0.365
Married	0.62	0.61	0.55	0.461
Number of Children	1.03	0.91	0.92	0.740
Joint Equality F-Stat (Control v T1)				0.930
Joint Equality F-Stat (Control v T2)				0.449
Joint Equality F-Stat (T1 v T2)				1.080

Notes: This table presents balance checks for the full sample of retailers based on pre-intervention data on business and owner characteristics. The first three columns present average values by experimental group. The fourth column presents the equality of means F-test. Statistically significant p-values are highlighted by: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Finally, to demonstrate balance in the sample of retailers present in the administrative dataset, in Table 10, we present balance checks for this sample. Across the 10 tests on balance variables (for equality of three means), we do not reject any of the null hypotheses of mean equality at the 5% level.

Table 10 BALANCE CHECKS FOR RETAILERS IN ADMINISTRATIVE DATA SAMPLE

	T1 Mean	T2 Mean	P-Value
Number of Employees: Unpaid and Paid	1.54	1.25	0.161
Total Assets (Pesos)	348,160	265,486	0.150
Weekly Customers (1 to 12)	4.79	4.88	0.820
Monthly Sales Estimate (Pesos)	45,559	46,412	0.896
Monthly Profits Estimate (Pesos)	8,747	9,161	0.672
Birth Year	45.27	43.95	0.411
Highest Education Level (1 to 13)	5.84	5.87	0.869
Gender (Male=1)	0.54	0.49	0.373
Married	0.59	0.57	0.711
Number of Children	0.91	0.93	0.881
Joint Equality F-Stat (T1 v T2)			0.638

Notes: This table presents balance checks for retailers in the administrative data sample. The first two columns present average values by treatment group. The third column presents the equality of means test p-value for each variable. The final row reports the joint equality F-statistic across all covariates.

Multiple Hypothesis Testing

Following the framework in List et al. (2019), we test multiple hypotheses across treatments, subgroup effects, and outcome variables[19]. Table 11 outlines the multiple hypotheses tested in the main analysis using audit data. To control the false discovery rate (FDR), we report sharpened q-values calculated using the code from Anderson (2008), which implements the procedure in Benjamini et al. (2006). This approach is commonly applied in analyses of randomized field experiments in developing economies (e.g., Anderson and McKenzie (2022)).

Table 11 ENUMERATION OF MULTIPLE HYPOTHESES TESTED USING AUDIT DATA

	Main Adoption DV ≥ 1 Digital Payment	**Other Adoption DV** ≥ 2% Digital Payments	**Extensive Margin** Functioning device	**Intensive Margin** Usage Scale
Treat × Subgroup				
SI: All retailers	✓	✓	✓	✓
VR: All retailers	✓	✓	✓	✓
SI × Low Dig. Cap.	✓	✓	✓	
SI × High Dig. Cap.	✓	✓	✓	
VR × Low Dig. Cap.	✓	✓	✓	
VR × High Dig. Cap.	✓	✓	✓	
SI × Low Savviness	✓	✓		
SI × High Savviness	✓	✓		
VR × Low Savviness	✓	✓		
VR × High Savviness	✓	✓		
SI × Low Market IB	✓	✓		
SI × High Market IB	✓	✓		
VR × Low Market IB	✓	✓		
VR × High Market IB	✓	✓		

Notes: This table lists the multiple hypotheses tested in the main analysis in paper, using data from the audit twelve months post-intervention.

Below, we replicate the main effect table (Table 3) from the manuscript, with sharpened q-values in square parentheses.

[19] Robustness checks on model specification are not treated as additional hypothesis tests; their purpose is to evaluate the sensitivity of our hypothesis testing results to modeling choices.

Table 12 Impact of Customer Success Management Interventions on Digital Payment Adoption with q-values

	In the month prior to audit ...			
	(1) ≥1 Digital Payment	(2) ≥1 Digital Payment	(3) ≥2% Transactions w. Digital Payments	(4) ≥2% Transactions w. Digital Payments
System Integration	0.214***	0.196**	0.146**	0.130*
	(0.0644)	(0.0678)	(0.0644)	(0.0704)
	[0.006]	[0.018]	[0.029]	[0.058]
Value Realization	0.134**	0.131**	0.122*	0.122*
	(0.0625)	(0.0649)	(0.0650)	(0.0658)
	[0.033]	[0.044]	[0.053]	[0.058]
Biz/Owner Controls	No	Yes	No	Yes
Strata FE	No	Yes	No	Yes
Mean of DV: Control	0.321	0.321	0.275	0.275
Effect Size in %: SI	66.64	60.97	52.98	47.32
Effect Size in %: VR	25.08	25.40	29.04	30.08
Obs.	350	350	350	350

Notes: Data underlying these regressions were collected during an audit 12 months post-intervention. Estimates presented are from linear probability model regressions of the DV on treatment assignment (i.e., intent-to-treat estimates) with observations at the retailer level. The DVs are binary indicators for whether a digital payment system was successfully adopted by the retailer. In Columns (1) and (2), the DV is whether at least one digital payment (card, mobile wallet, or QR-based) was received from a customer in the last month. In Columns (3) and (4), the DV is whether at least 1 in 50 transactions were completed using digital payments (card, mobile wallet, or QR-based) from customers in the last month. The indicated regressions include: baseline controls for owner and retailer characteristics (gender; age; marital status; children status; education; average customer basket size; monthly sales; total employees; 5 subsector fixed effects) and 9 strata dummies indicating which randomization/implementation batch the retailer was part of. Standard errors clustered by 4-digit postal code are in round parentheses. Sharpened q-values (Anderson 2008) to account for FDR due to multiple hypothesis testing are in square parentheses. q-values are highlighted as: $p < 0.1^{*}$ $p < 0.05^{**}$ $p < 0.01^{***}$

Robustness Checks: Main Effects

Table 13 Robustness: Conley Spatial Correlation Correction (Main Effects)

	0.5km Cutoff		1km Cutoff		2km Cutoff	
	(1) ≥1 Digital Payment	(2) ≥2% Digital Payments	(3) ≥1 Digital Payment	(4) ≥2% Digital Payments	(5) ≥1 Digital Payment	(6) ≥2% Digital Payments
System Integration	0.196***	0.130**	0.196***	0.130*	0.196***	0.130*
	(0.0629)	(0.0641)	(0.0692)	(0.0721)	(0.0717)	(0.0725)
	[0.008]	[0.041]	[0.02]	[0.057]	[0.026]	[0.062]
Value Realization	0.131**	0.122*	0.131**	0.122*	0.131*	0.122*
	(0.0583)	(0.0626)	(0.0641)	(0.0652)	(0.0735)	(0.0691)
	[0.039]	[0.041]	[0.057]	[0.062]	[0.062]	[0.062]
Biz/Owner Controls	Yes	Yes	Yes	Yes	Yes	Yes
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean of DV: Control	0.321	0.275	0.321	0.275	0.321	0.275
Effect Size in %: SI	60.97	47.32	60.97	47.32	60.97	47.32
Effect Size in %: VR	25.40	30.08	25.40	30.08	25.40	30.08
Obs.	350	350	350	350	350	350

Notes: Data underlying these regressions were collected during an audit 12 months post-intervention. Estimates presented are from linear probability model regressions of the DV on treatment assignment (i.e., intent-to-treat estimates) with observations at the retailer level. In Columns (1), (3) and (5), the DV is whether at least one digital (card, mobile wallet, or QR-based) payment was accepted from a consumer in the last month. In Columns (2), (4) and (6), the DV is whether at least 1 in 50 transactions were completed using digital (card, mobile wallet, or QR-based) payments from consumers in the last month. The indicated regressions include: baseline controls for owner and retailer characteristics (gender; age; marital status; children status; education; average customer basket size; monthly sales; total employees; 5 subsector fixed effects) and 9 strata dummies indicating which randomization/implementation batch the retailer was part of. Standard errors corrected for arbitrary cluster correlation based on spatial distance between retailers, per Conley (1999), are in round parentheses — cutoffs considered beyond which retailer residuals are assumed to be uncorrelated are 0.5km (Columns 1 to 2), 1km (Columns 3 to 4) and 2km (Columns 5 to 6). Sharpened q-values (Anderson 2008) to account for FDR due to multiple hypothesis testing are in square parentheses. P-values are highlighted as: $p < 0.1^{*}$ $p < 0.05^{**}$ $p < 0.01^{***}$

Table 14 Robustness: Specification Checks (Main Effects)

	(1) Includes Non-Operational	(2) Probit	(3) Logit	(4) Lasso for Control Vars
System Integration	0.168**	0.189***	0.189***	0.214***
	(0.0605)	(0.0621)	(0.0627)	(0.0650)
	[0.014]	[0.005]	[0.006]	[0.003]
Value Realization	0.138**	0.125**	0.122**	0.132**
	(0.0601)	(0.0599)	(0.0596)	(0.0632)
	[0.014]	[0.019]	[0.021]	[0.019]
Biz/Owner Controls	Yes	Yes	Yes	No
Strata FE	Yes	Yes	Yes	No
Obs.	451	350	350	350

Notes: Data underlying these regressions were collected during an audit 12 months post-intervention. Estimates presented are from regressions of the DV on treatment assignment (i.e., intent-to-treat estimates) with observations at the retailer level. The DV is whether at least one digital (card, mobile wallet, or QR-based) payment was accepted from a consumer in the last month. In Columns (2) and (3), instead of coefficient estimates, marginal effects are reported (dy/dx) at mean values of included variables. Standard errors cluster by 4-digit postal code are in round parentheses. Sharpened q-values (Anderson 2008) to account for FDR due to multiple hypothesis testing are in square parentheses. q-values are highlighted as: $q < 0.1^{*}$ $q < 0.05^{**}$ $q < 0.01^{***}$

Table 15 Robustness: SUTVA Checks (Main Effects)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
SI	0.203**	0.187**	0.188**	0.194**	0.203**	0.208**	0.281***	0.286***	0.210**
	(0.0895)	(0.0904)	(0.0849)	(0.0854)	(0.0886)	(0.102)	(0.0989)	(0.0935)	(0.0874)
SI×Dist to Closest Sample Firm	-0.00000696								
	(0.000152)								
VR	0.158*	0.160*	0.149*	0.149*	0.188*	0.114	0.0704	0.0626	0.127
	(0.0886)	(0.0876)	(0.0887)	(0.0834)	(0.0958)	(0.0925)	(0.0962)	(0.0915)	(0.0867)
VR×Dist to Closest Sample Firm	-0.0000852								
	(0.000143)								
SI×Dist to Closest T1/T2		0.0000298							
		(0.000139)							
VR×Dist to Closest T1/T2		-0.0000683							
		(0.000103)							
SI×Dist to Closest T2			0.00000903						
			(0.0000914)						
VR×Dist to Closest T2			-0.0000280						
			(0.0000775)						
SI×Low Dist to Closest Sample Firm				0.0109					
				(0.120)					
VR×Low Dist to Closest Sample Firm				-0.0395					
				(0.112)					
SI×Low Dist to Closest T1/T2					-0.0203				
					(0.112)				
VR×Low Dist to Closest T1/T2					-0.110				
					(0.125)				
SI×Low Dist to Closest T2						-0.0320			
						(0.119)			
VR×Low Dist to Closest T2						0.0338			
						(0.116)			
SI×# Sample Firms in Market							-0.0114		
							(0.00994)		
VR×# Sample Firms in Market							0.00781		
							(0.00818)		
SI×# T1/T2 Firms in Market								-0.0176	
								(0.0138)	
VR×# T1/T2 Firms in Market								0.0131	
								(0.0114)	
SI×# T2 Firms in Market									-0.00565
									(0.0167)
VR×# T2 Firms in Market									0.00160
									(0.0200)
Biz/Owner Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	350	350	350	350	350	350	350	350	350

Notes: Data underlying these regressions were collected during the audit 12 months post-intervention. Estimates presented are from regressions of the DV on treatment assignment (i.e., intent-to-treat estimates) with observations at the retailer level. The DV in all columns is whether at least one digital (card, mobile wallet, or QR-based) payment was accepted from a consumer in the last month. In Columns (1) to (3) we present interactions of treatment assignment with the distance in meters to the closest sample retailer by type (any, then any treatment, then T2 only). In Columns (4) to (6) we present interactions of treatment assignment with a dummy variable for low (below-sample-median) distance to the closest sample retailer by type (any, then any treatment, then T2 only). In Columns (7) to (9) we present interactions of treatment assignment with the number of in-sample retailers by type (any, then any treatment, then T2 only) in the same 4-digit Zipcode. The indicated regressions include: baseline controls for owner and retailer characteristics (gender; age; marital status; children status; education; average customer basket size; monthly sales; total employees; 5 sub-sector fixed effects i.e., set of two digit SIC codes) and 9 strata dummies indicating which randomization/implementation batch the retailer was part of. Standard errors cluster by 4-digit postal code are in round parentheses. P-values are highlighted as: $p < 0.1^{*}$ $p < 0.05^{**}$ $p < 0.01^{***}$

Table 16 ITT and ATT Estimates of Customer Success Management Interventions on Digital Payment Adoption

	≥1 Digital Payment		≥2% Digital Payments	
	(1) OLS: ITT	(2) IV: ATT	(3) OLS: ITT	(4) IV: ATT
System Integration	0.196***		0.130*	
	(0.0678)		(0.0691)	
Value Realization	0.131**		0.122*	
	(0.0649)		(0.0657)	
SI Complier		0.221***		0.147**
		(0.0722)		(0.0741)
VR Complier		0.144**		0.135*
		(0.0706)		(0.0734)
Biz/Owner Controls	Yes	Yes	Yes	Yes
Strata FE	Yes	Yes	Yes	Yes
Mean of DV: Control	0.321	0.321	0.275	0.275
Effect Size in %: SI	60.97	68.71	47.32	53.30
Effect Size in %: VR	25.40	26.54	30.08	32.09
Obs.	350	350	350	350

Notes: Data underlying these regressions were collected during an audit 12 months post-intervention. Estimates presented from linear probability model regressions of the DV on treatment assignment (i.e., intent-to-treat estimates) in Columns (1) and (3), or IV regressions with compliance to interventions instrumented by treatment assignment (i.e., treatment-on-treated estimates) in Columns (2) and (4). In Columns (1) and (2), the DV is whether at least one digital payment (card, mobile wallet, or QR-based) was received from a customer in the last month. In Columns (3) and (4), the DV is whether at least 1 in 50 transactions were completed using digital payments (card, mobile wallet, or QR-based) from customers in the last month. The indicated regressions include: baseline controls for owner and retailer characteristics (gender; age; marital status; children status; education; average customer basket size; monthly sales; total employees; 5 sector fixed effects) and 9 strata dummies indicating which randomization/implementation batch the retailer was part of. Standard errors clustered by 4-digit postal code are in round parentheses. Sharpened q-values are highlighted as: $p < 0.1^{*}$ $p < 0.05^{**}$ $p < 0.01^{***}$

Robustness Checks: Mechanisms and Heterogeneity

To support our model-free evidence on the effects of the Value Realization intervention on usage, we estimate random-effects models as assignment to the Value Realization intervention is time-invariant and randomized. We include month–year fixed effects to capture seasonality for more precise estimates. Across a range of dependent variables (two active usage indicators, monthly digital payment counts in levels and logs, and two standardized usage indices), the Value Realization intervention consistently exhibits large and statistically significant (1% level) impacts on digital payment usage. For instance, the Value Realization intervention raises a standardized usage index by 0.43 standard deviations over T1.

Table 17 Impact of Value Realization Intervention on Digital Payment Usage

	Monthly Active Usage (Binary)		Monthly No. of Payments		Composite	
	(1) Had ≥ 1 Transaction	(2) Had ≥ 4 Transactions	(3) Levels: Winsorized	(4) Log: IHS	(5) Usage Index (Avg.)	(6) Usage Index (PCA)
Value Realization	0.170***	0.156***	2.537***	0.531***	0.392***	0.690***
	(0.0390) [0.001]	(0.0351) [0.001]	(0.840) [0.001]	(0.125) [0.001]	(0.0907) [0.001]	(0.159) [0.001]
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Biz/Owner Controls	Yes	Yes	Yes	Yes	Yes	Yes
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Seasonality Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean of DV: SI	0.293	0.159	3.065	0.693	2.96e-09	-0.369
SD of DV: SI	0.455	0.366	8.390	1.275	0.913	1.605
Effect Size in %	58.03	98.38	82.75	76.70	—	—
Effect Size in SD	0.374	0.427	0.302	0.417	0.429	0.430
Obs.	3019	3019	3019	3019	3019	3019

Notes: Estimates presented are from panel regressions of the DV on treatment assignment, with observations at the retailer-month level (month referring to the number of elapsed months since the digital payment system was installed at the retailer). The DVs in Columns (1)-(2) are binary indicators for whether the digital payment system was actively used in that month, with active usage operationalized in two different ways: in Column (1) as whether it was used to accept at least one digital payment (card, mobile wallet, or QR-based) from consumers and in Column (2) as whether it was used to accept at least four digital payments (card, mobile wallet, or QR-based) from consumers (i.e., one per week). The DVs in columns (3)-(4) are based on the monthly number of digital payments (card, mobile wallet, or QR-based) accepted from consumers, operationalized in two different ways: in Column (3) as levels i.e., number of payments winsorized 2.5% on both tails and in Column (4) as logs i.e., the inverse-hyperbolic-sine transformation applied to the number of payments. The DV in column (5) is the average of all the DVs in Columns (1)-(4) after they are standardized, with T1 as the basis for standardization. The DV in column (6) is the predicted score for the first principal component of the four standardized DVs in Columns (1)-(4). The indicated regressions include: Time FE corresponding to the month post-installation, baseline controls for owner and retailer characteristics, 9 strata dummies indicating which randomization/implementation batch the retailer was part of, and controls for seasonal effects (calendar month FE interacted with calendar year FE). Clustered standard errors (by 4-digit postal code) are in round parentheses. Sharpened q-values (Anderson 2008) to account for FDR due to multiple hypothesis testing are in square parentheses. q-values are highlighted as: $q < 0.1^{*}$ $q < 0.05^{**}$ $q < 0.01^{***}$

These conclusions are robust to using alternative functional forms (e.g., Poisson for count data—see Column (1) of Web Appendix Table 18), different winsorization thresholds for the payment count DVs (see Columns (2)-(3) of Web Appendix Table 18), and excluding 'never-users' for intensive-margin effects using a principal stratification approach (see Web Appendix Table 19).

Table 18 Impact of Value Realization on Monthly Number of Digital Payments: Poisson Model and Winsorization Sensitivity

	(1) Monthly Number of Payments (Winsorized 2.5%)	(2) Monthly Number of Payments (Winsorized 5%)	(3) Monthly Number of Payments (Winsorized 1%)
Value Realization	1.076***	2.125***	2.921***
	(0.226)	(0.691)	(1.082)
Time FE	Yes	Yes	Yes
Biz/Owner Controls	Yes	Yes	Yes
Strata FE	Yes	Yes	Yes
Seasonality Controls	Yes	Yes	Yes
Obs.	3019	3019	3019

Notes: Estimates in Column (1) presented are from a Poisson regression (with random effects) of the DV on treatment assignment, with observations at the retailer-month level (month referring to the number of elapsed months since the digital payment system was installed at the retailer). The DV in Column (1) is the monthly number of digital payments (card, mobile wallet, or QR-based) accepted from consumers, winsorized 2.5% on both tails. The DV in Columns (2) and (3) are the monthly number of digital payments (card, mobile wallet, or QR-based) accepted from consumers, winsorized 5% and 1% on both tails respectively. Indicated regression include: Time FE corresponding to the month post-installation, baseline controls for owner and retailer characteristics (gender; age; marital status; children status; education; average customer basket size; monthly sales; total employees; 5 subsector fixed effects), 9 strata dummies indicating which randomization/implementation batch the retailer was part of, and controls for seasonal effects (calendar month FE interacted with calendar year FE). Clustered standard errors (by 4-digit postal code) are in round parentheses. P-values are highlighted as: $p < 0.1^{*}$ $p < 0.05^{**}$ $p < 0.01^{***}$

Table 19 Impact of Value Realization Intervention on Digital Payment Usage: Excluding Never-Users

	Monthly Active Usage (Binary)		Monthly No. of Payments		Composite	
	(1) Had ≥ 1 Transaction	(2) Had ≥ 4 Transactions	(3) Levels: Winsorized	(4) Log: IHS	(5) Usage Index (Avg.)	(6) Usage Index (PCA)
Value Realization	0.131***	0.149***	2.451***	0.478***	0.351***	0.618***
	(0.0471)	(0.0443)	(1.120)	(0.159)	(0.115)	(0.203)
	[0.004]	[0.004]	[0.007]	[0.004]	[0.004]	[0.004]
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Biz/Owner Controls	Yes	Yes	Yes	Yes	Yes	Yes
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Seasonality Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean of DV: T1	0.371	0.201	3.875	0.876	0.135	-0.132
SD of DV: T1	0.483	0.401	9.267	1.377	0.980	1.722
Effect Size in SD	0.271	0.372	0.265	0.347	0.358	0.359
Obs.	2647	2647	2647	2647	2647	2647

Notes: Estimates presented are from panel regressions of the DV on treatment assignment, with observations at the retailer-month level (month referring to the number of elapsed months since the digital payment system was installed at the retailer). We exclude from the sample the retailers that were never-users, i.e. not a single digital payment received using the digital payment system, and weight "users" in T2 by the predicted probability that they would have used even if they weren't in T2 (computed using a logit model of user status predicted by pre-treatment covariates for T1): a principal stratification approach. The DVs in Columns (1)-(2) are binary indicators for whether the digital payment system was actively used in that month, with active usage operationalized in two different ways: in Column (1) as whether it was used to accept at least one digital payment (card, mobile wallet, or QR-based) from consumers and in Column (2) as whether it was used to accept at least four digital payments (card, mobile wallet, or QR-based) from consumers (i.e., one per week). The DVs in columns (3)-(4) are based on the monthly number of digital payments (card, mobile wallet, or QR-based) accepted from consumers, operationalized in two different ways: in Column (3) as levels i.e., number of payments winsorized 2.5% on both tails and in Column (4) as logs i.e., the inverse-hyperbolic-sine transformation applied to the number of payments. The DV in column (5) is the average of all the DVs in Columns (1)-(4) after they are standardized, with T1 as the basis for standardization. The DV in column (6) is the predicted score for the first principal component of the four standardized DVs in Columns (1)-(4). The indicated regressions include: Time FE corresponding to the month post-installation, baseline controls for owner and retailer characteristics, 9 strata dummies indicating which randomization/implementation batch the retailer was part of, and controls for seasonal effects (calendar month FE interacted with calendar year FE). Clustered standard errors (by 4-digit postal code) are in round parentheses. Sharpened q-values (Anderson 2008) to account for FDR due to multiple hypothesis testing are in square parentheses. q-values are highlighted as: $q < 0.1^{*}$ $q < 0.05^{**}$ $q < 0.01^{***}$

Table 20 Mechanism Evidence and HTEs by Digital Capacity with q-values

	Extensive Margin DV: Functioning Digital Payment System In-Store and Operable by Staff				**Main Adoption DV**: ≥ 1 Digital Payment		**Alt. Adoption DV**: ≥ 2% Digital Payments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
System Integration	0.369***	0.346***	0.575***	0.592***	0.463***	0.450***	0.392***	0.385***
	(0.0570)	(0.0614)	(0.0987)	(0.110)	(0.103)	(0.125)	(0.0915)	(0.116)
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.007]	[0.001]	[0.009]
System Integration			-0.319**	-0.387**	-0.396***	-0.413**	-0.389***	-0.412**
× High Digital Capacity			(0.123)	(0.137)	(0.123)	(0.153)	(0.126)	(0.156)
			[0.017]	[0.018]	[0.006]	[0.019]	[0.008]	[0.02]
Value Realization	-0.0230	-0.0159	-0.125*	-0.108	-0.0760	-0.0701	-0.0882	-0.0710
	(0.0544)	(0.0596)	(0.0791)	(0.0855)	(0.0917)	(0.0943)	(0.0880)	(0.0911)
	[0.245]	[0.338]	[0.08]	[0.123]	[0.18]	[0.250]	[0.162]	[0.246]
Value Realization			0.172*	0.160*	0.356***	0.345**	0.356***	0.331***
× High Digital Capacity			(0.100)	(0.105)	(0.122)	(0.127)	(0.112)	(0.116)
			[0.066]	[0.097]	[0.01]	[0.019]	[0.007]	[0.018]
Biz/Owner Controls	No	Yes	No	Yes	No	Yes	No	Yes
Strata FE	No	Yes	No	Yes	No	Yes	No	Yes
Mean of DV: Control	0.394	0.394	0.394	0.394	0.321	0.321	0.275	0.275
Effect Size in %: SI	93.45	87.69	—	—	—	—	—	—
Effect Size in %: VR	*Null*	*Null*	—	—	—	—	—	—
Mean of DV: Control + Low DC	—	—	0.216	0.216	0.162	0.162	0.108	0.108
Effect Size in %: SI + Low DC	—	—	266.1	273.7	285.4	277.7	362.5	356.1
Effect Size in %: VR + Low DC	—	—	*Null*	*Null*	*Null*	*Null*	*Null*	*Null*
Mean of DV: Control + High DC	—	—	0.486	0.486	0.403	0.403	0.361	0.361
Effect Size in %: SI + High DC	—	—	52.73	42.03	*Null*	*Null*	*Null*	*Null*
q-value: SI + High DC = 0	—	—	0.002	0.017	0.166	0.303	0.340	0.338
Effect Size in %: VR + High DC	—	—	*Null*	*Null*	59.68	62.53	73.68	77.73
q-value: VR + High DC = 0	—	—	0.203	0.250	0.004	0.009	0.005	0.009
Obs.	350	350	350	350	350	350	350	350

Notes: Data underlying these regressions were collected during an audit 12 months post-intervention. Estimates presented are from linear probability model regressions of the DV on treatment assignment (i.e., intent-to-treat estimates) and in specified columns, treatment assignment interacted with a dummy variable indicating high (above-sample median) digital capacity index value. The main effect of high digital capacity ($\theta High_i$ in equation 2) is included in the estimation but omitted from the table for brevity. The DV in Columns (1) to (4) is a binary indicator for whether a functioning digital payment system was present in-store at the time of the audit, which staff were able to operate. In Columns (5) and (6), the DV is whether at least one digital (card, mobile wallet, or QR-based) payment was accepted from a consumer in the last month. In Columns (7) and (8), the DV is whether at least 1 in 50 transactions were completed using digital payments (card, mobile wallet, or QR-based) from a consumer in the last month. The indicated regressions include: baseline controls for owner and retailer characteristics (gender; age; marital status; children status; education; average customer basket size; monthly sales; total employees; 5 subsector fixed effects) and 9 strata dummies indicating which randomization/implementation batch the retailer was part of. Standard errors clustered by 4-digit postal code are in round parentheses. Sharpened q-values (Anderson 2008) to account for FDR due to multiple hypothesis testing are reported in square parentheses, highlighted as: q < 0.1* q < 0.05** q < 0.01***

Table 21 Mechanism Evidence: HTEs by Digital Capacity Sub-components with q-values

	Main Adoption DV: ≥ 1 Digital Payment in the Last Month			
	(1)	(2)	(3)	(4)
System Integration	0.363***	0.376**	0.322***	0.295***
	(0.115)	(0.131)	(0.0741)	(0.0784)
	[0.007]	[0.018]	[0.001]	[0.005]
System Integration	-0.218*	-0.277*		
× High Owner Savviness	(0.129)	(0.148)		
	[0.066]	[0.058]		
System Integration			-0.362**	-0.355**
× High Market Installed Base			(0.144)	(0.150)
			[0.019]	[0.025]
Value Realization	-0.0582	-0.0667	0.0583	0.0529
	(0.0950)	(0.0958)	(0.0795)	(0.0807)
	[0.215]	[0.250]	[0.203]	[0.250]
Value Realization	0.307**	0.320**		
× High Owner Savviness	(0.131)	(0.125)		
	[0.026]	[0.022]		
Value Realization			0.269*	0.280*
× High Market Installed Base			(0.154)	(0.165)
			[0.064]	[0.076]
Biz/Owner Controls	No	Yes	No	Yes
Strata FE	No	Yes	No	Yes
Mean of DV: Control + Low [HTE Var]	0.219	0.219	0.269	0.269
Effect Size in %: SI + Low [HTE Var]	165.8	171.9	119.6	109.7
Effect Size in %: VR + Low [HTE Var]	*Null*	*Null*	*Null*	*Null*
Mean of DV: Control + High [HTE Var]	0.364	0.364	0.452	0.452
Effect Size in %: SI + High [HTE Var]	39.8	*Null*	*Null*	*Null*
q-value: SI + High [HTE Var]=0	0.043	0.123	0.268	0.309
Effect Size in %: VR + High [HTE Var]	49.0	54.8	79.5	84.7
q-value: VR + High [HTE Var]=0	0.009	0.014	0.014	0.022
Obs.	350	350	350	350

Notes: Data underlying these regressions were collected during an audit 12 months post-intervention. Estimates presented are from linear probability model regressions of the DV on treatment assignment (i.e., intent-to-treat estimates) interacted with a dummy variable indicating high (above-sample median) owner savviness or market installed base. The main effect of high digital capacity ($\theta High_i$ in equation 2) is included in the estimation but omitted from the table for brevity. The DV is a binary indicator for whether at least one digital (card, mobile wallet, or QR-based) payment was accepted from a consumer in the last month. The indicated regressions include: baseline controls for owner and retailer characteristics (gender; age; marital status; children status; education; average customer basket size; monthly sales; total employees; 5 subsector fixed effects) and 9 strata dummies indicating which randomization/implementation batch the retailer was part of. Standard errors clustered by 4-digit postal code are in round parentheses. Sharpened q-values (Anderson 2008) to account for FDR due to multiple hypothesis testing are reported in square parentheses. Sharpened q-values are highlighted as: q < 0.1* q < 0.05** q < 0.01***

Table 22 Mechanism Evidence: Intermediate Outcomes and HTEs by Continuous Digital Capacity Index

	Extensive Margin DV: Functioning Digital Payment System In-Store and Operable by Staff				**Main Adoption DV**: ≥ 1 Digital Payment		**Alt. Adoption DV**: ≥ 2% Digital Payments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
System Integration	0.369***	0.346***	0.376***	0.353***	0.218***	0.197***	0.152**	0.134**
	(0.0570)	(0.0614)	(0.0518)	(0.0548)	(0.0615)	(0.0620)	(0.0593)	(0.0620)
System Integration × Digital Capacity (normalized 0–1; mean centered)			-0.695***	-0.793***	-0.709***	-0.774**	-0.730***	-0.796***
			(0.253)	(0.272)	(0.267)	(0.310)	(0.255)	(0.289)
Value Realization	-0.0230	-0.0159	-.0213	-0.0124	0.138**	0.137**	0.125**	0.126**
	(0.0544)	(0.0596)	(0.0529)	(0.0855)	(0.0601)	(0.0602)	(0.0626)	(0.0623)
Value Realization × Digital Capacity (normalized 0–1; mean centered)			0.383*	0.371*	0.615**	0.638**	0.563**	0.546**
			(0.218)	(0.220)	(0.265)	(0.288)	(0.236)	(0.241)
Biz/Owner Controls	No	Yes	No	Yes	No	Yes	No	Yes
Strata FE	No	Yes	No	Yes	No	Yes	No	Yes
Mean of DV: Control	0.394	0.394	0.394	0.394	0.321	0.321	0.275	0.275
Effect Size in %: SI	93.45	87.69	95.42	89.38	68.01	61.38	55.17	48.65
Effect Size in %: VR	*Null*	*Null*	*Null*	*Null*	25.52	26.34	29.30	30.93
Obs.	350	350	350	350	350	350	350	350

Notes: Data underlying these regressions were collected during an audit 12 months post-intervention. Estimates presented are from linear probability model regressions of the DV on treatment assignment (i.e., intent-to-treat estimates) and in specified columns, treatment assignment interacted with a continuous variable for the digital capacity index. This index is constructed using four measures (digital native age cohort, digital practices, share of neighbors accepting digital payments, estimate of share of local consumers owning payment cards), with indicators scored one if above the median value on this measure and zero if below the median. The sum of these four indicators was computed to construct an overall digital capacity index ranging from zero (lowest) to four (highest). The interaction analysis uses a continuous measure of this composite: normalized between zero (highest digital capacity) and one (lowest digital capacity) and then mean centered. The main effect of digital capacity ($\theta High_i$ in equation 2) is included in the estimation but omitted from the table for brevity. The DV in Columns (1) to (4) is a binary indicator for whether a functioning digital payment system was present in-store at the time of the audit, which staff were able to operate. In Columns (5) and (6), the DV is whether at least one digital (card, mobile wallet, or QR-based) payment was accepted from a consumer in the last month. In Columns (7) and (8), the DV is whether at least 1 in 50 transactions were completed using digital payments (card, mobile wallet, or QR-based) from a consumer in the last month. The indicated regressions include: baseline controls for owner and retailer characteristics (gender; age; marital status; children status; education; average customer basket size; monthly sales; total employees; 5 subsector fixed effects) and 9 strata dummies indicating which randomization/implementation batch the retailer was part of. Standard errors clustered by 4-digit postal code are in round parentheses. p-values are highlighted as: $p < 0.1^{*}$ $p < 0.05^{**}$ $p < 0.01^{***}$

Figure 5 **Predictors of Organic Adoption in Control Group**

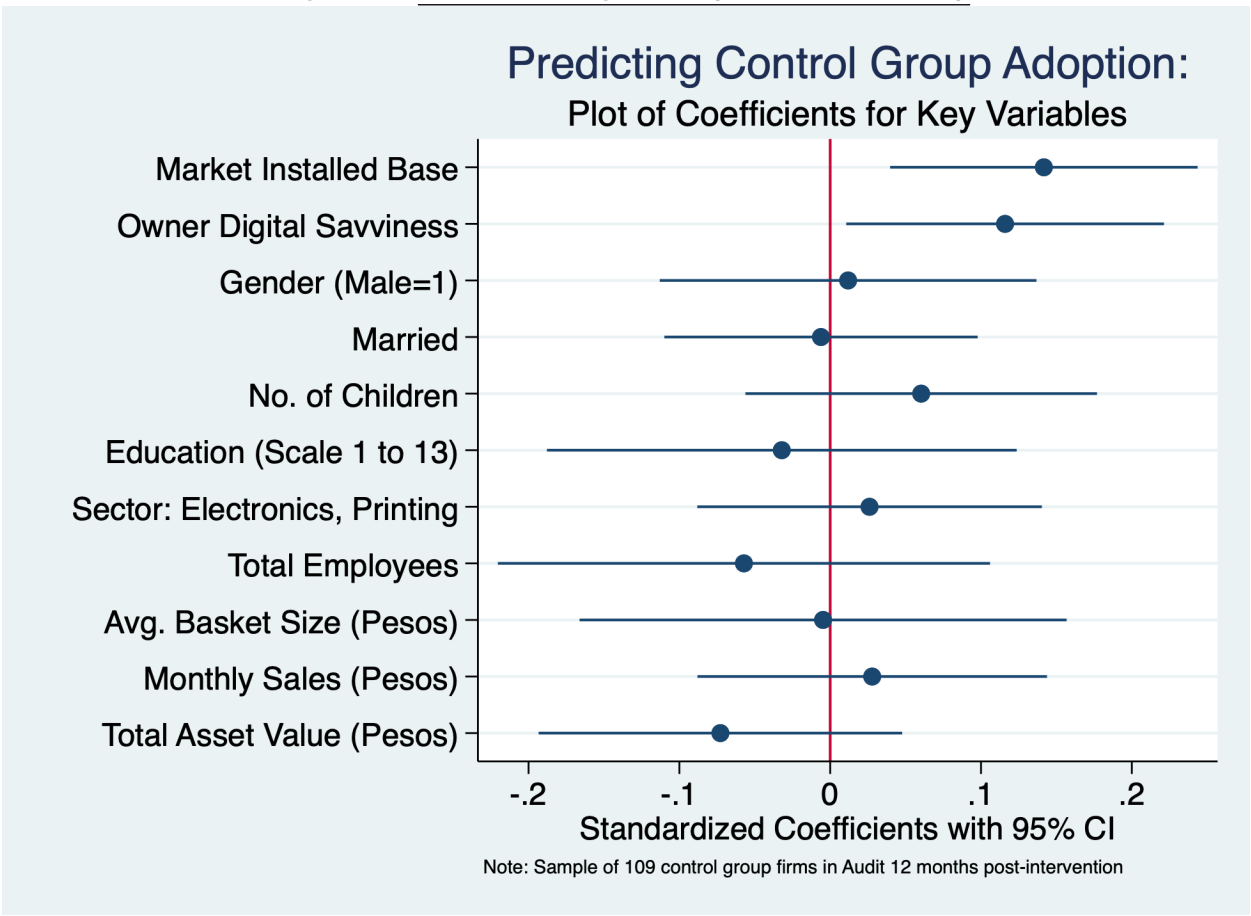


Table 23: Installation Challenges and Their Solutions

39 Installation Challenges and Their Solutions		
Total Retailers Receiving Customized CSM Solution	**123**	**100%**
1. Internet failures (for example: slow, patchy etc.)	47	38.21
CSM Solution: Resolve with TSP hotline or switch to Wifi		
2. No accessible bank account to receive payment deposits	42	34.14
CSM Solution: Open bank account online with Afirme Bank		
3. Forgetful of, or slow in learning, how to process payments	16	13.00
CSM Solution: Provide extra practice and video tutorials		
4. Lacked knowledge on banking system and online banking	13	10.75
CSM Solution: Provide banking 101 training		
5. Processing payment takes ≥ 1 min	9	7.32
CSM Solution: Update smartphone OS and payment app		
6. Transaction declined due to error	7	5.69
CSM Solution: Reset account and wait 24-48 hours		
7. Low attention span of entrepreneur	5	4.07
CSM Solution: Reschedule appointment, focus on training other employees		
8. Forgot email account password	3	2.44
CSM Solution: Reset password steps		

Notes: Data was collected from CSMs during the System Integration intervention (CSMs completed a report after each visit to a retailer). Frequencies of installation challenges and solutions are reported for retailers included in the analysis in Section 3.

(Continued)

39 Installation Challenges and Their Solutions		
9. Bank requires visit to complete registration process *CSM Solution: Open bank account online with Afirme Bank*	3	2.44
10. SIM card phone number lost *CSM Solution: Resolve with TSP hotline*	3	2.44
11. Entrepreneur missing debit card to withdraw deposit *CSM Solution: Train to withdraw funds without debit card*	3	2.44
12. Delays in registration process to accept foreign cards *CSM Solution: Resolve with payment app hotline*	3	2.44
13. Entrepreneur wants to change email account *CSM Solution: Create new account in payment app*	2	1.63
14. Entrepreneur misplaced card reader or it got stolen *CSM Solution: Provide replacement card reader*	2	1.63
15. Smartphone slow due to many apps *CSM Solution: Delete unnecessary apps*	2	1.63
16. Confusion between bank account number, debit card number, and IBAN number *CSM Solution: Explain differences and retrieve IBAN number*	2	1.63
17. Card reader battery issues *CSM Solution: Call payment app hotline to replace card reader*	2	1.63
18. Lack of understanding around default sales tax charges *CSM Solution: Explain sales tax system quickly*	2	1.63
19. No practice transaction is processed *CSM Solution: Reset account and extra practice visit*	2	1.63
20. Entrepreneur not approved for new bank account due to application inconsistencies *CSM Solution: Set up new bank account in spouse's name*	2	1.63
21. Smartphone battery failure *CSM Solution: Onboard on entrepreneur's personal phone*	2	1.63
22. Card processing failures after payment app update *CSM Solution: Re-install payment app and login again*	1	0.81
23. "Internal error message" during transaction *CSM Solution: Resolve with payment app hotline*	1	0.81
24. Transaction declined due to network error *CSM Solution: Resolve with payment app hotline*	1	0.81
25. Smartphone automatically downloads unauthorized apps *CSM Solution: Uninstall apps and clear cache*	1	0.81
26. Bank account change not approved for over 72 hours *CSM Solution: Resolve with payment app hotline*	1	0.81
27. Smartphone screen damaged *CSM Solution: Onboard on entrepreneur's personal phone*	1	0.81
28. Smartphone water damage *CSM Solution: Onboard on entrepreneur's personal phone*	1	0.81
29. Large transaction deposits not received by entrepreneur *CSM Solution: Assist entrepreneur to complete fraud verification steps in payment app*	1	0.81
30. Cannot find payment app in Play Store *CSM Solution: Update Play Store app*	1	0.81
31. Unable to login to payment app *CSM Solution: Call payment app hotline to reset account*	1	0.81
32. Intermittent electricity supply to charge hardware *CSM Solution: Assist with advice on portable charging*	1	0.81
33. Cards not detected in reader *CSM Solution: Call payment app hotline to replace card reader*	1	0.81

Notes: Data was collected from CSMs during the System Integration intervention (CSMs completed a report after each visit to a retailer). Frequencies of installation challenges and solutions are reported for retailers included in the analysis in Section 3.

(Continued)

39 Installation Challenges and Their Solutions		
34. Mobile banking app not support by smartphone OS *CSM Solution: Open new bank account with supported provider*	1	0.81
35. Payment app frozen *CSM Solution: Re-install app and try again*	1	0.81
36. Mobile (card-free) payments not being processed *CSM Solution: Resolve with payment app hotline*	1	0.81
37. Charger for card reader not working *CSM Solution: Call payment app hotline to replace charger*	1	0.81
38. Transaction cancellation not processed *CSM Solution: Resolve with payment app hotline*	1	0.81
39. Incorrect IBAN entered during registration *CSM Solution: Call payment app hotline to change IBAN*	1	0.81

Notes: Data was collected from CSMs during the System Integration intervention (CSMs completed a report after each visit to a retailer). Frequencies of installation challenges and solutions are reported for retailers included in the analysis in Section 3.

Table B1 and Web Appendix Table 23 were created using the following methodology:

• CSMs were required to record any challenge encountered in their report, and these were factual in nature, e.g., “Sin cuenta bancaria [no bank account]".

• The field manager built a master list of distinct challenges reported, throughout intervention and indexed each challenge (code of 1 to 39). This master list was built in real-time as the team was solving the challenges in the field—it was a growing document to which new challenges and troubleshooting solutions were added. CSMs would reference the document to learn how to solve a challenge based on prior experiences of the team.

• Post-intervention, the field manager and team leaders read reports for each treated merchant and manually-tagged whether and which challenge(s) were experienced per the index code from the master list, using original text reports as well as conversations with the CSM who completed the intervention.

• The authors were not involved in this hand-tagging. We performed a simple frequency tabulations to generate Table B1 and the full Web Appendix Table 20.

Winner's Curse Correction for Targeted Policy Analysis

In Table 6, we compare cost-per-acquisition (CPA) under four policy regimes: the base Tech-Drop program (Column 1), Tech-Drop with only System Integration added (Column 2), Tech-Drop with both onboarding interventions added (Column 3), and a targeted policy that assigns System Integration to low-digital-capacity retailers and both interventions to high-digital-capacity retailers (Column 4). For Columns (1)–(3), the policy evaluation does not require any correction for the winner's curse, since these columns report the CPA that would result if a fixed intervention were universally added. In other words, there is no model-based "choice" between alternative interventions. By contrast, the targeted policy in Column (4) is explicitly chosen based on subgroup-specific treatment effect estimates: low-capacity retailers are assigned the intervention that improves adoption most for their subgroup, and high-capacity retailers are assigned the intervention that improves adoption most for theirs. Because this rule is learned from the same estimates on which it is evaluated, the naive CPA estimate is biased downwards. This is the "winner's curse" problem.

To address this, we implement the bootstrap correction proposed in Xu et al. (2025). Specifically, we proceed in three steps: (i) For each bootstrap resample of the data, we re-estimate the heterogeneous treatment effects and re-compute the optimal targeting rule to determine the policy assignment; (ii) we compute the implied policy value using the bootstrap estimates ("estimation environment") and then re-compute it using the original full-sample estimates ("evaluation environment"); and (iii) the difference between these two values provides a draw of the winner's curse bias. Averaging across bootstrap replicates yields an estimate of the upward bias, which we subtract from the naive policy value to obtain the winner's-corrected CPA.

For the targeted regime, we report two main sets of numbers. First, the naive CPA, which is 30,242 MXN. Second, the winner's-curse–corrected CPA, which is 31,007 MXN. The correction corresponds to a bootstrap-estimated upward bias of 0.009 in the policy value V, or roughly a 2.5% adjustment. Diagnostics from the bootstrap show that the targeting rule is reasonably stable: across B≈500 resamples, the chosen intervention flipped in 12.7% of cases for low-capacity retailers and in none of the cases for high-capacity retailers. This stability means that more complex bootstrapping procedures discussed in Xu et al. (2025) yield little change and so we report results from the standard bootstrap in the main text of the paper.

That said, for completeness, here in the Web Appendix we report two additional robustness checks that are useful for non-smooth decision rules: (i) an *m-of-n* bootstrap that resamples $m = \lfloor n^{0.95} \rfloor$ per replication, and (ii) the *perturbation* correction that evaluates the bootstrap-chosen policy on perturbed parameters $\hat{\theta}^{p,b} = \hat{\theta} + \varepsilon_N \sqrt{N}\,(\hat{\theta}^b - \hat{\theta})$ with $\varepsilon_N = N^{-0.45}$ (Algorithm 2 in Xu et al. (2025)).

Web Appendix Table 23 reports the intermediate calculations underlying these results. The table shows how the naive policy value is obtained as a weighted average of subgroup ITT effects, how the winner's-curse bias $\widehat{WC}$ is estimated, and how subtracting this bias yields the corrected value and CPA.

Table 23 Winner's Curse Correction for Targeted Policy

Panel A: Inputs

	Value (V)
$Pr(\text{LowDC}) \times ITT_{\text{LowDC}}$	$0.3886 \times 0.450 = 0.175$
$Pr(\text{HighDC}) \times ITT_{\text{HighDC}}$	$0.6114 \times 0.312 = 0.191$
Overall policy value (V_{naive})	0.3659
Naive CPA (MXN $)	30,242

Panel B: Winner's Curse Corrections

	Standard bootstrap	m-of-n $m = \lfloor n^{0.95} \rfloor$	Perturbation ($\varepsilon_N = N^{-0.45}$)
$\widehat{WC}$	0.0090	0.0079	0.0044
V_{corr}	0.3568	0.3580	0.3615
CPA_{corr} (MXN $)	31,007	30,907	30,610

Notes: This table reports intermediate calculations for the targeted policy (Column 4 of Table 6). Policy value V is defined as the probability of acquiring a new adopter. Panel A reports the components used to form the naive value V_{naive} as the subgroup-weighted ITT. Panel B compares three estimation methods and corrections for the winner's-curse bias $\widehat{WC}$. "m-of-n" resamples $m = \lfloor n^{0.95} \rfloor$ per replication. "Perturbation" follows Algorithm 2 in Xu et al. (2025) with $\varepsilon_N = N^{-0.45}$ and the policy is chosen on bootstrap estimates and evaluated on the perturbed ones. CPAs to the nearest MXN.

Policy Implications: Spillovers

Table 24 Secret Shopper Audit of Neighbors: Potential Intervention Spillovers

	Total	Focal Retailer: Control	Focal Retailer: Treatment
Neighbors Audited	852	268	584
Accepting Digital Payments	144 *(16.9%)*	36 *(13.4%)*	108 *(18.5%)*
Newly Accepting Digital Payments	117 *(13.7%)*	30 *(11.5%)*	87 *(15.5%)*

Notes: This table presents data from secret shopper audits of 852 nearest neighbors who are out-of-sample, 18 months post-intervention.