

Market Power and the Supply-Side of Cash Transfers: Experimental Evidence from Chile

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October 9, 2026

Latest version: karnani.cl/jmp.pdf

Abstract

This paper studies the impacts of a cash transfer program on local businesses. We conducted a two-stage RCT in a Chilean municipality, transferring funds to the poorest 1/3 of households. We transferred these funds through a novel digital wallet with restricted redemption, so that households could use the proceeds only at participating local retailers. We randomly encouraged local businesses to join the digital wallet platform, so they could sell to households using this new payment method. We also randomized the fraction of firms treated at the local-market level to measure the supply-side responses to these cash transfers under different levels of market saturation. Firms respond to local cash transfers, increasing prices by about 9% and reducing competitive monitoring. However, retailers facing more competitive markets attenuated their price increases: a 10pp increase in the fraction of substitute retailers reduces prices by 3.4%. These results suggest that limiting local market power may avoid unintended price effects in the context of cash transfer programs. Finally, we estimate a structural model describing the use of and competition for cash transfers, identified using the experimental variation and transaction-level microdata. We use these estimates to run counterfactual exercises that evaluate the impact of different program designs on prices and welfare. Our estimates suggest markups average about 13% and most of the benefits of competition are achieved with a 60% participation rate within the wallet ecosystem.

*Job Market Paper. Email: karnani@mit.edu. We thank Esther Duflo and Parag Pathak for their guidance and support in this project. We also thank the participants of the MIT Development and Applied Microeconomics Lunches and the PUC-Chile Applied Microeconomics Seminar for their feedback. This project was funded by the United Nations Development Program (Fondo Chile 2022), the Inter-American Development Bank (RG-E1958), the Municipality of Pedro Aguirre Cerda (DJ-05906/2024) and the George and Obie Shultz Fund (2022-2023). We thank MIDE-UC and Consultorias Crecer for their data collection and logistic services. We are indebted to the team at Conectados, especially Dulce Frau and Carlos Martinez, for making this cash transfer program possible and sharing their data. This research project was reviewed and exempt by Harvard's Committee on the Use of Human Subjects (IRB24-0376) and by MIT's Committee On the Use of Humans as Experimental Subjects (E-4847). The study was registered on the AEA RCT registry (AEARCTR-0009502).

1 Introduction

Cash transfer programs are widely used as a tool to alleviate poverty in the developing world, and represent one of the most heavily studied anti-poverty interventions in development economics. More than 100 low- and middle-income countries have implemented either conditional or unconditional cash transfer programs, and over one sixth of the world population has received at least one cash transfer payment (Banerjee et al., 2024a; World Bank, 2025; Gentilini, 2022).

Many of these programs have been shown to have positive effects on direct recipient households, such as improving educational attainment (Baird et al., 2011; Marquez-Padilla et al., 2025), promoting productive investments (Gertler et al., 2012; Haushofer and Shapiro, 2016), and reducing child mortality (Walker et al., 2025). However, our understanding of the indirect effects of cash transfer programs on local markets and prices remains limited.¹ Every transfer is ultimately spent somewhere, and the businesses on the receiving end of that spending are not passive: they set prices, monitor competitors, and adjust capacity. Whether the surplus created by a transfer stays with the beneficiary or is captured by the supply side is, at its core, a question about market power.

This gap in knowledge is relevant. If cash transfers systematically increase local prices, they may generate two problematic outcomes. First, the real value of transfers to beneficiaries is partially eroded, requiring higher nominal amounts of cash to maintain the same level of welfare compared to the situation without a change in prices. Second, non-recipient households in the same communities face higher costs of living, potentially exacerbating local inequality (Filmer et al., 2023). Thus, understanding how cash transfers affect the supply-side pricing decisions of local markets is crucial to designing effective social assistance programs.

In this paper, we address this gap by studying the impact of cash transfers on local businesses, with a focus on how market structure and competition shape price dynamics and, ultimately, the division of program surplus between households and firms. We address the following question at the intersection of development economics and empirical industrial organization: how do cash transfers affect local prices, how does competition in local markets mediate this effect, and how large are the resulting welfare consequences for the households the programs intend to help?

The question has become more pressing as transfers migrate to digital instruments that restrict where balances can be spent. Electronic benefit cards (used to deliver SNAP benefits in the US), vouchers (like the World Food Program vouchers in Jordan and Lebanon) and digital wallets (like the COVID-19 cash assistance in Colombia²) are now used by governments and donors around the world to deliver assistance cheaply and transparently. However, they also usually confine spending to an enrolled network of stores. Restricted redemption is attractive precisely because it channels funds to small local businesses, but it is also, mechanically, a device that creates a captive customer

¹Notable exceptions studying this topic include Egger et al. (2022) and Cunha et al. (2019). We discuss these papers in more detail below.

²Beneficiaries of these cash transfers could cash out their transfers by visiting mobile money agents, but the transfers were, in principle, meant to be used with merchants accepting digital wallet payments to avoid agglomerations/queues at money agents in the middle of the pandemic (Londoño-Vélez and Querubin, 2022).

base.

Understanding this relationship is essential for optimal program design. If price effects are substantial and competition-sensitive, then policymakers face important trade-offs, such as choosing between restricted-redemption programs (price vouchers³), which can support some specific local retailers but may create market power, and unrestricted programs (like “pure” cash transfers), which may preserve competition but may not achieve local economic development objectives.

However, competition in local markets is itself determined by a variety of factors, many of which can be related to characteristics of a cash transfer program. For example, low-income communities, which are more likely to be targeted by cash transfer programs, could have fewer local businesses due to limited demand, and therefore less competition. At the same time, cash transfers could stimulate local economic activity and encourage new businesses to enter the market, thereby increasing competition. Thus, independently controlling the level of competition in local markets is central to understanding how cash transfers affect prices.

We make three main contributions. First, we provide experimental evidence on the impact of cash transfers on local prices and firm behavior, leveraging a novel digital wallet platform to conduct a randomized controlled trial (RCT) in Chile. The platform allows us to randomly vary the stores where beneficiaries can use their cash transfers. In other words, the design enables us to randomize on the supply side of the market, allowing us to measure the indirect effects of cash transfers on local businesses.

Second, we also provide experimental evidence on how local market structure and competition influence firm behavior in response to local cash transfer programs. Because we can manipulate the supply side, we can also experimentally change the level of competition among local businesses. We randomized the fraction of stores in the treatment at the local-market level, and then randomly encouraged firms within markets to join, measuring supply-side responses to the cash-transfer-induced demand shock under different levels of treatment saturation. This two-stage design isolates the causal role of competition in mediating the price effects of cash transfers.

Third, we estimate a structural model of household demand for retail stores under restricted redemption, identified by the experimental variation and by transaction-level microdata from the digital wallet. The model delivers objects the reduced form analyses cannot: heterogeneous price sensitivities, substitution patterns across stores, store-level markups, and household welfare. We then use the estimated model to quantify the price and welfare consequences of the program and to evaluate counterfactual designs.

1.1 Empirical Strategy and Research Design

Our empirical strategy leverages a two-stage RCT in Chile. A distinctive feature of our approach is the use of a novel digital wallet platform that, in principle, enables two-sided randomization: one can independently randomize which households receive transfers and which retailers can accept those transfers. In practice, we only randomize on the supply side of the market. These types of two-sided market experiments are

³Quantity vouchers, unlike price vouchers, could in principle shield beneficiaries from price fluctuations, but they also distort incentives for retailers. For example, [Meckel \(2020\)](#) shows that providing quantity vouchers for nutritious foods to low-income mothers and children in the WIC program led to increased prices for non-WIC customers.

common in the tech industry (Johari et al., 2022) but, to the best of our knowledge, we are the first to implement such supply-side randomization in the context of cash transfer programs.⁴

We partnered with a fintech company and the municipality of Pedro Aguirre Cerda (PAC), a low-income municipality in the Santiago Metropolitan Region, to deliver cash transfers to 10,571 households, accounting for about a third of the municipality’s households. The transfer took place through a digital wallet linked to the national identification card (the Chilean *Cédula de Identidad*), redeemable only at enrolled neighborhood stores. The digital wallet platform does not require beneficiaries to have a bank account or smartphone. This design ensures that the program is easily accessible and free of cost to low-income households who may not have access to traditional banking services. The transfer, CLP 50,000 (about US\$55), was worth roughly a tenth of a month’s income for the bottom third of the municipality’s households, who were the target population for this cash assistance program.

We implemented a two-stage design (Crépon et al., 2013) across 188 small retailers in 20 neighborhood markets: neighborhoods were first randomly assigned to a high (75%) or low (25%) treatment saturation, and within each neighborhood individual stores were then randomly encouraged to join the platform. The design generates, within the same municipality and the same transfer program, exogenous variation in whether a store faces the transfer-induced demand and in how many of its neighbors face it too. It therefore separates the two forces that a restricted-redemption program sets in motion: a demand shock at enrolled stores, and a change in the competitive pressure those stores face from other enrolled stores.

We measure the supply response with mystery-shopper prices of a standardized staple: a one-kilogram bag of sugar of the leading national brand. We also collected responses to store surveys at baseline and endline, and accessed the complete ledger of wallet transactions.

Our results reveal that encouraged stores raised their prices by almost 4% relative to the control stores in the neighborhood (the Intent To Treat, ITT), and stores that registered because they were encouraged (compliers) increased their prices by 9% (the Local Average Treatment Effect, LATE). In low-saturation neighborhoods, encouraged stores were 18-19 percentage points less likely to report checking competitors’ prices and products, whereas in high-saturation neighborhoods the same encouragement differentially *raised* monitoring by an additional 23-28 percentage points, reversing the competitive behavior. Accounting for non-compliance exacerbates these differences: in low-competition neighborhoods, and conditional on the complier subpopulation, treated retailers reduced competitive monitoring behavior by 35 percentage points, but the result is again reversed in high-competition neighborhoods. Owners thus recognized and responded to the saturation environment the lottery assigned them.

Their prices, however, did not directly respond to saturation: the price premium of encouraged over control stores is also close to 4% in low-saturation neighborhoods and is not statistically different in high-saturation neighborhoods. Indeed, the count or share of participating neighbors is a coarse measure of competition, as a bakery joining the

⁴Busso and Galiani (2019) is the closest example to our design, as they randomize the *entry*, in the margin, food providers in a CCT program in the Dominican Republic, where other existing incumbents were already in place. However, they do not randomize the market-level saturation, and the number of providers encouraged to enter was at most 3.

program probably does not discipline a greengrocer’s sugar price. Instead, we exploit the within-neighborhood lottery and find that competition disciplines prices at a finer resolution: the price response falls steeply with the share of a store’s encouraged rivals that sell the same product lines.

Using the business lines of every store in the baseline census, we classify two stores of the same neighborhood as close substitutes when they share a common line (aside from “minimarket” which all stores have in common), and measure for each store the share of its encouraged rivals that are its close substitutes, a quantity determined by which particular rivals the lottery drew. Because a store’s expected share under the lottery depends on its fixed position in the network of substitutes, we recenter the share and its interaction with the encouragement at their expectations over re-draws of the lottery (Borusyak and Hull, 2023). A store’s price response falls steeply with that share: the interaction of the encouragement with the recentered encouraged-substitute share is -0.167 in log points, and the corresponding effects on substitute enrollment imply that registration raises the price of a store with no registered close substitute by about 13% and of a store with a registered-substitute share of 1/3 by a nonsignificant 1.5%, a fall of about 3.4% per 10 percentage point increase in the share of local substitutes. Thus, what disciplines a store prices is not the count of participating neighbors, but the count of participating neighbors that are its close substitutes.

Finally, we use the experimental design to directly test for interference. We specifically test two different spillover effects: the effect of a market’s saturation on its non-encouraged stores and the effect of encouraged close substitutes on non-encouraged stores. We find no significant price spillovers on non-encouraged stores. We also find no spillovers on other business practices, except for a marginally significant effect on price monitoring.

These findings demonstrate that competitive intensity plays a causal role in determining whether retailers can extract rents from cash transfer programs. The reduced-form evidence strongly suggests that program design choices regarding retailer participation can substantially affect welfare outcomes.

1.2 Structural Model and Counterfactual Analysis

While the reduced-form evidence establishes that competition matters, there are other questions a program designer could care about: How much beneficiary welfare is lost to higher prices? Who bears that loss? And what would happen under designs we did not run, such as universal enrollment or more specific targeted participation?

To answer these questions, we estimate a random-coefficients discrete-choice model of store choice in the tradition of Berry et al. (1995), adapted to the institutional features of these restricted-redemption transfers: a captive wallet that must be spent within an enrolled-store network, spatial differentiation (Davis, 2006) in dense low-income neighborhoods, and assuming single-product price-setting retailers competing à la Nash-Bertrand. We estimate the model on the universe of wallet transactions in the experiment (over 21k purchases by over 10k beneficiary households) matched to mystery-shopper prices, household geolocations and census-block socioeconomic data.

Identification leverages the experimental design in ways that are rare in demand estimation. Randomized encouragement provides a store-level instrument that is orthogonal to unobserved characteristics by construction and the randomized saturation

arms provide market-level variation in competitive intensity. In addition, the geocoded location of every household and store lets us construct spatial price instruments and isolation-based micro-moments.

On the demand side, we find these low-income households are highly price sensitive, but also face substantial travel costs, so proximity, not price alone, governs which stores compete for each captive beneficiary. On the supply side, we estimate the Nash-Bertrand pricing game jointly with demand. This yields a structural decomposition of marginal cost and the markups: the mean Lerner index is 13%, and markups fall with the randomized enrollment intensity. We use these estimates to simulate counterfactual scenarios and quantify the relevance of expanding competition in the wallet’s ecosystem.

1.3 Related Literature

Our paper contributes to three literatures.

Cash transfers and local markets. First, we contribute to the extensive literature on cash transfer programs in development economics. While numerous studies document the direct benefits of transfers to recipient households (see [Banerjee et al. \(2024a\)](#) for a comprehensive review), evidence on general equilibrium effects remains limited. [Egger et al. \(2022\)](#) estimate the general equilibrium effects of a large cash transfer program in rural Kenya, finding substantial multiplier effects but minimal price impacts (1.2% at most). Similarly, [Cunha et al. \(2019\)](#) find no significant average price effects of transfers in Mexico, though prices respond more in remote, less competitive villages, a pattern our design tests directly. [Filmer et al. \(2023\)](#) document that cash transfers in the Philippines raised local food prices and increased stunting among non-beneficiary children, underscoring that supply-side responses have first-order welfare consequences. Our contribution is to demonstrate *experimentally* that market structure is a key moderator: price effects are substantial when competitive intensity is limited but largely disappear when participation is broad. This helps reconcile seemingly contradictory findings in the literature and provides a framework for predicting when price effects are likely to matter.

Market power and retail competition in developing economies. Second, we contribute to the empirical industrial organization literature on market power in low-income retail settings. Previous studies document substantial markups and imperfect pass-through in markets serving poor consumers ([Mitra et al., 2018](#); [Bergquist and Dinerstein, 2020](#); [Iacovone and McKenzie, 2022](#); [Jiménez-Hernández and Seira, 2022](#)) and that the structure of local retail matters greatly for the welfare of poor households ([Atkin et al., 2016](#)). Spatial differentiation is also central in our setting, as in [Davis \(2006\)](#) and [Houde \(2012\)](#): travel costs sustain fragmented neighborhood markets, each with its own set of oligopolistic suppliers ([Ramos-Menchelli and Sverdlin-Lisker, 2022](#)). Like [Busso and Galiani \(2019\)](#), our experimental variation in competitive intensity provides a direct identification strategy to test the effects of competition on prices, which we further embed in a structural model that recovers markups and welfare. The combination of structural estimation with randomized variation ([Todd and Wolpin, 2006](#); [Attanasio et al., 2012](#); [Low and Meghir, 2017](#); [Allende et al., 2019](#)) remains underutilized in empirical industrial organization, especially in low-income settings.

Digital payments and the delivery of social assistance. Finally, we contribute to the literature on digital financial inclusion and fintech in developing economies. The digital wallet platform we study shares features with mobile money systems used by over two billion people (Raithatha and Storchi, 2025), which have been shown to improve risk sharing and reduce poverty (Jack and Suri, 2014; Suri et al., 2023), and with electronic benefit systems that reduce leakage and improve state capacity (Muralidharan et al., 2016; Aker et al., 2016; Banerjee et al., 2024b). Digital disbursement also reshapes downstream financial behavior (Bachas et al., 2021; Higgins, 2024) and leaves transaction records that enable research and monitoring (Houeix, 2025). Our findings highlight a trade-off specific to these platforms: restricted redemption is what makes digital wallets attractive for targeting and local development, but it is also precisely what creates market power. Quantifying that trade-off, and showing it can be managed with participation rules rather than abandoned, is a central policy contribution of this paper.

1.4 Policy Implications

Our findings have direct implications for the design of cash transfer programs and social protection systems more broadly. First, they highlight the importance of considering local market structure when designing transfer programs: competition matters for program effectiveness. In areas with limited competition, program designers should be cautious about implementing restricted-redemption mechanisms that could create market power for participating retailers, as the price pass-through can erode the transfer value. Instead, unrestricted cash transfers or vouchers redeemable at a wide range of retailers may be preferable to preserve competition and minimize this passthrough. This is particularly relevant in settings with limited retail density or high spatial frictions (like informal settlements), where market power concerns may be more acute. In settings with dense retail markets, competition may limit price effects.

Second, our results suggest that policymakers can manage competitive intensity through program design choices. For example, by encouraging a higher fraction of local retailers to participate in the program (e.g., through outreach or incentives), they can increase competition and mitigate price passthrough effects without requiring sophisticated targeting or substantial administrative capacity. This approach can be particularly effective in communities where market structures may be more concentrated, and the marginal cost of expanding participation is small relative to the welfare benefits it generates (such as ours).

Third, trade-offs exist between local, targeted business development and competition. Programs seeking to support small businesses through restricted redemption, such as the one we study in this paper, must balance this objective against the efficiency costs of reduced competition.

Finally, and closely related to our previous points, while digital payment platforms offer many advantages, they can also facilitate market segmentation if redemption is restricted. Policymakers should carefully consider participation rules when implementing digital transfer systems, and ensure that they do not inadvertently create local oligopolies. Allowing beneficiaries to redeem funds at a wide range of retailers, or implementing mechanisms to promote competition among participating retailers, can help preserve the benefits of digital transfers while minimizing potential downsides.

The remainder of the paper is structured as follows. Section 2 describes the institutional context and the digital wallet platform used to implement our experiments. Section 3 describes the data and empirical strategies, and results for the experiment, with particular focus on competition effects, are presented in Section 4. Then, Section 5 develops the structural model, Section 6 discusses identification and estimation, and Section 7 presents the structural estimates, welfare analysis, and counterfactuals. Finally, Section 8 concludes.

2 The Intervention

Our experiments leverage a novel digital wallet platform developed by Conectados (in English, “Connected”), a Latin American fintech company specializing in targeted social assistance delivery. Understanding the institutional details of this platform is important for interpreting our results, as the specific design features both enable our experimental approach and shape the market environment we study. This section describes the platform’s operation, its advantages relative to traditional transfer systems, and the implications for market power.

2.1 Platform Architecture and Operation

Conectados provides municipalities and private donors with a technology platform for delivering targeted cash transfers to low-income households. The system consists of three integrated components: (1) a digital wallet infrastructure linked to national identification documents, (2) a merchant acceptance network of local retailers, and (3) a transaction processing system that handles payments and settlements.

Beneficiaries Each beneficiary, selected by a municipality or private donor, is issued a digital wallet linked to their national identification card (e.g. Chilean Cédula de Identidad). These national IDs contain machine-readable QR codes, which serve as the payment instrument. Beneficiaries do not need to have smartphones, bank accounts, or any prior relationship with financial institutions to use the platform. They only need their government-issued ID card, making the program accessible to economically marginalized communities or to unbanked or underbanked populations. Once enrolled, beneficiaries receive an SMS notification informing them of their wallet activation and the amount of funds available for spending.⁵

When beneficiaries wish to make a purchase, they simply present their ID card at a participating retailer. The retailer scans the barcode using a smartphone application, which verifies the wallet balance and processes the transaction in real-time. The purchase amount is debited from the beneficiary’s virtual wallet, and the retailer receives a digital receipt confirming payment. Beneficiaries can redeem their virtual wallet balances for food⁶ and unused funds expire after a predetermined period (typi-

⁵De facto, all beneficiaries had access to smartphones, and we also distributed information via WhatsApp messages. We show an example of a WhatsApp message sent to beneficiaries in Appendix Figure 3.

⁶In our experiments, beneficiaries could only use their wallets for food purchases. More broadly, Conectados has implemented cash transfers for other essential goods, such as school supplies or house-

cally 2 weeks in our experiments), creating a “use it or lose it” incentive that encourages prompt spending.⁷

Retailers Local retailers participate in the platform by downloading a proprietary smartphone application and completing a brief registration process.⁸ The application is designed for minimal technical literacy: the entire transaction process involves entering a purchase amount, scanning a QR code, and confirming the transaction.⁹ Importantly, retailers incur zero direct costs: there are no equipment requirements beyond a smartphone with internet access (which all small retailers in our experiment already possess), no transaction fees, and no monthly subscription charges.

After confirming a transaction, retailers receive their payment via bank transfer within a few hours (but never more than 72 hours). This rapid settlement represents a significant improvement over many government-run payment systems, where settlement delays can create cash flow challenges for small businesses.¹⁰ The transaction data is logged in real-time, creating a comprehensive record of all purchases that is valuable for both the program administration and our research purposes.

Donor/Municipality Public authorities or private donors interface with Conectados through a web-based dashboard. Program administrators upload a list of beneficiary identification numbers and specify transfer amounts. Conectados then creates and loads the digital wallets, manages the transaction infrastructure, and provides real-time reporting on program utilization. The company charges donors a fixed fee per cash transfer program, which covers platform maintenance, customer service, and transaction processing.

2.2 Comparison with Traditional Transfer Systems

The Conectados platform offers several advantages over traditional cash transfer delivery mechanisms, though it also introduces new considerations regarding market power.

Relative to Physical Cash Transfers Physical cash distribution faces well-documented challenges in developing economies: high distribution costs, corruption and leakage, and security risks for both beneficiaries and administrators (Muralidharan et al., 2016). Pure cash transfers also limit the ability to restrict expenditures for intended purposes, such as restricting alcohol or tobacco purchases, although empirically this does not seem to be a significant issue (Evans and Popova, 2017). Digital wallets eliminate these concerns while maintaining the flexibility benefits of cash over in-kind transfers.

Moreover, digital systems create detailed transaction data that enable monitoring and evaluation (Houeix, 2025). In our experiments, we observe every transaction with

hold items.

⁷Appendix Figure 7 shows a diagram of how Conectados works, which was also shared with cash transfer beneficiaries.

⁸Businesses are later verified as formal businesses by Conectados, and only then can start processing transactions in the platform.

⁹Appendix Figure 6 shows a diagram of how the transaction process works.

¹⁰For example, in Chile, the “30-day Payment Law” (Ley de Pago a 30 Días) requires government agencies to pay suppliers within 30 days, and in some cases extends this period up to 60 days.

complete information on timing, location, purchase amount, and retailer characteristics, data that would be impossible to collect with physical cash. This transparency benefits both program administrators (who can monitor utilization patterns) and researchers (who can study market responses).

Relative to Electronic Benefit Transfer (EBT) Cards Many developed and middle-income countries deliver social assistance through EBT cards or similar instruments (e.g., SNAP benefits in the United States, Bolsa Família cards in Brazil or Prospera in Mexico). These systems usually require specialized point-of-sale (POS) terminals and impose enrollment costs on retailers. Infrastructure requirements could exclude the smallest retailers, precisely the businesses that may be most important in low-income neighborhoods.¹¹

Conectados overcomes these barriers by leveraging existing smartphones rather than requiring specialized hardware. The zero-cost enrollment has the opportunity to increase retailer participation relative to traditional EBT systems.¹²

Relative to Mobile Money Mobile money systems like Kenya’s M-Pesa have substantially increased financial inclusion in many developing countries (Jack and Suri, 2014). However, these systems typically require beneficiaries to possess mobile phones and understand how to use the mobile money platform. By using national ID cards as the payment instrument rather than mobile phones, Conectados could in principle achieve universal accessibility. In our experimental setting, national ID cards are nearly universal for adults,¹³ and the user interface (presenting an ID card) requires no technical knowledge. This design choice substantially expands the potential beneficiary population relative to phone-based systems.

2.3 Implications for Market Power

While the Conectados platform offers operational advantages, it also creates a potential channel for market power that does not exist with unrestricted cash transfers. This trade-off lies at the center of our research question.

Restricted Redemption Beneficiaries can only redeem their digital wallet balances at select retailers onboarded into the Conectados platform. Unlike physical cash or mobile money, which can be spent anywhere, the digital wallet restricts expenditures to a subset of local retailers. This restricted redemption creates a captive demand pool for participating businesses, which is both a feature (it ensures transfers support local businesses and appropriate expenditure categories) and a potential bug (it creates market power for participating retailers).

From a beneficiary’s perspective, the restriction reduces their effective choice set. If only few local retailers participate, beneficiaries must either shop at these retailers

¹¹However, Higgins (2024) documents a substantial increase in the adoption of POS terminals in small Mexican firms in response to the rollout of Prospera’s debit cards.

¹²In our experiment, over 70% of eligible retailers agreed to participate after a simple marketing campaign.

¹³In Chile it is mandatory for residents to hold a national ID card after the age of 18.

(potentially at higher prices) or forgo the transfer entirely. This captive customer dynamic creates opportunities for participating retailers to raise prices above competitive levels, particularly if consumers face substantial search costs (Ellison, 2016).

From a retailer’s perspective, participation in the platform yields market power over a specific customer segment (the recipients). The strength of this market power depends on competitive intensity: if all nearby retailers participate, beneficiaries can easily substitute away from high-price retailers. But if only a few retailers participate, the limited alternatives constrain beneficiary responses to price increases. Participation is therefore a design parameter that governs the competitive environment.

Policy Trade-off This creates a trade-off for program designers. Restricting transfers to local small businesses may achieve important policy objectives: supporting local entrepreneurship, keeping money circulating within low-income communities, and preventing expenditures on non-essential goods (e.g. by excluding retailers that sell alcohol or tobacco). In fact, these objectives are at the core of Conectados’ mission as a business.¹⁴ However, these restrictions may also enable rent extraction that reduces program effectiveness.

Moreover, the optimal policy likely varies with context. In settings with dense retail markets, competitive pressure may limit market power even with restricted redemption. In settings with sparse retail density, market power concerns may be more severe. Our experimental variation in competitive intensity helps quantify these trade-offs.

2.4 A Platform for Two-Sided Experiments

The Conectados platform’s key innovation for research purposes is the ability to randomize on both sides of the market. Traditional cash transfer experiments randomize beneficiaries but not retailers, i.e. all retailers can accept purchases from any customer. This makes it challenging to identify supply-side effects.¹⁵

By contrast, the Conectados platform allows us to randomize which retailers are encouraged to join the platform while holding fixed the set of beneficiaries. This creates exogenous variation in the demand shock faced by different retailers, and the number of other competitors that receive it, enabling the identification of supply-side effects.

This two-sided randomization capability is the key feature that makes our experiments possible. It allows us to answer questions about market power and competition that have been difficult to address in previous work due to identification challenges. The remainder of the paper exploits this capability to experimentally study retailer responses to cash transfer programs.

¹⁴Conectados was incorporated as a *spinoff* of a Chilean NGO that began using digital wallets to provide social benefits in 2020, in response to the need for more efficient and targeted social assistance programs during the COVID-19 pandemic. They offered a “dual impact” model: digital wallets and restricted redemption could be used to help households in need while at the same time fostering local economic development by channeling funds to small businesses.

¹⁵Market-level cash transfer experiments offer an alternative research design to estimate supply-side responses to cash transfers. For example, Egger et al. (2022) randomize cash transfers to households in rural Kenya, where all local retailers in treated villages can accept cash from any customer. This creates an exogenous demand shock at the village level, but the magnitude of this shock could still be endogenous for local firms, as retailers may enter the market in response to the transfers, affecting the competitive landscape and thus the prices in response to increased demand.

3 Experimental Design and Data

We conducted our experiment in Pedro Aguirre Cerda (Santiago, Chile), which was specifically designed to test whether competitive intensity causally affects retailer responses to cash transfer programs. This section describes the design and data collection.

3.1 Setting

Pedro Aguirre Cerda (PAC) is a mid-sized urban municipality in the Santiago Metropolitan Region, located near Santiago’s city center. According to the 2024 Chilean Census, PAC has a population of 96,062 residents distributed across 31,034 households.

Within Chile, PAC is characterized by relatively high poverty rates and low quality of life indicators. Over 20% of households are classified as income poor by the Chilean government and in the Urban Quality of Life Index (ICVU), which ranks Chile’s urban municipalities across dimensions including housing, health, employment, environment, and connectivity, PAC consistently ranks in the bottom quartile.

The municipality’s economic landscape consists primarily of small-scale retail, personal services, and informal employment. Most households rely on small neighborhood stores (*almacenes*, or *negocios de barrio*) for daily purchases, which include small grocery stores, butcher shops, bakeries and greengrocers. These establishments are typically owner-operated, located within residential properties, and maintain limited inventory.

3.2 Experimental Design

Our experiment implements a two-stage randomization design that creates exogenous variation in competitive intensity while maintaining comparability of treated and control retailers.

Stage 1: Market Definition and Competition Randomization In partnership with the municipality, we identified 188 small neighborhood stores to participate in the program. Eligible businesses included small retail stores with no more than one cashier, excluding those selling alcohol or tobacco products. These businesses were located in different neighborhoods spread across the municipality, which we treat as separate markets.

Neighborhoods were defined based on the Chilean National Institute of Statistics’ (INE) Census Zones, and adjusted for the proximity of stores to each other, so as to avoid separating stores that were close to each other but in different census zones. Zones with very few stores (< 4) were merged with adjacent zones to ensure a minimum number of stores per neighborhood. The objective was to create markets where retailers compete for the same customers: close enough that beneficiaries could reasonably walk between retailers, but distinct enough that cross-market shopping is uncommon.

In total, with this procedure we defined 20 neighborhoods across the municipality, with an average of 9.4 stores per neighborhood (ranging from 4 to 15 stores). Ex-post validation using community detection algorithms on transaction data confirmed that

our market definitions align well with actual shopping patterns.¹⁶

We then randomized these 20 markets into two equally-sized treatment arms: high-saturation and low-saturation. In high-saturation neighborhoods, about 75% of stores were randomly encouraged to participate in the digital wallet ecosystem, whereas in low-saturation neighborhoods, only around 25% of stores were selected.

This first-stage randomization creates variation in saturation intensity across markets. In high-saturation markets, beneficiaries have many nearby options to use their transfers (approximately 7 treated retailers per market on average). In low-saturation markets, beneficiaries have limited options (less than 3 retailers per market on average), creating stronger captive-customer dynamics.

Stage 2: Retailer-Level Randomization Within Markets Within each market, we randomly selected specific retailers for treatment, maintaining the target participation rates determined in Stage 1. For example, if a market with 8 eligible retailers is randomly assigned to the high-competition treatment arm, we randomly selected 6 retailers for treatment. If the same market is randomly assigned to a low-competition condition, we only randomly selected 2 retailers for treatment.

The treatment arms were equally sized: 94 stores were assigned to each treatment condition in the second stage. This two-stage design yields a 2×2 structure with the following sample sizes in each cell: (1) 72 treated retailers in high-competition markets, (2) 22 control retailers in high-competition markets, (3) 22 treated retailers in low-competition markets, and (4) 72 control retailers in low-competition markets. The final allocation of stores and neighborhoods to their respective treatment conditions is shown in Figure 1. The design thus creates, for a store, exogenous variation in whether it was encouraged and, for its market, exogenous variation in how many of its competitors were.

Household Selection and Cash Transfers In this experiment, we distributed cash transfers to 10,571 low-income households through Conectados’ digital wallets. The direct individual recipients account for about 11% of the total population of PAC, and over a third of all households in the municipality, creating a substantial demand shock across the municipality. The cash transfers were directly funded by the municipality, and were intended to provide financial assistance to low-income families while supporting local businesses, i.e. the municipality was aware of the potential local economic impacts of conducting cash transfers with Conectados.

The cash transfers in PAC took place in 5 different waves. In each of the first 4 waves, 2,000 households received a transfer of about 55 USD (50,000 CLP). These transfers took place between September and December, 2024. In a final 5th wave, 2,571 households received the same amount in January 2025.¹⁷ Each household received only

¹⁶We used an unsupervised community detection algorithm (Blondel et al., 2008) to identify neighborhoods based only on the network formed by the transaction data coming from the digital wallet, without using any geographic location as input. The results were very similar to our initial neighborhood definitions: we identified 22 separate neighborhoods and the geographic distribution was largely consistent.

¹⁷We concluded the data collection for the experiment in December 2024. The remaining funds were distributed in January 2025, but we did not collect outcomes after that.

1 transfer, households had about two weeks to spend their proceeds, and unused funds expired after this period. Of the balances disbursed, 5.6% were forfeited.

We did not randomize the allocation of cash transfers at the household level. Instead, the municipality selected low-income households from the Social Registry of Homes (*Registro Social de Hogares*) for participation. The Social Registry is Chile’s unified system for targeting social programs, using a proxy means test to score households based on income, assets, and demographic characteristics.¹⁸ Thus, in the experiment we only conducted supply-side randomizations.

3.3 Implementation

Retailer Treatment In Chile, the Conectados application was publicly downloadable, meaning any retailer could potentially self-enroll to join the digital wallet ecosystem. As we were not allowed to fully restrict which retailers could participate, our treatment was an encouragement marketing intervention targeted at treatment retailers. Treated retailers were informed about Conectados by our field team and encouraged to register in the platform, while control retailers did not receive any information about the program.

Aside from being informed about the benefits of the digital wallet for their business, treated retailers also received promotional materials to display at their stores. Specifically, we distributed stickers and logos to treatment firms right after the baseline survey collection process (which we describe in the next subsection), once the interview was over. These materials were not shown in advance to avoid influencing their baseline responses.

Each treated business was provided with two large circular hanging logos and two smaller stickers for them to display prominently in their storefronts.¹⁹ We conducted posterior visits to verify the display of these materials. Examples of how they were displayed are shown in Appendix Figure 9. We did not provide any monetary incentives for businesses to join the platform, but we did offer technical assistance to help them register and use the digital wallet application.

Importantly, we did not disclose any information about the saturation of the treatment we were randomly inducing, i.e. treated retailers did not know what percentage of nearby stores were participating. We also did not guarantee any sales or revenue increases to the stores. We intended to measure “business-as-usual” responses to the cash transfer program, without priming firms with information about the demand shock or the competitive environment. The only benefit we communicated to them was the potential to attract more customers, as beneficiaries would be informed about the location of treatment stores where they could use their digital wallets.

¹⁸We do not have household-level data to characterize the recipients’ income distribution in PAC. However, the 2024 CASEN survey (similar to the US Current Population Survey, CPS) indicates that the median household income for the bottom tercile of the distribution was about 500,000 CLP. If the municipality targeted the lowest income households, we estimate the median transfer to be about 10% of the monthly household income.

¹⁹Logos were 40cm in diameter and the stickers 9cm in diameter each. See Appendix Figure 8 for an example of the promotional materials provided to treated stores.

Informing Beneficiaries One and a half months after visiting the retailers, we started the first cash transfer wave and loaded the beneficiaries’ wallets. Immediately after each cash transfer wave, households were informed via official SMS texts and WhatsApp messages from Conectados and the municipality about the location of treatment stores.²⁰

For this purpose, we created a simple web application where beneficiaries could navigate a map, locate the treatment stores, click to learn more about each store (e.g. name, address) and view travel times. The map was centered around each household’s location and zoomed in by default so users could easily see the nearest participating stores.²¹ The link to this application was shared with beneficiaries when they were informed about receiving the cash transfer and the URL was masked with Conectados’ official website. This ensured that recipients had guidance on where to redeem their benefits, fostering a direct connection between beneficiaries and local businesses in the treatment group. We tracked visits to this web application and clicks *within* the application (e.g. to view travel times) to measure engagement and search behavior. We provide more details about this and other data collection procedures below.

3.4 Data Collection

We use four sources of data, with the timeline in Figure 2. First, store surveys at baseline (July 2024, six weeks before the first wave) and endline (December 2024, at the end of the fourth wave) collected the owner’s characteristics, opening hours, employment, and a battery of business practices, including whether the store had checked competitors’ prices or products, raised its prices, run out of normally stocked products, or invested in advertising in the previous month.

Second, mystery shoppers purchased a one-kilogram bag of sugar of the leading national brand (Iansa) at every roster store at baseline (July) and at endline (December), recording the price paid for each purchase, and following a strict protocol designed to ensure consistency and to minimize detection. Sugar is a homogeneous staple carried by almost every store, so we use its price as a store-level price index.

Third, we obtained administrative transaction data from Conectados’ backend system, which records all transactions made through the digital wallet platform. This data includes beneficiary IDs, retailer IDs, transaction amounts and timestamps. This allows us to measure actual sales and transaction volumes via the wallet at each onboarded retailer over time.

Finally, we use administrative records collected at baseline by Conectados and the municipality. These records include the geocoded address of each recipient household and a roster with the location of each store, along with tags describing the product lines carried by each store.²² Our main descriptive facts are presented below.

²⁰A screenshot of the WhatsApp message sent to beneficiaries is shown in Figure 3.

²¹A screenshot of this map is shown in the Appendix Figure 10.

²²We also collected anonymous clickstream data from the map web-application used by beneficiaries to locate treatment stores. This auxiliary data includes timestamps of visits, IP addresses and retailers clicked on when users requested directions. We then connected the click stream data with the digital wallet’s backend data to correlate search behavior with real transactions. This descriptive analysis is not the main focus of our study, and we relegate it to Appendix 11.

3.5 Descriptive Statistics

Out of the 188 stores selected to participate in the program, 151 (80.32%) agreed to participate in the baseline survey, 78 treatment stores and 73 control stores. The response rate was similar across treatment and control groups, and there were no significant differences in refusal (or closure) rates between the two groups nor by the intensity of the intervention assigned at the market level. We report the differences in response rates in Appendix Table 25.

The endline mystery-shopper visits yielded a price at 140 stores²³, of which 137 also have a baseline price. Appendix Figure 12 traces the flow of stores through the samples.

Table 1 reports baseline balance by encouragement condition. About 70% of stores are owner-operated and 79% of them are located within the owner’s residency. The respondents were 42% male, and were on average 48 years old with about 31 years of work experience. Stores in PAC open for about 10.7 hours per day, except for Sundays, when they are open for about 8 hours. Stores have about 1.15 employees on average and about half of respondents know about digital wallet payments. Virtually everyone had a mobile phone with internet access, so they could use the digital wallet app.

Similarly, Table 2 shows the balance of store characteristics by the intensity of competition assigned at the neighborhood level. There are no significant differences in store characteristics between groups, neither when comparing individual treatment assignments or neighborhood competition levels.

Compliance When it comes to actual participation in the digital wallet platform, 68 out of the 94 treatment retailers (72.34%) successfully onboarded and began accepting payments from beneficiaries. In contrast, 30 out of the 94 control retailers (31.91%) independently registered in the platform, despite not being included in the marketing campaign or receiving any promotional materials.²⁴ This indicates a level of non-compliance in both the treatment and control groups, which we address in our empirical strategy.

Table 3 shows the (first-stage) results of the marketing campaign on onboarding rates, measured as whether a retailer registered in the digital wallet platform or not. The marketing campaign increased the probability of registering by over 40 percentage points, which is a substantial increase given that the baseline registration rate in the control group was only 31.91%. This indicates that our marketing intervention was effective in encouraging retailers to join the digital wallet ecosystem. We do not find any significant differences in compliance rates between high- and low-competition neighborhoods, suggesting there are no spillover effects in the adoption of the platform across retailers within the same neighborhood.

²³A store closed at the time of the visit or not carrying the product does not generate a price, but we attempted to recontact the store whenever possible.

²⁴Control retailers could have learned about the program through, for example, observing promotional materials at treated retailers, conversations with beneficiaries seeking to use transfers, or word-of-mouth from other retailers.

4 Experimental Results

4.1 Empirical Strategy

The second-stage lottery identifies the effect of the encouragement on a store’s outcomes, and the first-stage lottery how that effect varies with the share of the store’s neighbors that were encouraged. We estimate intent-to-treat (ITT) effects of the encouragement marketing campaign on business outcomes using the following regression model:

$$Y_j = \alpha + \beta T_j + \gamma X_j + \varepsilon_j,$$

where Y_j is the outcome of interest for store j (e.g. (log) prices, or different business practices), T_j is a treatment encouragement indicator for store j , X_j is a vector of store-level control characteristics (e.g. baseline characteristics, neighborhood fixed effects), and ε_j is an error term. As we used a two-stage randomization design, conditioning the store-level encouragement assignment to the neighborhood-level saturation of the encouragement, we cluster our standard errors at the neighborhood level (Abadie et al., 2023). The coefficient of interest β captures the ITT effect of being assigned to the marketing campaign.

To account for noncompliance, we estimate local average treatment effects (LATEs) using a two-stage least squares (2SLS) approach (Imbens and Angrist, 1994). In the first stage, we model participation in the digital wallet platform as a function of the encouragement treatment assignment:

$$D_j = \pi_0 + \pi_1 T_j + \nu_j,$$

where D_j is an indicator for whether store j registered in the digital wallet platform. The coefficient π_1 captures the effect of the marketing campaign on the likelihood of participation, which we already report in Table 3.

In the second stage, we estimate the effect of actual participation on business outcomes, using the predicted values of D_j from the first stage:

$$Y_j = \alpha + \beta D_j + \gamma X_j + \varepsilon_j,$$

where β now captures the LATE, i.e. the treatment effect, for the set of compliers, of participating in the digital wallet platform on outcome Y_j .

To estimate the impact of competition intensity, proxied by the saturation of the treatment, we first follow Crépon et al. (2013) and extend the above models by including an interaction term between the store-level treatment indicator and a neighborhood-level saturation intensity indicator. This allows us to assess whether the effects of the marketing campaign differ in high- versus low-saturation environments. Specifically, we estimate regression models of the form:

$$Y_j = \alpha + \beta_1 T_j + \beta_2 C_n + \beta_3 (T_j \times C_n) + \gamma X_j + \varepsilon_j,$$

where C_n is an indicator for whether neighborhood n (to which store j belongs) was assigned to a high-saturation environment. Then β_1 is the effect of the encouragement in low-saturation neighborhoods, $\beta_1 + \beta_3$ its effect in high-saturation ones, and β_2 the effect of a market’s saturation on its *non*-encouraged stores, which is informative

about spillovers. With neighborhood fixed effects β_2 is absorbed and the interaction is identified within the realized saturation of each neighborhood.

We similarly account for noncompliance in this heterogeneous effects framework by using a 2SLS approach, where we instrument D_j with T_j and $C_n \times D_j$ with $T_j \times C_n$. This allows us to estimate the LATE of participating in the digital wallet platform, conditional on the neighborhood-level saturation intensity.

The saturation arm measures competition coarsely: it counts every encouraged store in the neighborhood, whether or not it sells what store j sells. However, we expect that what should discipline a store’s price is the number of enrolled stores its customers regard as substitutes. Thus, as an additional measure of competitive intensity, we focus on the share of retailers that are *substitutes*. We define two retailers j, j' in the same market as substitutes if they carry the same product lines. For example, two greengrocers in the same neighborhood are substitutes, but a greengrocer is not a substitute for a butcher shop. When defining product lines, we use binary tags provided by the municipality indicating if a retailer is a greengrocer, a butcher shop, or a bakery.²⁵

For each retailer j , we define the share of encouraged substitutes as

$$Z_j = \sum_{j' \in T_{n(j)}} 1[j \text{ and } j' \text{ are substitutes}] / (N_{n(j)}^{enc} - T_j),$$

where $T_{n(j)}$ is the treatment set of encouraged retailers in neighborhood $n(j)$ and $N_{n(j)}^{enc}$ is the total number of encouraged retailers in the neighborhood.

However, Z_j is a nonlinear function of the random assignment and of a fixed network (which pairs of stores are close substitutes) so even though the assignment is random, a store’s *expected* share under the lottery is not. For instance, a store with many close substitutes in the baseline roster expects a high Z_j whatever the draw, and such stores may differ in their potential outcomes when compared to stores with fewer close substitutes. Following [Borusyak and Hull \(2023\)](#), we recenter this variable, i.e. for every design-dependent regressor we compute its expectation over the two-stage lottery by re-drawing both stages 2,000 times with the network held fixed, and use the realized value minus that expectation, $\tilde{Z}_j = Z_j - E[Z_j]$ and $\widetilde{T_j Z_j} = T_j Z_j - E[T_j Z_j]$. The recentered regressors are uncorrelated by construction with every fixed characteristic of the store, including its substitute density, so they identify the interaction without further controls.

The specification

$$Y_j = \alpha + \beta_1 T_j + \beta_2 \tilde{Z}_j + \beta_3 \widetilde{T_j Z_j} + \gamma' X_j + \varepsilon_j \quad (1)$$

then asks whether an encouraged store’s response is smaller when the stores that were encouraged alongside it are the ones it competes with, where now β_3 captures the average differential effect of encouraging a higher share of substitute retailers on the encouraged group.

Similarly, we define a measure of the share of *enrolled substitutes*, M_j , which is analogous to our measure Z_j , but using the set of enrolled retailers instead of encouraged

²⁵All retailers have a common tag of “minimarket” or “almacen”, which is a standard way of describing the mom-and-pops shops in our experiment. Thus, we do not consider these generic tags when defining substitutes.

retailers:

$$M_j = \sum_{j' \in D_{n(j)}} 1[j \text{ and } j' \text{ are substitutes}] / (N_{n(j)}^{enr} - D_j),$$

where $D_{n(j)}$ is the enrolled set of retailers in neighborhood $n(j)$ and $N_{n(j)}^{enr}$ is the total number of encouraged retailers in the neighborhood. Then, we estimate

$$Y_j = \alpha + \beta_1 D_j + \beta_2 M_j + \beta_3 (D_j \times M_j) + \gamma X_j + \varepsilon_j,$$

in a 2SLS framework, where we instrument D_j , M_j and $D_j M_j$ with T_j , $\tilde{Z}_j$ and $\widetilde{T_j Z_j}$. In this case, β_3 captures the average differential effect of enrolling a higher share of substitute retailers on the set of compliers. We control for neighborhood fixed effects in X_j and center M_j around its sample mean $\bar{M}$, so our results are again interpreted as deviations from the mean enrolled share of substitutes.

4.2 Results

4.2.1 Intent-to-Treat Effects

We first estimate the ITT effects of the marketing campaign on business outcomes. Even though there could be selection into the platform (firms that decide to enroll in the platform might be different from those off the platform), the ITT effects are still relevant for policy purposes, as they capture the average effect of being assigned to the treatment group, which is a controllable marketing campaign, regardless of compliance, which is not easily controlled by a policymaker.

Competitive Behavior Tables 4 and 5 present our main results on competitive monitoring behavior, a self-reported behavioral manifestation of market power. Table 4 examines whether retailers report checking competitors' prices in the past month. Columns (1) and (2) show pooled ITT estimates. These pooled treatment effects are small and insignificant, suggesting no average effect across both saturation regimes. However, this masks substantial heterogeneity by competitive intensity.

Column (3) includes the saturation dummy C_n and the interaction term $C_n \times T_j$. In low-saturation markets (when $C_n = 0$), treated retailers are 16.9 percentage points less likely to check competitors' prices relative to control retailers, a 100% reduction from the base mean of 16.9% ($\hat{\beta}_1 / \hat{\alpha} = 1$). This large negative effect suggests that when retailers face limited market saturation, they reduce competitive monitoring.

However, the interaction term coefficient is large and positive, indicating a differential increase of 28.4 percentage points. This implies that in high-competition markets, treated retailers are $-16.9 + 28.4 = 11.5$ percentage points more likely to check competitors' prices than control retailers in high-competition markets. This total effect in high-competition markets is significantly different from zero ($F = 5.02$, $p = 0.037$), which indicates a reversal in competitive monitoring in high-competition markets compared to low-competition markets.

Column (4) adds market fixed effects, which absorb the effect of competition intensity (β_2) and improve precision by accounting for market-specific factors. Results are nearly identical: $\hat{\beta}_1 = -17.9$ p.p. for the low-saturation treatment effect and $\hat{\beta}_3 = 28.0$ for the interaction.

Table 5 presents analogous results for product monitoring, i.e. whether retailers report checking competitors’ products (instead of prices) in the past month. Results mirror the price monitoring findings. There are no significant pooled effects, but in low-competition markets, treated retailers are 16.9 p.p. (100%) less likely to check competitors’ products.

In high-competition markets, this effect is offset by a 23.8 p.p. interaction term, yielding a total effect of approximately 7 p.p., although this joint effect is not significantly different from zero ($F = 2.14$, $p = 0.1597$). Column (4) again includes market fixed effects, with nearly identical results: $\hat{\beta}_1 = -19.2$ p.p. and $\hat{\beta}_3 = 22.6$ p.p.

These patterns provide evidence that our randomly induced saturation intensity moderates retailers’ self-reported competitive behavior in response to the cash transfer program. When few nearby retailers are encouraged, treated retailers face limited competitive pressure from other program participants. Their customer base of cash transfer recipients is effectively captive, as beneficiaries have few alternatives for redeeming their transfers. This market power reduces the need for competitive monitoring.

On the contrary, when most nearby retailers are encouraged, treated retailers compete for the same pool of beneficiaries. Even though beneficiaries can only use transfers at participating retailers, they have many potential retailers to choose from. This maintains competitive discipline, forcing treated retailers to monitor competitors. The reversal also validates the first-stage randomization: owners recognized and responded to the competitive environment the lottery assigned them, even though we never told them how many neighbors had been encouraged.

Other Business Practices The previous behavioral responses validate that our first-stage randomization successfully created variation in competitive pressure. Retailers recognize and respond to differences in competitive intensity. We now examine whether this translates into differences in other business practices, such as self-reported wages, sales goals, employment, opening hours, stockouts, advertising, and self-reported price increases.

We find no significant effects on self-wage nor on sales goals. In addition, we find no significant effects on employment outcomes²⁶ nor on daily opening hours in PAC. We also find no effects on stockouts (Table 6). Even though the experiment induced substantial demand for goods at participating stores, it could be that stores in PAC were prepared to handle the demand shock and might have easy access to upstream suppliers in case they need to quickly restock an item. PAC is located in central Santiago with nearby access to wholesale markets, so retailers may be able to rapidly restock items in high demand.²⁷

Table 7 examines whether retailers report investing in advertising in the past month. Without market fixed effects (Column 1), treated retailers are 11.7 p.p. more likely to report investing in advertising, although this effect is only marginally significant

²⁶We noticed many retailers in PAC reported an unusually high number of employees, including formal employees, such as 9 full-time formal employees. This is unlikely to be accurate in this context of small stores and may reflect reporting errors. There is a sharp drop in the number of retailers reporting having more than 3 employees. When truncating the data at 3 employees, our point estimates for the marketing campaign are positive (about 0.2 additional employees), but still non-significant.

²⁷In addition, our endline survey occurred after four transfer waves, so if stockouts were concentrated early in the program, we may have missed them.

($p = 0.072$). Moreover, this effect is not robust to including neighborhood fixed effects (Column 2). We find no significant interaction with competition intensity either. Interpreting these results is complicated because our treatment included providing promotional materials (stickers, logos). Retailers may interpret “investing in advertising” as including these materials even though they received them for free. For example, if investing in advertising is another measure of competitive behavior (which we expect to decrease for treated retailers in low-competition environments), then a negative effect in out-of-pocket advertising could be counteracted by positive reports regarding advertising efforts due to a reflection of the marketing treatment itself. Overall, the evidence on advertising is inconclusive.

Taken together, these results suggest that the marketing campaign did not significantly affect most business practices, except for competitive monitoring behavior. This is consistent with the idea that the cash transfer program created a demand shock, but retailers’ responses were primarily driven by their perception of competitive intensity rather than changes in other business practices, such as increasing their capacity or adjusting their opening hours. The lack of significant effects on employment, stock-outs, and advertising suggests that retailers may have been able to accommodate the increased demand without making substantial changes to their operations.

As a final self-reported outcome we asked storeowners in PAC to report whether they had increased prices in the last month (in addition to sending secret shoppers). Table 8 reports treatment effects on this outcome. We find that in low-competition markets, treated retailers are about 23 p.p. (43%) more likely to report increasing prices relative to control retailers. This large effect confirms that retailers in low-competition markets consciously adjusted their pricing strategies in response to the demand shock.

The interaction term is negative, and about the same size, but not statistically significant ($p = 0.136$). The implied total effect in high-competition markets is close to zero and statistically insignificant. This pattern suggests that competitive intensity may have constrained retailers’ ability or willingness to raise prices. Given this self-reported evidence, we now analyze the secret shopper data to objectively measure price changes in the field.

Price Effects We analyze the price data collected by secret shoppers to objectively measure the impact of the encouragement treatment on prices. Table 9 presents the simplest specification without distinguishing saturation intensity. In the preferred specifications with fixed effects (Columns 2 and 4), treated retailers charged 60 CLP (3.76%) higher prices for sugar than control retailers. Moreover, the results are robust to various specifications, yielding similar results when controlling for baseline prices, using a difference-in-differences approach, or changing the outcome variable to measure price changes instead of levels. We report the results of these additional specifications in the Appendix (Tables 26, 27 and 28).

Table 10 examines whether price effects differ by saturation intensity. Again, the main effect of the marketing campaign in low-competition markets β_1 is positive and significant: treated retailers charge about 3.5% higher prices than control retailers. This confirms that in low-competition environments, treated retailers raised prices in response to the demand shock induced by cash transfer recipients.

Our estimates for the saturation effects are not statistically significant, suggesting that prices did not differ systematically by market saturation level. Our results are

robust to different specifications, such as changing the outcome variable to measure price changes instead of levels (Appendix Table 29), where we similarly find a 4% difference in the rate of price increases between treated and control retailers in low-competition markets, but no significant effects for treated retailers in high-competition markets.²⁸ Unlike monitoring, the price response of encouraged stores relative to the control stores of their own neighborhood does not vary with the neighborhood’s saturation.

However, when measuring competition based on the fraction of *substitute* retailers randomly encouraged, our results differ. Table 11 re-examines whether price effects differ by competitive intensity, now based on our measure of encouraged substitutes Z_j . Just as before, the main effect of the marketing campaign in low-competition markets β_1 is positive and significant, indicating that treated retailers charge about 4.7% higher prices than control retailers in the presence of an average fraction of close substitute retailers.

When examining the effects of competition based on encouraging more close substitutes, our results show that the aforementioned price effects are attenuated by a higher share of encouraged substitute retailers. Specifically, a 10pp increase in the share of substitutes reduces prices by 1.7%. This negative effect is statistically significant and sizeable: our estimates suggest that an increase of under 30pp in the share of substitute retailers is enough to fully counteract the price increase caused by our encouragement.

These results are consistent with the self-reported evidence in Table 8. In low-competition markets, treated retailers raised prices in response to the cash transfer program, but high-competition markets disciplined this behavior.

Spillovers and Interference The store-level comparisons assume that a store’s outcome depends only on its own encouragement, implied by the stable-unit-treatment-value assumption, SUTVA (Imbens and Rubin, 2015). Our two-stage design is built to test this based on the saturation induced in the first stage (Hudgens and Halloran, 2008). Indeed, the presence of more encouraged substitutes do not significantly affect the pricing behavior of control retailers, as seen in β_2 reported in Table 8. The same pattern is observed in Table 10, where β_2 is insignificant. This suggests that competitive spillovers operate within the treatment group, rather than across treatment and control units.

Moreover, in all specifications, for all outcomes, the coefficient of the saturation of encouraged substitutes is not statistically significant. This indicates that the market-level effects of the program do not vary systematically with the level of saturation induced. In other words, while competitive intensity moderates the behavior of treated retailers, it does not appear to have a significant spillover effect on control retailers. The effects of the program are thus confined to treatment stores, and the ITT estimates are not contaminated by responses of the control group. We report qualitatively equivalent results for the 2SLS estimates in the next subsection, which, modulo one marginally significant coefficient, also show no significant spillovers on control retailers.

²⁸Without controlling for any baseline price levels, our results are imprecisely estimated and statistically insignificant. We report these results in Appendix Table 30.

4.2.2 2SLS Effects

Compliance with the marketing campaign was imperfect, with 72.3% of treated retailers registering on the digital wallet platform and 31.9% of control retailers self-enrolling (Table 3). To account for this noncompliance, we estimate LATEs using the two-stage least squares (2SLS) approach described above.

Table 12 presents LATE estimates for the price effects of actually registering on the digital wallet platform. The results are qualitatively similar to the ITT estimates, but significantly larger in magnitude. In specifications with market fixed effects (Columns 2 and 4), complier retailers that registered on the platform charged about 145 CLP (9.1%) higher prices than those that did not register. This is over twice the ITT effect, which is consistent with the ~ 40 p.p. increase in takeup caused by the marketing campaign.

Analogously, we estimate LATEs for all the business practice outcomes we discussed above. Tables 15 and 16 present LATE estimates for competitive monitoring behavior. The results are again qualitatively similar to the ITT estimates, but larger in magnitude. In low-competition markets, retailers that registered on the platform are about 35 p.p. less likely to check competitors' prices or products relative to off-platform retailers. However, in high-competition markets, this effect is offset in each case by a large positive interaction term, yielding an increase in price monitoring of about 28.8 p.p. relative to off-platform retailers in high-competition neighborhoods ($F = 4.73$, $p = 0.043$), and to no statistically significant changes in product monitoring against the same comparison group.

Onboarded retailers are also 46-49 p.p. more likely to report increasing prices in low-competition markets (Table 19), but this effect is again offset by a negative interaction term, yielding no statistically significant total effect in high-competition markets ($F = 0.02$, $p = 0.898$). As before, we find no significant effects on stockouts, and only marginally significant LATEs ($p = 0.093$) on advertising (Tables 17 and 18).

Like before, our price LATE estimates are robust to various specifications, like controlling for baseline prices or changing the outcome variable to measure price changes instead of levels. We report the results of these additional specifications in the Appendix (Tables 31 and 32).

Table 13 examines whether LATEs on prices differ by competitive intensity. The main effect of registering on the platform in low-competition markets is positive and significant: retailers who registered on the platform charge about 8.7% higher prices than non-registered retailers. This confirms that in low-competition environments, retailers that joined the digital wallet platform raised prices in response to the demand shock induced by cash transfer recipients.

Again, our estimates for the interaction effects based on market saturation are not statistically significant. As before, these results are robust to different specifications, such as changing the outcome variable to measure price changes instead of levels (Appendix Table 33).

When studying the effects of higher competition among substitute retailers, the results in Table 14 mimic those in Table 11, but with higher magnitudes. A 10pp increase in the fraction of close competitors reduces prices by 3.4%. In the extreme case where an average enrolled firm faces no substitutes, its prices are expected to be about 13% higher, having 1/3 of its substitutes enrolled would imply a non-significant

increase of only 1.5%.

Taken together, our results suggests that even in high-saturation markets, retailers that registered on the platform still raised prices, although to a lesser extent in cases where they competed against close substitutes. Thus, competition among substitutes moderated price increases and market structure matters. This is relevant for policy-makers designing cash transfer programs, as they should consider competitive effects when choosing redemption restrictions. The qualitative trade-off between supporting local retailers (via restrictions) and maintaining competition (via broad participation) is a key aspect we now study through the lens of a structural model.

5 A Structural Model of Demand and Competition Under Restricted Redemption

Our previous analyses establish that competitive intensity causally moderates retailer responses to cash transfer programs, but it leaves some design questions unanswered. How much beneficiary welfare is lost when enrolled stores reprice? Who bears that loss? And what would prices and welfare look like under program designs we did not implement?

Answering these questions requires a model of how beneficiaries choose among stores and how stores price against that demand. It also requires considering the equilibrium nature of the setting: treatment and control stores who joined the platform compete for the same captive beneficiaries within a market, and this competition might be more relevant across close substitutes. A structural model can internalize cross-store substitution and produce counterfactual scenarios (as in Section 7.3) describing the kind of general-equilibrium objects a program designer could be interested in.

This section develops such a model. We model the demand for enrolled stores as a random-coefficients discrete-choice problem (Berry et al., 1995; Nevo, 2001; Berry et al., 2004) in which households differ in their price sensitivity and in their valuation of store proximity, and we model the supply-side behavior with Nash-Bertrand price competition among single-product retailers. We estimate the model on the universe of digital-wallet transactions in PAC, matched to mystery-shopper prices, household geolocations, and census-block socioeconomic data.

Three features of the environment discipline our choices. First, the wallet is *captive*: balances must be spent within the enrolled network before they expire, so the relevant outside option is thin and the relevant competition is among enrolled neighbors. Second, the setting is spatial: stores are differentiated by where they sit relative to each household (Davis, 2006; Houde, 2012), and travel costs sustain fragmented neighborhood markets (Ramos-Menchelli and Sverdlin-Lisker, 2022). Third, the experiment itself generates the identifying variation: randomized onboarding, randomized saturation, and the quasi-random geometry of which households ended up near which enrolled stores.

5.1 Environment and Data Construction

Markets and choice sets. A *market* is a neighborhood-wave cell $t = (n, w)$, with $n = 1, \dots, 20$ neighborhoods and $w = 1, \dots, 5$ transfer waves, giving $T = 100$ markets.

Each market contains the J_t enrolled stores with valid price observations (we have $N = 426$ valid store-market observations on 89 distinct stores). Beneficiaries choose which enrolled store to patronize, and the outside option $j = 0$ is leaving wallet balances unspent.

Market shares. Shares are constructed from the wallet’s administrative transaction records: s_{jt} is store j ’s share of total wallet disbursements in market t . The outside share, i.e. forfeited wallet balances, averages $\bar{s}_0 = 5.6\%$, measured directly from the platform’s expiration logs.

Prices. Store-level prices come from the mystery-shopper purchases of sugar, collected in two rounds. We assign each transfer wave the price from a collection round: waves 1-3 carry the round-1 price and waves 4-5 the round-2 price.²⁹

Microdata. The wallet’s transaction ledger records all 21,142 purchases made by the redeeming beneficiary households over the five waves, with the buyer’s residential address and the store’s identity on every record. We collapse the ledger to household-level choices: each household’s *first choice* is the store receiving the largest share of its funds. We observe 74.1% of the households choosing only 1 retailer to make purchases (i.e. 3/4 of beneficiaries spend their entire wallet at their first choice, as seen in Appendix Figure 16), so about 1/4 of beneficiaries have a valid *second choice*. For households transacting at two or more stores in their market, the runner-up store by funds is the second choice. Households shopping in more than one neighborhood (16.6%) are assigned the neighborhood where they spent the most, so both choices always belong to the same market. The distribution of transaction amounts is highly skewed, with a majority of transactions being exactly the full amount of 50,000 CLP (Appendix Figure 15).

Requiring the chosen retailer to have a valid price (i.e. to carry the standardized sugar bag) yields 5,734 first-choice records and 518 first-second choice pairs for the micro-moments. For each record we observe the buyer’s geolocation, the chosen store(s), the full set of enrolled alternatives in the buyer’s market, the distance from the household to every alternative, and the household’s census-block poverty index (defined as the fraction of households in the bottom 2 quintiles of the country’s income distribution). All spending excluded from the choice records remains in the aggregate share data. These records generate the micro-moments of Section 6.

5.2 Household Demand

We model household demand for retailers based on a simple discrete choice problem because, as indicated above, three quarters of beneficiaries transact at a single store and the average number of distinct stores visited is below two. We assume that each household i in market t chooses the store $j \in \mathcal{J}_t$ that maximizes its indirect utility u_{ijt} , which depends on the store’s price, its accessibility, and its distance from the household.

²⁹This step assignment is due to the fact that we observe two price measurements, not five. We assigned waves 1-3 to the first round of prices and waves 4-5 to the second round to balance the number of observations assigned to each price-collection round. A linear-interpolation alternative delivers similar estimates. We implicitly assume that sugar serves as the store-level price index.

Specifically, household i 's indirect utility from store j in market t is

$$u_{ijt} = \alpha_i p_{jt} + \beta_i c_{jt} + \gamma d_{ij} + \xi_{jt} + \varepsilon_{ijt}, \quad (2)$$

where p_{jt} is the price, $c_{jt} \in \{0, 1\}$ indicates whether j is the most accessible store in its market (the one with the shortest average distance to a beneficiary in that neighborhood-wave), d_{ij} is the physical distance from household i to store j (so $\gamma < 0$ is a travel cost) and ε_{ijt} is i.i.d. Type-1 extreme value. We decompose unobserved store quality as $\xi_{jt} = \zeta_j + \zeta_t + \Delta\xi_{jt}$, absorbing both store fixed effects ζ_j and neighborhood-wave (market) fixed effects ζ_t .

The household-specific random coefficients are

$$\begin{aligned} \alpha_i &= \alpha^p + \pi_{p,d} d_i + \pi_{p,s} s_i + \sigma_p \nu_i^p, \\ \beta_i &= \beta^c + \pi_{c,d} d_i + \pi_{c,s} s_i + \sigma_c \nu_i^c, \end{aligned} \quad (3)$$

where d_i is household i 's isolation (the minimum distance to any enrolled store), s_i is its census-block deprivation (the share of households in household i 's census block in the bottom 2 quintiles of the income distribution) and $\nu_i^p, \nu_i^c \sim \mathcal{N}(0, 1)$. Equation (3) contains two heterogeneity components: the systematic components load on two observables and the idiosyncratic components capture dispersion beyond observables. This allows households to differ in their price sensitivity and in their attachment to the most accessible store in the market idiosyncratically (σ_p and σ_c), through isolation ($\pi_{p,d}$ and $\pi_{c,d}$) and through deprivation ($\pi_{p,s}$ and $\pi_{c,s}$).

Integrating over the T1EV shock, market shares are

$$s_{jt} = \int \frac{\exp(\delta_{jt} + \mu_{ijt})}{1 + \sum_{k \in \mathcal{J}_t} \exp(\delta_{kt} + \mu_{ikt})} dF(\nu_i, d_i, s_i), \quad (4)$$

where $\delta_{jt} = \alpha^p p_{jt} + \beta^c c_{jt} + \zeta_j + \zeta_t + \Delta\xi_{jt}$ is the mean utility component and $\mu_{ijt} = (\pi_{p,d} d_i + \pi_{p,s} s_i + \sigma_p \nu_i^p) p_{jt} + (\pi_{c,d} d_i + \pi_{c,s} s_i + \sigma_c \nu_i^c) c_{jt} + \gamma d_{ij}$ collects the household-specific components. The integral is simulated with 100 agents per market drawn from the empirical distribution of household locations, so the model's heterogeneity and the travel distances d_{ij} that enter every agent's utility are anchored to the actual geography of beneficiaries rather than to a parametric assumption.

5.3 Supply

We treat the supply side as a full structural object and estimate it *jointly* with demand. Each retailer is a single-product price setter playing a static³⁰ Nash-Bertrand game: store j in market t chooses p_{jt} to maximize variable profit $(p_{jt} - mc_{jt}) M_t s_{jt}(\mathbf{p}_t)$, taking rivals' prices as given, where M_t is the market size (which we can drop w.l.o.g.). The first-order condition is the Lerner rule

$$p_{jt} - mc_{jt} = - \frac{s_{jt}}{\partial s_{jt} / \partial p_{jt}}, \quad L_{jt} \equiv \frac{p_{jt} - mc_{jt}}{p_{jt}} = - \frac{1}{\varepsilon_{jjt}}, \quad (5)$$

³⁰We abstract away from dynamic considerations. As the experimental evidence suggests, retailers did not seem to adjust their capacity in response to changes in the market, but they did adjust their prices and react to competition, which are the two central aspects of this standard model.

in which the own-price semi-elasticity aggregates over households,

$$\frac{\partial s_{jt}}{\partial p_{jt}} = \int \alpha_i s_{ijt} (1 - s_{ijt}) dF(\nu_i, d_i, s_i), \quad (6)$$

so the equilibrium markup $\eta_{ij}(\theta) \equiv p_{jt} - mc_{jt}$ is a function of the demand parameters $\theta = (\alpha^p, \sigma_p, \pi_{p,d}, \pi_{p,s}, \beta^c, \sigma_c, \pi_{c,d}, \pi_{c,s}, \gamma)$ and the fitted shares.

We assume an explicit cost model. Marginal cost is log-linear in observed variables,

$$\log mc_{jt} = w'_{jt} \gamma_c + \omega_{jt}, \quad (7)$$

where $w_{jt} = [1, \text{Minimarket}_j, \text{Meat}_j, \text{Veggie}_j, \text{distance-to-wholesale}_j, \text{CPI}_w]$. The distance to the nearest wholesaler proxies restocking costs, the store-type indicators capture product-mix cost differences, CPI_w is the Chilean monthly consumer price index for each wave's calendar month and ω_{jt} is an unobserved cost shock. Combining (5) and (7), the supply unobservable is

$$\omega_{jt}(\theta, \gamma_c) = \log(p_{jt} - \eta_{jt}(\theta)) - w'_{jt} \gamma_c, \quad (8)$$

and the supply side contributes the moment condition $\mathbb{E}[Z_{jt}^S \omega_{jt}] = 0$, with the excluded supply instruments being the store-level randomized encouragement treatment T_j and the market-level randomized enrollment saturation λ_n . Neither are related to a store's cost shocks, so $\mathbb{E}[T_j \omega_{jt}] = \mathbb{E}[\lambda_n \omega_{jt}] = 0$ hold by design.

6 Identification and Estimation

Two classic endogeneity problems arise. First, prices are correlated with unobserved quality: better stores charge more and sell more, biasing naive estimates of α^p toward zero. Second, the heterogeneity parameters $(\sigma_p, \pi_{p,d}, \pi_{p,s}, \sigma_c, \pi_{c,d}, \pi_{c,s})$ and the travel cost γ are weakly identified by aggregate shares alone.

6.1 Identifying the Price Coefficient

Aside from relying on two sets of fixed effects on the demand specification to reduce the standard endogeneity concerns, we use two types of instruments to identify the price coefficient. First, on the demand side, we use a “donut” k-means instrument averaging the prices of the three nearest priced stores located outside each store's own neighborhood, along with the single nearest outside store's price as another instrument. Neighboring stores can face common wholesale costs, so their prices are informative about a store's own costs. However, a neighbor store in a different market is assumed to not enter the household's choice set, so its idiosyncratic quality cannot load on $\Delta \xi_{jt}$.

In addition, we include supply-side instruments using the random assignment of the treatment and competition-intensity conditions. The Nash-Bertrand first-order condition (5) ties each store's markup to the demand elasticity, which depends on α^p . The randomized program, which cannot shift a store's marginal cost but does shift its demand³¹ and hence its optimal markup, supplies excluded instruments. We use

³¹Recall that households were given a map with the stores that were randomly encouraged. Store lookup within that map correlates with sale volumes (Appendix Figure 11).

both the store-level onboarding treatment T_j and the neighborhood-level enrollment saturation λ_n . The supply moments $\mathbb{E}[T_j \omega_{jt}] = \mathbb{E}[\lambda_n \omega_{jt}] = 0$ require that the part of (log) price not explained by cost shifters and the model's markup be uncorrelated with the randomized variation. Matching these supply-side moments pins α^p through the markup FOC.

We assume no endogenous residential sorting, so the physical distances between beneficiaries and stores are treated as pre-determined geography. However, the realized distance to the chosen store is a selection outcome. Thus, we include a spatial instrument defined as the average distance between a store and the households in the market. We also discipline the travel-cost coefficient γ with a distance micromoment: the average distance households to their (first) chosen stores (this distribution is shown in Appendix Figure 14). We discuss this micromoment and the other micro-moments below.

6.2 Micro-Moments

The heterogeneity parameters are identified by 13 micro-moments: 5 from first choices, 7 from second choices and one travel-cost moment. Each micro-moment is matched to its model analog within the GMM system (Petrin, 2002). Repeated choices against a common price vector are informative about preference dispersion even without price variation over time (Berry et al., 2004), as a household with a strong (weak) price-sensitivity draw sorts both of its choices toward cheap (expensive) stores, so the joint distribution of first- and second-choice characteristics identifies the variance structure that single choices cannot.

Let $\tilde{p}_{jt} = p_{jt} - \bar{p}_t$ denote prices demeaned within market. From first choices (5,734 records) we include $\text{Cov}(d_i, \tilde{p}_{j(1)})$, $\text{Cov}(s_i, \tilde{p}_{j(1)})$, $\mathbb{E}[c_{j(1)}]$, $\text{Cov}(d_i, c_{j(1)})$ and $\text{Cov}(s_i, c_{j(1)})$.

From second choices (518 pairs), conditional on dropping the first choice, we include $\mathbb{E}[\tilde{p}_{j(1)} \tilde{p}_{j(2)}]$, $\mathbb{E}[\tilde{p}_{j(2)}]$, $\mathbb{E}[c_{j(2)}]$, $\text{Cov}(d_i, \tilde{p}_{j(2)})$, $\text{Cov}(s_i, \tilde{p}_{j(2)})$, $\text{Cov}(d_i, c_{j(2)})$ and $\text{Cov}(s_i, c_{j(2)})$.

Finally, we include the travel-cost micromoment $\mathbb{E}[d_{ij(1)}]$, the average distance from a household to its first-choice store. As households are likely to shop much nearer than a randomly drawn enrolled store, the gap pins down γ .

6.3 Estimation

We estimate the demand and supply systems jointly by two-step efficient GMM, stacking three blocks of moments: the aggregate demand moments $\mathbb{E}[Z^{D'} \Delta \xi] = 0$ (the two spatial price instruments), the 13 micro-moments of Section 6.2, and the supply moments $\mathbb{E}[Z^{S'} \omega] = 0$, whose excluded instruments are the randomized store-level treatment T_j and the randomized neighborhood saturation λ_n . We then transform the instruments in the moment conditions to obtain feasible optimal instruments (Chamberlain, 1987) and report standard errors based on these efficient instruments.

7 Structural Results

7.1 Demand

Table 20 reports the demand estimates. The OLS logit price coefficient is -1.8 , but instrumenting raises it more than sevenfold. This is a common characteristic of situations with price-quality endogeneity in store choice: better stores charge more, so OLS confounds quality with price and biases the price coefficient toward zero. The IV-logit estimate, without random coefficients nor a supply-side estimation, yields $\hat{\alpha}^p = -14.4$, which is very close to our estimate under the full random-coefficients model with supply-side moments, $\hat{\alpha}^p = -13.7$. The latter is the preferred estimate because it accounts for both endogeneity and heterogeneity, and it is the one we use to compute welfare effects.

The estimated structural travel cost is $\hat{\gamma} = -5.26/\text{km}$. Relative to the mean price coefficient, one kilometer of distance is worth about 261 CLP, i.e. close to a sixth of the mean store price, so proximity is a relevant determinant of store choice in these neighborhoods.

The two components of the price random coefficient yield relevant heterogeneity across households. The systematic isolation component is $\hat{\pi}_{p,d} = -3.92$: a household one standard deviation (0.53 km) more isolated than average is about 15% more price-sensitive. Intuitively, distance from the enrolled network could be a proxy for peripheral, lower-income locations with worse options, which could be the households for whom the transferred funds matters most. The deprivation component also suggests that households in poorer areas are more price sensitive, but this result is not statistically significant. The idiosyncratic component is $\hat{\sigma}_p = 3.26$, i.e. dispersion is equal to roughly a quarter of the mean coefficient, even conditional on location and poverty rates. Together they imply a cross-household standard deviation of α_i of 3.8 (28% of the mean). We plot the distribution of recovered price sensitivities in Figure 4.

The implied own-price elasticities average -13.9 . This is far more elastic than most retail food demand estimates. Moreover, the outside option is very limited in this setting: within a captive wallet, the relevant margin is store-versus-store substitution among close substitutes, with forfeiture (the outside option) accounting for only 5.6% of balances. Thus, these estimates suggest a high within-network elasticity.

7.2 Supply

Estimating the Nash-Bertrand supply side jointly with demand yields the markups in Table 21. The mean Lerner index is 12.7%, exceeding the representative-firm benchmark $1/|\varepsilon| \approx 7.2\%$ because markups and shares are positively correlated across heterogeneous stores, i.e. a minority of high-share stores carry the largest markups.

Our estimated markups line up with the experimental variation exactly as the reduced form suggests: stores in high-competition neighborhoods price closer to cost (Lerner 0.07-0.12) than stores in low-saturation neighborhoods (0.14-0.16), and their demand is correspondingly more elastic (-15 to -17 versus -12 to -13).

Marginal costs. Because we estimate the supply side rather than only inverting it, the model also delivers a structural decomposition of marginal cost (Table 22). Marginal costs rise with a store’s distance to the nearest supermarket (+0.34 in logs

per kilometer), i.e. stores farther from wholesale supply face higher restocking costs. In addition, specialized sellers have lower marginal costs (-0.39 for greengrocers and -0.55 for meat stores). The cost trend is the Chilean monthly CPI, linked to each wave’s calendar month, which yields a positive coefficient ($+0.12$) but it is not statistically significant. The estimated cost model is relevant for identification, as it is the channel through which the randomized onboarding treatment and the randomized neighborhood saturation, both orthogonal to these costs, pin down the price coefficient.

The markup-competition relationship reflects the randomized design. Regressing store-level Lerner indices on the randomly assigned enrollment intensity (Table 23) yields $\hat{\beta}_\lambda = -0.059$ (marginally significant). Each additional 10 percentage points of neighborhood enrollment lowers markups by about 0.6 percentage points (about 5% of the representative firm’s markup). This is the supply-side mirror of the reduced-form results, as saturation compresses the margins the model recovers from prices and shares alone. Even though the magnitude of the slope is small, our estimates suggest that stores already operate on thin margins, so there is limited scope for more competition to reduce prices. However, the opposite is not necessarily true, i.e. lower competition could in principle increase markups by a sizeable amount, which we show in our counterfactual exercises.

7.3 Counterfactuals

The estimated model allows us to simulate counterfactual scenarios that were not implemented in the experiment. We focus on changing the coverage of the enrolled network, i.e. the fraction of stores in a neighborhood that are enrolled in the digital wallet platform. We vary the fraction λ of each market’s stores that is enrolled, and re-solve for Nash-Bertrand equilibrium prices when beneficiaries can redeem only at the enrolled subset. For each λ we enroll a random fraction of each market’s priced stores, hold marginal costs at their recovered values, re-derive each market’s most-accessible-store indicator within the enrolled subset, and average over random enrollment draws.

Figure 5 and Table 24 report the results, and they quantify a central program design message. Restricting redemption to a quarter of each market’s stores raises equilibrium prices 23.7% above the full-coverage benchmark and more than doubles Lerner indices (0.30 against 0.13). Moreover, because utility considers γd_{ij} , part of the surplus loss at low coverage is the travel disutility of a thinner enrolled network rather than price alone. This effect is not considered in the price-competition trade-off, but it is a relevant welfare consideration for program designers.

Thus, we estimate the change in consumer welfare (ΔCS) following Small and Rosen (1981). The implied beneficiary surplus loss is about 15,704 CLP per wallet, i.e. roughly a third of the transfer. The loss shrinks rapidly as coverage expands: at $\lambda = 0.6$ the price increase is under 3% and the surplus loss about 6% of the transfer, and beyond $\lambda \approx 0.75$ the gains are essentially exhausted.

Because enrolling an additional neighborhood store costs the program almost nothing, the model implies that coverage is a cost-effective competition policy available to a restricted-redemption program like this one. Most of the market power problem we document is a consequence of partial enrollment/competition in the market, not of the digital-wallet design itself.

8 Conclusion

This paper provides experimental and structural evidence on how local market structure mediates the impact of cash transfers on prices, firm behavior and household welfare. Using a two-stage RCT in Chile, we demonstrate that restricted-redemption transfer programs can generate substantial price increases when competitive intensity is limited, and we quantify such price increases experimentally and through a model.

Our analysis yields five key findings. First, retailers respond strategically to demand shocks from cash transfer programs by adjusting prices and competitive monitoring. These responses indicate that program restrictions create market power for participating retailers, and thus the effects of cash transfers should not be evaluated in isolation from supply-side dynamics.

Second, competitive intensity causally moderates these responses. Treated retailers in low-saturation markets reduced competitive monitoring by 17 percentage points (LATE = 35 p.p.) and increased prices by 4% (LATE=9%), while those in high-saturation markets increased competitive monitoring in prices by almost 12 percentage points (LATE=29 p.p.). Moreover, the price effects are smaller in markets where retailers compete against more close substitutes, falling at a rate of about 3.4% per additional 10pp in the share of enrolled substitute retailers. This experimental variation in competition provides direct evidence that market structure shapes the general equilibrium effects of transfer programs.

Third, we estimate a structural model that leverages the experimental design. We estimate demand parameters that imply that beneficiaries are highly price sensitive, care substantially about proximity and have very heterogeneous preferences. Firm markups average about 13% of price, and increasing enrollment intensity further compresses these markups.

Fourth, while some previous studies have found minimal or modest price effects, we demonstrate that these effects depend critically on market structure and program design. Price effects are large when redemption is restricted and competition limited, but small or absent when markets are competitive or redemption unrestricted. This reconciles seemingly contradictory findings in the literature, and it sharpens the trade-off policymakers face between local economic development objectives (supporting small businesses through restricted redemption) and program efficiency (maximizing beneficiary welfare through competitive markets). Our estimates suggest that enrolling a quarter of each market's stores would raise prices about 23% above the full-coverage benchmark and cost beneficiaries roughly a third of the transfer. However, the benefits of competition are mostly exhausted at 75% coverage, where price increases are under 3%. Broad participation is thus a cheap and administratively simple lever that preserves most of the local development rationale while disciplining prices.

Fifth, we show how the Conectados platform itself represents a promising model for delivering social assistance via digital wallets linked to identification documents. Such platforms offer benefits in targeting, administrative cost, and data transparency, but they create new channels for market power precisely the redemption restrictions that make them attractive. As developing countries continue expanding social protection systems, and as digital platforms increasingly mediate these systems, attention to market structure and competitive effects will become more important. Our findings hope to inform the design of more effective, more equitable transfer programs.

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9 Figures and Tables

9.1 Figures

Figure 1: Map of PAC

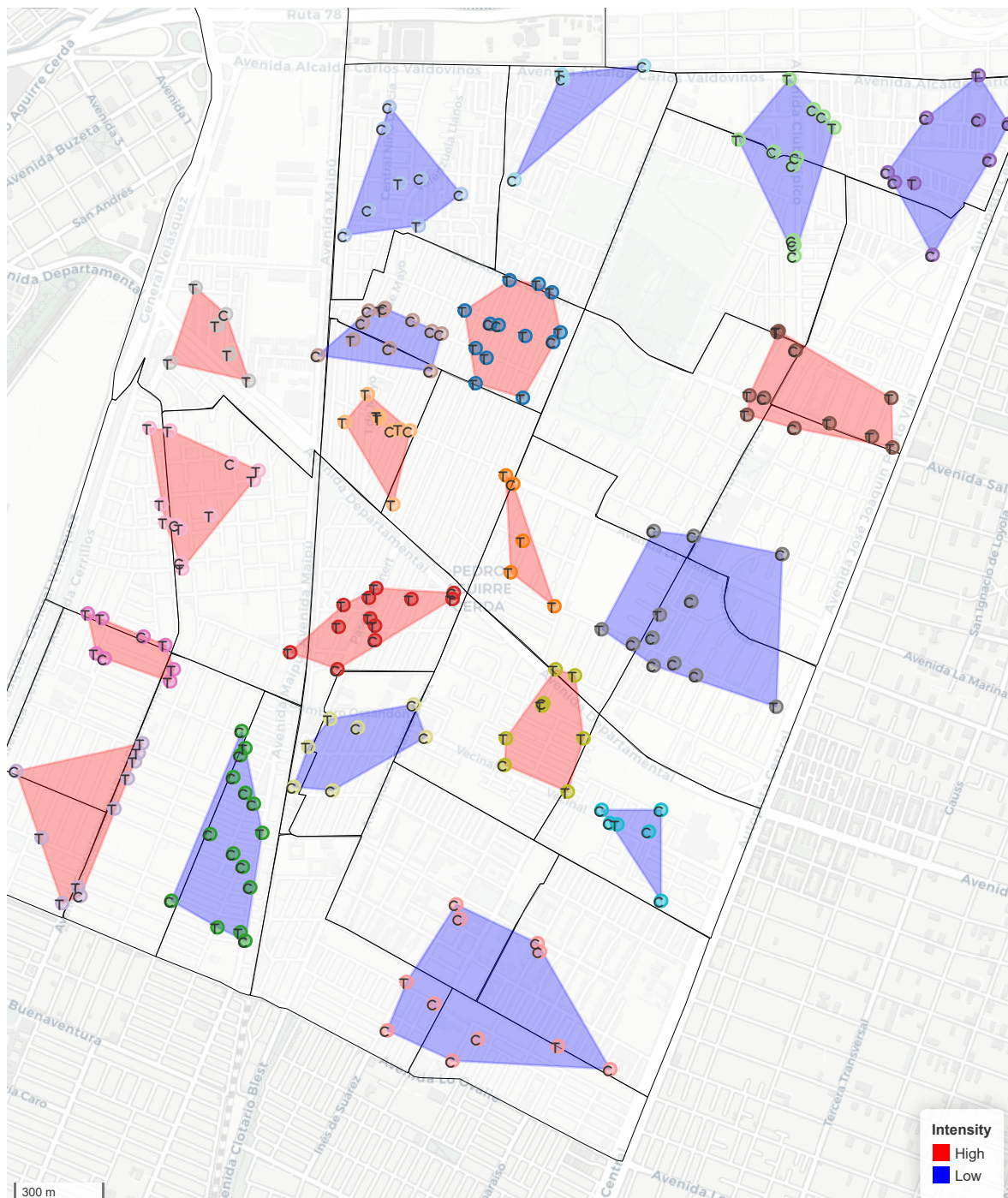


Figure 2: Timeline of the Experiment

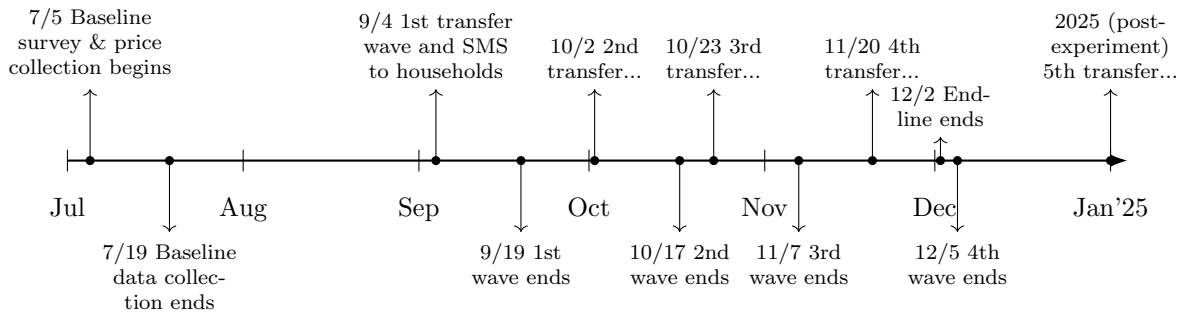
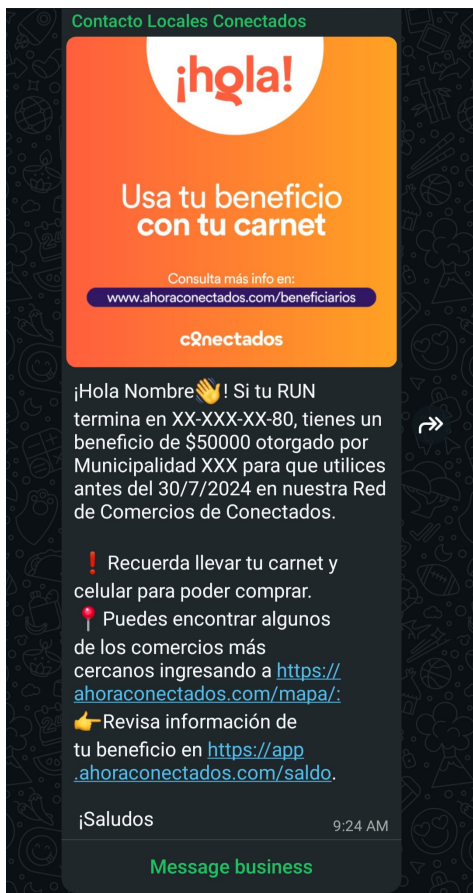


Figure 3: WhatsApp Message to Beneficiaries



→ Official Business account

→ Same graphics as stickers and logos for easy recognition

→ “Use your transfer with your ID”

→ “Get more information here [URL]”

→ “Hi [NAME]! If your ID number ends in [ID], you have a cash transfer of CLP 50,000 given by the Municipality of [PAC] to use before [DATE] in local businesses with Conectados.”

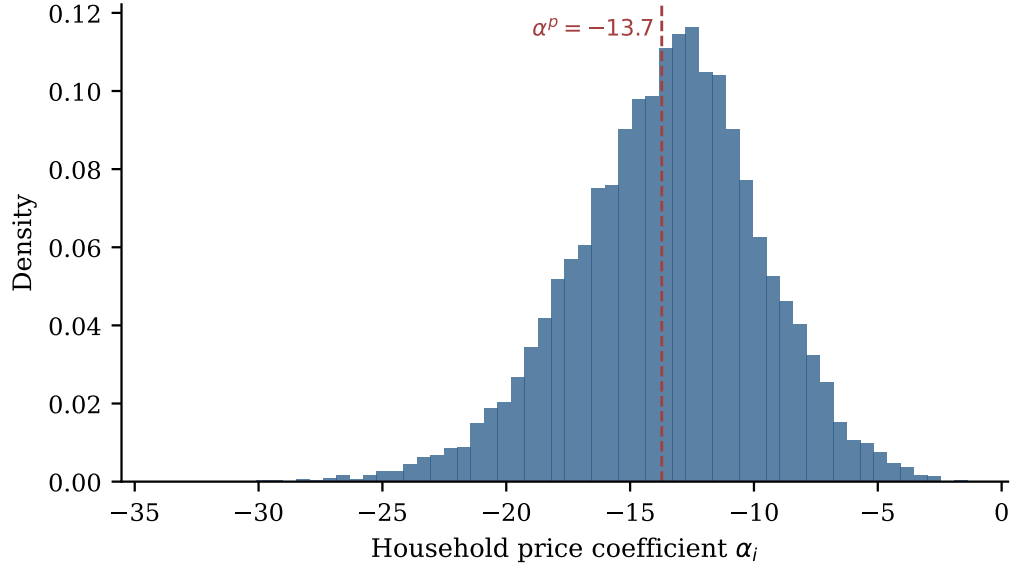
→ “Remember to take your ID and phone to make a purchase.”

→ “You can find stores near you clicking here [PERSONALIZED MAP].”

→ “Check more information about your transfer here [LINK TO CHECK BALANCE].”

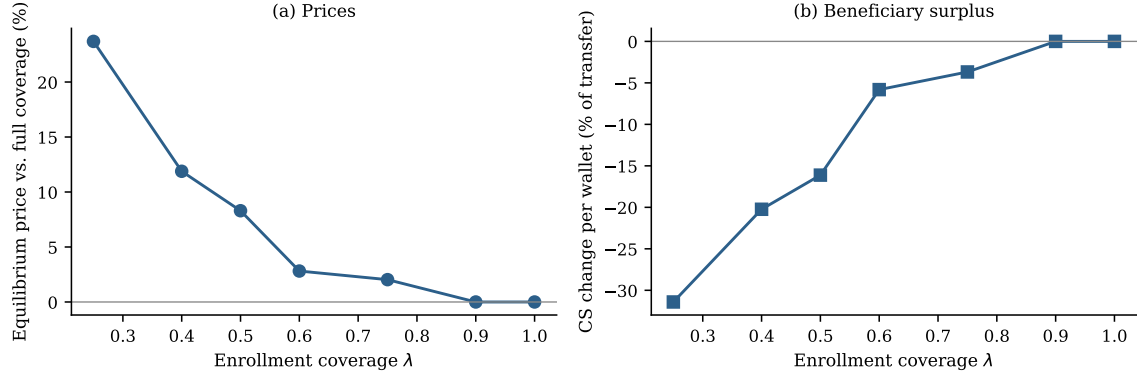
Note: Message sent to beneficiaries in PAC. The message included a link to a map application to locate treatment stores along with clear instructions on how to use the digital wallet. To increase trust, the message also includes the beneficiary’s ID number, mentions the beneficiary’s municipality, uses an official URL, and provides a contact number for support from an official business account.

Figure 4: Estimated Distribution of Household Price Coefficients



Note: Distribution of $\alpha_i = \hat{\alpha}^p + \hat{\pi}_{p,d}d_i + \hat{\pi}_{p,s}s_i + \hat{\sigma}_p\nu_i^p$ over simulated agents. The dashed line marks $\hat{\alpha}^p = -13.7$; the standard deviation is 3.8.

Figure 5: Counterfactual Enrollment Coverage: Prices and Beneficiary Surplus



Note: Nash-Bertrand equilibria given the model estimates when a random fraction of each market's stores is enrolled. Full coverage is normalized to zero.

9.2 Tables

Table 1: Experimental sample balance by store-level randomization

Variable	Mean (SD)	Control (SD)	Treatment (SD)	Difference [p-value]
Respondent is the owner	0.70 (0.46)	0.67 (0.47)	0.72 (0.45)	0.05 [0.66]
Store located within the apartment	0.79 (0.41)	0.82 (0.39)	0.76 (0.43)	0.08 [0.32]
Male	0.42 (0.50)	0.37 (0.49)	0.47 (0.50)	0.10 [0.39]
Age	48.39 (13.70)	46.15 (12.75)	50.59 (14.32)	5.39 [0.15]
Age started working	17.30 (4.56)	17.52 (4.30)	17.09 (4.80)	-0.60 [0.21]
No. hours store opens: Monday	10.52 (2.73)	10.55 (2.65)	10.48 (2.82)	-0.24 [0.55]
No. hours store opens: Tuesday	10.71 (2.34)	10.64 (2.42)	10.77 (2.28)	-0.25 [0.55]
No. hours store opens: Wednesday	10.75 (2.19)	10.74 (2.11)	10.77 (2.28)	-0.32 [0.41]
No. hours store opens: Thursday	10.75 (2.19)	10.74 (2.11)	10.77 (2.28)	-0.32 [0.41]
No. hours store opens: Friday	10.77 (2.21)	10.77 (2.15)	10.77 (2.28)	-0.35 [0.38]
No. hours store opens: Saturday	10.13 (2.97)	10.36 (2.60)	9.91 (3.28)	-0.99* [0.07]
No. hours store opens: Sunday	8.29 (4.24)	8.90 (3.92)	7.70 (4.47)	-1.33 [0.14]
Number of employees receiving a salary	1.15 (1.31)	1.21 (1.38)	1.09 (1.23)	-0.29 [0.33]
Number of family members receiving a salary	0.77 (0.88)	0.75 (0.88)	0.79 (0.89)	-0.08 [0.58]
Number of formal employees	0.69 (1.96)	0.67 (1.21)	0.72 (2.50)	-0.28 [0.21]
Number of assistants without regular salary	0.65 (0.78)	0.70 (0.78)	0.61 (0.78)	-0.12 [0.34]
Respondent knows digital wallet payment method	0.53 (0.50)	0.51 (0.50)	0.55 (0.50)	0.04 [0.66]
Respondent has a mobile phone	0.99 (0.12)	0.99 (0.12)	0.99 (0.11)	-0.01 [0.38]
Mobile phone has internet or credit	0.98 (0.14)	0.97 (0.17)	0.99 (0.11)	0.02 [0.31]
Observations	151	73	78	

Note: This table presents the balance of business characteristics between control and treatment groups in PAC. Standard errors are clustered at the neighborhood level. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. There are no statistically significant differences after correcting for multiple hypotheses testing. Differences control for neighborhood and enumerator fixed effects, so they might not equate the different between columns.

Table 2: Experimental sample balance by neighborhood-level randomization

Variable	Mean (SD)	Low (SD)	High (SD)	Difference [p-value]
Respondent is the owner	0.70 (0.46)	0.66 (0.48)	0.73 (0.45)	0.09 [0.16]
Store located within the apartment	0.79 (0.41)	0.86 (0.35)	0.72 (0.45)	-0.12 [0.17]
Male	0.42 (0.50)	0.39 (0.49)	0.45 (0.50)	0.01 [0.87]
Age	48.39 (13.70)	46.77 (12.43)	50.11 (14.83)	2.89 [0.27]
Age started working	17.30 (4.56)	17.49 (3.91)	17.11 (5.16)	-0.25 [0.69]
No. hours store opens: Monday	10.52 (2.73)	10.39 (2.66)	10.65 (2.81)	0.40 [0.36]
No. hours store opens: Tuesday	10.71 (2.34)	10.47 (2.44)	10.95 (2.23)	0.53 [0.20]
No. hours store opens: Wednesday	10.75 (2.19)	10.57 (2.15)	10.95 (2.23)	0.44 [0.24]
No. hours store opens: Thursday	10.75 (2.19)	10.57 (2.15)	10.95 (2.23)	0.44 [0.24]
No. hours store opens: Friday	10.77 (2.21)	10.59 (2.20)	10.95 (2.23)	0.40 [0.30]
No. hours store opens: Saturday	10.13 (2.97)	9.87 (3.13)	10.39 (2.79)	0.51 [0.24]
No. hours store opens: Sunday	8.29 (4.24)	8.55 (3.87)	8.01 (4.60)	-0.65 [0.36]
Number of employees receiving a salary	1.15 (1.31)	1.12 (1.29)	1.18 (1.33)	0.17 [0.53]
Number of family members receiving a salary	0.77 (0.88)	0.70 (0.86)	0.84 (0.91)	0.19 [0.30]
Number of formal employees	0.69 (1.96)	0.56 (1.04)	0.83 (2.61)	0.40 [0.15]
Number of assistants without regular salary	0.65 (0.78)	0.70 (0.78)	0.60 (0.78)	-0.10 [0.41]
Respondent knows digital wallet payment method	0.53 (0.50)	0.51 (0.50)	0.55 (0.50)	-0.00 [0.98]
Respondent has a mobile phone	0.99 (0.12)	0.99 (0.11)	0.99 (0.12)	-0.00 [0.74]
Mobile phone has internet or credit	0.98 (0.14)	0.97 (0.16)	0.99 (0.12)	0.01 [0.41]
Observations	151	73	78	

Note: This table presents the balance of business characteristics between businesses in high and low intensity neighborhoods in PAC. Standard errors are clustered at the neighborhood level. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$. There are no statistically significant differences after correcting for multiple hypotheses testing. Differences control for enumerator fixed effects, so they might not equate the different between columns.

Table 3: Compliance/Onboarding Effects (First Stage)

	(1) In Platform	(2) In Platform	(3) In Platform	(4) In Platform
Treatment	0.404*** (0.0426)	0.427*** (0.0426)	0.438*** (0.0398)	0.439*** (0.0555)
High Competition			-0.0625 (0.0753)	-0.0606 (0.0919)
Treatment x High Competition				-0.00379 (0.0798)
Constant	0.319*** (0.0385)	0.308*** (0.0213)	0.334*** (0.0431)	0.333*** (0.0444)
Observations	188	188	188	188

Note: This table presents the estimated treatment effects on platform onboarding. Standard errors are clustered at the neighborhood level and neighborhood fixed effects are included in column (2).

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 4: ITT Effects on Price Monitoring

	(1) Check Prices	(2) Check Prices	(3) Check Prices	(4) Check Prices
Treatment	-0.00917 (0.0561)	-0.0445 (0.0574)	-0.169*** (0.0562)	-0.179*** (0.0514)
High Competition			-0.107 (0.0854)	
Treatment x High Competition			0.284*** (0.0761)	0.280*** (0.0889)
Constant	0.147*** (0.0462)	0.165*** (0.0296)	0.169*** (0.0562)	0.122*** (0.0296)
Observations	155	155	155	155

Note: This table presents the estimated treatment effects on monitoring competitors' prices. Standard errors are clustered at the neighborhood level and neighborhood fixed effects are included in columns (2) and (4). * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 5: ITT Effects on Product Monitoring

	(1) Check Prod.	(2) Check Prod.	(3) Check Prod.	(4) Check Prod.
Treatment	-0.0100 (0.0562)	-0.0826 (0.0502)	-0.169*** (0.0494)	-0.192*** (0.0516)
High Competition			-0.0445 (0.0975)	
Treatment x High Competition			0.238*** (0.0681)	0.226** (0.0824)
Constant	0.160*** (0.0424)	0.197*** (0.0259)	0.169*** (0.0494)	0.163*** (0.0264)
Observations	155	155	155	155

Note: This table presents the estimated treatment effects on monitoring competitors' products. Standard errors are clustered at the neighborhood level and neighborhood fixed effects are included in columns (2) and (4). * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 6: ITT Effects on Stockouts

	(1) Out of Stock	(2) Out of Stock	(3) Out of Stock	(4) Out of Stock
Treatment	-0.0292 (0.0679)	-0.0627 (0.0888)	0.0424 (0.129)	-0.000181 (0.130)
High Competition			0.0424 (0.172)	
Treatment x High Competition			-0.123 (0.174)	-0.130 (0.180)
Constant	0.467*** (0.0624)	0.484*** (0.0458)	0.458*** (0.0671)	0.504*** (0.0517)
Observations	155	155	155	155

Note: This table presents the estimated treatment effects on stockouts. Standard errors are clustered at the neighborhood level and neighborhood fixed effects are included in columns (2) and (4).

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 7: ITT Effects on Advertising

	(1) Advertising	(2) Advertising	(3) Advertising	(4) Advertising
Treatment	0.117* (0.0616)	0.0466 (0.0783)	-0.00753 (0.0946)	-0.0332 (0.106)
High Competition			0.00636 (0.0886)	
Treatment x High Competition			0.157 (0.138)	0.166 (0.149)
Constant	0.120*** (0.0372)	0.157*** (0.0404)	0.119** (0.0426)	0.131*** (0.0436)
Observations	155	155	155	155

Note: This table presents the estimated treatment effects on advertising. Standard errors are clustered at the neighborhood level and neighborhood fixed effects are included in columns (2) and (4). * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 8: ITT Effects on Self-Reported Price Increases

	(1) Increased P.	(2) Increased P.	(3) Increased P.	(4) Increased P.
Treatment	0.0783 (0.0620)	0.111 (0.0822)	0.235** (0.104)	0.226* (0.116)
High Competition			0.0201 (0.155)	
Treatment x High Competition			-0.217 (0.143)	-0.238 (0.153)
Constant	0.547*** (0.0538)	0.530*** (0.0424)	0.542*** (0.0566)	0.566*** (0.0421)
Observations	155	155	155	155

Note: This table presents the estimated treatment effects on self-reported price increases. Standard errors are clustered at the neighborhood level and neighborhood fixed effects are included in columns (2) and (4). * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 9: ITT Price effects

	(1) Sugar price	(2) Sugar price	(3) log Sugar Price	(4) log Sugar Price
Treatment	52.84** (24.39)	60.31** (24.27)	0.0335** (0.0156)	0.0376** (0.0155)
Constant	1611.3*** (17.61)	1607.5*** (12.48)	7.380*** (0.0113)	7.378*** (0.00798)
Observations	140	140	140	140

Note: This table presents the estimated treatment effects on sugar prices. Standard errors are clustered at the neighborhood level and neighborhood fixed effects are included in columns (2) and (4). * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 10: ITT Price effects by competition intensity with baseline controls

	(1) Sugar price	(2) log Sugar Price	(3) Sugar price	(4) log Sugar Price
Treatment	62.23*** (20.82)	0.0369*** (0.0127)	55.10* (31.63)	0.0350* (0.0200)
High Competition	-19.76 (18.80)	-0.0112 (0.0117)	-27.93 (26.81)	-0.0134 (0.0161)
Baseline Price	0.461** (0.166)		0.459** (0.168)	
Baseline log Price		0.573*** (0.136)		0.572*** (0.138)
Treatment x High Competition			15.28 (42.11)	0.00413 (0.0259)
Constant	864.4*** (267.0)	3.150*** (1.005)	869.3*** (272.2)	3.157*** (1.022)
Observations	137	137	137	137

Note: This table presents the estimated treatment effects on sugar prices by competition intensity. Standard errors are clustered at the neighborhood level and baseline price controls are included in all specifications. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 11: ITT Price effects by competition among substitutes with baseline controls

	(1) Sugar price	(2) Sugar price	(3) log Sugar Price	(4) log Sugar Price
Treatment	80.79*** (23.98)	75.14*** (26.25)	0.0508*** (0.0153)	0.0473** (0.0168)
Share of Encouraged Competitors	21.98 (68.32)	-30.15 (83.02)	0.0193 (0.0403)	-0.0171 (0.0495)
Treatment x Share of Encouraged Competitors	-230.3* (127.1)	-268.8** (114.1)	-0.152* (0.0832)	-0.175** (0.0762)
Constant	1613.4*** (19.67)	1604.1*** (12.53)	7.381*** (0.0126)	7.375*** (0.00801)
Observations	140	140	140	140

Note: This table presents the estimated treatment effects on sugar prices by competition among substitute retailers. Standard errors are clustered at the neighborhood level, neighborhood fixed effects are included in columns (2) and (4) and baseline substitute share controls are included in all specifications. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 12: Price effects (2SLS)

	(1) Sugar price	(2) Sugar price	(3) log Sugar Price	(4) log Sugar Price
In Platform	128.8** (60.33)	145.4** (53.63)	0.0817** (0.0387)	0.0907** (0.0345)
Constant	1564.0*** (36.90)		7.350*** (0.0238)	
Observations	140	140	140	140

Note: This table presents the estimated local average treatment effects on sugar prices. Standard errors are clustered at the neighborhood level and neighborhood fixed effects are included in columns (2) and (4). * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 13: Price effects by competition intensity with baseline controls (2SLS)

	(1) Sugar price	(2) log Sugar Price	(3) Sugar price	(4) log Sugar Price
In Platform	147.3*** (49.75)	0.0917*** (0.0319)	132.1* (73.45)	0.0870* (0.0486)
High Competition	-21.04 (22.31)	-0.0117 (0.0142)	-40.25 (68.06)	-0.0177 (0.0428)
Baseline Price	0.461*** (0.151)	0.000294*** (0.0000977)	0.459*** (0.155)	0.000293*** (0.000100)
In Platform x High Competition			31.96 (105.7)	0.00985 (0.0673)
Constant	808.6*** (243.1)	6.868*** (0.158)	820.4*** (260.0)	6.872*** (0.168)
Observations	137	137	137	137

Note: This table presents the estimated local average treatment effects on sugar prices by competition intensity. Standard errors are clustered at the neighborhood level and baseline price controls are included in all specifications. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 14: Price effects by competition among substitutes with baseline controls (2SLS)

	(1) Sugar price	(2) Sugar price	(3) log Sugar Price	(4) log Sugar Price
In Platform	167.4** (69.28)	139.1** (64.07)	0.105** (0.0441)	0.0867** (0.0409)
Share of Enrolled Competitors	282.8 (307.3)	115.2 (283.3)	0.193 (0.185)	0.0775 (0.171)
In Platform x Share of Enrolled Competitors	-520.2* (252.0)	-521.3** (182.2)	-0.344** (0.161)	-0.343** (0.122)
Constant	1549.2*** (41.43)		7.340*** (0.0266)	
Observations	140	140	140	140

Note: This table presents the estimated local average treatment effects on sugar prices by competition among substitutes. Standard errors are clustered at the neighborhood level, neighborhood fixed effects are included in columns (2) and (4), and baseline substitute share controls are included in all specifications. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 15: Effects on Price Monitoring (2SLS)

	(1) Check Prices	(2) Check Prices	(3) Check Prices	(4) Check Prices
In Platform	-0.0229 (0.141)	-0.101 (0.122)	-0.352** (0.153)	-0.366** (0.139)
In Platform x High Competition			0.639*** (0.203)	0.627** (0.234)
High Competition			-0.358* (0.174)	
Constant	0.156 (0.0966)		0.312** (0.130)	
Observations	155	155	155	155

Note: This table presents the estimated local average treatment effects on monitoring competitors' prices. Standard errors are clustered at the neighborhood level and neighborhood fixed effects are included in columns (2) and (4). * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 16: Effects on Product Monitoring (2SLS)

	(1) Check Prod.	(2) Check Prod.	(3) Check Prod.	(4) Check Prod.
In Platform	-0.0250 (0.141)	-0.188 (0.112)	-0.352** (0.150)	-0.391** (0.155)
In Platform x High Competition			0.523** (0.191)	0.481** (0.223)
High Competition			-0.252 (0.172)	
Constant	0.170* (0.0859)		0.312** (0.122)	
Observations	155	155	155	155

Note: This table presents the estimated local average treatment effects on monitoring competitors' products. Standard errors are clustered at the neighborhood level and neighborhood fixed effects are included in columns (2) and (4). * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 17: Effects on Stockouts (2SLS)

	(1) Out of Stock	(2) Out of Stock	(3) Out of Stock	(4) Out of Stock
In Platform	-0.0729 (0.170)	-0.143 (0.191)	0.0879 (0.272)	-0.000369 (0.249)
In Platform x High Competition			-0.290 (0.413)	-0.338 (0.418)
High Competition			0.154 (0.320)	
Constant	0.496*** (0.117)		0.422** (0.158)	
Observations	155	155	155	155

Note: This table presents the estimated local average treatment effects on stockouts. Standard errors are clustered at the neighborhood level and neighborhood fixed effects are included in columns (2) and (4).

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 18: Effects on Advertising (2SLS)

	(1) Advertising	(2) Advertising	(3) Advertising	(4) Advertising
In Platform	0.294* (0.166)	0.106 (0.168)	-0.0156 (0.197)	-0.0678 (0.208)
In Platform x High Competition			0.389 (0.356)	0.412 (0.361)
High Competition			-0.140 (0.230)	
Constant	0.00250 (0.102)		0.125 (0.111)	
Observations	155	155	155	155

Note: This table presents the estimated local average treatment effects on advertising. Standard errors are clustered at the neighborhood level and neighborhood fixed effects are included in columns (2) and (4). * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 19: Effects on Self-Reported Price Increases (2SLS)

	(1) Increased P.	(2) Increased P.	(3) Increased P.	(4) Increased P.
In Platform	0.196 (0.157)	0.253 (0.171)	0.488** (0.226)	0.461* (0.230)
In Platform x High Competition			-0.443 (0.335)	-0.493 (0.335)
High Competition			0.202 (0.269)	
Constant	0.468*** (0.101)		0.344** (0.135)	
Observations	155	155	155	155

Note: This table presents the estimated local average treatment effects on self-reported price increases. Standard errors are clustered at the neighborhood level and neighborhood fixed effects are included in columns (2) and (4). * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 20: Demand Parameter Estimates

	(1) OLS logit market FE	(2) IV logit spatial IVs	(3) BLP-MM joint $D + S$
Price (α^p)	−1.816* (1.050)	−14.405** (6.031)	−13.722*** (5.027)
Most accessible store, mean (β^c)			0.152 (0.372)
Travel cost (γ , per km)			−5.256*** (1.500)
Price RC s.d. (σ_p)			3.255*** (1.242)
Price $\times$ isolation ($\pi_{p,d}$)			−3.920*** (1.044)
Price $\times$ deprivation ($\pi_{p,s}$)			−2.235 (4.055)
Accessible-store RC s.d. (σ_c)			1.315*** (0.466)
Accessible-store $\times$ isolation ($\pi_{c,d}$)			−0.668** (0.327)
Accessible-store $\times$ deprivation ($\pi_{c,s}$)			8.573*** (1.752)
Store fixed effects	No	No	Yes
Market fixed effects	Yes	Yes	Yes
Supply side estimated jointly	No	No	Yes
Observations	426	426	426

Note: This table presents the estimated structural parameters of the demand model. Columns (1)-(2) are logit share regressions with market fixed effects; column (2) instruments price with the two out-of-market spatial price instruments used in the aggregate demand moments. Column (3) is the preferred BLP random-coefficients demand and supply model with micro-moments. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 21: Recovered Markups and Own-Price Elasticities

Treatment	Competition	Mean Lerner	(s.d.)	Mean ε_{jj}	N
C	Low	0.138	(0.193)	−12.52	98
C	High	0.066	(0.021)	−16.45	30
T	Low	0.157	(0.204)	−12.03	78
T	High	0.120	(0.187)	−14.88	220
All stores		0.127	—	−13.9	426

Note: This table presents Lerner indices and own-price elasticities implied by the model estimates.

Table 22: Estimated Marginal-Cost Function

Dependent variable: $\log mc_{jt}$	Coefficient
Distance to nearest supermarket (per km)	0.338*** (0.116)
Veggie store	-0.391*** (0.125)
Meat store	-0.547* (0.296)
Monthly CPI (centered)	0.124 (0.156)
Constant	0.058 (0.123)
Excluded supply instruments	T_j, λ_n
Observations	426

Note: This table presents the coefficients of the log-linear marginal-cost equation estimated jointly with demand. Standard errors in parentheses.

Table 23: Decomposition of the Price Effect

<i>A. Markup wedge, by treatment $\times$ competition arm</i>					
Arm	Price	Marg. cost	Markup	Markup share	N
C-High	1.590	1.484	0.106	6.6%	30
T-High	1.627	1.438	0.189	12.0%	220
C-Low	1.637	1.415	0.222	13.8%	98
T-Low	1.628	1.373	0.256	15.7%	78
<i>B. Response to randomized neighborhood saturation (including wave FEs)</i>					
Outcome	$\hat{\beta}_\lambda$	(SE)			
Price p_{jt}	-0.0216	(0.0293)			
Marginal cost mc_{jt}	+0.0806	(0.0592)			
Markup $p_{jt} - mc_{jt}$	-0.1022	(0.0509)			
Lerner index L_{jt}	-0.0587	(0.0322)			

Note: This table presents the decomposition of the price effect into its components. Panel A splits each mystery-shopper price into recovered marginal cost and the residual markup, averaged by treatment-by-competition arm. Panel B regresses each object on randomized neighborhood saturation with wave fixed effects. All objects are generated from the recovered marginal costs and markups.

Table 24: Counterfactual Enrollment Coverage: Prices, Markups, and Beneficiary Surplus

Coverage λ	Price (CLP)	vs. full	Lerner	Δ CS/wallet (CLP)	% of transfer
0.25	2,017	+23.7%	0.303	-15,704	-31.4%
0.40	1,824	+11.9%	0.223	-10,117	-20.2%
0.50	1,766	+8.3%	0.190	-8,060	-16.1%
0.60	1,676	+2.8%	0.160	-2,907	-5.8%
0.75	1,664	+2.0%	0.147	-1,842	-3.7%
1.00	1,631	+0.0%	0.127	0	+0.0%

Note: This table presents the Nash-Bertrand equilibria given the model estimates when a random fraction of each market's stores is enrolled.

A Appendix

A.1 Additional Tables

Table 25: Response Rates by Treatment Status and Intensity

	(1) Response	(2) Response	(3) Response	(4) Response
Treatment	0.0532 (0.0525)	0.0825 (0.0766)	0.0821 (0.0732)	0.0126 (0.0783)
High Competition			-0.0543 (0.0842)	-0.124 (0.110)
Treatment x High Competition				0.139 (0.142)
Constant	0.777*** (0.0458)	0.762*** (0.0383)	0.789*** (0.0546)	0.806*** (0.0490)
Observations	188	188	188	188

Note: This table presents the estimated effects of the treatment assignment on survey response rates. Standard errors are clustered at the neighborhood level and neighborhood fixed effects are included in column (2). * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 26: Price effects with baseline controls

	(1) Sugar price	(2) Sugar price	(3) log Sugar Price	(4) log Sugar Price
Treatment	52.56** (22.11)	53.54** (22.41)	0.0314** (0.0137)	0.0317** (0.0137)
Baseline Price	0.463** (0.165)	0.481** (0.185)		
Baseline log Price			0.575*** (0.135)	0.595*** (0.148)
Constant	855.1*** (265.3)	826.5** (300.2)	3.129*** (0.999)	2.985** (1.091)
Observations	137	137	137	137

Note: This table presents the estimated treatment effects on sugar prices. Standard errors are clustered at the neighborhood level, baseline price controls are included in all specifications and neighborhood fixed effects are included in columns (2) and (4). * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 27: Δ Price effects

	(1) Δ Sugar Price	(2) Δ Sugar Price	(3) Δ log Sugar Price	(4) Δ log Sugar Price
Treatment	53.54* (27.14)	46.92* (23.87)	0.0304* (0.0148)	0.0278* (0.0142)
Constant	-19.25 (23.53)	-15.87 (12.20)	-0.00965 (0.0118)	-0.00833 (0.00723)
Observations	137	137	137	137

Note: This table presents the estimated treatment effects on changes in sugar prices. Standard errors are clustered at the neighborhood level and neighborhood fixed effects are included in columns (2) and (4). * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 28: Difference-in-Differences Price effects

	(1) Sugar price	(2) Sugar price	(3) log Sugar Price	(4) log Sugar Price
Treatment	-1.825 (27.44)	8.406 (22.93)	0.00242 (0.0139)	0.00750 (0.0121)
Post Period	-17.93 (23.59)	-16.98 (24.44)	-0.00878 (0.0119)	-0.00823 (0.0123)
Treatment x Post Period	54.67* (26.87)	56.04* (27.88)	0.0311** (0.0145)	0.0319** (0.0151)
Constant	1629.3*** (27.67)	1623.2*** (17.07)	7.389*** (0.0142)	7.385*** (0.00859)
Observations	277	277	277	277

Note: This table presents the estimated difference-in-differences treatment effects on sugar prices. Standard errors are clustered at the neighborhood level and neighborhood fixed effects are included in columns (2) and (4). * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 29: Δ Price effects by competition intensity

	(1) Δ Sugar Price	(2) Δ log Sugar Price	(3) Δ Sugar Price	(4) Δ log Sugar Price
Treatment	56.75** (22.83)	0.0334** (0.0131)	70.32* (37.05)	0.0402* (0.0209)
High Competition	-6.553 (24.78)	-0.00601 (0.0147)	8.873 (36.48)	0.00180 (0.0195)
Treatment x High Competition			-29.00 (44.74)	-0.0147 (0.0258)
Constant	-17.69 (27.10)	-0.00822 (0.0136)	-21.37 (30.54)	-0.0101 (0.0152)
Observations	137	137	137	137

Note: This table presents the estimated treatment effects on changes in sugar prices by competition intensity. Standard errors are clustered at the neighborhood level. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 30: Price effects by competition intensity

	(1) Sugar price	(2) log Sugar Price	(3) Sugar price	(4) log Sugar Price
Treatment	65.45** (24.20)	0.0407** (0.0154)	42.20 (35.83)	0.0280 (0.0233)
High Competition	-25.93 (22.27)	-0.0149 (0.0139)	-51.18 (31.05)	-0.0287 (0.0198)
Treatment x High Competition			48.26 (46.41)	0.0264 (0.0298)
Constant	1617.8*** (19.47)	7.384*** (0.0125)	1624.1*** (22.30)	7.387*** (0.0144)
Observations	140	140	140	140

Note: This table presents the estimated treatment effects on sugar prices by competition intensity. Standard errors are clustered at the neighborhood level. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 31: Price effects with baseline controls (2SLS)

	(1) Sugar price	(2) Sugar price	(3) log Sugar Price	(4) log Sugar Price
In Platform	123.1** (50.39)	128.6** (48.97)	0.0782** (0.0323)	0.0802** (0.0315)
Baseline Price	0.464*** (0.152)	0.479*** (0.158)	0.000296*** (0.0000983)	0.000307*** (0.000104)
Constant	808.0*** (244.6)		6.868*** (0.159)	
Observations	137	137	137	137

Note: This table presents the estimated local average treatment effects on sugar prices. Standard errors are clustered at the neighborhood level, baseline price controls are included in all specifications and neighborhood fixed effects are included in columns (2) and (4). * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 32: Δ Price effects (2SLS)

	(1) Δ Sugar Price	(2) Δ Sugar Price	(3) Δ Sugar Price	(4) Δ log Sugar Price	(5) Δ log Sugar Price
In Platform	125.4* (60.02)	125.4* (60.02)	112.7** (52.16)	0.0713** (0.0326)	0.0668** (0.0310)
Constant	-66.05 (43.12)	-66.05 (43.12)		-0.0362 (0.0221)	
Observations	137	137	137	137	137

Note: This table presents the estimated local average treatment effects on changes in sugar prices. Standard errors are clustered at the neighborhood level and neighborhood fixed effects are included in columns (2) and (4). * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 33: Δ Price effects by competition intensity (2SLS)

	(1) Δ Sugar Price	(2) Δ log Sugar Price	(3) Δ Sugar Price	(4) Δ log Sugar Price
In Platform	134.3** (54.47)	0.0790** (0.0316)	168.7* (81.53)	0.0965* (0.0472)
High Competition	-7.739 (28.04)	-0.00671 (0.0166)	35.58 (73.21)	0.0154 (0.0409)
In Platform x High Competition			-72.32 (104.5)	-0.0369 (0.0612)
Constant	-67.52 (41.56)	-0.0375* (0.0216)	-84.21 (55.28)	-0.0460 (0.0290)
Observations	137	137	137	137

Note: This table presents the estimated local average treatment effects on changes in sugar prices by competition intensity. Standard errors are clustered at the neighborhood level. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

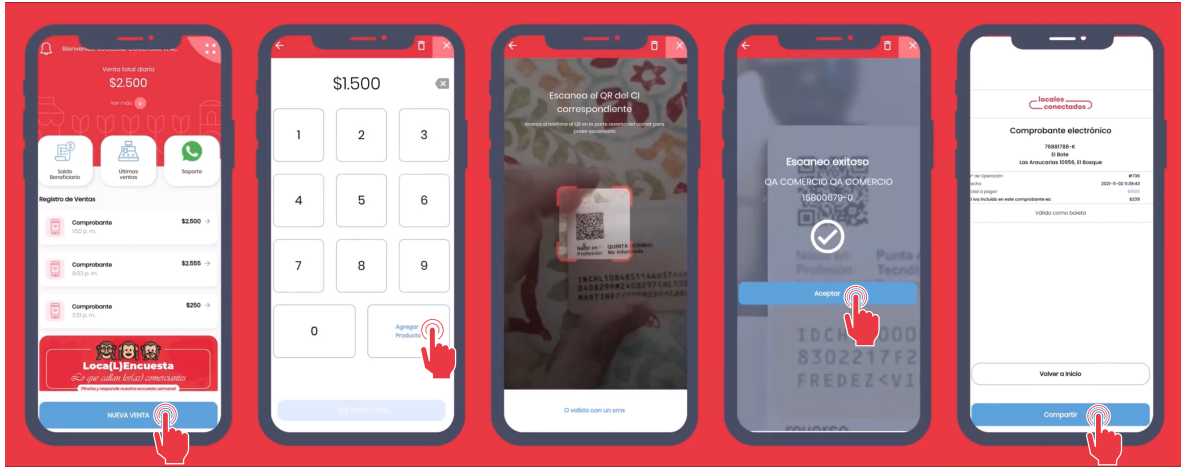
Table 34: Price effects by competition intensity (2SLS)

	(1) Sugar price	(2) log Sugar Price	(3) Sugar price	(4) log Sugar Price
In Platform	156.3** (57.78)	0.0973** (0.0369)	101.2 (85.33)	0.0672 (0.0559)
High Competition	-23.17 (23.03)	-0.0132 (0.0144)	-89.37 (67.99)	-0.0493 (0.0433)
In Platform x High Competition			113.8 (112.8)	0.0621 (0.0721)
Constant	1559.6*** (36.23)	7.347*** (0.0233)	1586.4*** (50.14)	7.362*** (0.0328)
Observations	140	140	140	140

Note: This table presents the estimated local average treatment effects on sugar prices by competition intensity. Standard errors are clustered at the neighborhood level. * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

A.2 Additional Figures

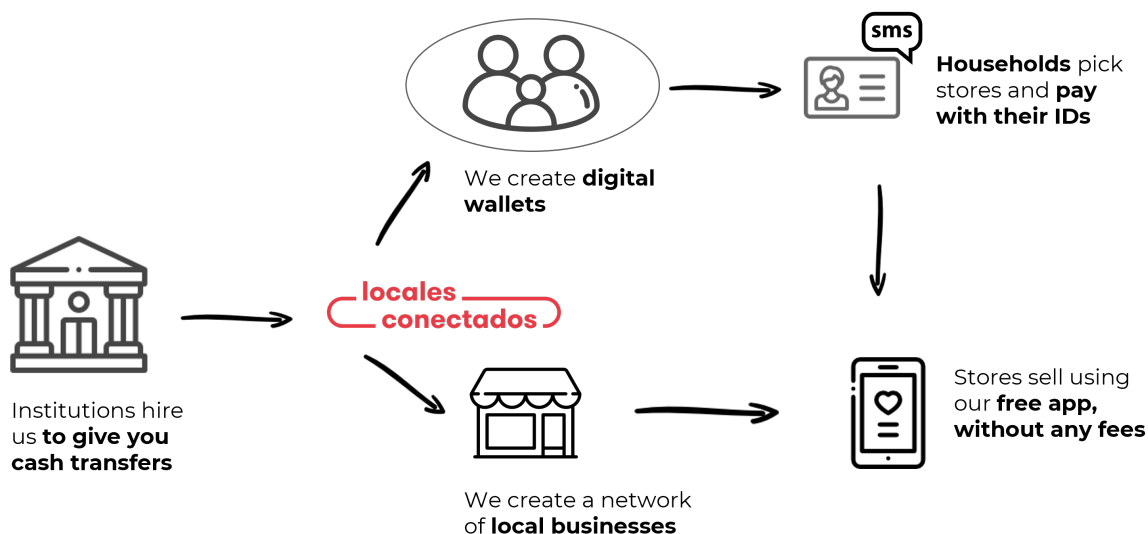
Figure 6: Diagram explaining the Conectados transaction process for businesses



Create new sale Input sale amount Scan ID's QR code Confirm purchase Send digital receipt

Note: This diagram summarizes the transaction process for businesses in the Conectados mobile application. The business creates a new sale in the app, inputs the sale amount, scans the beneficiary's ID QR code, confirms the purchase, and sends a digital receipt. A video-version of this diagram was shared with businesses to explain how the program works.

Figure 7: Diagram explaining the Conectados business model to beneficiaries



Note: This diagram summarizes the *Conectados* business model. Conectados is hired by institutions to provide cash transfers. The program connects beneficiaries of cash transfer programs with local businesses through a digital wallet platform. Beneficiaries receive cash transfers that can be used at participating local businesses by showing their ID cards. Businesses onboarded onto the digital wallet platform can sell to beneficiaries without incurring in additional costs. This diagram was shared with beneficiaries to explain how the program works.

Figure 8: Marketing Materials for Businesses



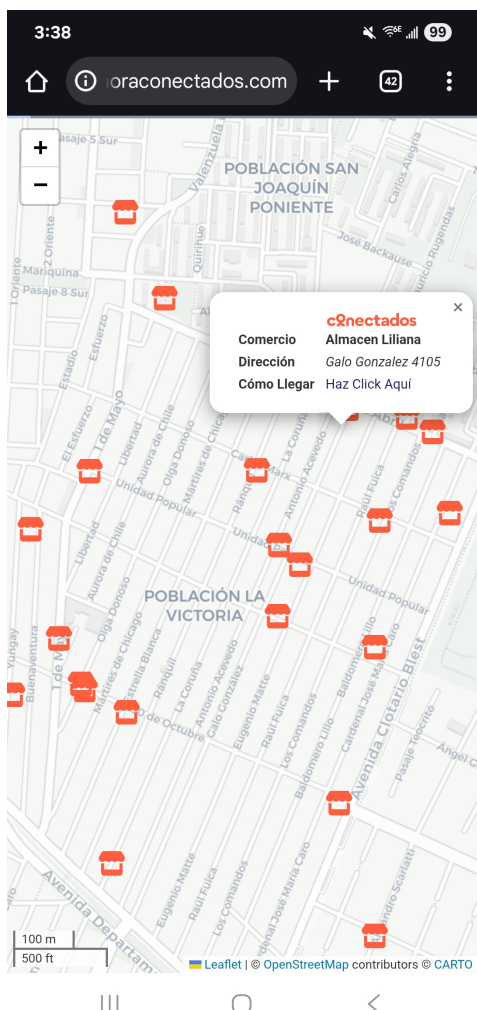
(a) “Here we are connected #LocalBusiness”

(b) “Buy here with your ID” (logo)

Figure 9: Examples of Storefronts with Marketing Materials



Figure 10: Map Application to Locate Treatment Stores



→ Custom URL for each beneficiary

→ Simple zoom in/out buttons (they can also use pinch-to-zoom)

→ Pop-up when clicking on a store:

→ Name of the clicked store

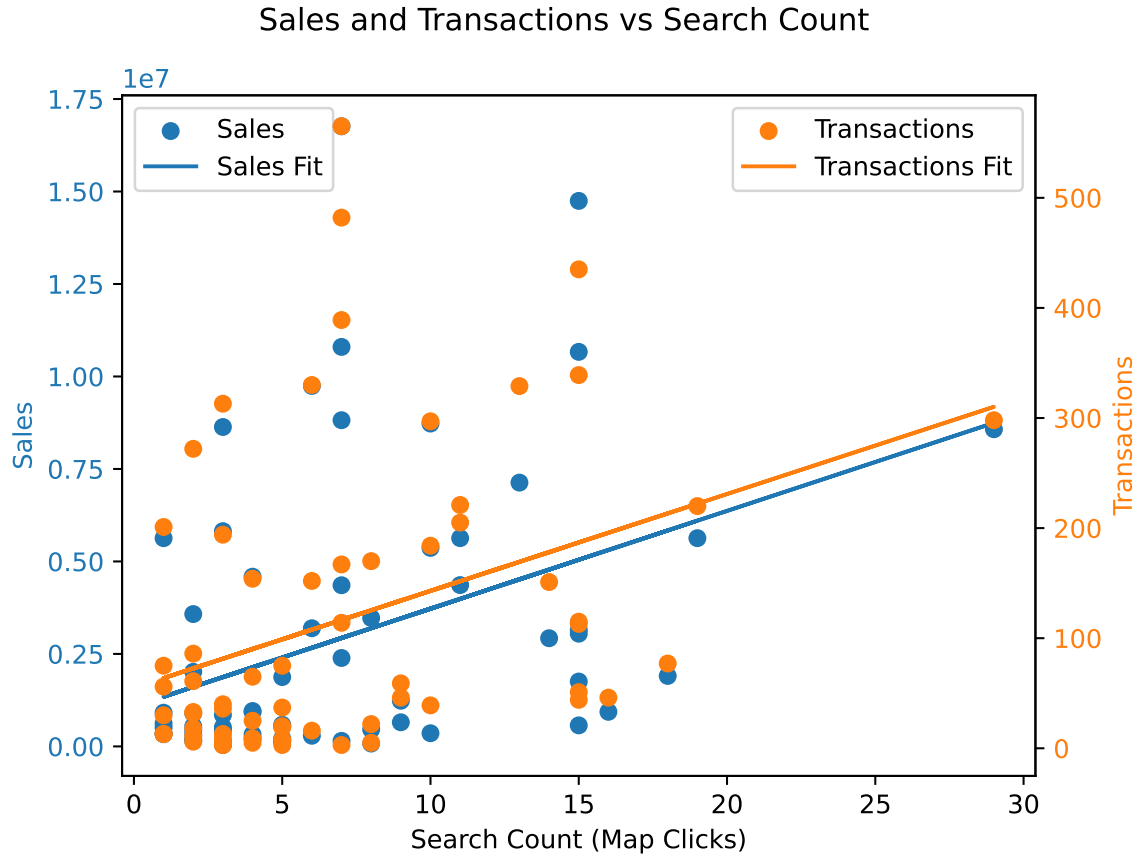
→ Address of the clicked store

→ Link for directions to the clicked store (tracked)

→ Map automatically centered in beneficiary's neighborhood (in this case, Población La Victoria)

Note: Screenshot of the map application sent to beneficiaries in PAC. The map shows the locations of nearby treatment stores and was automatically centered in each beneficiary's neighborhood. The application allows beneficiaries to click on specific stores and get directions to them. When they search for directions to a store, the application automatically opens Google Maps (web interface if app is not installed) with the route to the selected store, and we (anonymously) track this interaction.

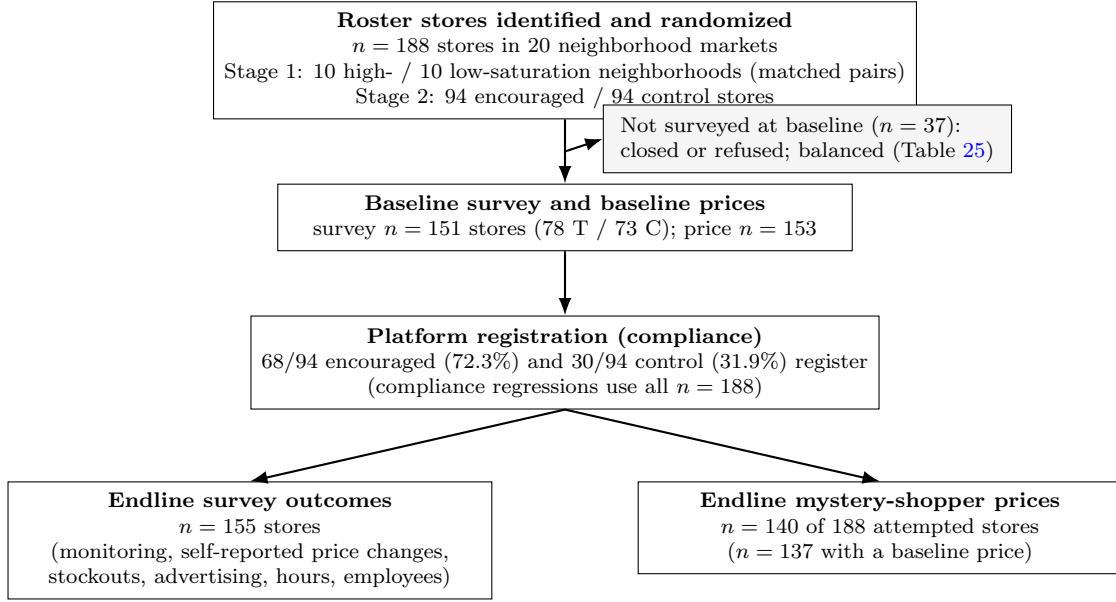
Figure 11: Search Behavior and Sales/Transactions



Note: Scatter plot of the number of searches for directions to treatment stores (x-axis) and total sales (y-axis, left panel) or total number of transactions (y-axis, right panel) during the intervention period. Each point represents a store.

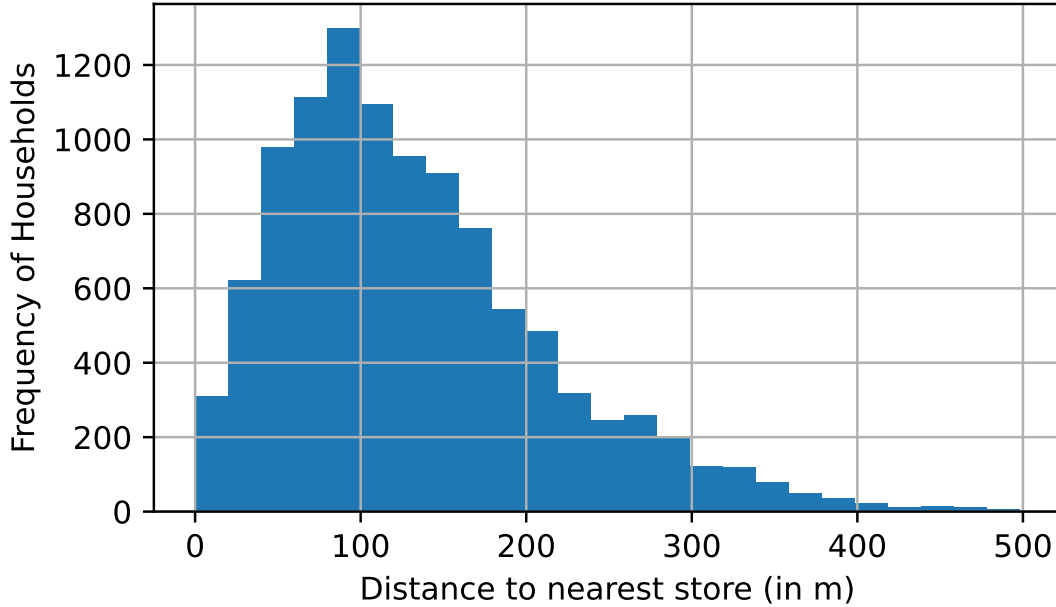
We found a positive correlation between search behavior and wallet transactions. Stores with more clicks had higher sales and transactions, suggesting that some beneficiaries used the application to locate stores and make purchases. On average, each search-click is associated to an increase in almost 25,000 CLP (27 USD) in sales. However, we suspect most beneficiaries did not heavily rely on this application to locate stores: out of the nearly 3000 visits to the application, only 1183 were unique IPs (although different users can share the same IP) and the maximum number of searches for any given store was only 29. Households could also locate stores using the address displayed in the map, without clicking on the search link, or by simply visiting stores they already knew.

Figure 12: Diagram of Store Enrollment, Survey Participation and Price Collection



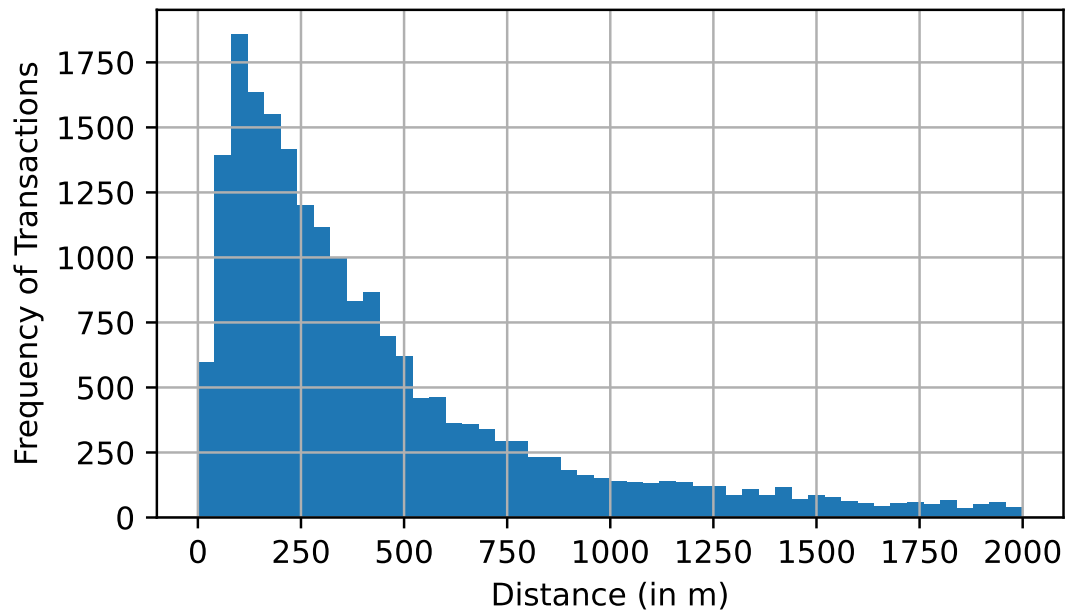
Note: The endline survey and the mystery-shopper price collection are parallel tracks: shoppers attempted a purchase at all 188 roster stores regardless of survey participation. Endline survey participation exceeds baseline participation because some stores that declined the baseline interview responded at endline; specifications with baseline controls use the overlap.

Figure 13: Minimum Distance Distribution



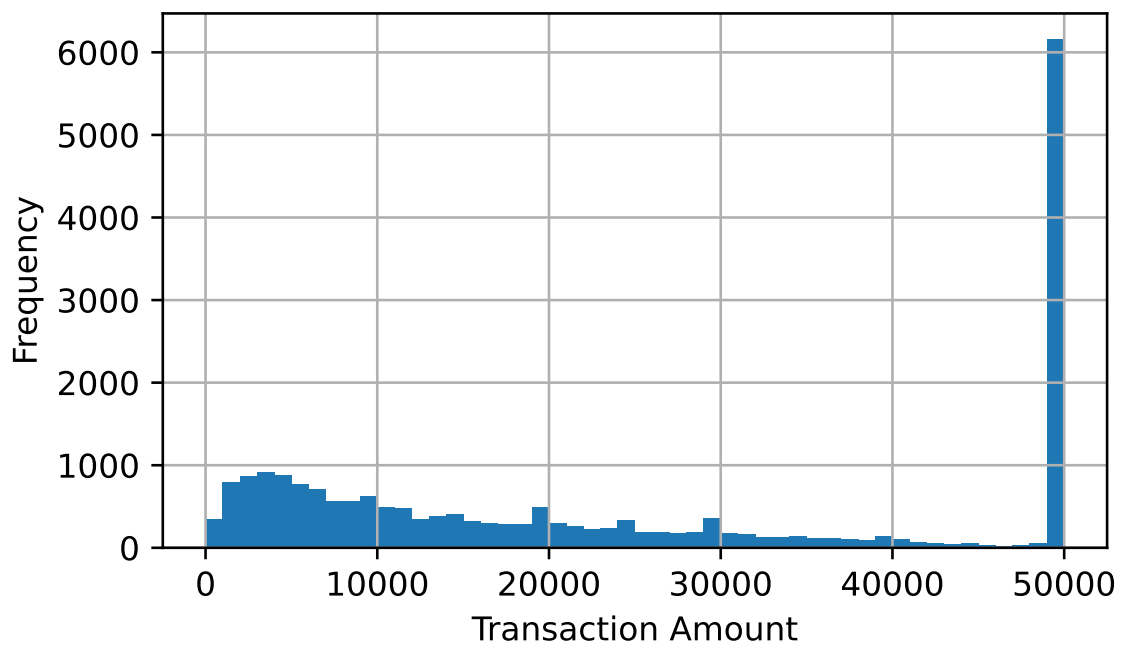
Note: Distribution of minimum distances from households to stores (under 500 meters). There are 25 bins, each representing 20 meter intervals.

Figure 14: Transaction Distance Distribution



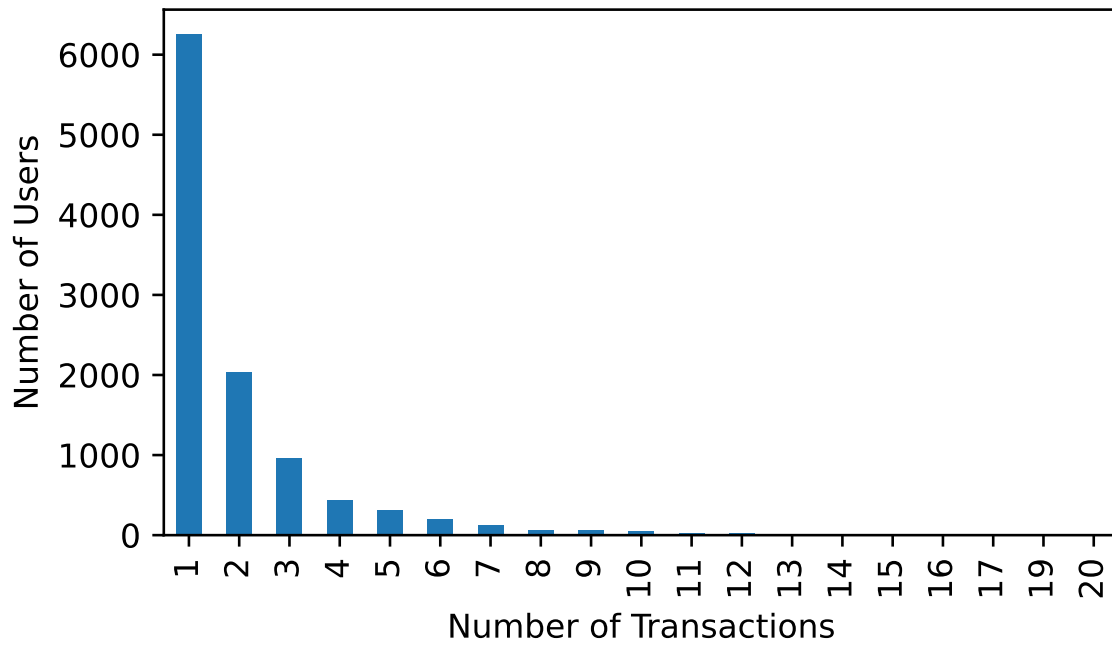
Note: Distribution of distances from households to stores for transactions under 2000 meters. There are 50 bins, each representing 40 meter intervals.

Figure 15: Transaction Amount Distribution



Note: Distribution of transaction amounts.

Figure 16: Transactions Per User



Note: Distribution of the number of transactions per user.