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ABSTRACT

The potential benefits of demand side interventions may leak into the profits of suppliers whenever there is market power. In those situations, governments could attempt to regulate the market or to increase competition. We provide the first experimental evidence on the effect of increased competition on prices and quality relying on an intervention that randomized the entry of retail firms into 72 local markets in the context of a conditional cash transfer program. Six months after the intervention, product prices decreased by about 5 percent while service quality perceived by consumers improved.

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1. Introduction

Governments often face uncertainty about how to best regulate markets. This is more manifest in developing countries where evidence is scant and state capacity is often low. In fact, whenever there is market power, demand side interventions may end up being leaked into the profits of suppliers (Laffont, 2005). Governments could then either attempt to regulate the market or, when possible, increase the degree of competition. In this paper we shed light on this issue in the context of a randomized control trial in the Dominican Republic that experimentally increase the level of competition across markets to which firm entry was restricted.

The experiment was part of an attempt to improve the operations of the conditional cash transfer program. This program provides monetary transfers to poor families that can only be used by means of a debit card that is only accepted by a network of grocery stores affiliated with the program. The program beneficiaries represent a large share of these stores' customers and sales. 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. In response to this situation, the Dominican government decided to fuel competition by expanding the retail network. 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.[2]

We use data on both retail stores and households collected at baseline and six months after the intervention. 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 ranging from two to six percent and to improvement in self-reported service quality.

Theoretical models of imperfect competition make various predictions about the competitive effects of market entry. Firms with market power may exploit their position to reduce quality, just as they may raise prices (Tirole, 1988). 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 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). Instead, the effect of competition on quality has been shown to be less clear-cut across the various models. Ultimately, the effect of competition on quality depends on the extent to which consumers perceive and value quality.

There is a well-established literature analyzing the effect of competition on prices and quality exploiting observational data.[3] Recently a large number of papers have analyzed the

[2]The National Statistics Office formally divides the country into provinces, municipalities, sections and neighborhoods. We use as our unit of analysis clusters of one or two adjacent neighborhoods which we call district.

effect of Walmart entry. Walmart's better technology allowed it to grow fast, and this growth, in turn reduced its operating costs through economies of scale (Basker, 2007), allowing then Walmart to achieve a dominant market position. Basker and Noel (2009) find that Walmart's entry into the grocery market caused competitors to respond by lowering prices by 1%, on average. They conclude that competitors' responses vary in line with their degree of differentiation from Walmart. The largest supermarket chains reduce their prices by less than half as much as smaller competitors. Low-end grocery stores, on the other hand, which compete more directly with Walmart, cut their prices by more than twice as much as higher-end stores. Ailawadi et al. (2010) find, using panel data, that incumbents suffer significant sales losses because of Wal-Mart entry.

The effect on Walmart entry is not limited to prices. Matsa (2011) finds that retail stores facing more intense from Walmart have fewer shortfalls and that competition provides an incentive to invest in service quality that provide consumers with a better shopping experience. In the same vein, Bennett and Yin (2014) 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 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.

Finally, in a very important recent contribution, Atkin et al. (2017) exploits an event study methodology to estimate the welfare effects of foreign retail entry in Mexico. Using a rich

[3] See Carlton (1983), Berry (1992), Bresnahan and Reiss (1989), Bresnahan and Reiss (1991), Goolsbee and Syverson (2008), Reiss and Spiller (1989) and Trapani and Oslon (1982) among others.

dataset, they decompose welfare gains into several distinct channels (three of them operating through the cost of living and three others through households income). They find that foreign retail entry causes large and significant welfare gains for the average household that are mostly driven by a reduction in the cost of living through entry affecting the prices of goods purchased by consumers both directly and indirectly. Indeed, about one quarter of the price index effect they estimate is due to pro-competitive effects on the prices charged by domestic stores, with the remaining three quarters due to the direct consumer gains from shopping at the new and different foreign stores.

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 of service. We show that in this market, populated by consumers that often walk to do their grocery shopping, entry of new stores is still effective even when on average there were about five stores per market. The design allows us to experimentally focus on the effect on the prices charged by incumbent stores. They are hence comparable to the pro-competitive effects estimated by Atkin et al. (2017). Given that the new stores entering the market in our study are very similar to the existing stores, and that we do not find exit of firms in our sample, this is most likely the only direct effect of firm entry in our environment.

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 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. One possibility would be to attempt to regulate the market. However, 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, when that is possible, provides an effective means of avoiding rent capture by suppliers.

2. 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). During 2011, the year of our study, the program had reached a plateau.

This CCT program includes two components. First, a health component provides households with a transfer of about US$ 19.5 per month. Transfers are contingent on parents bringing 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 and health. The program's second component focuses on education. Transfers depend 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.[4] The typical household (three children in school age) would receive a total monthly transfer of US$ 36, which would cover approximately 20% of the basic consumption.[5]

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 withdrawn from the bank but, instead, can only be spent by using a debit card[6] that works only in a network of program-affiliated retailers 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.[7] First, the government executing agency 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

[4] Households can also receive other money transfers that are deposited in their bank accounts, such as a subsidy for higher education, pensions for the elderly living in extreme poverty, a subsidy to buy gas and/or a subsidy to pay the electricity bill. These transfers could be used in the same retailers that are part of our study.
[5] The official value in the basic basket of goods is reflected in the extreme poverty line which at the time was 43 dollars per capita.
[6] This debit card can be used only by the head of household.
[7] 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" set by the Social Subsidies Administration (ADESS), the program executing agency.

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.

Entering the network can be costly for many stores. 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 places, 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.[8] This implies a loss of consumer surplus and therefore a potential welfare loss.

3. Experimental Design

In response to this situation, the authorities designed a plan for the expansion of the retail network so that to encourage competition and thus increase the effectiveness of the subsidies awarded under the program. We collaborated with the CCT executing agency to propose a way of expanding the network allowing to experimentally evaluate the effects of an increase in market competition on goods prices and service quality.

The intervention consists of an exogenous randomized increase in the number of retailers associated with the network across districts. The actual implementation of the experiment was the responsibility of the CCT executing agency based on our guidelines. In our setting, *incumbents* are the stores already operating in the program network before the intervention took place. *Market entry* means that a store is approved by the CCT executing agency to sell to program beneficiaries. These retailers entering the program network are the *entrants.*

The CCT executing agency identified the geographic areas in which to implement the expansion 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

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

urban. In addition, they had to have at least one non-affiliated retailer that would be interested in joining the network. Ultimately, 72 districts in 17 municipalities were included in the experiment. These districts were used to provide the framework for randomization.

The intervention was implemented in three stages. First, before randomization, between December 2010 and May of 2011, the CCT executing agency collected in the identified areas 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 initial goal was to have at least three candidates for entry in each neighborhood. In those cases in which the search for potential entrants yielded few candidates, the 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.

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. Panel A of Table 1 describes the research sample. Before treatment, there were some 341 retailers operating in the network within these 72 districts. These 72 districts were formed by 72 targeted neighborhoods and 25 adjacent non-targeted neighborhoods. 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 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*).

The actual affiliation into the network was carried out in May-June 2011 by the executing agency using a standardized procedure. Actual affiliation, however, differed from the intended/randomized affiliation. Panel B of Table 1 describes the distribution of districts by 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%) there was perfect compliance with randomization.[9] In 28 districts (39%) fewer retailers than intended by randomization actually entered the network. This happened because, during visits to the stores and store audits done by the executing agency, some of the applicants were determined to not satisfy the eligibility criteria to be part of the network.[10] In 6 districts (8%), the executing agency incorporated into the network more retailers than our randomization allowed for. This was a failure on the part of the CCT executing agency to follow the intervention protocol that was discovered when performing an independent audit of compliance.

4. Empirical Strategy

In a context of perfect compliance random assignment would guarantee no selection into treatment status, making the identification of the average treatment effects straightforward by simply comparing mean outcomes of treated and control districts. In a context with

[9] 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.

[10] In other words, it was not the case that firms defied randomization. Several firms applied and wanted to become part of the network of retailers serving the CCT beneficiaries but were deemed ineligible by the executing agency.

some noncompliance random assignment can be still be exploited using an instrumental variable approach.

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, compliance improves when considering the extensive margin (i.e., whether there is at least one entrant into the market). Table 1 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 of the districts (30%) were randomized to entry but 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:

$$Y_{is} = \alpha + \gamma Z_S + \beta X_{is} + \varepsilon_{is} \qquad (1)$$

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.[11] X_{is} is a vector of pre-treatment characteristics. As is common

[11] 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 includes the pre-treatment value of Y_{is}.[12] 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.[13] Thus, we also estimate the following equation using two-stage least squares (2SLS):

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

where $T_S$is a dummy variable that captures actual observed entry into the market. We instrument T_S with Z_S. In all specifications, standard errors clustered at the district level.

5. Data

We collected retailer and household data before treatment in April and May 2011 (baseline) and after treatment in December 2011, six months after the intervention was completed (endline). We also obtained administrative information from the executing agency with the list of establishments operating in the network of retailers. We 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.

[12] Control variables also include the following pre-treatment variables: number of incumbent retailers and number of beneficiaries at baseline in the district, province fixed effects, percent of beneficiaries over population, average household income in the district, percent of population with primary complete and secondary complete, an indicator variable equal to one if the district is urban.

[13] 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.

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 and, for budgetary restrictions, a very limited number of retailers operating outside the network or outside the districts under analysis.[14] The survey of beneficiaries was designed based on 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.[15]

The retailer questionnaire was designed to collect very detailed information on prices and on the products sold by the retailers–our main outcomes of interest. Similarly to DellaVigna and Gentzkow (2017) we focused on products that are frequently sold and widely available. We collected information on 15 product categories: bread, rice, pasta, cooking oil, sugar, flour, powdered milk, onions, eggs, beans, cod, canned sardines, chicken, salami and chocolate. According to Social Protection Expenditure Survey, a nationally representative household expenditure survey collected in 2010, these 15 goods represent 85% of all non-perishable food products and 60% of all food products bought by an average household.[16]

For each one of these 15 products, we pre-specified the unit of measurement and then asked for information on the price, variety, and brand of the cheapest available option at the

[14] Appendix Table A1 describes the sample sizes associated with each of these three samples both at baseline and at endline.
[15] The final sample has a mean and a median size of 30 households per district; the smallest district has 24 households and the largest 60.
[16] Appendix Table A2 shows the share spent on each type of food. Notice that households spend 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.)

store.[17] Surveyors were instructed to ask respondents about the price usually paid by customers using the CCT debit card for the specific product-variety-brand combination. These price quotes are likely to be a mixture of posted prices and prices that arose after some bargaining between customers and retailers took place.

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 questions on consumer behavior including a module on expenditure in which we asked about total spending, brands, varieties and quantities of the same 15 items included in the retailer questionnaire.

Price measurement. The main outcome of interest are the prices paid by consumers as reported in the retailer survey. 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 price measure was computed by weighting each product by the average proportion of total household expenditure spent on each of the 15 items. These weights (one for each product and constant across retailers/households) were computed using the Social Protection Expenditure Survey from 2010.[18] A second price measure was obtained by a simple average of the prices.

In addition, we use the information in the household survey to build an alternative and independent measure of the average price of the basket. For each item, we derive the price

[17] We decided to focus on the cheapest alternative for two reasons. First, it was a simple way of anchoring the survey responses provided by retailers. Second, 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.
[18] The last column of Appendix Table A2 shows the weights.

paid by the consumer from the ratio of the total expenditure on that item and the total number of units bought. Some households did not report expenditure for all 15 items. Thus, 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. 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.

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 reported by retailers and consumers in the district ($\bar{P}_{js}^{R}/\bar{P}_{js}^{C}$) is a useful statistic for assessing how close these two measures are. We find that the average relative price for all products and districts is 0.99.

Product identity (barcodes). We use the reported information on product, brand, variety and measurement unit to build barcode-equivalent observations. For instance, in the case of the product "cooking oil" one barcode corresponds to olive oil (variety) produced by Mazola (brand) sold in one-liter bottles (unit). Table 3 reports some statistics that describe the products in our data. Three facts are worth noting. First, because the survey asked about the cheapest available barcode for each product, the number of barcodes in our data is relatively small (331 barcodes for the 15 products).[19] Nine of the 15 products have 15 barcodes or less, five have less than 35 barcodes and only one product (rice) captures a third of all the barcodes in the data. As a comparison, Broda and Weinstein (2010) report

[19] Some products (onions, chicken, and bread) are typically sold without a brand or a variety. These "generic" varieties were considered to share the same barcode within product.

that the average product category in the US has more than 14,076 barcodes. Second, the different barcodes within each product seem to be close substitutes. The coefficients of variation of the prices of 11 of these 15 products is smaller than 0.13. Third, stores could report prices for different barcodes for the same product in the baseline and endline surveys. For eleven of the products, only a quarter of the stores changed the reported barcode. We will report treatment effects of competition on the probability of changing brands, varieties, or barcodes.

Quality and clients. We are also interested in the quality of the service provided by the stores.[20] We 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 information 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 also use a set of district-, consumer-, and retailer-level measures as control variables. For instance, we use administrative information, disaggregated by

[20] 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.

district, on the total number of beneficiaries and the number of retailers operating in the CCT network at baseline.[21]

6. Results

Market description. Table 3 presents some descriptive statistics on both customers and retailers in the areas under study. Recall that the 72 districts are in 17 municipalities These districts are relatively far from one another by construction. Many of them are in different municipalities while others, located in the same municipality, are on average 2.8 miles apart from each other.[22,23]

In these districts retailers are small, owner-run "mom-and-pop" shops. In incumbent stores, program beneficiaries can use the CCT transfer money to purchase only food products (of any brand or variety).[24] Typically, these are non-perishable food products and only a very limited number of fresh products (fruits, vegetables or dairy products). People who shop in the areas under study are poor, with low levels of schooling, and earnings equivalent to slightly more than one quarter of the country's per capita GDP. As is common 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.

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

[21] For a full description of all the outcome and control variables, see Appendix Table A3.
[22] In each district, we computed the centroid of all retailers as an approximation to the relevant economic centroid of the district.
[23] Given that in our setting most people walk to the retailers, it would take more than 40 minutes for a person in one district to shop in another district within the same city.
[24] The CCT executing agency listed a set 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.

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 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.[25] 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. On average there are about 4.6 incumbent retailers operating in the 72 districts under study and serving an average of about 630 beneficiaries. The average distance between pairs of retailers in these districts is 1.3 miles in a context in which program beneficiaries' mobility is limited: only 15% of them own a car or a motorcycle and 95% of them shop only in a retailer in the program network located within 10 blocks of their house (roughly half a mile). Beneficiaries usually shop in just one store. Retailers can therefore 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

[25] From administrative data, we know that: the average household receives a monthly transfer of 36 dollars, there are on average 630 beneficiaries and 4.6 incumbent retailers in each district. From the retailer survey, we know that the average sales is 8850 dollars. Thus, ((36 x 630)/4.6)/8850=0.56. On average, these households spend about 62 dollars on perishable foods. Thus, ((62 x 630)/4.6)/8850=0.96

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.[26]

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. In fact, in our experiment entrants are similar to incumbents they sell products of similar brands and varieties at similar prices. The stores' characteristics are also similar.[27]

Internal validity. Figure 1 shows pre-treatment sample means of relevant outcomes and covariates and plots the p-values of *t*-tests of the differences in these means between non-intention-to-treat and intention-to-treat groups. Overall these differences are small and not statistically significant. We find one statistically significant difference at conventional levels out of 25 variables tested. 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. Importantly, the share of districts with non-targeted neighborhoods, the share of incumbent retailers, the number of

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[26] Unfortunately, we lack information to empirically test the existence of third-degree price discrimination which would require price data for beneficiaries and non-beneficiaries

[27] See Appendix Table A4.

beneficiaries, the price of the basket of goods, and our measure of service quality are all statistically similar in the intention-to-treat and non-intention-to-treat groups.

Effects on product availability. Competition could in principle change the characteristics of the goods sold by retailers (which in the data would translate into changes in brands or varieties) by inducing them to switch to different brand/varieties. Table 4 shows the treatment effects estimated using equations (1) and (2). 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.[28] Columns 3 and 4 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.[29] Columns 5 and 6 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 errors at the district level in parenthesis and, for the case of IV.[30]

We computed the share of products that changed brand, variety, and barcodes for each store. We find that in general the changes were not different in treated and control areas. Moreover, we ranked the barcodes according to its average pre-treatment price and find no

[28] 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.

[29] 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.

[30] The correlation between randomized and observed entry is 0.48. In all first stage regressions the coefficient on the instrument is positive and is statistically significant at 5% (or 1% depending on the specification). In all cases the first stage F statistic ranges from 8 to 30 depending on the model. For reasons of space we do not show first stage estimates. They are available from the authors upon request.

evidence that stores switched to initially cheaper barcodes as a result of the increased competition in their districts.

Effects on Prices. In Table 5, we present the effect of entry on log prices. Panel A shows retail prices, while Panel B shows prices as measured using household information. 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. The estimators are larger for the sample of the targeted neighborhoods than they are for the sample as a whole. Considering the simplest IV it is estimated that entry into the network decreases prices by 5.2 to 6.0 percent in the case of the sample of incumbent stores in the targeted neighborhoods. Intention-to-treat yields smaller estimates: the decrease in prices is 1.9 to 2.5 percent, with the estimates not varying much across specifications. This is consistent with having better compliance in locations where larger price drops were observed (for instance, because they were originally less competitive).[31] The second panel of Table 5 shows estimates of competition on prices using price measures derived from the consumer data. The estimated effects are similar to those estimated using retailer-level data. This is not surprising, since these two price measures are similar and it is reassuring, because these measures are independent of one another.

Table 6 reports some alternative specifications that show that results on prices are robust. Column 1 shows the estimates reported in column 6 of Table 5. Column 2 shows that the estimates are similar when average prices are constructed using simple (i.e., unweighted) averages. We do not have any a priori preference for using one measure (weighted) over the

[31] It is worth noting that we could not find statistically significant differences between districts that complied and districts that did not complied with treatment. See Appendix Table A5

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 slightly more precise estimates and are easier to be interpreted as the value of a basket of goods consumed by this population.

Columns 3 and 4 in Table 6 shows results in which we estimate equations (1) and (2) but the outcome Y_{jis} is the log price of product j in retailer/household i in district s. We include product and product-brand 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 are of similar magnitude to those presented in Table 5. We have also estimated the treatment effects individually for each of the 15 products under analysis. 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.[32] Columns 4 and 5 restrict the estimation to those products that did not change barcodes between baseline and endline surveys. The point estimates are negative and of similar magnitudes as those in our benchmark specification but the standard errors are larger (since the sample size is smaller).

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 5. 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 effects are equal.

[32] The results are shown in Appendix Table A6.

Other effects of competition. Entry does appear to have some effect on the quality of the service provided by retailers in our sample. Table 8 shows the effect on targeted neighborhoods (results are very similar for the other samples). Panel A shows some indirect measures of service quality. Most of the results are not statistically significant although it is possible that we lack statistical power to detect positive effects. The only robust effect of competition seems to be on how customers rate their shopping experience, with that rating improving in treated areas. Panel B shows the effects on clients. The negative effect of market entry on prices seems to have been fueled by a reduction in the number of shoppers who went to retail stores in treated areas. These results are very noisy, however. 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. Finally, panel C 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-experimental 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.

7. 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 service quality. We found a significant and very robust reduction in prices that ranges from two to six percent as a result of the increase in competition and some evidence that quality of the service provided to consumers improved six months after the intervention. We find these results to be economically large. They are in line with those find in the literature in other developing countries. For instance, Atkin et al. (2017) find that in the case of Mexico, prices of incumbent domestic retailers decreased approximately 4 percent twelve quarters after the entry of a global retail chain into the market.

Our paper contributes to the literature on the effects of competition. To the extent of our knowledge, it is the first paper to provide field experimental evidence that increased competition significantly decreases prices. Our results show that at least partially removing that market barrier was conducive to consumer surplus gains among the poor. As in the case of any study on a given industry, it may not be possible to directly extrapolate our conclusions to other industries or populations. Our results are most relevant for situations in which entry is artificially restricted in areas in which consumers are not mobile.

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. 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. In settings like the one in our study, consumers would be better off with free entry. Moreover, governments could also consider facilitating entry, for instance, by helping stores opening bank accounts, obtaining a tax identification number, or paying for the debit card reading machine.

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Tables and Figures

TABLE 1: EXPERIMENT DESCRIPTION
(Research sample and compliance by number of retailers randomized to entry)

Number of retailers randomized to entry	Panel A: Intervention and Research Sample				Panel B: Compliance				
	Number of incumbent retailers in sample	Number of districts	Number of neighborhoods in each district		Observed entry (number of retailers)				
			Targeted	Not targeted	0	1	2	3	4
0	107	21	21	6	17	2	2	0	0
1	71	18	18	5	3	14	1	0	0
2	81	18	18	6	5	8	5	0	0
3	82	15	15	8	5	3	4	2	1
Total	341	72	72	25	30	27	12	2	1

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

TABLE 2: BRANDS, VARIETIES AND BARCODES BY PRODUCT

	Number of varieties	Number of brands	Number of variety-brands combinations (barcodes)	Coefficient of variation of the price	Share of retailers that changed barcode between baseline and endline surveys	
					Control districts	Treated districts
Rice (lb.)	4	112	118	0.10	0.40	0.41
Cooking oil (lb.)	2	7	7	0.13	0.25	0.26
Sugar (lb.)	2	11	12	0.07	0.14	0.18
Pasta (lb.)	1	8	8	0.10	0.36	0.41
Eggs (unit)	2	22	23	0.11	0.12	0.18
Powdered milk (125 gr.)	4	8	8	0.12	0.24	0.18
Chocolate (unit)	3	7	7	0.12	0.18	0.16
Sardines (unit)	2	22	23	0.72	0.50	0.49
Green beans (lb.)	5	30	34	0.09	0.26	0.30
Onions (lb.)	3	3	4	0.25	0.01	0.01
Salami (lb.)	3	22	28	0.26	0.29	0.31
Chicken (lb.)	2	9	10	0.12	0.20	0.24
Cod (lb.)	3	4	6	0.09	0.06	0.01
Flour (lb.)	1	15	15	0.12	0.19	0.20
Bread (unit)	1	28	28	0.29	0.22	0.19

TABLE 3. DESCRIPTIVE STATISTICS

	Mean	Standard Deviation
A. Retailer characteristics		
% of stores managed by owner	0.822	0.383
Number of employees	5.025	2.962
% of sales financed by CCT	0.561	0.310
CCT beneficiaries non-perishable food expenditure / sales	0.962	0.120
% of retailers that declare an increase in sales after entering the CCT program	0.964	0.187
% of stores with all prices posted for public view	0.415	0.493
% of stores that never bargain over prices	0.441	0.497
B. Consumer characteristics		
Individual income / GDP per capita	0.270	0.145
% of population with primary education or lower	0.621	0.077
% of households that own a car or motorcycle	0.163	0.370
% of households that shop in a retail store within 10 blocks of their house	0.550	0.498
% of households with a non-CCT retail store within 10 blocks of their house	0.958	0.200
Number of retailers in which households usually shop	1.034	0.181
% beneficiaries aware of prices before shopping	0.281	0.449
C. Market (district) characteristics		
Number of incumbent retailers per district	4.64	3.05
Number of beneficiaries per district	631	2.47
Distance between pairs of districts (within city, in miles)	2.81	2.10
Distance between pair of incumbent retailers (within district, in miles)	1.32	2.62
Minimum distance between pairs incumbent retailers (within district, in miles)	0.23	0.35
Number of entrants per district	0.85	0.49
Distance between pairs of incumbent and entrant retailers (within district, in miles)	1.63	2.81
Minimum distance between pairs incumbent and entrant retailers (within district, in miles)	0.42	0.81

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

TABLE 4: IMPACT OF PRODUCT AVAILABILITY
(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]
Percent of products that changed barcode	250	72	0.010	0.010	0.022	0.026
			[0.025]	[0.023]	[0.050]	[0.051]
Percent of products that changed to a cheaper barcode	250	72	0.006	-0.003	0.014	-0.008
			[0.024]	[0.018]	[0.048]	[0.045]
Percent of products that changed brand	250	72	0.011	0.022	0.025	0.055
			[0.024]	[0.026]	[0.050]	[0.058]
Percent of products that changed variety	250	72	0.002	-0.000	0.004	-0.000
			[0.007]	[0.007]	[0.016]	[0.016]
Baseline and District controls				X		X

Note: Each entry shows an estimate of the impact of an increase in competition on different outcomes at the retail level. The percent of product that changed barcode/brand/variety is defined as the percent of the 15 products that changed barcode/brand/variety between baseline and endline. Columns [1] and [2] report sample sizes. Columns [3] and [5] report the estimation with no controls. Columns [4] and [6] control for the baseline log(price), the baseline number of retailers, the baseline percentile rank of price of the product in control areas and 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 not reported and in most cases larger than 10. *** p<0.01, ** p<0.05, * p<0.1

TABLE 5. 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]
(A) Retailer measures	*All districts*						
	1(Entry>0)	399	72	-0.020**	-0.014**	-0.040**	-0.027*
				[0.007]	[0.007]	[0.018]	[0.014]
	Targeted neighborhoods						
	1 (Entry>0)	254	72	-0.026**	-0.020**	-0.056**	-0.052**
				[0.009]	[0.007]	[0.024]	[0.021]
	Incumbent retailers in targeted neigh.						
	1 (Entry>0)	212	70	-0.025**	-0.019**	-0.060**	-0.052**
				[0.009]	[0.007]	[0.028]	[0.023]
(B) Consumer measures	*Targeted neighborhoods*						
	1 (Entry>0)	2025	72	-0.024**	-0.015**	-0.043**	-0.028*
				[0.008]	[0.007]	[0.017]	[0.016]
	Shop at incument retailers						
	1 (Entry>0)	1493	71	-0.030**	-0.020**	-0.052**	-0.037*
				[0.010]	[0.009]	[0.020]	[0.020]
Baseline and 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 [5] report the estimation with no controls. Columns [4] and [6] control for the baseline log(price), the baseline number of retailers, the baseline percentile rank of price of the product in control areas and 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 not reported and in most cases larger than 10. *** p<0.01, ** p<0.05, * p<0.1

TABLE 6: IMPACT OF COMPETITION ON PRODUCT PRICES

Robustness (IV Estimation)

	Price index (weighted)	Price index (unweighted)	Individual product prices (pooled)	Individual product prices (pooled)	Individual product prices (pooled)	Price index (weighted)	Individual product prices (pooled)	Individual product prices (pooled)	Individual product prices (pooled)
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
All districts									
1(Entry>0)	-0.027*	-0.024	-0.026	-0.034**	-0.034**	-0.028	-0.028	-0.020	-0.020
	[0.015]	[0.016]	[0.018]	[0.016]	[0.016]	[0.040]	[0.019]	[0.019]	[0.019]
Targeted neighborhoods									
1 (Entry>0)	-0.052**	-0.062**	-0.055*	-0.059**	-0.062**	-0.056	-0.052*	-0.045	-0.047
	[0.021]	[0.029]	[0.030]	[0.027]	[0.028]	[0.062]	[0.031]	[0.030]	[0.030]
Incumbent retailers in targeted neigh.									
1 (Entry>0)	-0.052**	-0.060*	-0.054*	-0.058*	-0.062**	-0.056	-0.043	-0.035	-0.038
	[0.023]	[0.031]	[0.031]	[0.030]	[0.031]	[0.070]	[0.032]	[0.030]	[0.031]
Samples	Whole	Whole	Whole	Whole	Whole	Non-barcode change	Non-barcode change	Non-barcode change	Non-barcode change
Baseline and District controls	X	X	X	X	X	X	X	X	X
Product fixed effects			X				X		
Product-brand fixed effects				X				X	
Product-brand-variety fixed effects					X				X

Note: Each entry shows an estimate of the impact of an increase in competition on the weighted and unweighted price index and the log of individual product prices after treatment. Columns [1] through [5] report the estimations over the whole sample of products, while columns [6] through [9] report the results over the sample of products that did not change barcode. Baseline controls include the log(price) index, the baseline number of retailers and the baseline quality index of the product. District controls 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). Additional fixed effects in columns [4] and [8] include the interaction between product and brand, and columns [5] and [9] control for the triple interaction of product, brand and variety. Standard errors clustered at the district level are reported in brackets. IV first-stage F-statistics are not reported and in most cases larger than 10. *** 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: 1(Entry=j)=1(Observed entry=j), instrumented with 1(Randomized entry=j), j=1,2 or more	
	[1]	[2]	[3]	[4]	[5]	[6]
All neighborhoods						
1(Randomized entry=1)	399	72	-0.020**	0.002	-0.034**	0.016
			[0.009]	[0.009]	[0.016]	[0.023]
1(Randomized entry=2 or 3)			-0.020**	-0.019**	-0.047	-0.058*
			[0.008]	[0.008]	[0.029]	[0.033]
Targeted neighborhoods						
1(Randomized entry=1)	254	72	-0.023**	-0.008	-0.034*	0.010
			[0.011]	[0.010]	[0.020]	[0.031]
1(Randomized entry=2 or 3)			-0.027**	-0.025***	-0.071**	-0.087*
			[0.010]	[0.008]	[0.035]	[0.048]
Incumbent retailers in targeted neigh.						
1(Randomized entry=1)	212	70	-0.028**	-0.013	-0.043**	-0.006
			[0.011]	[0.010]	[0.021]	[0.024]
1(Randomized entry=2 or 3)			-0.023**	-0.021**	-0.072*	-0.070*
			[0.011]	[0.008]	[0.041]	[0.039]
Baseline and 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 [5] report the estimation with no controls. Columns [4] and [6] control for the baseline log(average price), the baseline number of retailers, the baseline percentile rank of price of the product in control areas and 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

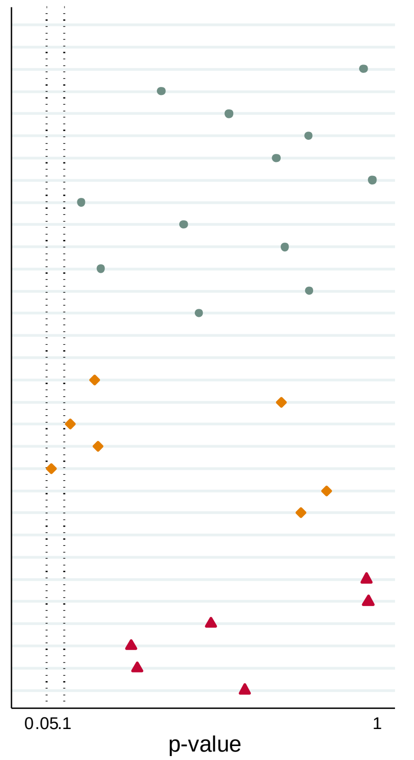
TABLE 8: IMPACT OF COMPETITION CLIENTS, SERVICE QUALITY AND SPILLOVERS
(Targeted Neighborhoods)

	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]	[2]	[3]	[4]	[5]	[6]
Panel A: Service quality						
Store cleanliness	254	72	0.105	0.142	0.228	0.372
			[0.290]	[0.295]	[0.614]	[0.721]
Time shopping (minutes)	2117	72	4.691	1.879	8.385	3.624
			[4.531]	[3.984]	[8.115]	[7.661]
Delivery	2118	72	0.056	0.040	0.100	0.076
			[0.063]	[0.042]	[0.111]	[0.079]
Service-quality rating	2116	72	0.213**	0.200**	0.380**	0.379**
			[0.090]	[0.069]	[0.192]	[0.159]
Panel B: Clients						
Number of customers on best day	254	72	-15.665	-11.160	-33.898	-27.090
			[37.136]	[35.894]	[84.195]	[85.438]
Share of customers CCT beneficiaries	228	70	-5.244	-4.810*	-12.211	-13.432
			[3.799]	[2.753]	[9.907]	[10.069]
Switch to entrant retailer	1400	71	0.057***	0.072**	0.099**	0.146**
			[0.018]	[0.024]	[0.035]	[0.056]
Panel C: Spillovers						
Log (average price after treatment) -weighted *of non-CCT retailers in experimental districts*	63	33	-0.003	0.028	-0.006	0.065
			[0.023]	[0.022]	[0.041]	[0.059]
Log (average price after treatment) -weighted of CCT retailers in non-experimental districts	136	25	-0.014	0.010	-0.024	0.012
			[0.013]	[0.033]	[0.024]	[0.037]
Baseline and District controls				X		X

Note: Each entry shows an estimate of the impact of an increase in competition on different outcomes at the retail level. Columns [1] and [2] report sample sizes. Columns [3] and [5] report the estimation with no controls. Columns [4] and [6] control for the baseline outcome, the baseline log(price), the baseline number of retailers, the baseline percentile rank of price of the product in control areas and 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 not reported and in most cases larger than 10. *** p<0.01, ** p<0.05, * p<0.1

FIGURE 1. DIFFERENCES IN PRE-TREATMENT SAMPLE MEANS

	T	C	N
A. District characteristics			
Log (total beneficiaries - 2010)	6.453	6.441	72
Change in log (total beneficiaries - 2009/2010)	0.172	0.211	72
Log (sales - 2010)	11.340	11.149	69
Change in log (sales -2009/2010)	1.231	1.033	67
Number of incumbent retailers - 2010	4.725	5.190	72
Change in log (number of retailers - 2009/2010)	0.444	0.442	72
% Solidaridad program beneficiaries / population	0.383	0.325	72
% of population with primary education or lower	0.613	0.628	72
% of population with incomplete secondary	0.208	0.213	72
% of population with completed secondary education	0.179	0.159	72
1 (if district is urban)	0.770	0.796	72
District includes non-targeted neighborhoods	0.373	0.286	72
B. Retailer characteristics			
Log-price index - pre-treatment (weighted)	-0.338	-0.323	400
Percentage male	0.839	0.853	401
1 (if the surveyed person is the retailer's owner)	0.623	0.688	401
1 (if has more than a completed primary education)	0.613	0.679	401
Log (total employees)	1.526	1.412	401
Log (sales)	9.106	9.088	388
Share of retailers in targeted neighborhood	0.651	0.615	401
C. Consumer characteristics			
Log demeaned price (weighted)	-3.588	-3.588	2125
Service quality (rating 1-10)	8.983	8.979	2248
Head of household's age	52.346	53.021	2250
% of heads of household who are married	0.538	0.576	2250
% of head of household who work	0.532	0.557	2250
% of head of household who are male	0.618	0.635	2250



Note: Panels A, B and C feature the differences in pre-treatment sample means for district, retailer and consumer characteristics, respectively. Column T, column C and column N present, in that order, the mean for the treatment and the control groups, and the sample size. Each point is the p-value of the difference in means between the treatment and control groups.

Appendix I: Price Variable Construction

1. Data Sources

Price data were 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 (barcodes) 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. 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. Measures

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.

4. 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.

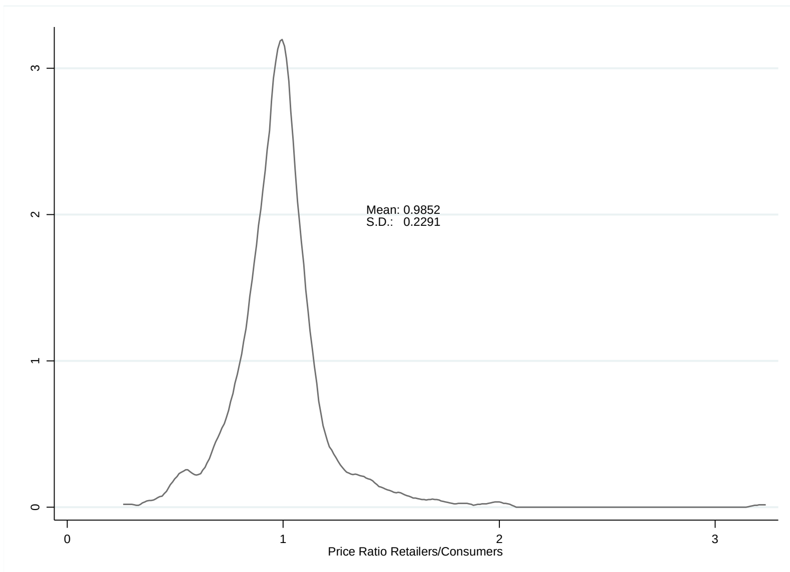
The weights W_k were compared with the results of a nationally representative survey of program beneficiaries, the Evaluation Survey of Social Protection (EEPS), which was conducted in 2010/2011. In this survey, households were queried about their expenditure on a broader set of products. Appendix Table A1 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.

5. 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 $\bar{P}_{ks}^{R}$ 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.

APPENDIX FIGURE 1: Distribution of ($\bar{P}^{R}_{ks}/\bar{P}^{C}_{ks}$)



Appendix Tables

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

APPENDIX TABLE A2. EEPS 2010 - 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.

APPENDIX TABLE A3. 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 education	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		
Average Price (weighted)	$\text{Log}(P_{i0})$, where: $P_{it} = \sum_{k=1}^{K} W_k * p_{itk}$ p_{ik} 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
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
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
Share of retailers in targeted neighborhood	1 (If retailer is in a targeted neighborhood)	Retailer survey

Consumer Characteristics		
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 homogenous. $\overline{p_{kt}}$ 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 homogenous. $\overline{p_{kt}}$ 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
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 TABLE A4. AVERAGE CHARACTERISTICS OF ENTRANT VS INCUMBENT RETAILERS AT BASELINE

	Entrants	Incumbents	p-value of difference	Number of observations
	[1]	[2]	[3]	[4]
Log-price index - pre-treatment (weighted)	-0.343	-0.332	0.443	400
	[0.094]	[0.080]		
Log-price index - pre-treatment (unweighted)	-0.258	-0.248	0.388	400
	[0.077]	[0.082]		
1 (retailer does special sales/promotions)	0.431	0.386	0.527	401
	[0.500]	[0.487]		
Log (sales)	8.989	9.117	0.371	388
	[0.904]	[0.821]		
Log (total employees)	1.399	1.509	0.028	401
	[0.378]	[0.484]		
Percent male	0.804	0.849	0.494	401
	[0.401]	[0.359]		
1 (if the surveyed person is the retailer's owner)	0.627	0.643	0.822	401
	[0.488]	[0.480]		
1 (if has more than complete primary education)	0.686	0.623	0.318	401
	[0.469]	[0.485]		
% Solidaridad Clients	49.25	48.037	0.8424	347
	[26.570]	[23.842]		

APPENDIX TABLE A5. DIFFERENCES IN NON-COMPLIERS AND COMPLIERS

	Compliers	Non-compliers	p-value of difference	Number of obs.
	[1]	[2]	[3]	[4]
A. District characteristics				
Log (total beneficiaries - 2010)	6.417	6.556	0.584	72
	[0.977]	[0.626]		
Change in log (total beneficiaries - 2009/2010)	0.168	0.232	0.184	72
	[0.146]	[0.236]		
Log (sales - 2010)	11.285	11.295	0.978	69
	[1.229]	[1.337]		
Change in log (sales -2009/2010)	0.971	1.774	0.327	67
	[2.577]	[3.703]		
Number of incumbent retailers - 2010	5.945	6.294	0.852	72
	[6.753]	[6.469]		
Change in log (number of retailers - 2009/2010)	0.402	0.579	0.316	72
	[0.596]	[0.744]		
% Solidaridad program beneficiaries / population	0.393	0.290	0.099	72
	[0.234]	[0.183]		
Average monthly household income (US$)	491.088	495.965	0.842	72
	[86.411]	[93.582]		
% of population with completed primary education or lo	0.618	0.615	0.887	72
	[0.080]	[0.067]		
% of population with incomplete secondary education	0.208	0.215	0.615	72
	[0.052]	[0.048]		
% of population with completed secondary education or	0.174	0.170	0.809	72
	[0.065]	[0.045]		
1 (if district is urban)	0.745	0.882	0.216	72
	[0.413]	[0.332]		
District includes non-targeted neighborhoods	0.400	0.176	0.093	72
	[0.494]	[0.393]		
B. Retailer characteristics				
Log-price index - pre-treatment (weighted)	-0.336	-0.338	0.235	400
	[0.084]	[0.082]		
Percentage male	0.837	0.839	0.543	401
	[0.370]	[0.368]		
1 (if the surveyed person is the retailer's owner)	0.642	0.623	0.897	401
	[0.480]	[0.485]		
1 (if has more than a completed primary education)	0.623	0.613	0.496	401
	[0.485]	[0.488]		
Log (total employees)	1.486	1.526	0.397	401
	[0.461]	[0.482]		
Log (sales)	9.083	9.105	0.429	388
	[0.822]	[0.856]		
Share of retailers in targeted neighborhood	0.601	0.651	0.117	401
	[0.491]	[0.478]		

APPENDIX TABLE A6. 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