NBER WORKING PAPER SERIES

THE CAUSAL EFFECT OF COMPETITION ON PRICES AND QUALITY: EVIDENCE FROM A FIELD EXPERIMENT

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Working Paper 20054
http://www.nber.org/papers/w20054

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
April 2014

We thank Francisca Müller and Juan Sebastian Muñoz for their excellent project management and research assistance. We thank very useful comments by Daron Acemoglu, Gary Libecap, James Poterba, Ernesto Schargrodsky, Gustavo Torrens, and seminar participants at various places. This experiment has been registered in the American Economic Association RCT Registry with number AEARCTR-0000343. The views expressed herein are those of the authors and should not be attributed to the Inter-American Development Bank or the National Bureau of Economic Research.

The Causal Effect of Competition on Prices and Quality: Evidence from a Field Experiment
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NBER Working Paper No. 20054
April 2014, Revised February 2015
JEL No. D4,I3,L1

ABSTRACT

This paper provides the first experimental evidence on the effect of increased competition on the prices and quality of goods. We rely on an intervention that randomized the entry of 61 retail firms (grocery stores) into 72 local markets in the context of a conditional cash transfer program that serves the poor in the Dominican Republic. Six months after the intervention, product prices in the treated districts had decreased by about 6%, while product quality and service quality had not changed. Using a theoretical model, we arrive at the conclusion that the poor segments of the population in these markets care the most about prices and much less about quality. Our results are also informative to the design of social policies. They suggest that policymakers should pay attention to supply conditions even when the policies in question will only affect the demand side of the market.

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1. Introduction

Ever since Adam Smith, economists have seen market competition as a way of achieving economic efficiency. If a competitive equilibrium exists, then the equilibrium is necessarily Pareto optimal in the sense that there is no other allocation of resources which would make all participants in the market better off. Adam Smith considered competition to be a form of rivalry between suppliers that eliminated excessive profits, did away with excessive supply and satisfied existing demand (Stigler, 1957). Competition also exerts downward pressure on costs, reduces slack periods and provides incentives for the efficient organization of production (Nickell, 1996). Price-taking implies that no supplier is able to exert market power, which means that firms do not price profitably above the marginal cost of production and that consumer surplus is therefore maximized. All these sound arguments notwithstanding, real-world experimental evidence on the welfare effects of competition has not, to the best of our knowledge, been presented.

In this paper we exploit a randomized control experiment to assess what impact the entry of new grocery stores into a given market has on prices, product quality (as defined by product brands and varieties) and service quality. The experiment was part of an attempt to improve the operations of the Dominican Republic's *Solidaridad* conditional cash transfer program. This program provides monetary transfers to poor families that can only be used by means of a debit card which is accepted only by a network of grocery stores that are affiliated with the program. The program beneficiaries represent a large share of these stores' customers and sales.

Only a certain number of stores are authorized to accept the program's debit cards. Because entry into this market is restricted by the program design, these retail stores can potentially wield market power. The government argued that they were using their market power to raise prices and to offer a more limited range of products than those offered by stores outside the network. This was seen as signaling a loss of consumer surplus and therefore a potential welfare loss. In response to this situation, we worked with the Dominican government to determine the extent to which the expansion of the retail network might fuel competition. The intervention was conducted during May and June 2011 and involved bringing 61 new grocery stores into the network in 72 districts. The experimental design allowed anywhere from zero to three stores to begin operating in each district.

We use data on both retail stores and households located in the areas concerned which was collected at baseline and six months after the intervention. Our data allow us to arrive at precise price measurements which we can then use to infer quality. The surveys also incorporate other independent measures of quality. We estimate average treatment effects using the randomization assignment in order to instrument the potentially endogenous entry of new stores induced by noncompliance with randomization.

We find that entry into the market leads to a significant and robust reduction in prices but that it does not lead to any change in the quality of the products or service (delivery of those goods) provided by the grocery stores. We also impose some structure in order to estimate the price-elasticity at entry, which we find to be 0.06.

Previous work analyzing the effects of competition has relied on observational data. Trapani and Oslon (1982) analyze the effect of the deregulation of the airline industry in the US on the price and quality of service by studying the relationship between fare level, open entry and service quality. This analysis exploits a cross-sectional sample of 70 markets within the United States in 1971 and 1977. The authors found that increasing competition in the airline industry leads to a reduction both in prices and in the average quality of service. Their paper shows that the independent effect of decreasing market concentration, which leads to a higher quality of service, is overshadowed by the independent effect of price competition (lower prices), which, in turn, lowers the quality of service. Bresnahan and Reiss (1990) study the interrelationships among potential entrants' profit levels and decisions using cross-sectional data on 149 geographically isolated US markets for new automobiles. They estimate that the second entrant has nearly the same costs and market opportunities as the first entrant. They also find that entry does not cause price-cost margins to fall by a significant amount. Bresnashan and Reiss (1991) examine the prices of tires in the United States to determine at what point further entry does not lead to any further price decrease, which the authors point out would be evidence that market competition had been achieved. Their study suggests that four retailers would be sufficient for the tire market to be (effectively) competitive.[2] Goolsbee and Syverson (2008) study the entry of a low-price competitor (Southwest Airlines) into the airline industry in the US. They find that large price decreases occur during the first three quarters of the time period that elapses between the announcement of entry and the point in time when actual entry occurs.

Several papers have developed econometric models to estimate the effects of market entry, including those of Carlton (1983), Berry (1992), Bresnahan and Reiss (1989), Bresnahan and Reiss (1991) and Reiss and Spiller (1989). Geroski (1989) examines a dynamic feedback model of entry and profit margins applied to panel data covering a six-year period (1974-1979) for 85 three-digit industries in the United Kingdom. He finds that entry barriers are rather high in most industries and that there are noticeable differences in the pace of competitive dynamics.

Besker and Noel (2009) analyze the effect of Wal-Mart's entry into the grocery market using a store-level price panel dataset. They find that competitors' response to the entry of a Wal-Mart store, which has a price advantage over competitors of about 10%, is a price

[2] Dufwenberg and Gneezy (2000) set up a lab experiment to address this question. They designed an experiment in which the model resembles an oligopolistic market with homogenous products and find that four competitors were sufficient to drive the equilibrium toward the competitive outcome.

reduction of 1%-1.2%, on average, with most of this reduction being accounted for by smaller-scale competitors. They conclude that competitors' responses vary in line with their degree of differentiation from Wal-Mart. At one extreme, the largest supermarket chains reduce their prices by less than half as much as smaller competitors. At the opposite extreme, low-end grocery stores, which compete more directly with Wal-Mart, cut their prices by more than twice as much as higher-end stores. Jia (2008) develops an empirical model –one which relaxes the assumption that entry into different markets is independent— to assess the impact of Kmart stores on Wal-Mart stores and other discount retailers and to quantify the size of the scale economies obtained within a given chain. She finds that the negative impact of Kmart's presence on Wal-Mart's profits was much stronger in 1988 than in 1997, while the opposite is true for the effect of Wal-Mart's presence on Kmart's profits.

In a more recent paper, Bennett and Yin (2013) explore the relationship between market development and drug quality by evaluating the impact of chain-store (Med-Plus) entry into the Indian pharmaceutical industry. They rely on a quasi-experimental variation and exploit a difference-in-differences identification strategy and find that the entry of a chain store leads to a relative 5% improvement in quality, measured on the basis of compliance with the standards of the Indian Pharmacopeia Commission, and a 2% decrease in prices. The authors conclude that the chain store increases retail competition by offering higher-quality drugs and lower prices. Although this evidence is compelling and very interesting, the effects associated with chain-store entry cannot be unequivocally attributed to an increase in competition, since the new stores operate on the basis of a completely different rationale than the incumbent family-run stores do. As the authors argue, it is better to interpret the evidence that they have gathered as also being indicative of the effects of market development in a developing country.

Although several important studies have used different setups to focus on the effects of the entry of new competitors into imperfectly competitive markets, our paper contributes to the literature by reporting on what is, to the best of our knowledge, the first randomized-controlled field experiment designed to assess the impact of increasing competition on prices and quality. Without neglecting the very important role of theory in the analysis of the data provided by previous works (see, among others, Einav and Levin (2010)), this is a significant contribution because, as has been acknowledged in the literature, competition in non-experimental studies is likely to be endogenous for the parameters of interest (see, among others, Blundell et al. (1999) and Aghion et al. (2014)).

Our paper addresses a fundamental question in economics. It exploits experimental variability in the entry of existing stores into a segment of a market where entry is constrained. The results do, in fact, indicate that our experiment induces a large and exogenous increase in competition among retail stores in that market segment. Naturally, even though our findings, in conjunction with economic theory, provide an outline of the general principles that are in play, as in the case of any study on a given market, it may not be possible to directly extrapolate our conclusions to other industries or populations.

The remainder of this paper is organized as follows. Section 2 presents a simple model that is used to guide the empirical analysis. Section 3 describes the setting in which the intervention took place. Section 4 discusses the experimental design. Section 5 presents the data used in this study. In Section 6, we present our empirical strategy. In Section 7, we present the empirical results. Section 8 concludes.

2. A Simple Model

Theoretical models of imperfect competition make various predictions about the competitive effects of market entry. Firms with market power may exploit their position to lower quality, just as they may raise prices (Tirole, 1988). Competition attenuates the incentive to do so and prompts firms to increase quality and/or decrease prices.

In most models, the entry of new competitors leads to price reductions by putting more competitive pressure on market incumbents. This is a prediction of most standard imperfect-competition models, such as differentiated-product Bertrand competition and spatial-competition models, as well as of many models with equilibrium price dispersion (such as that of Reinganum, 1979).

The effect of competition on product quality has been shown to be less clear-cut across the various models. Greater market power prompts firms to exploit their position in order to increase prices and reduce quality. Competition attenuates the incentives to do so; however, firms are likely to compete through quality if quality improvements translate directly into greater demand. The effect of competition on quality depends on the extent to which consumers perceive quality. Dranove and Satterthwaite (1992) explore the relationship between competition and quality by varying the precision of price and quality signals in a search model. They find that competition has an effect on quality when consumers have received quality signals that are at least somewhat informative.

We rely on a simple Cournot model of competition where n firms compete in price and quality. We impose a reasonable set of assumptions in relation to the experiment that we analyze in this paper and derive results for the effect of competition on both the price of the product supplied and the quality of the service provided.

We view the market that we are studying here as one that, in the absence of market entry restrictions, would operate much like a monopolistic competitive market, where retailers would differ mostly in terms of their location and, to some extent, the quality of the service provided. In our experiment retail stores offer a similar set of goods, but they can alter the varieties/brands they offer (the perceived product quality) and they can also vary the range of varieties/brands thus offering a different quality of service. They can also vary the customer service, another dimension of the quality of service provided.

In this section, in order to keep the model simple, we will abstract from the selection of multiple products/varieties and will consider firms that face a downward demand curve for a homogenous product in an oligopolistic market in which entry barriers enhance the market power of the incumbent retailers. Those retailers have also chosen the overall quality of the service they supply (which we model as one-dimensional). In the empirical analysis, we will seek to determine whether firms change the varieties/brands of the products they offer. In other words, we will try to establish whether, as a result of more competition, they provide products of a different quality at the same price or whether they offer products of the same quality at a lower price (or a combination of the two).

Assume that there are *n* identical firms that compete in a market of differentiated goods and that they choose the quantity of a homogenous product (q_i) that is produced and the quality of the service provided (v_i). We will suppose that the residual inverse demand curve that a firm faces is separable in terms of quantity and quality and that it depends not only on the quantity supplied by other firms, but also on the difference between the quality of the service provided by that firm and the service quality offered by the rest of the firms in that market, as follows:

$$p_i = F\left(v_i - \alpha \sum_{j \neq i} v_j\right) - \beta q_i - \delta \sum_{j \neq i} q_j \qquad (1)$$

where F is a strictly increasing function and α is such that the argument in F is always positive. Note that the lower the value of δ, the lower the degree of substitution between products. Similarly, the lower the value of α, the lower the degree of substitution between service qualities. At the limit, if both α and δ were zero, then increasing competition would not affect firm behavior.

For the sake of simplicity, we assume that there is no fixed cost and that the cost function is linear in the amount produced, but that it is increasing and convex in the level of quality supplied. This may reflect the fact that the initial increases in quality can be achieved by means of minor adjustments or improvements in inputs, while further improvements in quality are more costly. The cost function is then:

$$c_i(q_i, v_i) = c_q q_i + c_v \frac{(v_i)^2}{2}$$

Using both the inverse demand curve and the cost function, we can write the profit function of a firm *i* as:

$$\pi_i = \left[F\left(v_i - \alpha \sum_{j \neq i} v_j\right) - \beta q_i - \delta \sum_{j \neq i} q_j\right] q_i - c_q q_i - c_v \frac{(v_i)^2}{2}$$

The problem that the firm faces is then:

$$\max_{q_i; v_i} \pi_i$$

The first-order conditions for this optimization problem are then:

$$\frac{\partial \pi_i}{\partial q_i} = \left[F\left(v_i - \alpha \sum_{j \neq i} v_j \right) - 2\beta q_i - \delta \sum_{j \neq i} q_j \right] - c_q = 0$$

$$\frac{\partial \pi_i}{\partial v_i} = F'\left(v_i - \alpha \sum_{j \neq i} v_j \right) q_i - c_v v_i = 0$$

If a symmetric Nash equilibrium exists such that $(p_i, q_i, v_i) = (p, q, v)$ for all firms, then the previous two equations become:

$$F(v[1 - \alpha(n-1)]) - [2\beta + \delta(n-1)]q = c_q$$

$$F'(v[1 - \alpha(n-1)])q = c_v v$$

We now use this model to investigate how the number of firms in the market affects the equilibrium values of the quantity offered and the quality chosen by each firm. If F is concave, which we will assume it is, it then follows that $\frac{dv}{dn} > 0$ and $\frac{dq}{dn} < 0$. In other words, as the number of firms in the market increases, the amount of the product sold by each firm in the symmetric equilibrium decreases, while product quality rises. To see how the equilibrium price reacts to entry, we then turn to equation (1) and replace the arguments with their equilibrium values:

$$p = F([1 - \alpha(n-1)]v) - [\beta + \delta(n-1)]q$$

If we differentiate this expression with respect to *n*, *w*e easily find that $\frac{dp}{dn} < 0$. This means that the effect of increased competition in the symmetric equilibrium is a reduction of prices.

Thus, if customers value the increase in quality, then firms, in a symmetric Nash equilibrium, will react to an exogenous increase in the number of firms by reducing prices and increasing quality. If, instead, customers do not value quality (*F*'= 0), firms will compete only on price, as is the case in the Cournot model.

3. Setting

Our study exploits the design and implementation of a conditional cash transfer (CCT) program in the Dominican Republic. CCT programs have been extensively used since the mid-1990s as one of the main tools for providing social protection to people in low- and middle-income developing countries. The Dominican Republic introduced the *Solidaridad* CCT program in 2005.

The program provides monetary transfers to families living in poverty. Eligibility is determined on the basis of a quality-of-life score that is used to classify households into different socioeconomic groups. All households identified as extremely-to-moderately poor are eligible. In 2005 the program initially reached about 200,000 households. It then underwent two big expansions: one in 2007 (when it reached 460,000 households) and another in 2010 (when its coverage expanded to 520,000 households).[3] In the interim periods, the number of beneficiaries stayed relatively constant. During 2011, the year of our study, the program had reached a plateau, with the number of program beneficiaries increasing by around 3% during the year.

This CCT program includes two components. First, a health component ("*Comer es Primero"/* "Eating comes First") provides households with a transfer of about US$ 19.5 per month.[4] Transfers are contingent on parents bringing those of their children who are under five years of age to the community health center on a regular basis for developmental monitoring and immunizations. In addition, they are expected to attend workshops that provide instruction in nutrition, family planning, self-care and hygiene. The program's second component focuses on education ("*Incentivo a la Asistencia Escolar"/* "Incentives for School Attendance") and transfers a given amount of money to households depending on the composition of the family: Households with one or two eligible children (aged 6-16) receive US$ 8.4 per month; those with three children receive US$ 12.5; and those with four or more children receive US$ 16.7 per month. Transfers are contingent on school enrollment and attendance of children between 6 and 16 years of age.[5] The typical household (three children of school age) would receive a total transfer of US$ 36, which represents 17% of the median monthly food expenditure of the target population.[6]

Households' monetary transfers are deposited into individual bank accounts. More importantly, in order to ensure that the transfer is spent on food, the money cannot be

[3] Expanding coverage is costly, since the government has to conduct a population census in poor areas in order to determine household eligibility.
[4] Based on a 2010 exchange rate of DR$ 35.9 to the dollar.
[5] Students must not repeat a grade more than once and must have an 85% attendance record, as a minimum.
[6] In principle, households might receive other money transfers that are deposited in their bank accounts, such as a subsidy for higher education ("*Incentivo a la educación superior"*), a pension for the elderly living in extreme poverty ("*Programa de protección a la vejez en pobreza extrema"*), a subsidy to buy gas ("*Bonogas"*) and/or a subsidy to pay the electricity bill ("*Bonoluz"*). Some of these transfers could be used in the same retailers that are under analysis here.

withdrawn from the bank but, instead, can only be spent by using a debit card[7] that works only in a network of program-affiliated retailers (the network is known as the "*Red de Abastecimiento Social"* / "Social Supply Network"), most of which are grocery stores. This network of retailers and its interaction with program beneficiaries (the stores' customers) play a central role in this study.

There is a standardized procedure for joining the network.[8] First, the government executing agency[9] regularly opens calls for applications in certain districts and, via a community liaison, distributes application forms and encourages local stores to apply. Second, interested retailers fill in and submit the application. Third, the application is reviewed and checked by the executing agency. Inspectors visit the stores and record information on the applicants' infrastructure and access to basic services, including a phone line – a potentially costly item for the stores, but one that is necessary in order for the debit card or magnetic stripe reader to operate. Finally, scores are assigned to the applications and stores are allowed to join the network or not, depending on their score and on the number of affiliated stores already in the district in question.

Prima facie, this application procedure is cost-free for retailers. However, entering the network can still be costly for two reasons. First, many of these stores operate informally. The application requires them to provide a tax identification number and to have a bank account, which increases the (perceived) probability of being audited. Second, some retailers may be asked to do some upgrading, which could involve buying a card reader, connecting to a phone line, having a power generator and satisfying some minimum sanitary conditions.

The retailer's payoff for participating in the network may be a larger sales volume and higher profits, if the retailer enjoys some market power. In fact, in 2005, at the outset of the program, it was unclear to many retailers what the benefits of participating in the network might be. It was not yet clear how many CCT program beneficiaries (i.e. these stores' customers) there would be or how many nearby competitor retailers would be in the network. As a consequence, only a few retailers applied for entry in 2005. As a way of making affiliation attractive to retailers, the authorities decided to limit the number of stores that could join the network based on the number of beneficiaries in each district. In many districts, this effectively gave local market power to some retailers. In fact, the executing agency discovered that some stores had increased their prices and were offering a more limited variety of products than stores outside the network.[10]

[7] This debit card can be used only by the head of household.

[8] The standard process of affiliation and the operation of the retail network are governed by a set of administrative rules detailed in "Reglamento de Funcionamiento de la Red de Abasto Social" from ADESS.

[9] The Social Subsidies Administration (Administradora de Subsidios Sociales (ADESS)).

[10] See the report by ADESS entitled "Proyecto de Ampliación de la Red de Abasto Social" (pp.11-13).

In our setting, *market entry* means that a store is allowed to sell to program beneficiaries in the relevant district. Retailers entering the program network (*entrants*) were already operating in the non-CCT market. Entrants are very similar to retailers already affiliated with the program (*incumbents*). They sell products of similar quality at similar prices, and the stores' characteristics are also similar. The only meaningful difference is that the entrants are smaller, as they have, on average, about 12% fewer sales and 11% fewer employees than incumbents.

Table 1 presents some descriptive statistics on both customers and retailers in the areas under study. There are several important facts we would like to highlight.

Retailers are small, owner-run "mom-and-pop" shops. They sell mainly non-perishable food products and typically supply a very limited number of fresh products (fruits, vegetables or dairy products). This is partly due to storage limitations and partly to the types of goods that the population in these areas consumes. In fact, 75% of the total amount spent on food by consumers in these areas is spent on non-perishable goods.[11]

The people who shop in the areas under study are typically poor, with their earnings being equivalent to slightly more than one quarter of the country's per capita GDP. As is typical in many Latin American countries, residential segregation is prevalent in the Dominican Republic, with poor households clustered in different areas than middle- and high-income households (Bouillon, 2012). Thus, the markets under analysis are segmented by income, and most of the households whose members shop in the retail stores that we are analyzing are poor. Within these markets, from the retailers' point of view, the only difference between program beneficiaries and non-beneficiaries is the usage of the CCT debit card.[12] Beneficiaries shop during regular business hours. In incumbent stores, program beneficiaries can use the CCT transfer money to purchase only food products (of any brand or variety). A few items, such as alcoholic beverages and tobacco products, are specifically excluded from purchases made with the debit card, although, naturally, they could be bought by program beneficiaries if they pay for those products with cash.[13]

CCT beneficiaries represent a large share of the market for these retailers. Using information on sales, on the number of beneficiaries in the areas under study and on the program transfers, we estimate that about 56% of these retailers' sales are financed directly by the CCT transfers. However, when program beneficiaries shop in these stores, they buy

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[11] For this reason, in our analysis we focus on a limited set of products that capture about 85% of this set of non-perishable goods. See the Appendix I for more details about the types of goods under analysis.

[12] Simply being poor is not sufficient to make a person eligible for the program. People also need to prove that they are citizens of the Dominican Republic. It is estimated that 30% of households whose members are living in extreme poverty lack the proper documentation to be eligible for social programs.

[13] The CCT executing agency drew up a list of products that cannot be sold to beneficiaries using the debit card (e.g., alcohol). The CCT program regulations also explicitly prohibit fictitious transactions in exchange for cash.

products both with the CCT debit card and with cash. Because they typically shop in only one store on any given day, since the transaction costs of going to more than one shop are high, a store's membership in the CCT retail network provides it with some measure of market power. Using information on food expenditure, and assuming that all spending on groceries is done within the district where the members of the household live, we estimate that as much as 96% of an incumbent's sales could potentially come from program beneficiaries. The importance of program beneficiaries for these retailers is also confirmed by self-reported measures: 96% of retailers located in the areas under study and currently in the network (incumbents) claim that being affiliated with the CCT program has increased their sales.

The data suggests that there is room for local market power and for price discrimination. Program beneficiaries' mobility is limited: only 15% of them own a car or a motorcycle and, as a consequence, 95% of them shop only in a retailer in the program network located within 10 blocks of their house. As a consequence, retailers can potentially wield local market power. In the smaller shops, items are placed on shelves located behind the counter while, in the larger establishments, items are on shelves that can be browsed by the customer. The prices of the different items are not always in plain view. Only about 41% of retailers have prices posted where the customer can see them. Although we do not have direct evidence of it, this setup seems to provide an opportunity for third-degree price discrimination, since, because retailers know that certain customers are CCT beneficiaries who will be paying with a debit card, the retailers could charge them a different price. In fact, only 44% of retailers stated that they never bargain over prices with their customers.

Despite the beneficiary population's low degree of mobility, the market could be much more competitive if the government's entry restrictions were not in place. Almost 95% of customers could identify a non-affiliated store within a 10-block radius from their house. These potential entrant stores are very similar to the incumbent stores and have entered freely into the non-CCT market, which is a more competitive environment.[14]

4. Experimental Design

The particular context in which the network of retailers operates has been a cause of concern for the government. The market power wielded by the stores belonging to the network allows them to increase prices and to offer a more limited variety of products than stores outside the network. This implies a loss of consumer surplus and therefore a potential welfare loss. In addition, over time the CCT program has been increasing the number of beneficiaries, which exacerbates these problems. In response to this situation, the authorities have designed a plan for the expansion of the retail network. The goals of the

[14] See Appendix Table A1.

plan are to address the needs of all the beneficiary clientele of each store and encourage competition among those stores in order to increase the effectiveness of the subsidies awarded under the program.

In this context we worked with the CCT executing agency and the Inter-American Development Bank (IDB) to propose a way of expanding the network as a possible means of responding to the concerns raised by the government of the Dominican Republic. We proposed and designed an experimental evaluation. The actual implementation of the experiment was the responsibility of the CCT executing agency based on guidelines provided by the IDB.

The intervention consists of an exogenous randomized increase in the number of retailers associated with the network across districts.[15] We use this randomized variability in the entry of new retail stores into the network servicing the CCT beneficiaries as a means of evaluating the effect of an increase in competition on prices, product quality and retail service quality.

The districts used in this experiment were identified by the CCT executing agency with two considerations in mind. First, there needed to be, before treatment, a relatively strong demand for consumption goods per retailer and, second, it had to be feasible, at least a priori, to expand the number of stores in the district. Relatively high-demand districts were defined as those expected to have more than 100 program beneficiaries per retailer. in order to increase the possibilities of expanding the product supply by recruiting new retailers, it was decided that the districts should be located in municipalities with a population of over 15,000 in which at least 30% of the population was urban. In addition, they had to have at least one non-affiliated retailer that would be interested in joining the network. Ultimately, 72 districts were included in the experiment. The intervention was implemented in three stages.

First, before randomization, between December 2010 and May of 2011, the CCT executing agency collected applications from retailers that wanted to become part of the network. Each one of the 72 districts was built up starting from a targeted neighborhood that was in an area in which the executing agency was particularly interested in expanding the retail network. The aim was to have at least three candidates for entry in each neighborhood. However, as it turned out, this was not always possible, either because there were not enough applicants or because some of the applicants were not eligible. Eligibility was assessed by the executing agency on the basis of visits to the stores and store audits. In those cases in which the search for potential entrants yielded few feasible candidates, the

[15] The National Statistics Office divides the country into provinces, municipalities, sections and neighborhoods. This classification was being used by the CCT executing agency and, to simplify the project's implementation, the evaluation was based on that same convention. Districts are composed of either one neighborhood or two adjacent neighborhoods.

executing agency expanded the search area to include nearby areas (which we will refer to as "non-targeted neighborhoods"). The search for candidates was undertaken in all the neighborhoods covered by the study. Non-targeted neighborhoods were adjacent to targeted areas and were also places in which, according to administrative data, program beneficiaries went to do their shopping. Given the way in which they were defined, these districts are akin to local markets. The 72 selected districts were then used to provide the framework for randomization.

Table 2 presents statistics that provide an overview of the distribution of distances between retailers within districts and the distances between districts (computed using pre-intervention data).[16] The median distance between retailers within districts was about 246 meters, and the median distance between districts was approximately 3.4 kilometers. Within the corresponding provinces, the districts were far apart.

Each district was then assigned a random number in the set {0, 1, 2, 3}. This defined the number of potential new entrant retailers that the executing agency would try to recruit. Actual affiliation could, in principle, differ from the intended/randomized affiliation because of a shortage of eligible applicants for entry into the network (*noncompliance*). Another source of noncompliance could be a failure on the part of the CCT executing agency to follow the intervention protocol.[17]

Table 3 shows that, before treatment, there were some 341 retailers operating in the network within these 72 districts. Under full compliance, the design was such that a total of 99 new retailers would enter the network, which would represent an intended increase of 29% in the number of stores. A total of 21 districts were randomized to receive no entry of new retailers (*non-intention-to-treat districts*), while 51 districts were randomized {1, 2, or 3} for retailers to enter the network (*intention-to-treat districts*).

Affiliation occurred as indicated in the protocol. When the number of eligible applicants was less than or equal to the number of randomized new entrants, all of them were affiliated with the network. In those cases in which the number of applicants was larger than the number assigned by randomization, the entrants were selected randomly from among the eligible stores. The actual enrollment in the network was carried out in May-June 2011 by the executing agency using a standardized procedure.

Table 4 describes randomized and actual entry. A total of 61 retailers entered the network in these districts, thereby increasing the number of retailers operating in these markets by 26% in the treated areas. In 38 districts (53%), randomization was achieved (*perfect*

[16] Even though we collected information on the location of the retailers in our sample, the National Statistics Office of the Dominican Republic does not have the type of information that would be needed in order for us to map these neighborhoods and districts. We have therefore computed the location of the district as the centroid of the location of the retailers in our sample for each district.

[17] We performed an independent audit of compliance by calling all the retailers in the randomization sample.

compliance), while, in 28 (39%) districts, fewer retailers than expected, according to our randomization exercise, actually entered the network (*noncompliance*) and, in 6 districts (8%), the executing agency partnered with more retailers than had originally been provided for.

5. Data and Measures

Baseline retailer and household data was commissioned by the IDB and collected by the *Centro de Estudios Sociales y Demográficos* (Social and Demographic Research Center), a highly qualified local firm, in April and May 2011. The endline data was collected in December 2011, six months after the intervention was completed. Throughout the project, we also obtained administrative information from the executing agency.

We will consider three samples: the sample of retailers (both incumbents and entrants in targeted and non-targeted neighborhoods) located in the entire randomization sample of 72 districts; the sample of all retailers and consumers located in targeted neighborhoods within these districts; and the sample of incumbent retailers or consumers that patronize those retailers in targeted neighborhoods.

The survey of retailers included the majority of incumbent retailers in the targeted neighborhoods (95%) and a large share of incumbent retailers in the non-targeted neighborhoods (65%). It also covered all entrant retailers.[18] The survey of beneficiaries was designed on the basis of a sampling frame that included all beneficiaries in the 72 targeted neighborhoods. The survey did not collect information on beneficiaries located in non-targeted neighborhoods, however. Its sample included about 30 households per neighborhood; these households were drawn randomly from the sampling frame.[19]

The retailer questionnaire was designed to collect information on the stores' geographic location; on the owners; on their participation in the CCT retail network; on sales, marketing and competition; and on employees and investment. It was also designed to obtain very detailed information on prices and on the products sold by the retailers. The household questionnaire was to be answered by the person in possession of the debit card and therefore the one who did the shopping for the household. The questionnaire included queries on the physical characteristics and composition of the households, CCT program participation, the socioeconomic characteristics of the members of the households, and consumer behavior and spending.

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[18] Table A1 describes the sample sizes associated with each of these three samples both at baseline and at endline.
[19] The final sample has a mean and a median size of 30 per district; the smallest district has 24 beneficiaries, and the largest 60.

The retailer survey included questions about product prices –our main outcomes of interest. During the pilot stage, we determined that, typically, only a limited number of products in these stores were bought by program beneficiaries. These goods included bread, rice, pasta, cooking oil, sugar, flour, powdered milk, onions, eggs, beans, cod, canned sardines, chicken, salami and chocolate. These goods represent 85% of all non-perishable food products and 60% of all food products bought by an average household.[20, 21] In order to guarantee comparability, for each one of these 15 products, we pre-specified the unit of measurement, asked owners if the product was typically available at their stores, and then asked for information on the price, variety and brand of the cheapest available option.[22]

Since individual prices vary substantially, in order to gain statistical power in the analysis, we will focus on the average price of the basket of 15 products sold by the retailers. The retail price of the basket is computed as the average price of items included in the survey. We study two versions of this basket price: one that was computed by weighting each product by the proportion of total household expenditure (on the 15 items) that it represented, measured at baseline, and another in which a simple average was used for the computations. Additionally, we present results for a pooled model of all individual prices.

The household survey questionnaire included a module on expenditure in which we asked about total expenditure, brands, varieties and quantities of the same 15 items included in the retailer questionnaire. We use this to build an alternative and independent measure of the average price of the basket. For each item, we derive the price paid by the consumer from the ratio of the total expenditure on that item and the total number of units bought. Since some households did not report expenditure for all 15 items, in order to avoid a composition effect based on possible non-random non-responses on prices, we standardize each household product price by dividing it by the average price of that good as reported by all households in our sample. We then use these inferred demeaned prices to construct a weighted and an unweighted average price, just as we did in the case of retailers. In addition, the household survey includes questions that allow us to match households to retailers. We use this information to measure the prices in the retail stores that are in our sample more accurately.

[20] The other 40% of food expenditure corresponds to expenditure on dairy products, fruits, vegetables and meat products. These products are rarely sold by the retailers included in our study. (These types of products are typically sold in specialized stores or in street markets.)

[21] A secondary motive for focusing on a limited set of products was that it greatly simplified data collection and therefore reduced its cost.

[22] We decided to focus on the cheapest alternative for three reasons. First, it was a simple way of anchoring the survey responses provided by retailers. Second, as we discuss below, it allows us to capture changes in the quality of the products sold. Third, many of the consumers located in these areas are program beneficiaries, and the executing agency was interested in assessing the availability of inexpensive options in these product groups.

Let $\bar{P}_{js}^{R}$ be the average price in district s of product j computed using retailer information R that considers the cheapest available option for each product. Similarly, let $\bar{P}_{js}^{C}$ be the average price in district s computed using consumer information C that considers the goods actually bought by consumers. The average relative price in the district ($\bar{P}_{js}^{R}/\bar{P}_{js}^{C}$) is a useful statistic for assessing how close these two measures are. Note that, without measurement error in the measures of prices, this statistic is bounded from above at 1 by the way the data was collected. We find that the average relative price for all products and districts is 0.99.

In order to assess the quality (as captured by the brand and variety) of the goods sold by the stores, we use the brand/variety information gathered in the retail survey.[23] Conceptually, we want to measure whether observed changes in prices are the effect of a drop in the prices of goods of the same quality or the effect of a change in the quality of the products sold by the stores. Figure 1 shows the quality ladder for a given good in terms of price/product quality. Before treatment, the quality ladder is the solid line and the cheapest product carried by the store has a price/quality combination that is depicted as point A. Suppose that, after treatment (entry of a potential new competitor), we observe a decrease in price. This could be the result of either of two opposite effects (or a combination thereof). In one scenario, in response to more competition, the retailer chooses to sell a product of lower quality at a lower price, thereby moving along the ladder to point B. In another scenario, the quality ladder itself shifts down, and the retailer sells a product of the same quality as before treatment but at a lower price (point C). In the empirical analysis, we attempt to distinguish between these different possible scenarios.

For each store, we have information on the price and quality of the cheapest option available at that store. For each product, we rank all brands/varieties reported in the sample according to their average retail price as observed at baseline (with a higher rank assigned to more expensive brands). We then divide that rank by the total number of brands available in the economy at large to obtain a percentile rank. We compute a quality index per store as the average percentile ranking of the 15 products.[24] Thus, for example, if a store carries the most expensive brands/varieties of all 15 items, its percentile rank will be equal to one and we infer that its average quality is higher than a store which carries the cheapest brands for all 15 items (whose percentile rank will be close to zero).

[23] There is a large body of literature that relies on prices or unit values of goods to infer the quality of products. See, among others, Schott (2003), who documents systematic differences in US import unit values that support this assumption.

[24] In the endline survey, 47% of the stores reported all 15 items, 76% reported at least 14 items, and 91% reported at least 13 items. In the cases of stores that did not report all 15 items, we left the missing items out and computed a simple average of the reported items or a weighted average (with weight rescaled to sum to 1).

We are also interested in the quality of the service provided by the stores.[25] One dimension of service quality is the range of product choice available to consumers. Figure 2 shows the quality ladder for a given good. The range of prices for that product in the economy at large is (G-D), and the corresponding range of qualities is (K-H). We define the price range offered to consumers as (F-E)/(G-D) and the quality range offered to consumers as (J-I)/(K-H). One possible effect of competition is to trigger an increase in service quality in the form of an expansion of the ranges of prices or product qualities offered to consumers.

To capture this latter effect, we asked the retailers to name the three products, among the list of 15, that they sell the most to persons using the CCT debit card. For each of these three products, we asked about the price, the variety, the brand and the unit of measurement. For each product we first rank the brands/varieties in the sample in order to assess product quality in the same fashion as explained above. Then we compute a quality range as the (percentile) difference between the highest- and the lowest-ranked brands. Once we have computed the quality range for each product, we calculate the average quality range by store as a simple average.[26] We also measure the range of choice using the average price range by store. For each of these three products, we take the price difference between the most expensive and the cheapest available options and then compute the average price range across the three products.

We also assess service quality by looking at direct measures. To do so, we measured the average number of brands offered in each district and asked consumers to rate –from 1 (very bad) to 10 (excellent)— their latest experience shopping in a retailer affiliated with the network and to provide information on the amount of time they spent during their visits to the retailer. In addition, we have measures of store cleanliness and of the number of employees working on site to serve shoppers, as well as an indication as to whether or not the store offers home delivery service.

Increased competition can affect not only prices but also the quantities sold. In order to truly capture this effect, we would have had to have retailers report on the product quantities that they sold, but this proved to be infeasible in practice. As an alternative measure, we analyze the number of clients per day, the share of program beneficiaries who visit the participating stores and total retail sales. We also study the probability that beneficiaries may switch to a new entrant retailer within the network.

Throughout this paper, we use a set of district-, consumer-, and retailer-level measures as control variables to assess the validity of the design. For instance, we use administrative information, disaggregated by district, on the total number of beneficiaries, the number of

[25] We do not focus on aspects of service quality that would require large investments, since these kinds of changes would probably take longer than six months to complete.

[26] In this case, we did not use all 15 products but instead focused on the 8 most popular products (rice, oil, sugar, pasta, eggs, milk, beans and salami). In all, 97% of the stores reported 3 products in that set, and the other 3% reported 2 products in that set.

retailers operating in the CCT network and reported sales.[27] For a full description of all these outcome and control variables, see the Online Appendix.

6. Empirical Strategy

The advantage of random assignment is that the intention to treat is exogenous. Under random assignment and perfect compliance, there is no selection into treatment status, and therefore identification of the average treatment effects is straightforward. As we have shown in Section 4, we have noncompliance especially, but not only, in districts in which the entry of two or three stores was randomized. In order to gain statistical power, we base our analysis on a parsimonious model in which we pool all the treatments into a single-treatment categorical dummy variable that captures whether the district was randomized to receive one or more new stores, Z_S.

Although we had almost 50% noncompliance in the intensive margin of entry, we have better compliance when considering the extensive margin (i.e., whether there is at least one entrant into the market). Table 4 shows that in 51 districts (70%) we had entry in places randomized to entry and we observed no-entry in places randomized to no-entry. On the other hand, 21 (30%) of the districts were randomized to entry and actually observed no entry (noncompliance). Ceteris paribus, compliance was in fact better in places where we randomized fewer stores to entry. This is consistent with the idea that rents largely dissipate quickly as the number of competitors in the market rises (Bresnahan and Reis (1991)).

Thus, in our main specifications, we estimate the following equation:

$$Y_{is} = \alpha + \gamma Z_S + \beta X_{is} + \varepsilon_{is} \qquad (2)$$

where i could be a store or a consumer (depending on the outcome) located in district s. Y_{is} represents any of the outcomes under study observed after treatment. The parameter γ captures the intention-to-treat effect of increased levels of competition on the outcome under consideration.[28] X_{is} is a vector of pre-treatment characteristics. As is common

[27] We have administrative records on total sales for 2009-2010 as reported by the banks operating the debit cards. Data for 2011 was not made available to us. In the case of that data, as opposed to what would be possible when using scanner data, we cannot disaggregate individual product items, quantities or prices.

[28] Some of the variables under study are limited dependent variables (LDVs). The problem of causal inference with LDVs is not fundamentally different from the problem of causal inference with continuous outcomes. If there are no covariates or the covariates are sparse and discrete, linear models (and associated estimation techniques such as 2SLS) are no less appropriate for LDVs than they are for other types of dependent variables. This is certainly the case in a randomized experiment where controls are included for the sole purpose of improving efficiency, but where their omission would not bias the estimates of the parameters of interest.

practice in the literature, this vector always includes the pre-treatment value of Y_{is}. ε_{is} is the error term, which is assumed to be independent across districts but is allowed to display flexible correlations within districts.

Naturally, we are interested in the actual causal effect of increased competition on prices and quality.[29] Thus, we also estimate the following equation using two-stage least squares (2SLS):

$$Y_{is} = \alpha + \gamma T_S + \beta X_{is} + \varepsilon_{is} \qquad (3)$$

where T_S is a dummy variable that captures actual observed entry into the market. We instrument T_S with Z_S.

Randomization occurred at the district level. Therefore, the majority of our analysis uses data at the retail or consumer level, with standard errors clustered at the district level, and is robust to heteroscedasticity.

7. Results

Internal validity. When treatment is randomly manipulated, it is expected that the intention- and non-intention-to-treat groups are equivalent before treatment in every important sense (including observable and unobservable characteristics). The only significant difference between the two groups is that one has been randomized into treatment and the otherwise probabilistically identical group has not. It is therefore common practice to test for a statistical balance of pre-treatment observable variables in order to assess the success of randomization.

Table 5 shows the mean characteristics of districts, retailers and consumers in the non-intention-to-treat (column 1) and intention-to-treat (column 2) groups. Column 3 shows the p-value of the null hypothesis that both means are equal. We show the balance table before treatment for three sets of variables: pre-treatment outcomes, variables that are included as control variables (covariates) in the models estimated, and a few other informative characteristics. There are three sets of results that we would like to highlight.

First, overall we observe that the mean characteristics of these groups are well balanced. We find one statistically significant difference at conventional levels out of 32 variables tested. In spite of this, as a robustness analysis, we added these variables as controls in the estimated models and, overall, the results do not change.

[29] We do not expect general equilibrium effects to result from this experiment, given that the intervention did not manipulate the transfers to poor households. Moreover, the number of markets involved in the intervention was very small relative to the whole country.

Second, as we mentioned in Section 4, the districts are composed by (originally) targeted and non-targeted neighborhoods. The share of districts with non-targeted neighborhoods is statistically similar in the intention-to-treat and non-intention-to-treat groups. The share of retailers in targeted neighborhoods is also balanced across groups.

Third, Table 5 also provides a better picture of the setting in which the experiment took place. The districts under analysis had about 630 consumers using the CCT debit card and an average of 6 stores already operating within the retail network at baseline. Both the demand (number of beneficiaries) and the supply (retailers in the network) had been increasing in the years prior to the experiment. These characteristics are balanced across intention-to-treat groups. These two groups also have similar populations in terms of their demographic characteristics: both have populations whose members have low levels of education, are relatively poor and are living in urban areas. The average store in our sample has 4 employees and monthly sales of approximately US$ 9,000. All retail-level control variables, including all the outcome variables as measured before treatment and some demographic characteristics of the owner, are balanced. The statistically unbalanced variable is the number of employees, with retailers in the intention-to-treat group having about 0.5 employees more than the average retailer in the non-intention-to-treat group. The last panel shows that the mean characteristics of consumers (households) in our sample are also balanced between intention- and non-intention-to-treat groups.

Prices. In Table 6, we present the effect of entry on log prices. Panel A shows retail prices, while Panel B shows prices as measured using household information. In the case of retail prices, we provide estimates for three samples: the whole sample, the sample of retailers located in target neighborhoods, and the sample of incumbent retailers in those target neighborhoods. In the case of households, we provide estimates for all households in the target neighborhoods and all households that bought their goods from incumbent retailers located in target neighborhoods.[30] Column 1 shows the number of observations used in the estimation and Column 2 shows the number of clusters (districts) where those observations were located.[31] Columns 3-5 show intention-to-treat estimates in which the main independent variable is a dummy for randomized entry (i.e., 1 (Randomized entry>0)). Each model in those columns includes a different set of control variables, which is specified in the bottom panel of the table.[32] Columns 6-8 show instrumental variable results in which the dummy for observed entry (i.e., 1 (Observed entry>0)) was instrumented using the randomized entry dummy. In each model we report point estimates, clustered standard

[30] The reader will recall that we did not collect household information in non-targeted neighborhoods.

[31] There is some variation in the number of districts/clusters across samples. Two districts only have incumbent retailers located in non-targeted neighborhoods. Therefore, the sample of incumbent retailers in targeted neighborhoods has 70 clusters. Also, there is one district in which there are no consumers who buy products in an incumbent retailer, so in that sample we have 71 clusters.

[32] There is very little missing data, as there is complete information for all variables used in all columns for 97% of the sample of retailers.

errors at the district level in parenthesis and, for the case of IV, the first-stage F-statistic to assess the strength of the first-stage regression (shown in brackets).

Across all samples and models, we find sizable, statistically significant decreases in prices. Since there is noncompliance, the estimates of the average causal effects are always larger than the estimates of the intention-to-treat effects. Also, for both estimands (though more pronounced in the case of the IV), the estimates are larger in absolute value for the sample of incumbent stores in targeted neighborhoods. The estimators are also larger for the sample of the targeted neighborhoods than they are for the sample as a whole. However, the effects are not statistically different.

Regarding the size of the effect, considering the simplest IV model in Column 6, it is estimated that entry into the network decreases prices by 5.6% in the case of the sample of incumbent stores in the targeted neighborhoods. Intention-to-treat yields smaller estimates: in the same specification in Column 3, the decrease in prices is 2.6%, with the estimates not varying much across specifications. This is also consistent with having better compliance in locations with fewer incumbent retailers.

In the second panel of Table 6 we show estimates of completion on prices using price measures derived from the consumer data. The estimated effects are similar to those estimated using retailer-level data. On the one hand, this is not surprising, since, as we showed in Section 5, these two price measures are similar. On the other hand, it is reassuring because these measures are independent of one another.

We run a number of robustness analyses which, to save space, have been relegated to the Online Appendix. First, as expected in an experimental setup like ours, adding control variables does not change the point estimates noticeably. However, in our case, it does not add to (and sometimes worsens) the precision of the estimates. Appendix Table B1 presents results in which we control for several sets of pre-treatment variables. In each panel we also show the joint significance of each set of covariates using a standard Wald test. As can be seen in the table, many sets of coefficients are (jointly) not significantly different from zero at standard confidence levels. This can increase the standard errors (since it is a problem akin to adding irrelevant regressors). Second, Appendix Table B2 shows that the point estimates are similar when average prices constructed using simple (i.e., unweighted) averages are used as the dependent variable.[33] Third, Appendix Table B3 shows results in which we estimate equations (2) and (3) but the outcome Y_{jis} is the log price of product *j* in retailer/household *i* in district *s*. We include product fixed effects in the model. In other words, rather than estimating the effect on an average price, we pool all the prices and estimate an average treatment effect over all prices. Point estimates in this pooled model

[33] We do not have any a priori preference for using one measure (weighted) over the other (unweighted). The point estimates are similar across models and samples using both measures. The only difference is that the results for the weighted average price are more precise estimates.

are of similar magnitude to those presented in Table 6. Fourth, we estimate individual treatment effects for each of the 15 products under analysis. The results are shown in Appendix Table B4. Overall, the point estimates are negative. Statistical significance varies across products and, as expected, we have less power to reject the null of no treatment effect in some equations. Overall, we consider this set of the results of entry on prices to be robust.

In Table 7, we look at the intention-to-treat effect in districts where one store was randomized for entry and in locations where more than one store was randomized for entry. The effects are of the same order of magnitude as the ones presented in Table 6. More importantly, they are larger in districts where the entry shock is larger (i.e., where more than one store was randomized for entry), although the results are not precise enough to rule out the possibility that the estimands are equal.

We use the experiment to approximate a price-elasticity of entry by estimating the following model:

$$\log(p_{is}) = \alpha + \delta \log\left(\frac{n_1}{n_0}\right) + \beta X_{is} + \xi_{is} \qquad (4)$$

where $\log(p_{is})$ is the log of the average price, n_0 is the number of retailers before treatment and n_1 is the number of retailers observed in the market after treatment. As a result of noncompliance, the causing variable (i.e. $\log(n_1/n_0)$) is potentially endogenous. Therefore, we estimate equation (4) by 2SLS using $\log\left(n_1^{Rand}/n_0\right)$ as an instrument for $\log(n_1/n_0)$, where n_1^{Rand} is the number of retailers that would have been observed under full compliance (considering the intensive margin of randomization).

The results are presented in Table 8. Using the retailer data, we find that the price-elasticity of entry is about 0.052. The results are larger for incumbent retailers, which suggests that, after entry, they adjust their prices more than the entrants do. The results are a bit smaller in absolute values and more imprecise when using household-level information to measure this elasticity. However, it is nonetheless reassuring that the result holds when an independent source of information is used.[34]

Table 9 presents treatment effects of entry on prices for two samples of retailers: those that are not in the CCT market and those that are located in non-targeted neighborhoods. We found no treatment effect for either of these two samples. In the case of the non-CCT retailers, this was to be expected because they operate in a different (competitive) market. However, we take these results with a grain of salt: notice that the number of districts covered by these samples is smaller than the ones involved in the experiment, and the size of the sample of retailers is also small.

[34] Appendix Table B5 presents the results obtained when control variables are added. The results are robust, and point estimates are in general larger in absolute values than the ones shown in Table 8.

Product quality. As discussed in Section 5, it is possible that a price decrease may occur because there has been a decrease in the quality of the products available in the store. Table 10 shows the effect of entry on the index of product quality in the three samples. Overall, we find very small effects and cannot reject the null of zero effect of entry on product quality in any of the specifications or samples. We interpret this result as evidence that, after entry, there was no change in the quality of the products sold by the stores. This result also helps us to better interpret the results on prices as a pure price effect while product quality is held constant.

The effect on entry on product quality is also very robust. Appendix Table C1 presents a set of results in which we add controls. We cannot reject the null hypothesis of zero effect on product quality in any specification. Appendix Table C2 presents results on product quality similar to Table 10, but in this case the index is unweighted. Again, we find no significant effects, and the point estimates are smaller in absolute values than those shown in Table 10. Appendix Table C3 presents results for a pooled model similar to the one used to estimate prices. The results are again small and not statistically significant. Appendix Table C4 shows the results for quality for each individual product. In general, coefficients are not statistically significant. Moreover, for those products for which the coefficients are statistically different from zero, the point estimates are positive, which suggests that, if anything, product quality actually increased in some cases.

Service quality. Entry does not appear to have a strong effect on the quality of the service provided by retailers in our sample. Table 11 shows the effect on targeted neighborhoods. The top panel shows a set of results that show whether the variety of products has increased since entry. And, in fact, there seems to have been some increase in the range of products offered to consumers, even though the estimates are not statistically significant at conventional levels. Stores seem to have introduced other brands or varieties at similar prices. The bottom panel presents more direct measures of service quality. Again, most of the results are not statistically significant. The only impact seems to be on how customers rate their shopping experience, with that rating improving in treated areas.[35]

Other effects of competition. Table 12 presents treatment effects for customers. The negative effect of entry on prices seems to have been fueled by a reduction in the number of shoppers who went to retail stores in treated areas. We find that entry increased the probability that shoppers would switch to an entrant retailer and that the percentage of customers who are CCT beneficiaries declined.[36]

[35] Results for the sample of all districts and for the sample of incumbent retailers are shown in Appendix Tables D1 and D2 and are very similar to the ones shown in Table 11.
[36] Results for the sample of all districts and for the sample of incumbent retailers are shown in Appendix Tables E1 and E2 and are very similar to the ones shown in Table 12.

8. Conclusion

We conducted a randomized field experiment to evaluate the effect of increased competition on prices and quality in the context of a CCT program in the Dominican Republic. This program provides monetary transfers to poor families which can be spent only by using a debit card that is not accepted anywhere except in a network of grocery stores that are affiliated with the program. The CCT executing agency was concerned that the grocery stores in the network might be capturing rents from these transfers. We proposed an expansion of the network as a possible solution for this potential problem.

Randomization was conducted at the district level. In all, 72 districts were randomized to {0, 1, 2, 3} new entrant retailers. Actual affiliation was subject to noncompliance, which was greater in the districts that were randomized to a large number of new entrant grocery stores. In order to gain statistical power, we based our analysis on a parsimonious model in which we considered only the extensive margin of entry. Thus, we studied the effect of market entry on prices and quality. We found a significant and very robust reduction in prices as a result of the increase in competition, but we did not find robust improvements in product quality or service quality six months after the intervention. We did find, however, that shoppers consistently gave a higher quality rating to stores that were facing increased competition.

We then explored the impact on prices further by imposing some degree of structure. We estimated the price-elasticity of entry at 0.08. This means that, if competition increases by 1% (measured as the percentage increase in the number of stores operating in the market), then prices drop by 0.06%.

Our paper is informative for the literature on competition and efficiency. It is the first paper to provide field experimental evidence that increased competition significantly affects prices, even when the initial number of stores, on average, was not that small. As has long been argued by economists, competition increases consumer welfare. One possible interpretation for this result, which follows from our simple model, is that members of the poor population in developing countries mainly care about prices when shopping for groceries and are much less concerned about the types of quality dimensions that may come into play in the short term.

Our results are also informative for the design of social policies. They suggest that policymakers should pay attention to supply conditions even when they only affect the demand side of the market. Often, social programs subsidize consumer demand by transferring resources to households. If the supply side does not operate in a very competitive environment, part of the resources targeted for the needy population will leak into the profits of the firms that are serving them. Naturally, the government could envision other options for dealing with this potential problem. As was discussed at one point in the

Dominican Republic, one obvious possibility would be to attempt to regulate the market. However, it has been widely recognized that the government would have to deal with an array of informational constraints in order to do so. Regulation capture is another threat that has often been highlighted in the literature as an impediment to successful market regulation. Our findings, on the other hand, indicate that introducing competition provides an effective means of avoiding rent capture by suppliers.

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Tables

TABLE 1. DESCRIPTIVE STATISTICS

	Mean
A. Retailer characteristics	
Owner works in the store	0.822
Number of employees	5.025
% of sales financed by CCT	0.561
CCT beneficiaries' food expenditure / sales	0.962
Retailers with an increase in sales after entering the CCT program	0.964
All prices posted for public view	0.415
Never bargain over prices	0.441
C. Consumer characteristics	
% of households that own a car or motorcycle	0.163
% of households that shop in a retail store within 10 blocks of their house	0.550
% of households with a non-CCT retail store within 10 blocks of their house	0.958
Individual income / GDP per capita	0.270
Number of retailers in which households usually shop	1.034
Beneficiaries aware of prices before shopping	0.281

Note: The mean shown for each variable corresponds to the entire sample at baseline.

TABLE 2. WITHIN- AND BETWEEN-DISTRICT DISTANCES BEFORE TREATMENT

	Number of districts	Number of retailers in the network (pre-treatment)	Distance (in meters) between retailers within districts				Distance (in meters) between districts			
			25th percentile	Median	75th percentile	Mean	25th percentile	Median	75th percentile	Mean
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
All districts	72	341	166	246	509	586	1,182	3,416	15,705	12,246
By province										
Barahona	7	11	43	83	115	80	671	928	1,114	905
Distrito Nacional	11	99	134	170	465	710	1,038	1,862	3,567	2,280
Duarte	9	31	168	222	307	240	600	796	1,320	1,077
La Vega	5	12	45	182	1,012	413	1,329	1,924	2,450	1,917
San Cristobal	6	34	191	211	606	374	855	4,719	13,472	6,397
San Pedro de Macoris	5	23	233	248	577	353	3,682	4,353	7,449	4,979
Santiago	8	31	441	576	1,042	650	1,318	2,083	2,763	2,131
Santo Domingo	17	88	222	332	589	963	5,839	34,647	40,575	24,407
Valverde	4	12	265	294	323	294	968	38,235	38,444	25,813

Note: Column [1] displays the number of districts per province. Column [2] shows the number of retailers per province. Columns [3] to [10] show the different average distances between retailers, within and across districts, in all districts and per province. The values are expressed in meters.

TABLE 3. INTERVENTION AND RESEARCH SAMPLE

Number of retailers randomized for entry	Number of districts	Number of neighborhoods in each district		Number of incumbent retailers in sample
		Targeted	Not targeted	
0	21	21	6	107
1	18	18	5	71
2	18	18	6	81
3	15	15	8	82
Total	72	72	25	341

TABLE 4. RANDOMIZED AND ACTUAL ENTRY

Randomized entry (number of retailers)	Observed entry (number of retailers)					Number of districts
	0	1	2	3	4	
0	17	2	2	0	0	21
1	3	14	1	0	0	18
2	5	8	5	0	0	18
3	5	3	4	2	1	15
Number of districts	30	27	12	2	1	72

Note: Each entry shows the number of districts by randomized/observed treatment.

TABLE 5. DIFFERENCES IN PRE-TREATMENT SAMPLE MEANS

	Control: no entry	Treatment: some entry	p-value of difference	Number of obs.
	[1]	[2]	[3]	[4]
A. District characteristics				
Log (total beneficiaries - 2010)	6.441	6.453	0.960	72
	[1.016]	[0.865]		
Change in log (total beneficiaries - 2009/2010)	0.211	0.172	0.380	72
	[0.200]	[0.160]		
Log (sales - 2010)	11.149	11.340	0.573	69
	[1.466]	[1.165]		
Change in log (sales -2009/2010)	1.033	1.231	0.803	67
	[2.709]	[2.991]		
Number of incumbent retailers - 2010	6.714	5.745	0.577	72
	[7.590]	[6.273]		
Change in log (number of retailers - 2009/2010)	0.442	0.444	0.987	72
	[0.614]	[0.647]		
Other variables				
% Solidaridad program beneficiaries / population	0.325	0.383	0.151	72
	[0.151]	[0.244]		
Average monthly household income (US$)	475.252	499.273	0.293	72
	[91.933]	[85.583]		
% of population with completed primary education (	0.628	0.613	0.444	72
	[0.075]	[0.078]		
% of population with incomplete secondary educatio	0.213	0.208	0.735	72
	[0.053]	[0.051]		
% of population with completed secondary education	0.159	0.179	0.207	72
	[0.050]	[0.064]		
1 (if district is urban)	0.796	0.770	0.806	72
	[0.400]	[0.400]		
District includes non-targeted neighborhoods	0.286	0.373	0.489	72
	[0.463]	[0.488]		
B. Retailer characteristics				
Outcomes				
Log-price index - pre-treatment (weighted)	-0.323	-0.338	0.189	400
	[0.080]	[0.082]		
Product-quality index	0.407	0.407	0.992	400
	[0.052]	[0.059]		
Price range	0.329	0.281	0.277	361
	[0.326]	[0.307]		
Product-quality range	1.501	1.509	0.316	361
	[1.713]	[1.559]		
Brand-quality range	0.893	0.874	0.907	361
	[0.949]	[0.895]		
Variety-quality range	1.048	1.183	0.973	361
	[0.981]	[0.970]		
Covariates				
Percentage male	0.853	0.839	0.725	401
	[0.356]	[0.368]		
1 (if the surveyed person is the retailer's owner)	0.688	0.623	0.119	401
	[0.465]	[0.485]		
1 (if has more than a completed primary education)	0.679	0.613	0.197	401
	[0.469]	[0.488]		
Log (total employees)	1.412	1.526	0.064	401
	[0.440]	[0.482]		
Other Variables				
Log (sales)	9.088	9.106	0.855	388
	[0.767]	[0.857]		
Share of retailers in targeted neighborhood	0.615	0.651	0.781	401
	[0.489]	[0.478]		
C. Consumer characteristics				
Outcomes				
Log demeaned price (weighted)	-3.588	-3.588	0.970	2125
	[0.092]	[0.092]		
Service quality (rating 1-10)	8.979	8.983	0.975	2248
	[1.600]	[1.413]		
Covariates				
Head of household's age	53.021	52.346	0.523	2250
	[15.642]	[15.257]		
Monthly household income (US$)	475.369	498.549	0.315	2250
	[265.401]	[262.765]		
% of heads of household who are married	0.576	0.538	0.293	2250
	[0.495]	[0.499]		
% of head of household who work	0.557	0.532	0.311	2250
	[0.497]	[0.499]		
% of head of household who are male	0.635	0.618	0.620	2250
	[0.482]	[0.486]		

Note: Columns [1] and [2] report the mean and standar deviation (in square brackets) of each variable for the neighborhoods with no (randomized) entry and with some (randomized) entry. Column [3] reports the p-value of a t-test of the difference between the two samples (using clustered standard errors at the district level). Column [4] shows the number of observations used.

TABLE 6. IMPACT OF COMPETITION ON PRODUCT PRICES

Dependent variable: Log (average price after treatment) - weighted		Observations	Clusters (number of districts)	Intention-to-treat: OLS estimation: 1(Entry>0) = 1(Randomized entry>0)			Average treatment effect: IV estimation: 1 (Entry>0) = 1 (Observed entry>0), instrumented with 1 (Randomized entry>0)		
		[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
(A) Retailer measures	*All districts*								
	1(Entry>0)	399	72	-0.020**	-0.015**	-0.014**	-0.040**	-0.028*	-0.027*
				[0.007]	[0.007]	[0.007]	[0.018]	[0.017]	[0.014]
							{10.700}	{13.500}	{15.000}
	Targeted neighborhoods								
	1 (Entry>0)	254	72	-0.026**	-0.018**	-0.019**	-0.056**	-0.039**	-0.049**
				[0.009]	[0.008]	[0.007]	[0.024]	[0.018]	[0.020]
							{8.400}	{8.900}	{7.800}
	Incumbent retailers in targeted neigh.								
	1 (Entry>0)	212	70	-0.025**	-0.019**	-0.018**	-0.060**	-0.045**	-0.048**
				[0.009]	[0.008]	[0.008]	[0.028]	[0.021]	[0.022]
							{6.500}	{7.000}	{6.700}
(B) Consumer	*Targeted neighborhoods*								
	1 (Entry>0)	2025	72	-0.024**	-0.021***	-0.015**	-0.043**	-0.037**	-0.028*
				[0.008]	[0.006]	[0.007]	[0.017]	[0.013]	[0.016]
							{27.200}	{31.000}	{22.000}
	Shop at incument retailers								
	1 (Entry>0)	1493	71	-0.030**	-0.027***	-0.020**	-0.052**	-0.047**	-0.037*
				[0.010]	[0.008]	[0.009]	[0.020]	[0.017]	[0.020]
							{26.100}	{27.800}	{19.800}
Baseline measures					X	X		X	X
District controls						X			X

Note: Each entry shows an estimate of the impact of an increase in competition on the log (average price) after treatment. Panel A uses the weighted log-price in the retailer database, while panel B uses the weighted log-price in the beneficiary database. Columns [1] and [2] report sample sizes. Columns [3] and [6] report the estimation with no controls. Columns [4] and [7] control for the baseline log(price), the baseline number of retailers and the baseline product quality. Columns [5] and [8] report the estimates with neighborhood controls which include: 1 (if neighborhood is targeted), province fixed effects, the average education and income of households in the district and 1 (if neighborhood is urban). Standard errors clustered at the district level are reported in brackets. IV first-stage F-statistics are included in braces. *** p<0.01, ** p<0.05, * p<0.1

TABLE 7. IMPACT OF ENTRY ON PRICES

(Heterogeneity)

Dependent variable: Log(average price after treatment) - weighted	Observations (number of retailers)	Clusters (number of districts)	Intention-to-treat OLS estimation: 1(Entry=1) = 1(Randomized entry=1) 1(Entry=2,3,4) = 1(Randomized entry=2,3)			Average treatment effect IV estimation: 1(Entry=j)=1(Observed entry=j), instrumented with 1(Randomized entry=j), j=1,2 or more		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
All neighborhoods								
1(Randomized entry=1)	399	72	-0.020**	-0.011	0.002	-0.034**	-0.016	0.016
			[0.009]	[0.009]	[0.009]	[0.016]	[0.016]	[0.023]
1(Randomized entry=2 or 3)			-0.020**	-0.016**	-0.019**	-0.047	-0.037	-0.058*
			[0.008]	[0.008]	[0.008]	[0.029]	[0.026]	[0.033]
						{17.200}	{16.800}	{7.900}
Targeted neighborhoods								
1(Randomized entry=1)	254	72	-0.023**	-0.014	-0.008	-0.034*	-0.020	0.010
			[0.011]	[0.010]	[0.010]	[0.020]	[0.019]	[0.031]
1(Randomized entry=2 or 3)			-0.027**	-0.020**	-0.025***	-0.071**	-0.054*	-0.087*
			[0.010]	[0.009]	[0.008]	[0.035]	[0.029]	[0.048]
						{10.700}	{10.800}	{5.500}
Incumbent retailers in targeted neigh.								
1(Randomized entry=1)	212	70	-0.028**	-0.019*	-0.013	-0.043**	-0.028	-0.006
			[0.011]	[0.010]	[0.010]	[0.021]	[0.021]	[0.024]
1(Randomized entry=2 or 3)			-0.023**	-0.018**	-0.021**	-0.072*	-0.058	-0.070*
			[0.011]	[0.009]	[0.008]	[0.041]	[0.035]	[0.039]
						{9.100}	{9.400}	{5.100}
Baseline measures				X	X		X	X
District controls					X			X

Note: All entries report the estimation of a model in which the dependent variable is the log(average price) and the independent variables are dummies indicating the level of treatment (D=1,2,3) and controls. Columns [1] and [2] report sample sizes. Columns [3] and [6] report the estimation with no controls. Columns [4] and [7] control for the baseline log(average price), the baseline number of retailers and the baseline product quality. Columns [5] and [8] report the estimates with neighborhood controls: 1 (if neighborhood is targeted), province fixed effects, the average education and income of households in the district and 1(if neighborhood is urban). Standard errors clustered at the district level are reported in brackets. IV first-stage F-statistics are included in braces. *** p<0.01, ** p<0.05, * p<0.1

TABLE 8. PRICE ELASTICITY OF ENTRY

Dependent variable: Log (average price after treatment) - weighted		Observations	Clusters (number of districts)	IV estimates		
		[1]	[2]	[3]	[4]	[5]
(A) Retailer measures	*All districts*					
	$\text{Log}\left(\frac{\# \, Retailers_1}{\# \, Retailers_0}\right)$	399	72	-0.033	-0.035	-0.062**
				[0.026]	[0.024]	[0.027]
				{36.100}	{35.700}	{36.300}
	Targeted neighborhoods					
	$\text{Log}\left(\frac{\# \, Retailers_1}{\# \, Retailers_0}\right)$	254	72	-0.052**	-0.053**	-0.096***
				[0.026]	[0.024]	[0.030]
				{26.900}	{28.000}	{27.600}
	Incumbent retailers in targeted neigh.					
	$\text{Log}\left(\frac{\# \, Retailers_1}{\# \, Retailers_0}\right)$	212	70	-0.077**	-0.075**	-0.105**
				[0.038]	[0.037]	[0.040]
				{19.500}	{20.300}	{17.700}
(B) Consumer measures	*Targeted neighborhoods*					
	$\text{Log}\left(\frac{\# \, Retailers_1}{\# \, Retailers_0}\right)$	2025	72	-0.029*	-0.024	-0.017
				[0.016]	[0.015]	[0.016]
				{56.600}	{54.200}	{74.900}
	Shop at incument retailers					
	$\text{Log}\left(\frac{\# \, Retailers_1}{\# \, Retailers_0}\right)$	1493	71	-0.037**	-0.031*	-0.031
				[0.018]	[0.018]	[0.021]
				{36.200}	{34.400}	{54.500}
Baseline measures					X	X
Districts controls						X

Note: Each entry shows an estimate of the impact of an increase in competition on the log weighted average price. Panel A uses the weighted log-price in the retailer database, while panel B uses the weighted log-price in the consumer database. Columns [1] and [2] report sample sizes. Column [3] reports the estimation with no controls. Column [4] controls for the baseline log(price) and product-quality index. Columns [5]] reports the estimates with district controls (province fixed effects and total number of beneficiaries within the district at baseline). Standard errors clustered at the district level are reported in brackets. IV first-stage F-statistics are included in braces. *** p<0.01, ** p<0.05, * p<0.1

TABLE 9. IMPACT OF COMPETITION ON PRODUCT PRICES
(Non-CCT stores - No target areas)

Dependent variable: Log (average price after treatment) -weighted		Observations (number of retailers)	Clusters (number of districts)	Intention-to-treat: OLS estimation: 1(Entry>0) = 1(Randomized entry>0)			Average treatment effect: IV estimation: 1 (Entry>0) = 1(Observed entry>0), instrumented with 1(Randomized entry>0)		
		[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
(A) Retailer measures	*All districts*								
No CCT retailers	1(Entry>0)	63	33	-0.003	0.004	0.028	-0.006	0.007	0.065
				[0.023]	[0.019]	[0.022]	[0.041]	[0.031]	[0.059]
							{10.100}	{11.600}	{3.300}
(B) Retailer measures	*No targeted neighborhoods*								
CCT retailers	1(Entry>0)	136	25	-0.014	-0.010	0.010	-0.024	-0.015	0.012
				[0.013]	[0.011]	[0.033]	[0.024]	[0.019]	[0.037]
							{5.900}	{9.500}	{32.000}
Baseline measures					X	X		X	X
District controls						X			X

Note: Each entry shows an estimate of the impact of an increase in competition on the log (average price) after treatment. Panel A uses the weighted log-price in the retailer database, while panel B uses the weighted log-price in the beneficiary database. Columns [1] and [2] report sample sizes. Columns [3] and [6] report the estimation with no controls. Columns [4] and [7] control for the baseline log(price), the baseline number of retailers and the baseline product quality. Columns [5] and [8] report the estimates with neighborhood controls : 1 (if neighborhood is targeted), province fixed effects, the average education and income of households in the district and 1(if neighborhood is urban). Standard errors clustered at the district level are reported in brackets. IV first-stage F-statistics are included in braces.
*** p<0.01, ** p<0.05, * p<0.1

TABLE 10. IMPACT OF COMPETITION ON PRODUCT QUALITY

Outcome: Log(Product quality index) - weighted	Observations	Clusters (number of districts)	Intention-to-treat OLS estimation: 1 (Entry>0) = 1 (Randomized entry>0)			Average treatment effect (Observed entry>0), instrumented with 1 (Randomized entry>0)		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
All districts	399	72	-0.006	-0.005	-0.002	-0.011	-0.01	-0.004
1(Entry>0)			[0.007]	[0.007]	[0.006]	[0.013]	[0.013]	[0.012]
						{10.700}	{13.500}	{15.000}
Targeted neighborhoods	254	72	-0.013	-0.010	-0.008	-0.028	-0.021	-0.019
1 (Entry>0)			[0.011]	[0.011]	[0.007]	[0.020]	[0.022]	[0.017]
						{8.400}	{8.900}	{7.800}
Incumbent retailers in targeted neigh.	212	70	-0.01	-0.007	-0.003	-0.024	-0.017	-0.009
1 (Entry>0)			[0.011]	[0.011]	[0.008]	[0.024]	[0.025]	[0.019]
						{6.500}	{7.000}	{6.700}
Baseline measures				X	X		X	X
District controls					X			X

Note: Each entry shows an estimate of the impact of an increase in competition on quality after treatment. Panel A uses the weighted log-price in the retailer database, while panel B uses the weighted log-price in the beneficiary database. Columns [1] and [2] report sample sizes. Columns [3] and [6] report the estimation with no controls. Columns [4] and [7] control for the baseline log(price), the baseline number of retailers and the baseline product quality. Columns [5] and [8] report the estimates with neighborhood controls : 1(if neighborhood is targeted), province fixed effects, the average education and income of households in the district and 1(if neighborhood is urban). Standard errors clustered at the district level are reported in brackets.IV first-stage F-statistics are included in braces. *** p<0.01, ** p<0.05, * p<0.1

TABLE 11. IMPACT OF COMPETITION ON QUALITY
(Targeted districts)

Outcome	Observations	Clusters (number of districts)	Intention-to-treat: OLS estimation: 1 (Entry>0) = 1 (Randomized entry>0)			Average treatment effect: IV estimation: 1 (Entry>0) = 1(Observed entry>0), instrumented with 1(Randomized entry>0)		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Service quality (quality ladder)								
Price range	235	72	-0.031	-0.019	0.008	-0.068	-0.042	0.021
			[0.075]	[0.068]	[0.061]	[0.170]	[0.151]	[0.152]
						{8.400}	{9.500}	{8.000}
Product-quality range	235	72	0.597**	0.496*	0.236	1.324**	1.134*	0.632
			[0.250]	[0.276]	[0.213]	[0.606]	[0.631]	[0.561]
						{8.400}	{8.000}	{6.900}
Quality range brand	235	72	0.337	0.187	0.006	0.748	0.427	0.016
			[0.249]	[0.271]	[0.215]	[0.485]	[0.556]	[0.546]
						{8.400}	{8.000}	{6.800}
Variety-quality range	235	72	0.090	0.076	-0.034	0.201	0.177	-0.092
			[0.109]	[0.109]	[0.104]	[0.226]	[0.235]	[0.273]
						{8.400}	{7.800}	{6.800}
Service quality								
Number of brands offered in district	72	72	0.090	0.071	0.045	0.162	0.139	0.093
			[0.249]	[0.073]	[0.087]	[0.431]	[0.126]	[0.142]
						{27.000}	{21.000}	{14.300}
Store cleanliness	254	72	0.105	0.142	0.142	0.228	0.309	0.372
			[0.290]	[0.266]	[0.295]	[0.614]	[0.552]	[0.721]
						{8.400}	{8.100}	{7.300}
Log (employees)	254	72	0.067	-0.019	-0.038	0.145	-0.041	-0.095
			[0.087]	[0.056]	[0.048]	[0.168]	[0.127]	[0.127]
						{8.400}	{8.800}	{7.700}
Time shopping (minutes)	2117	72	4.691	2.384	1.879	8.385	4.242	3.624
			[4.531]	[4.111]	[3.984]	[8.115]	[7.311]	[7.661]
						{27.200}	{30.500}	{21.500}
Delivery	2118	72	0.056	0.044	0.040	0.100	0.078	0.076
			[0.063]	[0.044]	[0.042]	[0.111]	[0.077]	[0.079]
						{27.200}	{30.800}	{21.900}
Service-quality rating	2116	72	0.213**	0.235***	0.200**	0.380**	0.416**	0.379**
			[0.090]	[0.076]	[0.069]	[0.192]	[0.168]	[0.159]
						{27.200}	{30.900}	{21.900}
Baseline measures				X	X		X	X
District controls					X			X

Note: Each entry shows an estimate of the impact of an increase in competition on quality after treatment. Panel A uses the weighted log-price in the retailer database, while panel B uses the weighted log-price in the beneficiary database. Columns [1] and [2] report sample sizes. Columns [3] and [6] report the estimation with no controls. Columns [4] and [7] control for the baseline log(price), the baseline number of retailers, the baseline product quality, and the baseline value of the dependent variable. Columns [5] and [8] report the estimates with neighborhood controls : 1 (if neighborhood is targeted), province fixed effects, the average education and income of households in the district and 1 (if neighborhood is urban). Standard errors clustered at the district level are reported in brackets.IV first-stage F-statistics are included in braces. *** p<0.01, ** p<0.05, * p<0.1

TABLE 12. IMPACTS OF COMPETITION ON CLIENTELE
(Targeted districts)

Outcome	Observations	Clusters (number of districts)	Intention-to-treat: OLS estimation: 1 (Entry>0) = 1 (Randomized entry>0)			Average treatment effect: IV estimation: 1 (Entry>0) =1 (Observed entry>0), instrumented with 1 (Randomized entry>0)		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
(A) Retailer measures								
Number of customers on best day	254	72	-15.665	-9.667	-11.160	-33.898	-19.883	-27.090
			[37.136]	[37.985]	[35.894]	[84.195]	[78.688]	[85.438]
						{8.400}	{11.000}	{8.700}
Share of customers CCT beneficiaries	228	70	-5.244	-4.613	-4.810*	-12.211	-10.861	-13.432
			[3.799]	[3.096]	[2.753]	[9.907]	[8.708]	[10.069]
						{8.400}	{8.200}	{6.300}
(B) Consumer measures								
Switch to entrant retailer	1400	71	0.057***	0.055***	0.072**	0.099**	0.095**	0.146**
			[0.018]	[0.016]	[0.024]	[0.035]	[0.032]	[0.056]
						{27.200}	{31.000}	{22.000}
Baseline measures				X	X		X	X
District controls					X			X

Note: Each entry shows an estimate of the impact of an increase in competition on several outcomes after treatment. Panel A uses information from the retailer database, while panel B uses information from the beneficiary database. Columns [1] and [2] reports sample sizes. Columns [3] and [6] report the estimation with no controls. Columns [4] and [7] control for the baseline log(price), the baseline number of retailers, the baseline product quality, and the baseline value of the dependent variable. Columns [5] and [8] report the estimates with neighborhood controls : 1(if neighborhood is targeted), province fixed effects, the average education and income of households in the district and 1(if neighborhood is urban). Standard errors clustered at the district level are reported in brackets. IV first-stage F-statistics are included in braces. *** p<0.01, ** p<0.05, * p<0.1

Figures

Figure 1

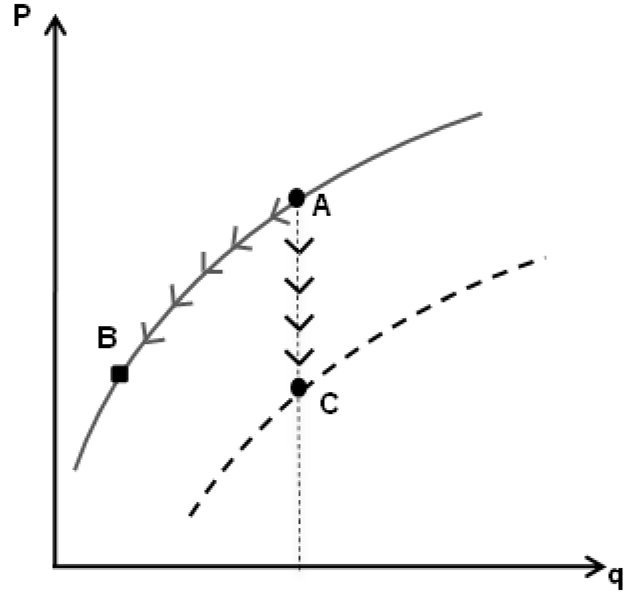
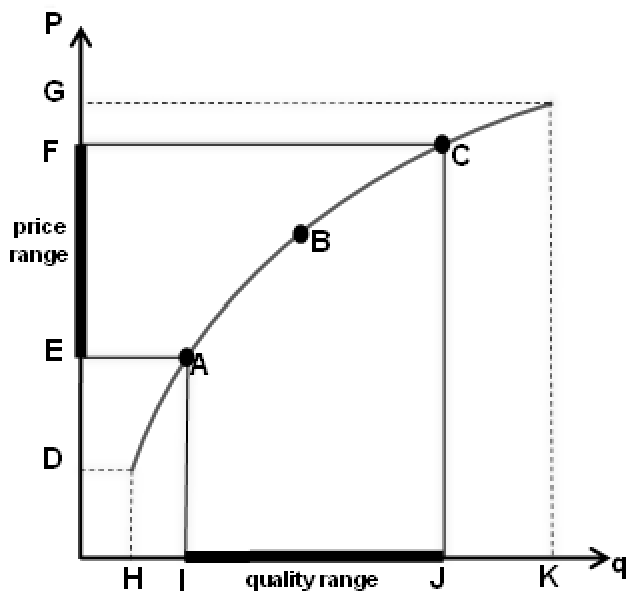


Figure 2



Appendix I: Measures of Prices, Product Quality and Service Quality

1. Data Sources

Price and product-quality data are obtained from the responses to three questions (sources). First, the retail survey questionnaire included a question (Question Q1) about 15 products. Retailers were asked about the brand and price of the cheapest brand that is normally available at their stores. This question pre-specified the unit of measurement. Second, in Question Q2, retailers were asked to identify the three products that they sell the most of to program beneficiaries and to provide information about the price, brand, variety and unit of measurement for three different versions of these three products. Finally, in Question Q3, consumers were asked about their weekly expenditure and the physical amount that they bought of each of the 15 products in the last 7 days.

2. Coding Varieties and Brands

In order to code all possible combinations of brand-variety for each product, we pooled all three sources of information. A unique code was assigned to each combination of brand-variety for each of the 15 products. Q1 and Q3 were intended to only deal with brands. In some instances, however, survey respondents mixed brands with varieties. For some products, information about the variety could be recovered from the question even when the respondent did not identify the variety, since in some cases the brand is associated with a particular variety. This imputation of missing information was based on data obtained from the webpages for each product.

	Number	
	Varieties	Brands
Rice	5	364
Cocking Oil	5	14
Sugar	2	30
Pasta	5	15
Eggs	3	42
Milk	5	29
Chocolate	2	4
Sardines	3	31
Beans	5	64
Onions	2	11
Salami	2	71
Chicken	2	32
Cod	3	7
Flour	3	34
Bread	0	102

Two issues warrant discussion. First, the variety of the products is often not associated with a single characteristic. This is more frequently the case for some products than for others. For instance, the variety of eggs could differ because of their size, yolk quality, etc. So in those cases, varieties were grouped together even though the relevant attributes differ. Second, neither retailers nor consumers provided information about varieties of bread. The previous table showed the complete list of brands and varieties for each product in our sample.

3. Rankings

An average price was calculated for each brand/variety combination based on the information provided in the retailers' responses to Q1 and Q2 at baseline. Whenever the information was not available at baseline, we used endline information from retailers in the control areas. If there was any brand/variety combination for which no average price was available, we calculated it using information obtained from retailers in treated areas.

Source	Brand/Variety		Brand		Variety	
	N	%	N	%	N	%
Baseline	833	0.654	509	0.648	58	0.951
Endline control areas	214	0.168	146	0.186	2	0.033
Endline treated areas	226	0.178	130	0.166	1	0.016
	1273		785		61	

Using these average prices, we calculated a percentile ranking of brand/varieties for each one of the 15 products: (i) for each product, brand/variety combinations were ranked in ascending order; (ii) this ranking was divided by the total number of possible combinations of brand/varieties for each product. Call this measure v_{itk} for each product k, retailer i and time t (t=baseline, endline). Therefore, the higher the average price, the higher the ranking. We interpret this ranking as a measure of product quality.

4. Measures

We use this price and product-quality information to calculate price and quality indices for retailers and consumers.

Average Price (retailers). For each retailer i at time t (t=baseline, endline), we computed the average over all 15 products (k):

$$P_{it} = \sum_{k=1}^{15} W_k * p_{itk}$$

In the case of the weighted average price, W_k is the share of expenditure on product k (see below). In the case of the unweighted average price, W_k=1/15 for all k.

Average Price (consumer). For each consumer *i* at time *t* (t=baseline, endline), we computed the average (relative) price over all 15 products (*k*):

$$P_{it} = \sum_{k=1}^{K} W_k * \left[\frac{p_{itk}}{\overline{P_{kt}}}\right]$$

In the case of the weighted average price, W_k is the share of expenditure on product *k* (see below). In the case of the unweighted average price, W_k=1/K for all *k*. Many consumers did not report spending for all 15 products. To avoid differences in average prices due to bundle composition, we standardized the price of each product using its average price in the sample.

Quality Index (retailers). For each retailer *i* at time *t* (t=baseline, endline), we computed the average over all 15 products (*k*):

$$V_{it} = \sum_{k=1}^{K} W_k * v_{itk}$$

In the case of the weighted quality index, W_k is the share of expenditure on product *k* (see below). In the case of the unweighted quality index, W_k=1/15 for all k.

Number of Brands (district). In each district *s* at time *t*, we computed:

$$Brands_{st} = \sum_{k=1}^{K} \frac{N_{stk}}{15}$$

This is the average number of brands over the total number of products *k* that were available in district *s* at time *t* according to the answers provided by retailers.

Price Range (retailers). Using Q2 for each retailer *i* at time *t* (t=baseline, endline), we computed the average over three products for the price range:

$$\frac{\sum_{k=1}^{3}(Pmax_k/Pmin_k)}{3}$$

where $Pmax_k = Max(\{price_{kl}\}_{l=1}^{3})$ and $Pmin_k = Min(\{price_{kl}\}_{l=1}^{3})$. This is the average price range for the prices reported by the retailers for the three products (brands/varieties) that they sell the most of to their customers. The price range is the ratio of the higher and lower prices for each of these three products.

Quality Range (retailers). Using Q2 for each retailer *i* at time *t* (t=baseline, endline), we computed the average over three products for the price range:

$$\frac{\sum_{k=1}^{3}(Vmax_k/Vmin_k)}{3}$$

where $Pmax_k = Max(\{v_{kl}\}_{l=1}^{3})$ and $Pmin_k = Min(\{v_{kl}\}_{l=1}^{3})$. This is the average quality range for the qualities (brand/varieties) reported by the retailers for the three products (brand/varieties) that they sell the most of to their customers. The quality range is the ratio of the higher and lower quality for each of these three products. A version of this quality-range measure was calculated using the ranking that was estimated using just the rankings of the brands or the rankings of the varieties separately. We call these measures the "*Quality-range brand*" and "*Quality-range variety*".

5. Weights

The weights W_k for the 15 products were created using the household survey. The weights represent the share of monthly expenditure on product k made by all the surveyed households at baseline. In all measures, the weights add up to 1.

The weights W_k were compared with the results of a nationally representative survey of program beneficiaries, the Evaluation Survey of Social Protection (EEPS), which was conducted in 2010/2011. In this survey, households were queried about their expenditure on a broader set of products. Appendix Table F1 indicates the results of this comparison. The first column shows the product and the second column, the sample size. The third column shows the percentage of households that reported having consumed a given product in the previous week. The fourth column shows the average share of expenditure on each product. Panel A gives the corresponding information for the 15 products that were covered in our survey. Panel B summarizes the information about other non-perishable products that may be sold by small-scale retailers. Panel C shows the measures for other fresh or perishable products typically not sold by the retailers in question.

Several facts are worth mentioning here. First, the 15 products included in our survey account for 60% of total food expenditure. Second, the other products that are sold by the retailers under analysis represent 12% of total food expenditure. Third, most households bought these 15 products. Fourth, the weights calculated in our sample are very close to those observed in the EEPS.

APPENDIX TABLE F1: EEPS 2010/2011 - SHARE OF EXPENDINTURE ON ALL PRODUCTS

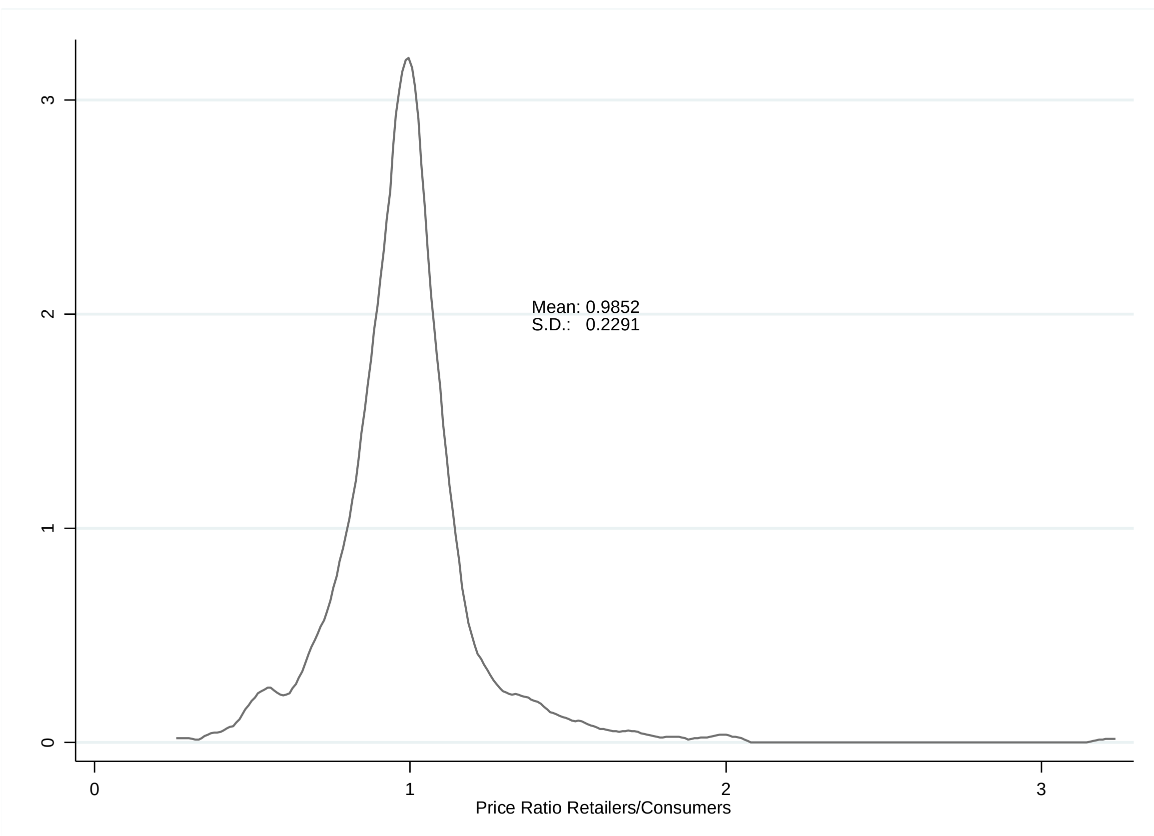
Product	EEPS 2010				
	N	Percentage consumption	Share of expenditure	Share of expenditure in price index	Survey weightings
Fifteen survey products			*0.601*	*1.000*	*1.000*
Rice	6783	0.962	0.157	0.262	0.293
Chicken	6784	0.784	0.089	0.148	0.170
Oil	6786	0.936	0.059	0.099	0.094
Milk	6786	0.338	0.045	0.075	0.062
Sugar	6785	0.955	0.045	0.075	0.052
Beans	6785	0.849	0.043	0.072	0.063
Salami	6786	0.758	0.039	0.064	0.048
Eggs	6785	0.792	0.030	0.051	0.050
Bread	6785	0.755	0.028	0.046	0.074
Pasta	6786	0.771	0.019	0.032	0.017
Onion	6785	0.886	0.018	0.030	0.020
Cod	6785	0.192	0.011	0.018	0.018
Sardines	6786	0.216	0.009	0.014	0.014
Chocolate	6784	0.366	0.007	0.011	0.015
Flour	6786	0.278	0.002	0.003	0.010
Other non-perishable products			*0.121*		
Powdered chicken bouillon	6786	0.874	0.025	-	-
Coffee	6785	0.708	0.023	-	-
Water	6786	0.485	0.017	-	-
Tomato paste	6786	0.715	0.017	-	-
Soda	6786	0.296	0.012	-	-
Smoked cutlets	6785	0.142	0.008	-	-
Powdered juice	6786	0.287	0.007	-	-
Ice	6786	0.329	0.005	-	-
Pigeon peas	6785	0.123	0.004	-	-
Dried coconut	6785	0.085	0.002	-	-
Canned green beans	6785	0.026	0.001	-	-
Fresh or persihable products			*0.264*		
White cheese	6785	0.336	0.017	-	-
Milk	6784	0.237	0.007	-	-
Yellow cheese	6786	0.113	0.005	-	-
Butter	6786	0.255	0.004	-	-
Orange juice	6786	0.072	0.003	-	-
Plantains	6785	0.723	0.037	-	-
Avocados	6784	0.787	0.022	-	-
Garlic	6785	0.900	0.022	-	-
Beef	6785	0.240	0.020	-	-
Pork	6785	0.232	0.019	-	-
Yucca	6784	0.526	0.014	-	-
Green bananas	6785	0.650	0.014	-	-
Chili peppers	6782	0.749	0.009	-	-
Fresh fish	6782	0.096	0.008	-	-
Potatoes	6785	0.252	0.007	-	-
Other vegetables	6784	0.604	0.006	-	-
Eggplants	6785	0.303	0.005	-	-
Squash	6785	0.399	0.005	-	-
Peas	6786	0.134	0.005	-	-
Clupea (fish)	6785	0.147	0.005	-	-
Lemons	6783	0.401	0.004	-	-
Tomatoes	6785	0.243	0.004	-	-
Chayote	6785	0.237	0.003	-	-
Cabbage	6784	0.194	0.003	-	-
Bananas	6786	0.271	0.003	-	-
Carrots	6786	0.175	0.003	-	-
Sweet potatoes	6785	0.114	0.002	-	-
Yautia	6785	0.073	0.002	-	-
Other fruits	6786	0.095	0.002	-	-
Beetroot	6785	0.064	0.001	-	-
Oranges	6786	0.115	0.001	-	-
Mangos	6786	0.055	0.001	-	-

Note: The products in each of the three product groups are listed in descending order of share of expenditure.

6. Price Validation

In order to assess the validity of our price measures, we compare price measures obtained using retailer data with those obtained using beneficiary data (an independent source of information). For each product and brand in all the districts, we calculated an average price based on the prices reported by the retailers and by the beneficiaries. Let $\bar{P}^R_{ks}$ be the average price in district *s* of product *k* computed using retailer information R, which corresponds to the cheapest available option for each product. Similarly, let $\bar{P}^C_{ks}$ be the average price in district *s* computed using consumers' information C which corresponds to the products actually bought by consumers. The average district relative price $(\bar{P}^R_{ks}/\bar{P}^C_{ks})$ is a useful statistic for assessing how close these two measures are. Note that, without measurement error in the measures of prices, this statistic is bounded from above at 1. The next figure shows a kernel density estimation of that price ratio. We find that the average relative price over all products and districts is 0.99.

$(\bar{P}^R_{ks}/\bar{P}^C_{ks})$ **Distribution**



Appendix II: Variables

Variable	Description	Source
District Characteristics		
Log (total beneficiaries - 2010)	Number of beneficiaries in January 2010 at the district level	Administrative
Change in log (total beneficiaries -2009/2010)	Change in the number of beneficiaries at the district level from January 2009 to January 2010	Administrative
Log (sales -2010)	Total sales from January to May 2010 at the district level	Administrative
Change in log (sales -2009/2010)	Change in total sales from January-May 2009 to January-May 2010 at the district level	Administrative
Number of incumbent retailers 2010	Number of active retailers per district as of February 2011	Administrative
Number of brands	Average number of brands available in each distict $Brands_{s,t} = \sum_{k=1}^{K} N_{k,S,t}/15$	Retailer survey
Change in log (number of retailers 2009/2010)	$\frac{\#\,retailers\ 2010 - \#\,retailers\ 2009}{0.5 * (\#\,retailers\ 2010 + \#\,retailers\ 2009)}$	Administrative
% Solidaridad program beneficiaries / population	Solidaridad program beneficiaries as a percentage of the total population (above 18 years)	Administrative
Average household monthly income (US$)	Average household income in the district (above 18 years)	Household survey
% of population with completed primary	Percentage of beneficiaries with incomplete primary education or lower (above 18 years)	Household survey
% of population with incomplete secondary	Percentage of beneficiaries with incomplete secondary education	Household survey
% Population with secondary complete or higher	Percen of beneficiariies with secondary complete or higher education	Household survey
Urban	1 (if district is urban)	Administrative
District includes non-targeted neighborhoods	1 (if district includes a non-targeted neighborhood)	Administrative

Retailer Characteristics

Outcomes

Average Price (weighted)	$\text{Log}(P_{i0})$, where: $P_{it} = \sum_{k=1}^{K} W_k * p_{itk}$ p_{itk} Price of product k in retailer i W_k Weight computed from the household survey $W_k = \frac{w_k}{\sum_{k=1}^{K} w_k}$ K is the number of products available at the store	Household and retailer surveys
Average Price (unweighted)	$P_{it} = \sum_{k=1}^{K} W_k * p_{itk}$ $W_k = \frac{1}{K}$ K is the number of products available at the store	Retailer survey
Product quality index (weighted)	$\log(Q_{i0})$, where: $V_{it} = \sum_{k=1}^{K} W_k * v_{itk}$ v_{itk} Quality (ranking in prices) of product k in retailer i W_k Weight computed from the household survey $W_k = \frac{w_k}{\sum_{k=1}^{K} w_k}$ Quality is computed by ranking the different product brands based on their average price. Therefore, a more expensive product is considered to be of better quality.	Household and retailer surveys
Producer quality index (unweighted)	$V_{it} = \frac{\sum_{k=1}^{K} v_{itk}}{K}$ q_{itk} : Quality (ranking in prices) of product k in retailer i Quality is computed by ranking the different product brands based on their average price. Therefore, a more expensive product is considered to be of better quality.	Retailer survey

Retailer Characteristics (cont.)

Outcomes		
Price range	$\frac{\sum_{j=1}^{3} price\ range_j}{3}$ Where, $Price\ range_i = \frac{Pmax_i}{Pmin_i}$ $Pmax_i = Max(price_{i1}, price_{i2}, price_{i3})$ $Pmin_i = Min(price_{i1}, price_{i2}, price_{i3})$ $price_{ij}$: Price of product i, brand j.	Retailer survey
Product-quality range	$\frac{\sum_{j=1}^{3} quality\ range_j}{3}$ Where, $Quality\ range_i = \frac{Vmax_i}{Vmin_i}$ $Vmax_i = Max(v_{i1}, v_{i2}, v_{i3})$ $Vmin_i = Min(v_{i1}, v_{i2}, v_{i3})$ v_{ij} : Price ranking of product i, brand j. Quality is computed by ranking the different product brands based on their average price. Therefore, a more expensive product is considered to be of better quality.	Retailer survey
Log (sales)	Log (self-reported sales)	Retailer survey
Log (employees)	Log (self-reported total number of employees)	Retailer survey
Share of CCT beneficiary customers	Percentage of customers who, according to the retailer, are program beneficiaries	Retailer survey
Number of customers - best day	Number of customers on the best day for sales	Retailer survey
Store cleanliness	Hygienic conditions in the store - scale of 1 to 10	Retailer survey
Covariates		
Retailer's gender	Gender of retailer's owner	Retailer survey
Retailer's ownership	1 (owns the retail store)	Retailer survey
Retailer's education	1 (if retailer has more than a completed primary education)	Retailer survey
Other Variables		
Share of retailers in targeted neighborhood	1 (If retailer is in a targeted neighborhood)	Retailer survey

Consumer Characteristics

Outcomes

Weighted demeaned price	$\text{Log}(P_{i0})$, where: $P_{it} = \sum_{k=1}^{K} W_k * \left[\frac{p_{itk}}{\overline{P_{kt}}}\right]$ p_{ik} Price of product k for household i (computed by dividing the amount of money spent on product i in the last week by the physical amount acquired). Units used in questions were $\overline{p_{kt}}$ is the average price of product k at time t. W_k Weight computed from the household survey $W_k = \frac{w_k}{\sum_{k=1}^{K} w_k}$ K is the number of products reported by each beneficiary	Household survey
Unweighted demeaned price	$P_{it} = \sum_{k=1}^{K} W_k * \left[\frac{p_{itk}}{\overline{P_{kt}}}\right]$ p_{ik} Price of product k for household i (computed by dividing the amount of money spent on product i in the last week by the physical amount acquired). Units used in questions were $\overline{p_{kt}}$ is the average price of product k at time t. W_k Weight computed from the household survey $W_k = \frac{1}{K}$	Household survey
Service quality	Quality scale (1- 10)	Household survey
Delivery	1 (retail has delivery)	Household survey
Switch to entrant retailer	1 (household change to entrant retailer between baseline and endline)	Household survey
Time shopping	Average minutes the household needs to shop	Household survey

Covariates

Household head or spouse working	Head of household or spouse is working	Household survey
Head of household's gender	Head of household’s gender	Household survey
Percentage of head of household married	1 (Head of household is married)	Household survey
Head of household's age	Head of household’s age	Household survey
Household log-income	Household’s income	Household survey

Appendix III: Robustness and Extensions

TABLE A1. SAMPLE SIZE

	At baseline	At endline
Universe of retailers in area under study	432	425
Universe of entrant retailers	61	61
Sample size: Retailers (in surveys)	401	400
By type		
Incumbent	350	341
Entrant	51	59
Located in targeted neighborhood	257	254
Incumbent in targeted neighborhood	215	212
Sample size: Beneficiaries (in surveys)	2250	2118
By type		
Shop in incumbent retailers	1620	1563
Located in targeted neighborhood	2250	2118
Number of districts	72	72

TABLE B1. IMPACT OF COMPETITION ON PRODUCT PRICES AND GROUP OF CONTROLS (ROBUSTNESS)

Dependent variable: Log (average price after treatment) - weighted		Observations	Clusters (number of districts)	Intention-to-treat: OLS estimation: 1(Entry>0) = 1(Randomized entry>0)					Average treatment effect: IV estimation: 1 (Entry>0) = 1 (Observed entry>0), instrumented with 1 (Randomized entry>0)				
		[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
(A) Retailer measures	*All districts*												
	1(Entry>0)	399	72	-0.020**	-0.015**	-0.014**	-0.012*	-0.014*	-0.040**	-0.028*	-0.027*	-0.023	-0.031
				[0.007]	[0.007]	[0.007]	[0.007]	[0.008]	[0.018]	[0.017]	[0.014]	[0.014]	[0.020]
	Wald test joint significance (p-values)								{10.700}	{13.500}	{15.000}	{15.200}	{11.900}
	Baseline measures				0.000	0.000	0.000	0.000		0.000	0.000	0.000	0.000
	District controls					0.000	0.000	0.000			0.000	0.000	0.000
	Retailer controls						0.590	0.540				0.520	0.440
	Household controls							0.000					0.000
	Targeted neighborhoods												
	1 (Entry>0)	254	72	-0.026**	-0.018**	-0.019**	-0.018**	-0.014*	-0.056**	-0.039**	-0.049**	-0.044**	-0.039
				[0.009]	[0.008]	[0.007]	[0.007]	[0.008]	[0.024]	[0.018]	[0.020]	[0.020]	[0.027]
	Wald test joint significance (p-values)								{8.400}	{8.900}	{7.800}	{8.000}	{6.600}
	Baseline measures				0.000	0.000	0.000	0.000		0.000	0.000	0.000	0.000
	District controls					0.010	0.090	0.060			0.020	0.060	0.020
	Retailer controls						0.670	0.610				0.570	0.480
	Household controls							0.030					0.150
	Incumbent retailers in targeted neigh.												
	1 (Entry>0)	212	70	-0.025**	-0.019**	-0.018**	-0.018**	-0.016*	-0.060**	-0.045**	-0.048**	-0.049**	-0.054
				[0.009]	[0.008]	[0.008]	[0.008]	[0.009]	[0.028]	[0.021]	[0.022]	[0.024]	[0.037]
	Wald test joint significance (p-values)								{6.500}	{7.000}	{6.700}	{6.700}	{5.000}
	Baseline measures				0.000	0.000	0.000	0.000		0.000	0.000	0.000	0.000
	District controls					0.020	0.070	0.150			0.030	0.140	0.060
	Retailer controls						0.860	0.820				0.770	0.780
	Household controls							0.130					0.510
(B) Consumer	*Targeted neighborhoods*												
	1 (Entry>0)	2025	72	-0.024**	-0.021***	-0.015**	-0.019**	-0.020**	-0.043**	-0.037**	-0.028*	-0.037*	-0.039**
				[0.008]	[0.006]	[0.007]	[0.008]	[0.008]	[0.017]	[0.013]	[0.016]	[0.019]	[0.020]
	Wald test joint significance (p-values)								{27.200}	{31.000}	{22.000}	{17.000}	{16.800}
	Baseline measures				0.000	0.010	0.010	0.010		0.000	0.000	0.000	0.000
	District controls					0.000	0.000	0.000			0.000	0.000	0.000
	Retailer controls						0.190	0.190				0.070	0.070
	Household controls							0.000					0.000
	Shop at incument retailers												
	1 (Entry>0)	1493	71	-0.030**	-0.027***	-0.020**	-0.021**	-0.022**	-0.052**	-0.047**	-0.037*	-0.042*	-0.044**
				[0.010]	[0.008]	[0.009]	[0.009]	[0.009]	[0.020]	[0.017]	[0.020]	[0.021]	[0.022]
	Wald test joint significance (p-values)								{26.100}	{27.800}	{19.800}	{15.600}	{15.400}
	Baseline measures				0.000	0.030	0.050	0.050		0.000	0.010	0.020	0.020
	District controls					0.000	0.000	0.000			0.000	0.000	0.000
	Retailer controls						0.150	0.160				0.060	0.060
	Household controls							0.000					0.000

Note: Each entry shows an estimate of the impact of an increase in competition on the log (average price) after treatment. Panel A uses the weighted log-price in the retailer database, while panel B uses the weighted log-price in the beneficiary database. Columns [1] and [2] report sample sizes. Columns [3] and [8] report the estimation with no controls. Columns [4] and [9] control for the baseline log(price), the baseline number of retailers and the baseline product quality. Columns [5] and [10] report the estimates with neighborhood controls : 1(if neighborhood is targeted), province fixed effects, the average education and income of households in the district and 1(if neighborhood is urban).Columns [6] and [11] add retailer controls to the specification (owner's gender, education and number of employees at baseline). Finally, columns [7] and [12] include household characteristics at the district level. Panel A includes market averages for the age of the head of household, household income, the percentage of people who are married and the percentage of people who work. Panel B uses the same controls as panel A, but here they have been computed at the individual household level. Standard errors clustered at the district level are reported in brackets. F-statistics for instrument at IV estimations are included in braces. p-values of Wald tests of joint significance for each group of controls are shown for each specification. *** p<0.01, ** p<0.05, * p<0.1

TABLE B2. IMPACT OF COMPETITION ON UNWEIGHTED PRODUCT PRICES (ROBUSTNESS)

Dependent variable: Log (average price after treatment) - unweighted		Observations	Clusters (number of districts)	Intention-to-treat: OLS estimation: 1(Entry>0) = 1(Randomized entry>0)			Average treatment effect: IV estimation: 1 (Entry>0) = 1 (Observed entry>0), instrumented with 1 (Randomized entry>0)		
		[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
(A) Retailer measures	*All districts*								
	1(Entry>0)	399	72	-0.012	-0.012*	-0.012	-0.023	-0.022	-0.024
				[0.008]	[0.007]	[0.008]	[0.019]	[0.016]	[0.016]
							{10.700}	{13.600}	{14.900}
	Targeted neighborhoods								
	1 (Entry>0)	254	72	-0.029**	-0.026**	-0.024**	-0.063*	-0.054*	-0.062**
				[0.010]	[0.009]	[0.008]	[0.034]	[0.028]	[0.029]
							{8.400}	{8.800}	{7.600}
	Incumbent retailers in targeted neigh.								
	1 (Entry>0)	212	70	-0.027**	-0.025**	-0.022**	-0.066*	-0.058*	-0.060*
				[0.010]	[0.010]	[0.009]	[0.039]	[0.032]	[0.031]
							{6.500}	{6.800}	{6.400}
(B) Consumer measures									
	Targeted neighborhoods								
	1 (Entry>0)	2025	72	-0.024**	-0.020**	-0.015**	-0.042**	-0.035**	-0.029*
				[0.008]	[0.007]	[0.007]	[0.016]	[0.014]	[0.016]
							{27.200}	{31.000}	{22.000}
	Shop at incument retailers								
	1 (Entry>0)	1493	71	-0.028***	-0.024**	-0.018**	-0.049**	-0.042**	-0.033*
				[0.009]	[0.008]	[0.008]	[0.020]	[0.017]	[0.019]
							{26.100}	{27.700}	{19.700}
Baseline measures					X	X		X	X
District controls						X			X

Note: Each entry shows an estimate of the impact of an increase in competition on the log (price) after treatment. Panel A uses the unweighted log-price in the retailer database, while panel B uses the unweighted log-price in the beneficiary database. Columns [1] and [2] report sample sizes. Columns [3] and [6] report the estimation with no controls. Columns [4] and [7] control for the baseline log(price), the baseline number of retailers and the baseline product quality. Columns [5] and [8] report the estimates with neighborhood controls : 1 (if neighborhood is targeted), province fixed effects, the average education and income of households in the district and 1 (if neighborhood is urban). Standard errors clustered at the district level are reported in brackets. F-statistics for instrument at IV estimations are included in braces. p-values of Wald tests of joint significance for each group of controls are shown for each specification. *** p<0.01, ** p<0.05, * p<0.1

TABLE B3. IMPACT ON POOLED PRICES (ROBUSTNESS)

Dependent variable: Log (average price after treatment)		Observations	Clusters (number of districts)	Intention-to-treat: OLS estimation: 1(Entry>0) = 1(Randomized entry>0)			Average treatment effect: IV estimation: 1 (Entry>0) = 1 (Observed entry>0), instrumented with 1 (Randomized entry>0)		
		[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
(A) Retailer measures	*All districts*								
	1(Entry>0)	5645	72	-0.003	-0.005	-0.013	-0.005	-0.010	-0.025
				[0.012]	[0.010]	[0.009]	[0.023]	[0.018]	[0.017]
							{10.600}	{13.600}	{15.600}
	Targeted neighborhoods								
	1 (Entry>0)	3585	72	-0.025**	-0.020*	-0.020**	-0.054*	-0.042*	-0.051*
				[0.012]	[0.012]	[0.009]	[0.032]	[0.024]	[0.029]
							{8.400}	{8.900}	{8.100}
	Incumbent retailers in targeted neigh.								
	1 (Entry>0)	2985	70	-0.020	-0.018	-0.018**	-0.048	-0.043	-0.048
				[0.013]	[0.012]	[0.009]	[0.035]	[0.027]	[0.029]
							{6.500}	{6.900}	{6.900}
(B) Consumer measures	*Targeted neighborhoods*								
	1 (Entry>0)	16531	72	-0.026**	-0.018**	-0.015**	-0.046**	-0.031**	-0.029*
				[0.009]	[0.009]	[0.007]	[0.018]	[0.016]	[0.015]
							{27.200}	{31.300}	{21.700}
	Shop at incument retailers								
	1 (Entry>0)	12102	71	-0.029**	-0.022**	-0.019**	-0.052**	-0.039**	-0.036**
				[0.010]	[0.009]	[0.007]	[0.021]	[0.018]	[0.017]
							{26.100}	{25.300}	{18.900}
Baseline measures					X	X		X	X
District controls						X			X

Note: Each entry shows an estimate of the impact of an increase in competition on the log (price) after treatment. Panel A uses the weighted log-price in the retailer database, while panel B uses the weighted log-price in the beneficiary database. Columns [1] and [2] report sample sizes. Columns [3] and [6] report the estimation with no controls. Columns [4] and [7] control for the baseline log(price), the baseline number of retailers and the baseline product quality. Columns [5] and [8] report the estimates with neighborhood controls : 1 (if neighborhood is targeted), province fixed effects, the average education and income of households in the district and 1 (if neighborhood is urban). Standard errors clustered at the district level are reported in brackets. F-statistics for instrument at IV estimations are included in braces. p-values of Wald tests of joint significance for each group of controls are shown for each specification. *** p<0.01, ** p<0.05, * p<0.1

TABLE B4: IMPACT ON INDIVIDUAL PRODUCT PRICES (ROBUSTNESS)

Outcome Log(Product Price)	Weighting	All		Targeted		Incumbents	
		ITT	IV	ITT	IV	ITT	IV
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Rice (lb.)	0.293	-0.008	-0.015	-0.010	-0.022	-0.009	-0.022
		[0.013]	[0.023]	[0.017]	[0.033]	[0.018]	[0.037]
Cooking oil (lb.)	0.094	-0.030**	-0.057	-0.050***	-0.110**	-0.052***	-0.129**
		[0.015]	[0.038]	[0.015]	[0.046]	[0.016]	[0.059]
Sugar (lb.)	0.052	-0.001	-0.002	-0.003	-0.007	-0.008	-0.019
		[0.011]	[0.020]	[0.009]	[0.020]	[0.010]	[0.023]
Pasta (lb.)	0.017	-0.027**	-0.051**	-0.048***	-0.102**	-0.048**	-0.113*
		[0.013]	[0.024]	[0.015]	[0.047]	[0.016]	[0.058]
Eggs (unit)	0.050	-0.022	-0.042	-0.025	-0.055	-0.025	-0.059
		[0.026]	[0.044]	[0.023]	[0.046]	[0.022]	[0.052]
Powdered milk (125 gr.)	0.062	0.032	0.060	0.019	0.040	0.006	0.015
		[0.023]	[0.042]	[0.025]	[0.054]	[0.022]	[0.053]
Chocolate (unit)	0.015	0.002	0.004	-0.008	-0.017	-0.009	-0.022
		[0.011]	[0.021]	[0.014]	[0.028]	[0.014]	[0.030]
Sardines (unit)	0.014	0.028	0.053	0.015	0.032	0.017	0.040
		[0.032]	[0.062]	[0.044]	[0.097]	[0.042]	[0.100]
Green beans (lb.)	0.063	-0.005	-0.009	-0.005	-0.011	-0.003	-0.007
		[0.006]	[0.011]	[0.008]	[0.017]	[0.008]	[0.020]
Onions (lb.)	0.020	-0.013	-0.024	-0.047**	-0.104*	-0.038*	-0.092
		[0.022]	[0.044]	[0.022]	[0.062]	[0.022]	[0.066]
Salami (lb.)	0.048	-0.051*	-0.096*	-0.060	-0.132	-0.046	-0.111
		[0.028]	[0.054]	[0.039]	[0.091]	[0.040]	[0.099]
Chicken (lb.)	0.170	-0.014	-0.023	-0.008	-0.016	-0.008	-0.016
		[0.009]	[0.017]	[0.014]	[0.025]	[0.014]	[0.028]
Cod (lb.)	0.018	-0.010	-0.019	-0.020**	-0.045	-0.023**	-0.057
		[0.009]	[0.016]	[0.010]	[0.031]	[0.010]	[0.039]
Flour (lb.)	0.010	-0.038**	-0.066**	-0.042**	-0.086*	-0.040*	-0.092
		[0.015]	[0.031]	[0.020]	[0.051]	[0.021]	[0.060]
Bread (unit)	0.074	0.088*	0.151	0.021	0.039	0.035	0.073
		[0.052]	[0.107]	[0.057]	[0.107]	[0.057]	[0.121]

Note: Each entry shows an estimate of the impact of an increase in competition on the price of different products. Column [1] shows the weighting of each product in the final retailer price. Columns [2]-[3] use all the retailers; columns [4]-[5] use retailers in targeted neighborhoods; and columns [6]-[7] use incumbent retailers in targeted neigborhoods. All columns report the estimations while controlling for the baseline log(price). *** p<0.01, ** p<0.05, * p<0.1

TABLE B5. PRICE ELASTICITY OF ENTRY GROUP OF CONTROLS

Dependent variable: Log (Average price after treatment) - weighted		Observations	Clusters (number of districts)	IV estimates				
		[1]	[2]	[3]	[4]	[5]	[4]	[5]
(A) Retailer measures	*All districts*							
	$\text{Log}\left(\frac{\#\ Retailers_1}{\#\ Retailers_0}\right)$	399	72	-0.033	-0.035	-0.062**	-0.061**	-0.068**
				[0.026]	[0.024]	[0.027]	[0.026]	[0.030]
	Wald test joint significance (p-values)			{36.100}	{35.700}	{36.300}	{36.200}	{27.300}
	Baseline measures				0.000	0.000	0.000	0.000
	District controls					0.000	0.000	0.000
	Retailer controls						0.510	0.430
	Household controls							0.000
	Targeted neighborhoods							
	$\text{Log}\left(\frac{\#\ Retailers_1}{\#\ Retailers_0}\right)$	254	72	-0.052**	-0.053**	-0.096***	-0.094***	-0.097**
				[0.026]	[0.024]	[0.030]	[0.030]	[0.035]
	Wald test joint significance (p-values)			{26.900}	{28.000}	{27.600}	{27.300}	{21.900}
	Baseline measures				0.000	0.000	0.000	0.000
	District controls					0.010	0.020	0.240
	Retailer controls						0.680	0.570
	Household controls							0.240
	Incumbent retailers in target neigh.							
	$\text{Log}\left(\frac{\#\ Retailers_1}{\#\ Retailers_0}\right)$	212	70	-0.077**	-0.075**	-0.105**	-0.105**	-0.115**
				[0.038]	[0.037]	[0.040]	[0.040]	[0.049]
	Wald test joint significance (p-values)			{19.500}	{20.300}	{17.700}	{17.200}	{14.200}
	Baseline measures				0.000	0.000	0.000	0.000
	District controls					0.000	0.000	0.000
	Retailer controls						0.830	0.830
	Household controls							0.400
(B) Consumer measures	*Targeted neighborhoods*							
	$\text{Log}\left(\frac{\#\ Retailers_1}{\#\ Retailers_0}\right)$	2025	72	-0.029*	-0.024	-0.017	-0.030*	-0.032*
				[0.016]	[0.015]	[0.016]	[0.017]	[0.017]
	Wald test joint significance (p-values)			{56.600}	{54.200}	{74.900}	{66.600}	{66.300}
	Baseline measures				0.000	0.010	0.040	0.040
	District controls					0.000	0.000	0.000
	Retailer controls						0.120	0.130
	Household controls							0.000
	Shop at Incument Retailers							
	$\text{Log}\left(\frac{\#\ Retailers_1}{\#\ Retailers_0}\right)$	1493	71	-0.037**	-0.031*	-0.031	-0.032	-0.035*
				[0.018]	[0.018]	[0.021]	[0.020]	[0.020]
	Wald test joint significance (p-values)			{36.200}	{34.400}	{54.500}	{52.900}	{52.500}
	Baseline measures				0.020	0.070	0.140	0.150
	District controls					0.000	0.000	0.000
	Retailer controls						0.090	0.100
	Household controls							0.000

Note: Each entry shows an estimate of the impact of an increase in competition on the log average price weighted . Panel A uses the weighted log-price in the retailer database, while panel B uses the weighted log-price in the beneficiary database. Columns [1] and [2] report sample sizes. Column [3] reports the estimation with no controls. Column [4] controls for the baseline log(price) and product-quality index. Column [5] reports the estimates with neighborhood controls : 1 (if neighborhood is targeted), province fixed effects, the average education and income of households in the district and 1 (if neighborhood is urban).Column [6] adds retailer controls to the specification (owner's gender, education and number of employees at baseline). Finally, column [7] includes household characteristics at the district level. Standard errors clustered at the district level are reported in brackets. F-statistics for instrument at IV estimations are included in braces. p-values of Wald tests of joint significance for each group of controls are shown for each specification. *** p<0.01, ** p<0.05, * p<0.1

TABLE C1. IMPACT OF COMPETITION ON QUALITY AND GROUP OF CONTROLS (ROBUSTNESS)

Outcome	Observations (number of retailers)	Clusters (number of districts)	Intention-to-treat					Average treatment effect				
			OLS estimation: 1(Entry>0) = 1(Randomized entry>0)					IV estimation: 1 (Entry>0) = 1 (Observed entry>0), instrumented with 1 (Randomized entry>0)				
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
All districts												
Log(Product quality index)- weighted	399	72	-0.006	-0.006	-0.003	-0.002	-0.006	-0.013	-0.011	-0.006	-0.004	-0.013
			[0.007]	[0.007]	[0.006]	[0.006]	[0.007]	[0.013]	[0.013]	[0.012]	[0.012]	[0.015]
Wald test joint significance (p-values)								{10.700}	{13.500}	{15.000}	{15.200}	{11.900}
Baseline measures				0.000	0.000	0.000	0.000		0.000	0.000	0.000	0.000
District controls					0.170	0.100	0.000			0.120	0.060	0.000
Retailer controls						0.030	0.060				0.010	0.030
Household controls							0.020					0.010
Targeted neighborhoods												
Log(Product quality index)- weighted	254	72	-0.014	-0.011	-0.009	-0.009	-0.011	-0.031	-0.024	-0.023	-0.023	-0.032
			[0.010]	[0.011]	[0.007]	[0.007]	[0.008]	[0.020]	[0.022]	[0.018]	[0.019]	[0.024]
Wald test joint significance (p-values)								{8.400}	{8.900}	{7.800}	{8.000}	{6.600}
Baseline measures				0.020	0.010	0.020	0.020		0.020	0.010	0.010	0.010
District controls					0.000	0.000	0.000			0.000	0.000	0.000
Retailer controls						0.770	0.780				0.680	0.630
Household controls							0.320					0.110
Incumbent retailers in targeted neigh.												
Log(Product quality index)- weighted	212	70	-0.011	-0.009	-0.005	-0.006	-0.010	-0.027	-0.021	-0.013	-0.017	-0.034
			[0.011]	[0.011]	[0.008]	[0.008]	[0.008]	[0.024]	[0.025]	[0.021]	[0.022]	[0.029]
Wald test joint significance (p-values)								{6.500}	{7.000}	{6.700}	{6.700}	{5.000}
Baseline measures				0.030	0.030	0.040	0.040		0.020	0.010	0.010	0.010
District controls					0.000	0.000	0.000			0.000	0.000	0.000
Retailer controls						0.860	0.890				0.820	0.810
Household controls							0.350					0.290

Note: Each entry shows an estimate of the impact of an increase in competition on quality after treatment. Panel A uses district measures, while panel B uses product quality and panel C uses a product-quality ladder. Columns [1] and [2] report sample sizes. Columns [3] and [8] report the estimation with no controls. Columns [4] and [9] control for the baseline log(price), the baseline number of retailers and the baseline product quality. Columns [5] and [10] report the estimates with neighborhood controls : 1 (if neighborhood is targeted), province fixed effects, the average education and income of households in the district and 1 (if neighborhood is urban).Columns [6] and [11] add retailer controls to the specification (owner's gender, education and number of employees at baseline). Finally, columns [7] and [12] include household characteristics at the district level, market averages for the age of the head of household, household income, the percentage of people married and the percentage of people who work. Standard errors clustered at the district level are reported in brackets. F-statistics for instrument at IV estimations are included in braces. p-values of Wald tests of joint significance for each group of controls are shown for each specification. *** p<0.01, ** p<0.05, * p<0.1

TABLE C2. IMPACT OF COMPETITION ON UNWEIGHTED PRODUCT QUALITY (ROBUSTNESS)

Dependent variable: Log (product-quality index) - unweighted		Observations	Clusters (number of districts)	Intention-to-treat			Average treatment effect		
				OLS estimation: 1(Entry>0) = 1(Randomized entry>0)			IV estimation: 1 (Entry>0) = 1 (Observed entry>0), instrumented with 1 (Randomized entry>0)		
		[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
(A) Retailer measures	*All districts*								
	1(Entry>0)	399	72	-0.006	-0.005	0.004	-0.012	-0.009	0.007
				[0.007]	[0.007]	[0.006]	[0.014]	[0.012]	[0.012]
							{10.700}	{13.700}	{14.600}
	Targeted neighborhoods								
	1 (Entry>0)	254	72	-0.012	-0.006	0.001	-0.027	-0.014	0.003
				[0.009]	[0.009]	[0.006]	[0.018]	[0.018]	[0.015]
							{8.400}	{8.200}	{7.400}
	Incumbent retailers in targeted neigh.								
	1 (Entry>0)	212	70	-0.01	-0.005	0.005	-0.025	-0.011	0.014
				[0.009]	[0.009]	[0.006]	[0.021]	[0.020]	[0.018]
							{6.500}	{6.400}	{6.300}
Baseline measures					X	X		X	X
District controls						X			X

Note: Each entry shows an estimate of the impact of an increase in competition on quality after treatment. Panel A uses the weighted log-price in the retailer database, while panel B uses the weighted log-price in the beneficiary database. Columns [1] and [2] report sample sizes. Columns [3] and [6] report the estimation with no controls. Columns [4] and [7] control for the baseline log(price), the baseline number of retailers and the baseline product quality. Columns [5] and [8] report the estimates with neighborhood controls : 1 (if neighborhood is targeted), province fixed effects, the average education and income of households in the district and 1 (if neighborhood is urban). Standard errors clustered at the district level are reported in brackets.F-statistics for instrument at IV estimations are included in braces. *** p<0.01, ** p<0.05, * p<0.1

TABLE C3. IMPACT ON POOLED QUALITY (ROBUSTNESS)

Dependent variable: Log(quality index) weighted		Observations	Clusters (number of districts)	Intention-to-treat: OLS estimation: 1(Entry>0) = 1(Randomized entry>0)			Average treatment effect: IV estimation: 1 (Entry>0) = 1 (Observed entry>0), instrumented with 1 (Randomized entry>0)		
		[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
(A) Retailer measures	*All districts*								
	1(Entry>0)	5985	72	-0.006	-0.007	-0.004	-0.013	-0.012	-0.007
				[0.007]	[0.007]	[0.006]	[0.013]	[0.013]	[0.012]
							{10.600}	{13.600}	{15.600}
	Targeted neighborhoods								
	1 (Entry>0)	3810	72	-0.014	-0.012	-0.009	-0.031	-0.024	-0.023
				[0.010]	[0.011]	[0.007]	[0.020]	[0.022]	[0.018]
							{8.400}	{8.900}	{8.100}
	Incumbent retailers in targeted neigh.								
	1 (Entry>0)	3180	70	-0.011	-0.009	-0.005	-0.027	-0.022	-0.014
				[0.011]	[0.012]	[0.008]	[0.024]	[0.025]	[0.021]
							{6.500}	{6.900}	{6.900}
Baseline measures					X	X		X	X
District controls						X			X

Note: Each entry shows an estimate of the impact of an increase in competition on the log (price) after treatment. Panel A uses the weighted log-price in the retailer database, while panel B uses the weighted log-price in the beneficiary database. Columns [1] and [2] report sample sizes. Columns [3] and [6] report the estimation with no controls. Columns [4] and [7] control for the baseline log(price), the baseline number of retailers and the baseline product quality. Columns [5] and [8] report the estimates with neighborhood controls : 1 (if neighborhood is targeted), province fixed effects, the average education and income of households in the district and 1 (if neighborhood is urban). Standard errors clustered at the district level are reported in brackets.F-statistics for instrument at IV estimations are included in braces. *** p<0.01, ** p<0.05, * p<0.1

TABLE C4: IMPACT ON INDIVIDUAL PRODUCT QUALITY

Outcome Log(Product quality)	Weighting	All		Targeted		Incumbents	
		ITT	IV	ITT	IV	ITT	IV
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Rice (lb.)	0.293	-0.027	-0.051	-0.028	-0.059	-0.024	-0.057
		[0.020]	[0.032]	[0.032]	[0.059]	[0.033]	[0.068]
Cooking oil (lb.)	0.094	0.001	0.002	-0.010	-0.021	-0.013	-0.031
		[0.016]	[0.029]	[0.021]	[0.047]	[0.021]	[0.054]
Sugar (lb.)	0.052	0.014***	0.026**	0.009**	0.020*	0.007*	0.016
		[0.004]	[0.010]	[0.004]	[0.012]	[0.004]	[0.011]
Pasta (lb.)	0.017	0.003	0.006	-0.009	-0.020	-0.012	-0.027
		[0.014]	[0.026]	[0.018]	[0.038]	[0.018]	[0.042]
Eggs (unit)	0.050	-0.039	-0.074	-0.044	-0.094	-0.052	-0.123
		[0.031]	[0.053]	[0.038]	[0.083]	[0.038]	[0.099]
Powdered milk (125 gr.)	0.062	0.042***	0.079***	0.044***	0.097**	0.046***	0.110**
		[0.012]	[0.022]	[0.014]	[0.039]	[0.014]	[0.047]
Chocolate (unit)	0.015	0.030	0.056	0.023	0.051	0.024	0.059
		[0.026]	[0.057]	[0.039]	[0.097]	[0.040]	[0.110]
Sardines (unit)	0.014	0.023	0.044	0.035*	0.076	0.039*	0.093
		[0.014]	[0.032]	[0.019]	[0.051]	[0.020]	[0.061]
Green beans (lb.)	0.063	-0.007	-0.014	0.000	-0.001	-0.004	-0.010
		[0.011]	[0.021]	[0.015]	[0.032]	[0.016]	[0.038]
Onions (lb.)	0.020	-0.014	-0.027	-0.023	-0.049	-0.021	-0.049
		[0.014]	[0.030]	[0.020]	[0.055]	[0.021]	[0.062]
Salami (lb.)	0.048	-0.023	-0.044	-0.005	-0.012	0.004	0.011
		[0.020]	[0.041]	[0.028]	[0.061]	[0.027]	[0.064]
Chicken (lb.)	0.170	0.008	0.015	0.004	0.008	0.008	0.019
		[0.011]	[0.021]	[0.015]	[0.033]	[0.016]	[0.040]
Cod (lb.)	0.018	-0.018	-0.034	-0.022	-0.046	-0.026	-0.060
		[0.018]	[0.038]	[0.018]	[0.043]	[0.018]	[0.050]
Flour (lb.)	0.010	-0.019	-0.036	-0.015	-0.033	-0.008	-0.021
		[0.020]	[0.039]	[0.027]	[0.057]	[0.027]	[0.065]
Bread (unit)	0.074	0.030	0.057	0.001	0.003	0.010	0.023
		[0.021]	[0.046]	[0.020]	[0.044]	[0.020]	[0.049]

Note: Each entry shows an estimate of the impact of an increase in competition on the price of different products. Column [1] shows the weighting of each product in the final retailer price. Columns [2]-[3] use all the retailers; columns [4]-[5] use retailers in targeted neighborhoods; and columns [6]-[7] use incumbent retailers in targeted neigborhoods. All columns report the estimations while controlling for the baseline log(price). *** p<0.01, ** p<0.05, * p<0.1

TABLE D1. IMPACT OF COMPETITION ON QUALITY
(All districts)

Outcome	Observations	Clusters (number of districts)	Intention-to-treat: OLS estimation: 1 (Entry>0) = 1 (Randomized entry>0)			Average treatment effect (Observed entry>0), instrumented with 1 (Randomized entry>0)		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Service quality (quality ladder)								
Number of brands	72	72	0.090	0.071	0.045	0.162	0.139	0.093
			[0.249]	[0.073]	[0.087]	[0.431]	[0.126]	[0.142]
						{27.000}	{21.000}	{14.300}
Price range	376	72	-0.048	-0.040	0.012	-0.097	-0.076	0.023
			[0.055]	[0.054]	[0.047]	[0.125]	[0.106]	[0.090]
						{10.700}	{14.000}	{15.500}
Quality range	376	72	0.083	0.163	0.241	0.168	0.313	0.494
			[0.257]	[0.226]	[0.213]	[0.492]	[0.432]	[0.409]
						{10.700}	{12.200}	{13.400}
Brand-quality range	376	72	0.139	0.158	0.075	0.280	0.303	0.153
			[0.196]	[0.175]	[0.187]	[0.346]	[0.321]	[0.354]
						{10.700}	{12.400}	{13.200}
Variety-quality range	376	72	0.065	0.040	-0.046	0.130	0.077	-0.094
			[0.078]	[0.082]	[0.088]	[0.168]	[0.160]	[0.183]
						{10.700}	{12.200}	{13.200}
Service quality								
Store cleanliness	400	72	-0.037	-0.133	0.151	-0.074	-0.251	0.305
			[0.213]	[0.192]	[0.229]	[0.425]	[0.376]	[0.434]
						{10.700}	{12.700}	{14.100}
Log (employees)	400	72	0.125*	0.044	-0.006	0.248**	0.082	-0.011
			[0.065]	[0.044]	[0.044]	[0.107]	[0.075]	[0.086]
						{10.700}	{13.200}	{14.800}
Time shopping (minutes)	2117	72	4.691	2.384	1.879	8.385	4.242	3.624
			[4.531]	[4.111]	[3.984]	[8.115]	[7.311]	[7.661]
						{27.200}	{30.500}	{21.500}
Delivery	2118	72	0.056	0.044	0.040	0.100	0.078	0.076
			[0.063]	[0.044]	[0.042]	[0.111]	[0.077]	[0.079]
						{27.200}	{30.800}	{21.900}
Service-quality rating	2116	72	0.213**	0.235***	0.200**	0.380**	0.416**	0.379**
			[0.090]	[0.076]	[0.069]	[0.192]	[0.168]	[0.159]
						{27.200}	{30.900}	{21.900}
Baseline measures				X	X		X	X
District controls					X			X

Note: Each entry shows an estimate of the impact of an increase in competition on quality after treatment. Panel A uses the weighted log-price in the retailer database, while panel B uses the weighted log-price in the beneficiary database. Columns [1] and [2] report sample sizes. Columns [3] and [6] report the estimation with no controls. Columns [4] and [7] control for the baseline log(price), the baseline number of retailers and the baseline product quality. Columns [5] and [8] report the estimates with neighborhood controls : 1 (if neighborhood is targeted), province fixed effects, the average education and income of households in the district and 1 (if neighborhood is urban). Standard errors clustered at the district level are reported in brackets. F-statistics for instrument at IV estimations are included in braces. *** p<0.01, ** p<0.05, * p<0.1

TABLE D2. IMPACT OF COMPETITION ON QUALITY
(Incumbent retailers in targeted districts)

Outcome	Observations	Clusters (number of districts)	Intention-to-treat OLS estimation: 1 (Entry>0) = 1 (Randomized entry>0)			Average treatment effect (Observed entry>0), instrumented with 1 (Randomized entry>0)		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Service quality (quality ladder)								
Number of brands	70	70	0.083	0.065	0.043	0.153	0.129	0.089
			[0.252]	[0.074]	[0.088]	[0.445]	[0.129]	[0.144]
						{25.400}	{20.400}	{13.900}
Price range	199	67	-0.017	-0.017	0.025	-0.041	-0.041	0.071
			[0.076]	[0.068]	[0.060]	[0.188]	[0.162]	[0.151]
						{6.500}	{8.200}	{7.100}
Quality range	199	67	0.702**	0.563*	0.268	1.717**	1.369*	0.758
			[0.279]	[0.291]	[0.225]	[0.808]	[0.731]	[0.607]
						{6.500}	{6.900}	{6.100}
Brand-quality range	199	67	0.353	0.234	0.062	0.862	0.568	0.176
			[0.258]	[0.272]	[0.220]	[0.557]	[0.580]	[0.572]
						{6.500}	{6.900}	{6.000}
Variety-quality range	199	67	0.115	0.097	-0.020	0.282	0.238	-0.058
			[0.112]	[0.111]	[0.102]	[0.255]	[0.248]	[0.281]
						{6.500}	{6.800}	{6.000}
Service quality								
Store cleanliness	212	70	0.053	0.123	0.120	0.126	0.297	0.340
			[0.295]	[0.279]	[0.319]	[0.692]	[0.639]	[0.831]
						{6.500}	{6.500}	{6.300}
Log (employees)	212	70	0.088	-0.018	-0.043	0.211	-0.044	-0.117
			[0.090]	[0.056]	[0.047]	[0.184]	[0.142]	[0.141]
						{6.500}	{6.700}	{6.600}
Time shopping (minutes)	1563	71	7.656	5.169	5.331	13.656	9.292	10.326
			[4.981]	[4.298]	[4.006]	[8.996]	[7.819]	[7.929]
						{26.100}	{27.200}	{19.300}
Delivery	1563	71	0.062	0.037	0.019	0.110	0.066	0.037
			[0.071]	[0.048]	[0.048]	[0.125]	[0.086]	[0.090]
						{26.100}	{27.300}	{19.700}
Service-quality rating	1561	71	0.264**	0.273***	0.211**	0.470**	0.487**	0.402**
			[0.106]	[0.087]	[0.078]	[0.225]	[0.196]	[0.172]
						{26.100}	{27.600}	{19.700}
Baseline measures				X	X		X	X
District controls					X			X

Note: Each entry shows an estimate of the impact of an increase in competition on quality after treatment. Panel A uses the weighted log-price in the retailer database, while panel B uses the weighted log-price in the beneficiary database. Columns [1] and [2] report sample sizes. Columns [3] and [6] report the estimation with no controls. Columns [4] and [7] control for the baseline log(price), the baseline number of retailers and the baseline product quality. Columns [5] and [8] report the estimates with neighborhood controls : 1 (if neighborhood is targeted), province fixed effects, the average education and income of households in the district and 1 (if neighborhood is urban). Standard errors clustered at the district level are reported in brackets. F-statistics for instrument at IV estimations are included in braces.*** p<0.01, ** p<0.05, * p<0.1

TABLE E1. IMPACTS OF COMPETITION ON CLIENTELE
(All districts)

Outcome	Observations	Clusters (number of districts)	Intention-to-treat: OLS estimation: 1 (Entry>0) = 1 (Randomized entry>0)			Average treatment effect: IV estimation: 1 (Entry>0) =1 (Observed entry>0), instrumented with 1 (Randomized entry>0)		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
(A) Retailer measures								
Number of customers on best day	400	72	-31.156	-39.497	-15.33	-61.893	-71.822	-29.384
			[39.666]	[39.963]	[30.937]	[77.982]	[72.597]	[59.618]
						{10.700}	{15.200}	{16.800}
Share of customers CCT beneficiaries	366	70	0.464	-0.487	-1.258	0.946	-0.957	-2.682
			[3.619]	[3.437]	[2.964]	[7.189]	[6.775]	[6.233]
						{10.700}	{12.100}	{12.200}
(B) Consumer measures								
Switch to entrant retailer	1400	71	0.057***	0.055***	0.072**	0.099**	0.095**	0.146**
			[0.018]	[0.016]	[0.024]	[0.035]	[0.032]	[0.056]
						{27.200}	{31.000}	{22.000}
Baseline measures				X	X		X	X
District controls					X			X

Note: Each entry shows an estimate of the impact of an increase in competition on several outcomes after treatment. Panel A uses information from the retailer database, while panel B uses information from the beneficiary database. Columns [1] and [2] reports sample sizes. Columns [3] and [6] report the estimation with no controls. Columns [4] and [7] control for the baseline log(price), the baseline number of retailers, the baseline product quality, and the baseline value of the dependent variable. Columns [5] and [8] report the estimates with neighborhood controls : 1(if neighborhood is targeted), province fixed effects, the average education and income of households in the district and 1(if neighborhood is urban). Standard errors clustered at the district level are reported in brackets. IV first-stage F-statistics are included in braces. *** p<0.01, ** p<0.05, * p<0.1

TABLE E2. IMPACT OF COMPETITION ON CLIENTELE
(Incumbent retailers in targeted districts)

Outcome	Observations	Clusters (number of districts)	Intention-to-treat			Average treatment effect		
			OLS estimation: 1 (Entry>0) = 1 (Randomized entry>0)			IV estimation: 1 (Entry>0) =1 (Observed entry>0), instrumented with 1 (Randomized entry>0)		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
(A) Retailer measures								
Number of customers on best day	212	70	-25	-17.025	-20.666	-59.866	-38.511	-53.615
			[38.883]	[38.453]	[36.308]	[101.946]	[89.762]	[96.822]
						{6.500}	{8.800}	{7.500}
Share of customers CCT beneficiaries	193	64	-5.325	-4.732	-4.081	-13.763	-12.012	-12.027
			[3.793]	[3.096]	[2.695]	[11.338]	[9.779]	[9.856]
						{6.500}	{6.900}	{5.600}
Baseline measures				X	X		X	X
District controls					X			X

Note: Each entry shows an estimate of the impact of an increase in competition on several outcomes after treatment. Panel A uses information from the retailer database, while panel B uses information from the beneficiary database. Columns [1] and [2] reports sample sizes. Columns [3] and [6] report the estimation with no controls. Columns [4] and [7] control for the baseline log(price), the baseline number of retailers, the baseline product quality, and the baseline value of the dependent variable. Columns [5] and [8] report the estimates with neighborhood controls : 1(if neighborhood is targeted), province fixed effects, the average education and income of households in the district and 1(if neighborhood is urban). Standard errors clustered at the district level are reported in brackets. IV first-stage F-statistics are included in braces. *** p<0.01, ** p<0.05, * p<0.1