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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, Revised September 2018

The authors declare to have no relevant interests related to the research describe in this paper. This research was made possible by the collaboration of the Administradora de Subsidios Sociales of the Dominican Republic and the Inter-American Development Bank. The collaboration placed no constraints on the conclusions of the analysis. The opinions expressed in this publication are those of the authors and do not necessarily reflect the views of the Inter-American Development Bank, its Board of Directors, any of the countries they represent, or the National Bureau of Economic Research. This experiment has been registered with the American Economic Association RCT Registry under number AEARCTR-0000343. We thank Francisca Müller, Juan Sebastian Muñoz, Dario Romero and Dario Salcedo for their excellent project management and research assistance. We are grateful for the very useful comments made by Daron Acemoglu, Alberto Chong, Matthew Gentzkow, Gary Libecap, David McKenzie, James Poterba, Ernesto Schargrodsky, Gustavo Torrens, participants at various seminars, and three anonymous referees. We also thank the valuable collaboration and feedback provided during the early stages of the project by Van Elder Espinal, Mayra Guzman, Carlos Leal and Mario Alberto Sanchez. All errors are our own.

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JEL No. D4,I3,L1

ABSTRACT

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The Causal Effect of Competition on Prices and Quality:

Evidence from a Field Experiment*

By MATIAS BUSSO AND SEBASTIAN GALIANI

This paper provides experimental evidence on the effect of increased competition on prices and quality in the retail sector. We randomized the entry of 61 firms into 72 markets serving the beneficiaries of a conditional cash transfer program in the Dominican Republic. Six months after the intervention entry into the market led to reductions in prices ranging from 2 to 6 percent and to a statistically significant improvement in self-reported service quality. Prices dropped more in areas where the number of entrants was larger. Competition seems to have driven part of the clientele away from incumbent retailers. (JEL I31, I38, D4)

The retail sector accounts for 20 percent of global GDP. In developing countries, the sector is populated mostly by small independent stores and small wholesalers that capture almost 60 percent of the sector's revenue (Bronnenberg and Ellickson 2015). A large body of empirical literature analyzes the effect of competition on prices and quality in the sector. This literature has focused primarily on market responses to the entry of modern, large "big box" retailers (see, for instance, Basker and Noel 2009 and Atkin, Faber, and Gonzalez-Navarro 2018). This paper

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contributes to the existing literature by providing experimental evidence on the effect of competition on prices and quality. We do this by assessing the effect of the entry of small retail shops rather than large retailers.

The experiment was part of an attempt to improve the operation of a conditional cash transfer (CCT) program in the Dominican Republic. This program provides monetary transfers to poor families that can only be used, by means of a debit card, in grocery stores affiliated with the program. Because entry into this market was restricted by program design, these stores could potentially wield market power. Our paper assesses the effect on product prices and quality of a relaxation of those entry restrictions. At the outset of the CCT program, the Dominican Republic's government was grappling with the problem of how to deliver basic consumption goods to the poor sectors of its population. It had potentially three alternative strategies for controlling the prices and quality of those goods: directly selling these goods to the program beneficiaries; outsourcing the provision of such goods to the private sector and regulating their prices; or outsourcing to the private sector and ensuring competition in the market. In the end, the last of these three policy options was chosen by the government. The efficiency considerations associated with these alternative strategies are often assessed on the basis of poorly understood tradeoffs. These tradeoffs hinge on three factors: first, on how effective the government is at providing incentives to public officials relative to providing incentives to private contractors, which, in turn, depends critically on whether quality is fully contractible (Hart, Shleifer, and Vishny 1997); second, on the government's capacity to directly regulate prices and quality (Laffont 2005); and third, on how much competition there is when outsourcing contracts (Banerjee et al. 2018). Our paper contributes to the literature by providing new evidence that governments can contract out public services and achieve lower

prices without sacrificing quality if they can ensure that there is enough competition among potential providers.

We carried out a randomized control trial in 72 districts of the Dominican Republic.¹ Each district is considered to be a market. In these markets, program beneficiaries represent a large share of the customers and sales. The intervention consisted of an exogenous randomized increase in the number of retailers. The actual implementation of the intervention was the responsibility of the CCT executing agency and was based on our guidelines. The design allowed anywhere from zero to three new stores to begin operating in each market. A total of 61 new grocery stores entered these markets, for a 26 percent increase in the number of stores in the treated areas. It is important to note that, in our setting, the stores referred to as “incumbents” are the ones that were already operating in the program network before the intervention took place; market entry is considered to have occurred when a store is approved by the CCT executing agency to sell its products to program beneficiaries. We refer to the retailers who are entering the program network as “entrants”.

We collected data on both retail stores and households (i.e. consumers) before and six months after the intervention took place. Our data includes information on product attributes (units, price, variety, and brand) that allows us to assess changes in the characteristics of the goods sold by retailers. As is also true of other papers that have contributed to the literature on this subject (Basker, 2005a; Hausman and Leibtag 2007; Basker and Noel 2009), we focus on the effect of competition on a relatively small set of goods. For each of those goods, we used the reported information on product, brand, variety, and measurement unit to

¹ The National Statistics Office divides the country into provinces, municipalities, sections, and neighborhoods. We use clusters of one or two adjacent neighborhoods, which we refer to as a district, as our unit of analysis.

build barcode-equivalent observations. Based on the retailer and household data, we built two independent measures of prices that proved to be very closely correlated. In addition, the surveys included measures of consumers' self-reported shopping experiences, which we use to measure quality.

We estimate average treatment effects using the randomization assignment as an instrument for the potentially endogenous entry of new stores induced by non-compliance with randomization.

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). By contrast, the effect of competition on quality has been less clear-cut across the various models. Ultimately, it appears that the effect that competition has on quality depends on the extent to which consumers perceive and value quality.

We find that entry into the market led to reductions in prices ranging from 2 to 6 percent and to a statistically significant improvement in self-reported service quality. Since, owing to the increased competition that they faced, stores did not change the varieties or brands of the goods that they sold, our point estimates do not change significantly when we restrict the estimation sample to the set of stores and goods that sold products with the same barcodes before and after treatment. We also find that prices dropped more in areas where the number of entrants was larger. Competition seems to have driven part of the clientele away from

incumbent retailers, since the entry of new stores increased the probability that consumers would switch to an entrant retailer.

This paper is closely related to a well-established literature on industrial organization, marketing, trade, and development that analyzes the effect of increased competition in the retail sector on prices and quality. Several studies have looked at the effect of the entry of Wal-Mart into local markets in the United States on a variety of outcomes, including prices (Basker 2005a; Basker 2007; Hausman and Leibtag 2007; Basker and Noel 2009), quality (Matsa 2011), sales (Stone 1995; Singh, Hansen, and Blattberg 2006; Ailawadi et al. 2010), employment (Basker 2005b; Neumark, Zhang, and Ciccarella 2008; Ellickson and Grieco 2013), and the entry and exit decisions of competitors (Jia 2008). In the case of developing countries, there is a smaller body of work on the effect that the entry of modern retail chains has on prices, quality, productivity, employment, and welfare in Chile (Lira, Rivero, and Vergara 2007), India (Minten, Reardon, and Sutradhar 2010; Bennett and Yin 2014), Madagascar (Minten, Randrianarison, and Swinnen 2009), Mexico (Iacovone et al. 2015; Atkin, Faber, and Gonzalez-Navarro 2018), Romania (Javorcik and Li 2013), and Uruguay (Borraz et al. 2014), among others.² The paper that comes the closest to our research is the study by Atkin, Faber, and Gonzalez-Navarro (2018), which provides a comprehensive analysis of the economic impact of the entry of foreign chain stores in Mexico. They find that prices charged by incumbent domestic retailers decreased by approximately 4 percentage points after the entry and that the foreign stores seem to offer a product mix of higher quality.

² The literature also includes studies on the effect of the entry of other hypermarkets and supermarkets (Courtemanche and Carden 2014; Zimmerman 2012) as well as other developed countries (Zhu, Singh, and Dukes 2011; Abe and Kawaguchi 2010; Holmes 2011; Gómez-Lobo, Jiménez, and Perdiguero 2015; Hosken, Olson, and Smith 2017). Other work related to the economics effects of entry into non-retail markets include Trapani and Oslon (1982), Carlton (1983), Bresnahan and Reiss (1987 and 1991), Reiss and Spiller (1989), Berry (1992), Goolsbee and Syverson (2008), and Gaynor and Town (2011).

Relative to the existing literature we show that entry of small retailers causes price effects of similar magnitudes as those observed when entrants are large retail chains. The existing literature found that objective measures of quality (e.g., reduced product shortfalls) improved after entry. Our paper shows that consumers perceive that quality improves. As in Bresnahan and Reiss (1991) we show that competition is effective even when there are as many as four to five incumbent retailers already operating in the market. Methodologically, our paper is, to the extent of our knowledge, the first one to randomize the entry of firms into the market.

This paper also relates to a central issue in public finance: the desirability of contracting public services out to the private sector (Megginson and Netter 2001; Jensen and Stonecash 2004; Blochliger 2008). Two important factors influence the effectiveness of contracting-out. First, the ability of governments to write complete contracts that ensure that providers do not achieve cost reductions by adjusting quality (Hart, Shleifer, and Vishny 1997).³ Second, the degree of competition measured, for instance, by the number of ex-ante bidders (Hong and Shum 2002; Li and Zheng 2009). Governments around the world have outsourced the provision of a wide array of services to the private sector including the operation of prisons in the United States (Hart, Shleifer, and Vishny 1997), the provision of medical care in the United States (Duggan 2004) and in Guatemala (Cristia, Evans, and Kim 2015), the supply of local government services ranging from solid waste collection and vehicle towing and storage to the operation of daycare centers in the United States (Levin and Tadelis 2010), the maintenance of public roads in Denmark (Blom-Hansen 2003) and the delivery of in-kind targeted transfers to the poor in Indonesia (Banerjee et al. 2018). The paper in the

³ Levin and Tadelis (2010) construct a measure of performance contracting difficulty and find that local governments in the United States tend to contract out precisely those services for which it is easier to write and administer performance contracts.

experimental literature on developing countries that comes the closest to our paper is the study by Banerjee et al. (2018) on the impact of outsourcing service delivery to the private sector for Indonesia's largest targeted transfer program. As in our study, they show that prices were lower only where there was more competition; which in their setting was achieved by modifying the bidding rules to encourage more bidders to participate.

Our paper contributes to the public finance literature by presenting experimental evidence suggesting that governments that choose to outsource to the private sector the delivery of services, can use competition to achieve lower prices without sacrificing quality. In our setting competition also matters for incidence. The benefits of social programs that subsidize consumers (via monetary or in-kind transfers) can potentially leak into the profits of the private providers that are serving them if the supply side does not operate in a competitive environment. Our findings indicate that introducing competition, when that is possible, provides an effective means of avoiding rent capture by suppliers.

I. 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 percent 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 card⁶ 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.

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.

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 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

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.

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.

II. 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

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

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 percent 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 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 percent 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*).

–Table 1 here–

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 percent in the treated areas. In 38 districts (53 percent) there was perfect compliance with randomization.⁹ In 28 districts (39 percent) 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 percent), 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.

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.

III. 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 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 percent 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 percent) 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 percent) 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:

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

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 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):

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

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.

IV. 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

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.

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.

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.

The survey of retailers included the majority of incumbent retailers in the targeted neighborhoods (95 percent) and a large share of incumbent retailers in the non-targeted neighborhoods (65 percent). 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. 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

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.

represent 85 percent of all non-perishable food products and 60 percent 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 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

¹⁶ Appendix Table A2 shows the share spent on each type of food. Notice that households spend 40 percent 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.)

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.

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 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 and the correlation between \bar{P}_{js}^R and \bar{P}_{js}^C is 0.96.¹⁹

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 2

¹⁸ The last column of Appendix Table A2 shows the weights.

¹⁹ Appendix Figure 1 plots the density of $\bar{P}_{js}^R/\bar{P}_{js}^C$ and provides some descriptive statistics.

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).²⁰ 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 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.

–Table 2 here–

Quality and clients. We are also interested in the quality of the service provided by the stores.²¹ 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

²⁰ 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.

²¹ 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.

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. We define an indicator variable “switch to an entrant retailer” as the product of two indicator variables. The first is equal to 1 if the client changed retailers between the time we conducted the baseline and endline surveys (and zero otherwise). The second is equal to 1 if the retailer where the client shopped at the time of the endline survey is an entrant retailer (and zero otherwise).

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 district, on the total number of beneficiaries and the number of retailers operating in the CCT network at baseline.²²

V. 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.^{23,24}

In these districts retailers are small, owner-run “mom-and-pop” shops. In incumbent stores, program beneficiaries can use the CCT transfer money to

²² For a full description of all the outcome and control variables, see Appendix Table A3.

²³ In each district, we computed the centroid of all retailers as an approximation to the relevant economic centroid of the district.

²⁴ 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.

purchase only food products (of any brand or variety).²⁵ 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.

–Table 3 here–

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 percent 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 percent of an incumbent's sales could potentially come from program beneficiaries.²⁶ The importance of program beneficiaries for

²⁵ 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.

²⁶ 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, $((36 \times 630)/4.6)/8850=0.56$. On average, these households spend about 62 dollars on perishable foods. Thus, $((62 \times 630)/4.6)/8850=0.96$

these retailers is also confirmed by self-reported measures: 96 percent 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 percent of them own a car or a motorcycle and 95 percent 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 of the different items are not always in plain view. Only about 41 percent 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 percent of retailers stated that they never bargain over prices with their customers.²⁷

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 percent 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

²⁷ Unfortunately, we lack information to empirically test the existence of third-degree price discrimination which would require price data for beneficiaries and non-beneficiaries

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.²⁸

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 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.

–Figure 1 here–

*Effects on product availability.*²⁹ 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.³⁰ Columns 3 and 4 show intention-to-treat estimates in which the main

²⁸ See Appendix Table A4.

²⁹ In Appendix Table A5 we provide means and standard deviations of all outcomes analyzed in this and the next subsections.

³⁰ 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

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.³¹ 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.³²

–Table 4 here–

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 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-

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.

³¹ There is very little missing data, as there is complete information for all variables used in all columns for 97 percent of the sample of retailers.

³² 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 percent (or 1 percent 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.

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).³³ 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 5 here–

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 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.

–Table 6 here–

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

³³ 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 A6

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.³⁴ 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.

–Table 7 here–

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

³⁴ The results are shown in Appendix Table A7.

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. It was not necessary for customers to switch stores in order for prices to drop due to increased competition. In fact, the variable “switch to an entrant retailer” could have been zero for a majority of consumers in treated districts if, after the entry of new retailers, very few consumers changed the store in which they did their shopping or switched to another incumbent store (if one was available). In that situation, prices could still have declined if, for instance, entrant retailers lowered their prices and incumbents copied them. The fact that we find that consumers actually switched to an entrant retailer suggests that part of the price drop was probably a response on the part of incumbent stores to the loss of part of their clientele.

–Table 8 here–

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.

VI. 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 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 found in the literature in other developing countries. For instance, Atkin, Faber, and Gonzalez-Navarro (2018) 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.

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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.)	2	15	15	0.12	0.19	0.20
Bread (unit)	1	28	28	0.29	0.21	0.19

TABLE 3. DESCRIPTIVE STATISTICS

	Mean	Standard Deviation
<i>Panel A: Retailer characteristics</i>		
% 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
<i>Panel B: Consumer characteristics</i>		
Individual income / GDP per capita	0.270	0.145
% of population with primary education or lower	0.619	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
<i>Panel C: Market (district) characteristics</i>		
Number of incumbent retailers per district	4.86	4.80
Number of beneficiaries per district	631	2.47
Distance between pairs of districts (within city, in miles)	2.81	2.11
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	0.90
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		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]
Percent of products that changed barcode	250	72	0.010 [0.025]	0.010 [0.023]	0.022 [0.050]	0.026 [0.051]
Percent of products that changed to a cheaper barcode	250	72	0.006 [0.024]	-0.003 [0.018]	0.014 [0.048]	-0.008 [0.045]
Percent of products that changed brand	250	72	0.011 [0.024]	0.022 [0.026]	0.025 [0.050]	0.055 [0.058]
Percent of products that changed variety	250	72	0.002 [0.007]	-0.000 [0.007]	0.004 [0.016]	-0.000 [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, the baseline number of beneficiaries and neighborhood controls which include: 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.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE 5. IMPACT OF COMPETITION ON PRODUCT PRICES

Dependent variable: Log (average price after treatment) - weighted		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]
<i>Panel A: Retailer measures</i>	<i>All districts</i> 1(Entry>0)	399	72	-0.020*** [0.007]	-0.014* [0.007]	-0.040** [0.018]	-0.027* [0.015]
	<i>Targeted neighborhoods</i> 1(Entry>0)	254	72	-0.026*** [0.009]	-0.020*** [0.007]	-0.056** [0.024]	-0.052** [0.021]
	<i>Incumbent retailers in targeted neigh.</i> 1(Entry>0)	212	70	-0.025*** [0.009]	-0.019** [0.007]	-0.060** [0.028]	-0.052** [0.023]
<i>Panel B: Consumer measures</i>	<i>Targeted neighborhoods</i> 1(Entry>0)	2025	72	-0.024*** [0.008]	-0.015** [0.007]	-0.043** [0.017]	-0.028* [0.016]
	<i>Shop at incumbent retailers</i> 1(Entry>0)	1493	71	-0.030*** [0.010]	-0.020** [0.009]	-0.052** [0.020]	-0.037* [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 corresponding to ITT with no controls (sample sizes of other estimators are approximately the same). 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, the baseline number of beneficiaries and neighborhood controls which include: 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.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE 6: IMPACT OF COMPETITION ON PRODUCT PRICES
Robustness (Instrumental variables estimation)

	Log (average price after treatment) - unweighted	Log individual product prices after treatment (pooled)	Log individual product prices after treatment (pooled)	Log individual product prices after treatment (pooled)	Log (average price after treatment) - weighted	Log individual product prices after treatment (pooled)	Log individual product prices after treatment (pooled)	Log individual product prices after treatment (pooled)
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
<i>All districts</i> 1(Entry>0)	-0.025 [0.017]	-0.026 [0.018]	-0.034** [0.016]	-0.034** [0.016]	-0.028 [0.040]	-0.028 [0.019]	-0.020 [0.019]	-0.020 [0.019]
Observations	379	5368	5368	5368	379	2724	2724	2724
<i>Targeted neighborhoods</i> 1 (Entry>0)	-0.066** [0.031]	-0.055* [0.030]	-0.059** [0.027]	-0.062** [0.028]	-0.056 [0.062]	-0.052* [0.031]	-0.045 [0.030]	-0.047 [0.030]
Observations	250	3529	3529	3529	250	1745	1745	1745
<i>Incumbent retailers in targeted neigh.</i> 1 (Entry>0)	-0.066** [0.033]	-0.054* [0.031]	-0.058* [0.030]	-0.062** [0.031]	-0.056 [0.070]	-0.043 [0.032]	-0.035 [0.030]	-0.038 [0.031]
Observations	209	2944	2944	2944	209	1459	1459	1459
Samples	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
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 [4] report the estimations over the whole sample of products, while columns [5] through [8] 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, the baseline percentile rank of price of the product in control areas and the baseline number of beneficiaries. District controls include: province fixed effects, the average education and income of households in the district and 1 (if neighborhood is urban). Additional fixed effects in columns [3] and [7] include the interaction between product and brand, and columns [4] and [8] control for the triple interaction of product, brand and variety. Standard errors clustered at the district level are reported in brackets.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

ABLE 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		Average treatment effect	
			OLS estimation: 1(Entry=1) = 1(Randomized entry=1) 1(Entry=2,3,4) = 1(Randomized entry=2,3)		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]
<i>All neighborhoods</i>						
1(Randomized entry=1)	399	72	-0.020** [0.009]	0.002 [0.009]	-0.034** [0.016]	0.016 [0.024]
1(Randomized entry=2 or 3)			-0.020** [0.008]	-0.019** [0.008]	-0.047 [0.029]	-0.059* [0.033]
<i>Targeted neighborhoods</i>						
1(Randomized entry=1)	254	72	-0.023** [0.011]	-0.009 [0.010]	-0.034* [0.020]	0.010 [0.033]
1(Randomized entry=2 or 3)			-0.027** [0.010]	-0.026*** [0.008]	-0.071** [0.035]	-0.090* [0.049]
<i>Incumbent retailers in targeted neigh.</i>						
1(Randomized entry=1)	212	70	-0.028*** [0.011]	-0.013 [0.010]	-0.043** [0.021]	-0.006 [0.026]
1(Randomized entry=2 or 3)			-0.023** [0.011]	-0.022*** [0.008]	-0.072* [0.041]	-0.075* [0.040]
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 corresponding to ITT with no controls (sample sizes of other estimators are approximately the same). 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, the baseline number of beneficiaries and neighborhood controls which include: 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.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

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

	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]
Panel A: Service quality						
Store cleanliness	254	72	0.105 [0.290]	0.132 [0.293]	0.228 [0.614]	0.348 [0.726]
Time shopping (minutes)	2117	72	4.691 [4.531]	1.879 [3.983]	8.385 [8.115]	3.625 [7.659]
Delivery	2118	72	0.056 [0.063]	0.040 [0.042]	0.100 [0.111]	0.076 [0.079]
Service-quality rating	2116	72	0.213** [0.090]	0.200*** [0.069]	0.380** [0.192]	0.379** [0.159]
Panel B: Clients						
Number of customers on best day	254	72	-15.665 [37.136]	-15.217 [36.302]	-33.898 [84.195]	-37.589 [89.050]
Share of customers CCT beneficiaries	228	70	-5.244 [3.799]	-4.328 [2.855]	-12.211 [9.907]	-12.215 [10.094]
Switch to entrant retailer = 1(client changed retail store between baseline and endline) x 1(endline store is entrant)	1400	71	0.057*** [0.018]	0.072*** [0.024]	0.099*** [0.035]	0.146*** [0.056]
Panel C: Spillovers						
Log (average price after treatment) -weighted of non-CCT retailers in experimental districts	63	33	-0.003 [0.023]	0.028 [0.022]	-0.006 [0.041]	0.065 [0.059]
Log (average price after treatment) -weighted of CCT retailers in non-experimental districts	136	25	-0.014 [0.013]	0.010 [0.032]	-0.024 [0.024]	0.012 [0.037]
Baseline and District controls				X		X

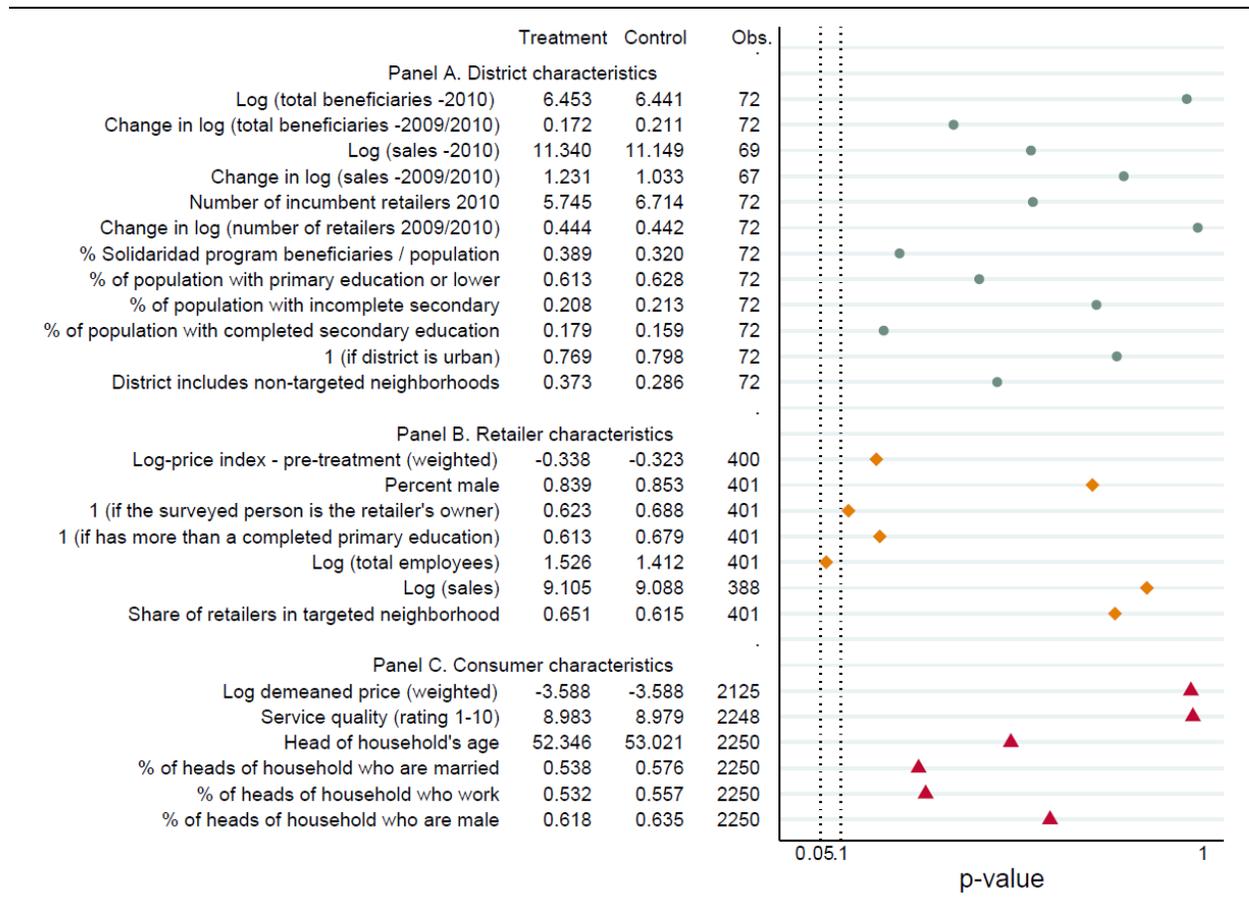
Note: Each entry shows an estimate of the impact of an increase in competition on different outcomes at the retail level. Columns [1] and [2] report sample sizes corresponding to ITT with no controls (sample sizes of other estimators are approximately the same). Columns [3] and [5] report the estimation 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, the baseline number of beneficiaries and neighborhood controls which include: 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.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

FIGURE 1. DIFFERENCES IN PRE-TREATMENT SAMPLE MEANS



Note: Panels A, B and C feature pre-treatment sample means for district, retailer and consumer characteristics, respectively. Each marker (circle, diamond or triangle) shows the p-value of the t-test of difference in means between the treatment and control groups.

**The Causal Effect of Competition on Prices and Quality:
Evidence from a Field Experiment***

By MATIAS BUSO AND SEBASTIAN GALIANI

Appendix I: Price Variable Construction

A. 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.

B. 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

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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.

C. 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):

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

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.

D. 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

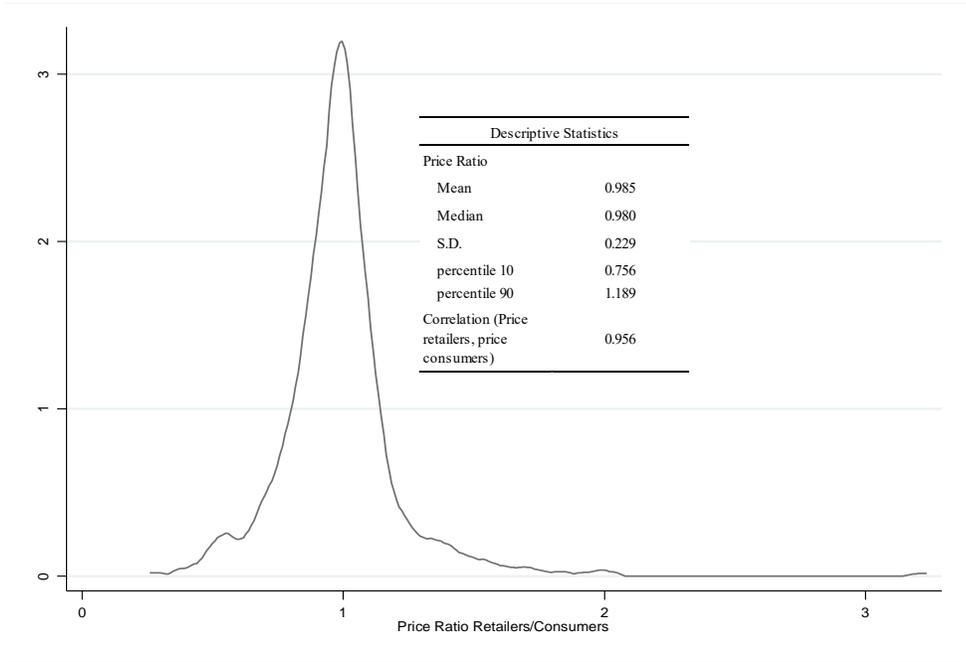
be sold by small-scale retailers. Panel C shows the measures for other fresh or perishable products typically not sold by the retailers in question.

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.

E. 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.

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



Note: The figure plots the ratio between the (simple) average price of product j in district s as reported by retailers and the (simple) average price of product j in district s as reported by consumers. The table reports statistics that describe the distribution of that ratio and, in the last row, the correlation between those two prices.

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}_{ks}^C 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 ($\bar{P}_{ks}^R/\bar{P}_{ks}^C$) 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. 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 II: 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
<i>By type</i>		
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
<i>By type</i>		
Shop in incumbent retailers	1620	1563
Located in targeted neighborhood	2250	2118
Number of districts	72	72

APPENDIX TABLE A2. EEPS 2010 - SHARE OF EXPENDITURE ON ALL PRODUCTS

Product	EEPS 2010				Survey weightings
	N	Percentage consumption	Share of expenditure	Share of expenditure in price index	
<i>Fifteen survey products</i>			<i>0.601</i>	<i>1.000</i>	<i>1.000</i>
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
<i>Other non-perishable products</i>			<i>0.121</i>		
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	-	-
<i>Fresh or perishable products</i>			<i>0.264</i>		
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 district	Retailer survey
Change in log (number of retailers 2009/2010)	$Brands_{s,t} = \sum_{k=1}^K N_{k,s,t}/15$ $\frac{\# \text{retailers 2010} - \# \text{retailers 2009}}{0.5 * (\# \text{retailers 2010} + \# \text{retailers 2009})}$	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 beneficiaries 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)	$\text{Log}(P_{it0})$, where: $P_{it} = \sum_{k=1}^K W_k * p_{itk}$ <p>p_{itk} Price of product k in retailer i W_k Weight computed from the household survey</p> $W_k = \frac{w_k}{\sum_{k=1}^K w_k}$ <p>K is the number of products available at the store</p>	Household and retailer surveys
Average Price (unweighted)	$P_{it} = \sum_{k=1}^K W_k * p_{itk}$ $W_k = \frac{1}{K}$ <p>K is the number of products available at the store</p>	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	$\text{Log}(P_{it0})$ $P_{it} = \sum_{k=1}^K W_k * \left[\frac{p_{itk}}{\bar{p}_{kt}} \right]$ <p> p_{itk} 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. Household survey \bar{p}_{kt} is the average price of product k at time t. W_k Weight computed from the household survey $W_k = \frac{w_k}{\sum_{k=1}^K w_k}$ </p>	
Unweighted demeaned price	$P_{it} = \sum_{k=1}^K W_k * \left[\frac{p_{itk}}{\bar{p}_{kt}} \right]$ <p> p_{itk} 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. Household survey \bar{p}_{kt} is the average price of product k at time t. W_k Weight computed from the household survey $W_k = \frac{1}{K}$ </p>	
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 [0.094]	-0.332 [0.080]	0.443	400
Log-price index - pre-treatment (unweighted)	-0.258 [0.077]	-0.248 [0.082]	0.388	400
1 (retailer does special sales/promotions)	0.431 [0.500]	0.386 [0.487]	0.527	401
Log (sales)	8.989 [0.904]	9.117 [0.821]	0.371	388
Log (total employees)	1.399 [0.378]	1.509 [0.484]	0.028	401
Percent male	0.804 [0.401]	0.849 [0.359]	0.494	401
1 (if the surveyed person is the retailer's owner)	0.627 [0.488]	0.643 [0.480]	0.822	401
1 (if has more than complete primary education)	0.686 [0.469]	0.623 [0.485]	0.318	401
% Solidaridad Clients	49.25 [26.570]	48.037 [23.842]	0.8424	347

Note: Columns [1] and [2] report the mean and standard deviation (in square brackets) of each variable for the entrant retailers and incumbent retailers at baseline. 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.

APPENDIX TABLE A5. SUMMARY STATISTICS OF OUTCOME MEASURES

Outcome	All districts		Targeted neighborhoods		Incumbent retailers in targeted neighborhoods	
	Mean	Standard deviation	Mean	Standard deviation	Mean	Standard deviation
<i>Prices</i>						
Price index (weighted)	0.719	0.058	0.719	0.059	0.720	0.058
Price index (weighted) - Non-barcode change	0.603	0.224	0.593	0.234	0.597	0.235
Price index (unweighted)	0.782	0.063	0.783	0.062	0.784	0.062
Price index (weighted) - Random entry=1	0.719	0.058	0.719	0.058	0.719	0.058
Price index (weighted) - Random entry=2 or 3	0.719	0.058	0.719	0.058	0.719	0.058
Price index - weighted (Consumers)	-	-	0.028	0.002	0.028	0.003
Price index (weighted) of non-CCT retailers in experimental districts	-	-	0.727	0.060	-	-
Price index (weighted) of CCT retailers in non-experimental districts	-	-	0.719	0.056	-	-
<i>Product Availability*</i>						
Percent of products that changed barcode	0.481	0.124	0.484	0.126	0.484	0.126
Percent of products that changed to a cheaper barcode	0.228	0.100	0.228	0.102	0.228	0.102
Percent of products that changed brand	0.446	0.156	0.452	0.169	0.452	0.169
Percent of products that changed variety	0.018	0.064	0.012	0.050	0.012	0.050
<i>Service quality, Clients and Spillovers</i>						
Store cleanliness	7.474	2.023	7.417	1.992	7.362	2.011
Time shopping (minutes)	-	-	61.0	47.6	62.7	48.9
Delivery	-	-	0.454	0.498	0.464	0.499
Service-quality rating	-	-	8.982	1.467	8.987	1.449
Number of customers on best day	106.0	117.9	113.2	124.9	113.8	123.8
Share of customers CCT beneficiaries	48.1	24.0	47.9	24.2	47.6	24.2
Switch to entrant retailer*	-	-	0.041	0.199	-	-

Note: For most variables summary statistics are measured at baseline and correspond to the samples located in treated and control districts. In the case of variables related to product availability (marked with *), which can only be defined at endline, we report the summary statistics for the control group.

APPENDIX TABLE A6. DIFFERENCES IN NON-COMPLIERS AND COMPLIERS

	Compliers	Non-compliers	p-value of difference	Number of obs.
	[1]	[2]	[3]	[4]
<i>A. District characteristics</i>				
Log (total beneficiaries - 2010)	6.417 [0.977]	6.556 [0.626]	0.584	72
Change in log (total beneficiaries - 2009/2010)	0.168 [0.146]	0.232 [0.236]	0.184	72
Log (sales - 2010)	11.285 [1.229]	11.295 [1.337]	0.978	69
Change in log (sales -2009/2010)	0.971 [2.577]	1.774 [3.703]	0.327	67
Number of incumbent retailers - 2010	5.945 [6.753]	6.294 [6.469]	0.852	72
Change in log (number of retailers - 2009/2010)	0.402 [0.596]	0.579 [0.744]	0.316	72
% Solidaridad program beneficiaries / population	0.393 [0.234]	0.290 [0.183]	0.099	72
Average monthly household income (US\$)	491.088 [86.411]	495.965 [93.582]	0.842	72
% of population with completed primary education or lo	0.618 [0.080]	0.615 [0.067]	0.887	72
% of population with incomplete secondary education	0.208 [0.052]	0.215 [0.048]	0.615	72
% of population with completed secondary education or	0.174 [0.065]	0.170 [0.045]	0.809	72
1 (if district is urban)	0.745 [0.413]	0.882 [0.332]	0.216	72
District includes non-targeted neighborhoods	0.400 [0.494]	0.176 [0.393]	0.093	72
<i>B. Retailer characteristics</i>				
Log-price index - pre-treatment (weighted)	-0.336 [0.084]	-0.324 [0.074]	0.235	400
Percentage male	0.837 [0.370]	0.864 [0.345]	0.543	401
1 (if the surveyed person is the retailer's owner)	0.642 [0.480]	0.636 [0.484]	0.897	401
1 (if has more than a completed primary education)	0.623 [0.485]	0.659 [0.477]	0.496	401
Log (total employees)	1.486 [0.461]	1.529 [0.513]	0.397	401
Log (sales)	9.083 [0.822]	9.164 [0.865]	0.429	388
Share of retailers in targeted neighborhood	0.601 [0.491]	0.784 [0.414]	0.117	401

Note: Columns [1] and [2] report the mean and standard deviation (in square brackets) of each variable at both the district and retailer level for compliers and non-compliers. 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.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Source: Author's calculations

APPENDIX TABLE A7. 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 [0.013]	-0.015 [0.023]	-0.010 [0.017]	-0.022 [0.033]	-0.009 [0.018]	-0.022 [0.037]
Cooking oil (lb.)	0.094	-0.030** [0.015]	-0.057 [0.038]	-0.050*** [0.015]	-0.110** [0.046]	-0.052*** [0.016]	-0.129** [0.059]
Sugar (lb.)	0.052	-0.001 [0.011]	-0.002 [0.020]	-0.003 [0.009]	-0.007 [0.020]	-0.008 [0.010]	-0.019 [0.023]
Pasta (lb.)	0.017	-0.027** [0.013]	-0.051** [0.024]	-0.048*** [0.015]	-0.102** [0.047]	-0.048** [0.016]	-0.113* [0.058]
Eggs (unit)	0.050	-0.022 [0.026]	-0.042 [0.044]	-0.025 [0.023]	-0.055 [0.046]	-0.025 [0.022]	-0.059 [0.052]
Powdered milk (125 gr.)	0.062	0.032 [0.023]	0.060 [0.042]	0.019 [0.025]	0.040 [0.054]	0.006 [0.022]	0.015 [0.053]
Chocolate (unit)	0.015	0.002 [0.011]	0.004 [0.021]	-0.008 [0.014]	-0.017 [0.028]	-0.009 [0.014]	-0.022 [0.030]
Sardines (unit)	0.014	0.028 [0.032]	0.053 [0.062]	0.015 [0.044]	0.032 [0.097]	0.017 [0.042]	0.040 [0.100]
Green beans (lb.)	0.063	-0.005 [0.006]	-0.009 [0.011]	-0.005 [0.008]	-0.011 [0.017]	-0.003 [0.008]	-0.007 [0.020]
Onions (lb.)	0.020	-0.013 [0.022]	-0.024 [0.044]	-0.047** [0.022]	-0.104* [0.062]	-0.038* [0.022]	-0.092 [0.066]
Salami (lb.)	0.048	-0.051* [0.028]	-0.096* [0.054]	-0.060 [0.039]	-0.132 [0.091]	-0.046 [0.040]	-0.111 [0.099]
Chicken (lb.)	0.170	-0.014 [0.009]	-0.023 [0.017]	-0.008 [0.014]	-0.016 [0.025]	-0.008 [0.014]	-0.016 [0.028]
Cod (lb.)	0.018	-0.010 [0.009]	-0.019 [0.016]	-0.020** [0.010]	-0.045 [0.031]	-0.023** [0.010]	-0.057 [0.039]
Flour (lb.)	0.010	-0.038** [0.015]	-0.066** [0.031]	-0.042** [0.020]	-0.086* [0.051]	-0.040* [0.021]	-0.092 [0.060]
Bread (unit)	0.074	0.088* [0.052]	0.151 [0.107]	0.021 [0.057]	0.039 [0.107]	0.035 [0.057]	0.073 [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 neighborhoods. All columns report the estimations while controlling for the baseline log(price).

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.