

TRADE CREDIT AND ORGANIZATIONAL FORM: EVIDENCE FROM THE REGULATION OF BUYER-SUPPLIER CONTRACTS^{*}

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ABSTRACT. We study how restrictions to the set of feasible contracts affect buyer-supplier relationships and the organizational form of the firm. We exploit a regulation change that forced a large Chilean retailer to reduce the time it took to pay some of its small suppliers, defined by an arbitrary sales cutoff, from approximately 90 to no more than 30 days. Using a within-product differences-in-differences identification strategy, we find that firms restricted in their ability to extend trade credit are 9% less likely to make a sale to the retailer and receive a 4.6% price reduction when they do make a sale. The large retailer responds by internalizing the procurement of some products previously sold by affected firms to its own subsidiaries. We interpret this as evidence that trade credit allows small firms to post a bond to guarantee product quality. When this guarantee is no longer possible, the retailer has an incentive to vertically integrate. To support this mechanism, we find evidence that the negative impacts of the regulation are mitigated for older firms, which have been able to establish a reputation, and for products with easily observable quality.

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I. Introduction

Contracting problems between suppliers and their buyers motivate a vast literature on the boundaries of the firm (e.g. Coase (1937), Williamson (1975), Grossman and Hart (1986)).¹ Firms choose to enter into procurement contracts with suppliers or source goods internally depending on the degree of contractual incompleteness between the two parties. We investigate the role trade credit plays as a contracting lever in allowing supplier firms to signal quality or guarantee their goods. Namely, we ask whether changes to the feasible set of trade credit terms affects the organizational structure of the buyer.

Trade credit is one of the most prevalent contracting features in procurement relationships (Petersen and Rajan (1997); Cuñat and Garcia-Appendini (2011)). Even large corporations, which have a relative financing advantage over their suppliers, rely heavily on trade credit (Wilson and Summers (2003)). For example, Walmart uses four times more supplier financing than short term external financing.² Wilson and Summers (2003) argue that this type of relationship—small suppliers financing large clients—is very prevalent. These observations suggest that trade credit has intrinsic value to corporations beyond traditional access-to-finance-based explanations. Delayed payment for goods may provide incentives to the suppliers and may permit more trade between the buyer and outside firms.

We exploit a natural experiment that restricted the terms of trade credit that suppliers could offer to study their common client’s decision to integrate or procure from a third party. To our knowledge, this is the first study to provide causal estimates of the effect of restrictions to the set of feasible contracts on the organizational form of a firm and its supply chain. The experiment occurred as follows. In January 2007 one of the two large supermarket chains that operate in Chile (the “Supermarket”) agreed with the Chilean government to reduce the number of days in which it paid its small suppliers from approximately 90 to no more than 30 days (the “Agreement”). The

¹See also Alchian and Demsetz (1972); Hart and Moore (1990); Holmstrom and Tirole (1991); Holmstrom (1999), among others.

²Based on internal calculations from publicly available information as of January 2013.

Agreement was put in place because of the government’s concern that the Supermarket exerted monopsonistic power over its small suppliers. As per the Agreement, suppliers were categorized as small by an arbitrary yearly revenues cutoff (roughly US\$4.0 million in Chilean pesos). We study the effect of this policy change on the contractual relationship between the Supermarket and its suppliers. We identify the causal effect that the restriction on the ability to extend trade credit had on these relationships by comparing the same product sold by two very similar firms, one exposed to the lower payment days and one not, before and after the Agreement was put in place.

We obtain from the Supermarket a proprietary dataset that details purchases made between 2006 and 2011 at the month-product-supplier level (e.g., soccer balls sold by firm A on July 2007). We use a within-product differences-in-differences empirical strategy. In particular, we denote suppliers that sold less than the cutoff during 2006 as the Treated group and suppliers that sold more than the cutoff as the Control group. We control non-parametrically for the effect of firm size on contractual outcomes by focusing our analysis on firms whose 2006 yearly revenues were within a relatively tight range of yearly sales above and below the cutoff.³ We compare changes in the price and quantities procured for each product sold by Treated firms before and after the Agreement relative to the *same* product category—as defined by the Supermarket—but sold by Control firms. Identification relies on the assumption that any secular trends or shocks (after controlling for product-level time trends) not related to the Agreement would affect the price and quantity of the product sold by Treated and Control firms in the same manner.

The intuition of this identification strategy is better grasped through an example. Suppose firms A and B both sold soccer balls (one of 1,871 product categories) to the Supermarket during 2006. Both firms were paid 90 days after delivery as, according to management, standard trade credit terms (i.e., net 90 terms) were in place for all clients except the very largest ones. Suppose firm A’s 2006 revenues were slightly lower than the cutoff and firm B’s 2006 revenues were slightly higher than the cutoff. Starting

³We do not observe total revenues to all clients, which makes it impossible to implement a regression discontinuity design.

in 2007 and as long as its yearly revenues were below the cutoff, firm A could have sold its soccer balls to the Supermarket and receive payment in 30 days. On the other hand, firm B would have still received payment after 90 days. Changes in the demand for soccer balls from final consumers or to the production of soccer balls after 2007 should affect both firms in a similar way. Thus, any difference in the Supermarket's decision to procure soccer balls from firms A or B after the Agreement, including the price and quantity, can be causally linked to the reduction in days payable.

Our analysis suggests that the restriction on the set of feasible contracts had important effects on the organizational form of the Supermarket and its supply chain. First, we show that after the Agreement, the probability that an affected supplier sells the same product to the Supermarket falls by 9% relative to an unaffected supplier. Further, for those products that are still sold after the Agreement, the reduction in days payables results in a 4.6% average reduction in prices. The reduction in prices implies a yearly interest rate of 32% given the 60 day change in days payable. Notably, this rate is significantly higher than the contemporary bank rates of 7%-11% that the Supermarket could obtain in formal credit markets. A placebo test on firms unaffected by the Agreement suggests the effects are not driven by the relative difference in size between Treated and Control firms. These results suggest a very high value of trade credit for a large, less financially constrained client that is implicit in unobservable variation in contract terms.

Second, we document how the Supermarket responds to the restriction in the ability of its suppliers to extend trade credit. For products that were mostly procured by affected firms, the Supermarket shifts purchases both to unaffected suppliers and to fully owned subsidiaries. Thus, for some products, the Supermarket chooses to modify its boundaries and vertically integrate to supply its inputs. The ability of suppliers to extend trade credit makes it more likely that the Supermarket procures externally. We interpret the 32% implied yearly discount as a lower bound on the price elasticity of vertical integration due to the lack of trade credit. Interestingly, we find that the procurement of products that were mostly bought from affected firms is reduced

after the Agreement. We interpret this as evidence that by shifting procurement to its subsidiary, the Supermarket is not fully able to replicate the preperiod market equilibrium (e.g., Baker, Gibbons, and Murphy (2001))

There are many reasons why firms might demand credit from their suppliers. One commonly argued reason is that the supplier may face a lower cost of capital than the buyer or may have an information advantage over the banking sector.⁴ In our setting, the buyer is orders of magnitude larger than the privately held supplier firms in our sample and has the ability to raise capital in the public market.⁵ Therefore, any rationale for supplying credit (at any price) must come from mechanisms outside of the relative financing advantage. Similarly, suppliers may have a stronger enforcement technology than banks or debt markets. This would suggest that buyers that rely heavily on suppliers in the product market (i.e. monopolist suppliers) would have strong incentives to not default on trade credit contracts (see Petersen and Rajan (1997)). Fisman and Raturi (2004) suggest the opposite mechanism, whereby competitive suppliers may engage in trade credit relationships to both learn about the creditworthiness of their buyers and to lock them in. Empirically, Fabbri and Klapper (2008) also find that in China, suppliers with lower market power do in fact extend more trade credit. In our setting, the Supermarket is one of the largest retailers in Chile, thus any information problems relating to payment by the Supermarket are likely to be quite small.

We estimate that the ability for suppliers to extend trade credit is valuable, but recognize that the standard trade credit paradigm does not fit our setting. We find that the effects of the Agreement are significantly reduced for older firms which have been able to establish a reputation, and, following Nelson (1970) and Milgrom and Roberts (1986), for products whose quality can be easily ascertained (“experience”

⁴Smith (1987), Petersen and Rajan (1997), and Fisman and Love (2003) provide evidence for the financing advantage explanation of trade credit. Klapper, Laeven, and Rajan (2011) and Murfin and Njoroge (2012) also study supplier-buyer relationships where the suppliers tend to be substantially smaller than the buyers. They rule out financing constraints as the main driver of the provision of trade credit, as well.

⁵The suppliers in our sample are generally privately held firms with annual sales between \$1 million and \$24 million, and most likely face higher borrowing costs than the Supermarket.

goods). We interpret these results as evidence that trade credit allows suppliers to post a bond to its client in the presence of information asymmetries about the quality of the good or of the firm.⁶ If the Supermarket cannot distinguish a product's quality, high quality suppliers can distinguish themselves by providing longer trade credit terms.⁷ If trade credit in fact helps to mitigate information asymmetries, then once the contract space is restricted, some suppliers may no longer be able to supply their goods to retailers at feasible prices. This is exactly what we see. In this sense, our paper is complementary to Murfin and Njoroge (2012), who investigates effects to firms' capital structure and investment policy from changes in the payments policies of their buyers and interprets the prevalence of small-financing-big relationships to uncertainty about product quality.

Our paper is closely related to the literature on the theory of the firm and its boundaries and the decision to vertically integrate.⁸ In our setting, the Agreement removed contracting levers previously used by suppliers to overcome an information problem. Once those contractual levers are removed, we observe how firms endogenously manage to find other means to overcome the contractual incompleteness. We add to this literature by providing causal estimates of how firms endogenously manage and integrate their supply chain when the set of feasible contracts is constrained. Our findings are also consistent with evidence provided by Barrot (2013) that trade credit terms can act as a barrier to entry for financially constrained suppliers.

Our study is also related to the relational contracting literature and the analysis of Baker, Gibbons, and Murphy (2002). The authors lay out a dynamic model where the value of the future supplier-client relationship must be large enough for both parties not to renege, consistent with our interpretation that older firms may have been able to acquire a reputation as good quality providers which mitigates the need for trade credit. Finally, our paper is related to a vast empirical literature that tests competing

⁶See Smith (1987).

⁷We present in the Appendix a simplified model of trade credit in the presence of information asymmetries about product- or supplier-quality that gives rise to a value of trade credit.

⁸See Williamson (1979), Grossman and Hart (1986), and Hart and Moore (2007) among many others.

theories of trade credit in the cross section (see Ng, Smith, and Smith (2002), Giannetti, Burkart, and Ellingsen (2011), Klapper, Laeven, and Rajan (2011), among others).

We begin with a description of the data and the empirical setting in Section II. We present our identification strategy in Section III. Section IV presents our results. Finally, Section V concludes.

II. Empirical Setting

A. The supermarket industry in Chile and the Agreement

The Chilean supermarket industry is composed of two large firms (one of which corresponds to the Supermarket) and a host of smaller, geographically concentrated firms. The two large Chilean supermarket chains have some characteristics in common with US supermarkets and discount retailers, including store format (large superstores), means of payments (own credit cards), and relationship with suppliers. Indeed, one of these two large Chilean supermarket chains was recently acquired by Walmart. Through a series of aggressive acquisitions and organic growth, these two large chains accounted for 63% of total supermarket revenues during 2006.⁹ As a comparison, the third largest firm accounted for less than 3% of total sales. In contrast, the US supermarket sector exhibits less concentration, with the 4 (8) largest firms representing 32% (46%) of total industry revenues.¹⁰

In August 2006 the Chilean government's pro-competition agency issued a report that forced the two large supermarket chains to modify the terms of their relationships with small suppliers. In its report, the agency expressed its concern over the industry's concentration as a source of monopsonistic power.¹¹ The agency's strategy consisted on denying both supermarkets regulatory approval for their new acquisitions until the modifications were put in place. This prompted both supermarkets to agree to

⁹Information taken from Chilean pro-competition agency website, www.fne.cl.

¹⁰Figure taken from 2007 Economic Census, NAICS code 44511 in factfinder2.census.gov

¹¹See "Requerimiento contra Cencosud y D&S", www.fne.cl.

modifications, the Supermarket in December 2006 and its large competitor in July 2008.

Before the Agreement, the Supermarket frequently used terms of 90 days payable, especially with its smaller suppliers. However, larger suppliers were typically able to negotiate shorter days payable. The Agreement was mainly motivated by the Chilean government's concern over the large differences in size between the Supermarket and its smaller suppliers. Indeed, there is no evidence that the Agreement might have been motivated by secular trends in the retail sector that would have differentially affected small and large firms. In particular, the Agreement's precise timing and chosen cutoff would have been nearly impossible to anticipate for firms.

The Agreement established new terms of payments from the Supermarket to its small suppliers. For the purpose of the Agreement, small suppliers were defined by the following criteria:

- (1) Suppliers who in the last 12 months had sold no more than 60,000 UF to the Supermarket,
- (2) Suppliers who in the last 12 months had sold no more than 100,000 UF to all their clients, as accredited by their sales tax information.
- (3) For new suppliers, maximum monthly revenues no higher than 5,000 UF.

Firms that met the three selection criteria were eligible to be paid by the Supermarket in 30 days. In practice, we find that all but one existing supplier are categorized as small or not small based on the second criteria only. That is, the first restriction is not active except for one supplier. Prior to the Agreement, the Supermarket had a flat days payable policy of 90 days with all its suppliers (except for a group of very large suppliers). Hence, the Agreement reduced the account payable days for eligible firms by 60 days.

The Chilean government has actively monitored the Agreement’s implementation since it was put in place. Publicly available reports suggest the Supermarket has indeed complied with the shorter payment period for small firms.¹²

III. Empirical Strategy

A. Baseline model: Product by firm by time variation

We obtain from the Supermarket a proprietary dataset that contains all the monthly transactions with its suppliers between January 2006 and August 2011, and contains observations at the supplier-product-month level. We estimate the effect of changes in trade credit on product-level contractual outcomes (i.e., apples sold by supplier XYZ) . The identification concern is that trade credit is an equilibrium outcome that depends, in part, on all other contractual outcomes including price and quantity, an importantly the fact that a procurement relationship exists in the first place. Formally, denote $TC_{i,j,t}$ as the equilibrium trade credit offered by firm i on product j and period t . We assume that $TC_{i,j,t}$ represents Supermarket days payable and may take on one of two values, $TC_{i,j,t} \in \{30, 90\}$. Let $y_{i,j,t}$ denote the outcome of interest for product j sold by firm i on time t . We consider a range of outcomes, $y_{i,j,t}$ including the incidence of a sale, the log of the procurement price, and the total revenue received by the supplier. We are interested in the coefficient β of the regression,

$$(1) \quad y_{i,j,t} = \alpha + \beta TC_{i,j,t} + \epsilon_{i,j,t}.$$

Estimating 1 in the cross-section will lead to a biased estimate as long as strict exogeneity doesn’t hold. This is likely to be the case, if, as is expected, terms of trade credit are a function of prices.

We address this problem in the following way. First, we use the Agreement as a source of exogenous variation in days payable in the time series. In particular, let

¹²See, for example, www.fne.gob.cl/wp-content/uploads/2012/03/arch_054_2009.pdf and www.fne.gob.cl/wp-content/uploads/2011/11/arch_022_2011.pdf

$t = pre$ represent 2006 and $t = post$ represent all years after 2006. Further, we assume that each supplier negotiates trade credit terms for all its products sold, that is, $TC_{i,j,t} = TC_{i,t}$. Then, $TC_{i,pre} = 90$ for all firms i , while $TC_{i,post} = 30$ for firms i that are affected by the Agreement (Treated firms) and $TC_{i,post} = 90$ for firms that are unaffected by the Agreement (Control firms). Because contracts may be negotiated at lower frequencies, we define the post period as three years after the Agreement is in place (i.e., 2007, 2008, and 2009; results are robust to dropping 2009, and to adding 2010 and 2011). We estimate a reduced form regression by replacing $TC_{i,t}$ with a dummy that equals 1 in the post period interacted with a dummy that equals one for Treated firms, $1(t = post) \times 1(i \in Treated)$. Thus, our estimates should be interpreted as Intent-to-Treat effects.

A second concern with the identification is that Treated and Control firms may sell different products. This would confound the effect of the Agreement if these products had differential trends in our relevant outcomes irrespective of the terms of trade credit. There are two levels at which we address this concern. First, since treatment is defined by yearly sales, we might worry that the prices of products sold by large suppliers would have evolved in a manner different to the products of small suppliers, irrespective of the change in trade credit terms, precisely because of the size differential. We control non-parametrically for this possibility by restricting our sample to firms with total yearly revenues, as defined by the IRS categorization, sufficiently close to the 100,000 UF cutoff. In particular, we define our “main sample” as firms with 2006 revenues between 25,000 UF and 600,000 UF (roughly US\$1.0 million to US\$24 million). Treated firms are firms with total 2006 revenues between 25,001 UF and 100,000 UF (US\$1.0 million to US\$4.0 million), and Control firms as those with total 2006 revenues between 100,001 UF and 600,000 UF (US\$4.0 million and US\$24 million).

Table 1 shows descriptive statistics at the firm level for our main sample of suppliers during 2006, before the Agreement. The sample includes 734 firms, 342 Treated and 392 Control. Panel A shows yearly statistics at the firm level. On average, firms

in our sample had yearly revenues to the Supermarket of approximately US\$752,830. The median firm in the sample has 1 department, 1 line, 2 sections, and 3 products, and the average firm sold 6.5 products. The table also shows the same statistics for Treated and Control firms, and confirms that by construction Control firms are larger than Treated firms. Interestingly, the median Control firm sold the same number of products as the median Treated firm during 2006 (three).

Table 1 (Panel B) provides sample statistics at the firm-product level during 2006 for our sample of firms. There are 877 firm-product pairs, of which 548 correspond to Treated firms and 329 to Control firms. The table shows that Treated firms sell less (in \$ and units) of each of their products than Control firms. Interestingly, the average price of products sold by both groups is similar (and, based on a simple hypothesis test, not statistically different). Finally, Control firms exhibit a higher probability of having a sale on any given month during the year.

Table 1 (Panel C) reports the concentration of product-firm transactions each month (by number) at the department level, the broadest level used by the Supermarket, for all months in the sample. This evidence suggests that Treated and Control firms may sell different products. Thus, we use the Supermarket’s own categorization system to differentiate products in our regressions. This categorization distinguishes the 16 departments shown in Table 1, each with its own lines, sections, and products. We operationalize this by including a product-time fixed effect, $\alpha_{j,t}$ that controls for differential trends in each product. This also allows to interpret the coefficient β in regression 1 as the average effect of trade credit terms on outcomes of the *same product*. We also include firm-product fixed effects, $\omega_{i,j}$. These account for firm and firm-product specific factors that may influence the estimated coefficient, including, for example, size, relative importance of the product on the firm’s portfolio, relative differences in firm focus accross products, and access to formal credit markets, among others. Finally, we include overall time trend dummies, δ_t .

In sum, we use the following model as the baseline for our estimation:

$$(2) \quad y_{i,j,t} = \beta 1(t = post) \times 1(i \in Treated) + \alpha_{j,t} + \omega_{i,j} + \delta_t + \epsilon_{i,j,t}.$$

The identification assumption for regression 2 is that any other factor affecting the difference in the outcomes of the same product sold by Treated and Control firms, $\epsilon_{i,j,t}$, is not correlated with the terms of trade credit. This would be violated if, for example, the incidence that a product is sold by Treated firms would have evolved in a systematically different manner than that of a product sold by Control firms within the *same* product category as defined by the Supermarket, irrespective of the Agreement.

We provide evidence that supports our identification assumption in Figure 1. The figure is based on the regression of $\log(price)$ and a dummy that equals one if a supplier records a sale to the Supermarket in Panels A and B, respectively, on the interaction term $1(i \in Treated) \times 1(t = month_t)$, a set of monthly dummies and product-firm fixed effects ($\omega_{i,j}$). The regression is ran on the main sample of firms (i.e., between 25k UF and 600k UF) and, to maximize comparability with our main regression results, which include product x time fixed effects, is restricted to those products that were sold by at least one Treated firm and one Control firm during 2006. The plots show the coefficients and 95% confidence interval on the interaction variable. This coefficients represent the differential price (and propensity to make a sale) for each product of Treated relative to Control firms, in each month of 2006 before the Agreement was put in place. None of the coefficients in both plots is significantly different from 0—even though the plot shows the 95% confidence interval, this is true even at a 90% level of confidence. More importantly, the coefficients show no discernible trend in these two variables, which suggests that any difference between both firms in terms of the prices and propensity to sell products did not exhibit differential trends before the Agreement was put in place.

To avoid estimation problems due to potential collinearity of the covariance matrix, we collapse the data in two periods, one before the Agreement (i.e., the year 2006)

and one after the Agreement.¹³ We restrict our sample after the Agreement to the first three years after 2006, that is, 2007, 2008, and 2009. Our results are the same if we include the latter years of our sample and if we drop 2009, and are attenuated if we include only one year in the post period (2007). This suggests that suppliers may enter into medium-long term contracts with the Supermarket, and that we are able to capture the timing of the negotiation of a new contract. To estimate regression (2) we first difference the data to eliminate all time invariant fixed effects and obtain the model we estimate,

$$y_{i,j,post} - y_{i,j,pre} = \beta 1(t = post) \times 1(i \in Treated) + [\alpha_{j,post} - \alpha_{j,pre}] + [\delta_{post} - \delta_{pre}] + \epsilon_{i,j,post} - \epsilon_{i,j,pre}.$$

This equation is estimated in the cross section of supplier-products that are at least present in the pre-period ($post = 0$). Logarithmic outcomes are only observable for product-firm pairs with transactions in both periods.

B. Testing for changes in the organization of the firm and the supply chain: product by time variation

In addition to measuring the effects of the Agreement on the transactions of Control vs. Treated firms, we are interested in measuring the overall response to this restriction of the contracting space by the Supermarket. To do this, we ask how procurement was affected as a function of treatment intensity. Namely, for goods that were sold mainly by Treated firms in 2006, did the Supermarket change its overall purchasing behavior, and from whom it purchased?

We are interested in two outcomes: the overall units procured of each good j in each time period t , and the incidence of the Supermarket purchasing each good j from one of its own subsidiaries. The first outcome, units solds, allows us to estimate the effect of the restriction of trade credit on the overall procurement of the product. The incidence of the Supermarket purchasing the good, in turn, allows us to measure the

¹³In particular, firm-product prices exhibit a high degree of stickiness and seasonality: the first order (monthly) autoregression coefficient of (log) prices is 0.9882.

Supermarket’s decision to bring the procurement of a particular product within the boundaries of the firm. We complement this analysis by also analyzing the market share of units supplied by Treated firms, Control firms, and by the Supermarket itself.

We denote $y_{j,t}$ to represent the outcome variable for product j in month t . We are interested in estimating the following differences-in-differences specification:

$$(3) \quad y_{j,t} = \alpha_j + \delta_t + \beta 1(t = post) \times treatmentintensity_j + \epsilon_{j,t}$$

Here, the treatment variable, $treatmentintensity_j$, is a measure of the fraction of procurement of good j made by the Supermarket from treatment firms in 2006. The coefficient of interest, β , measures the relative effect of the Agreement on product-level outcomes for products that were procured entirely by Treated firm in the preperiod, relative to products that were procured entirely by Control firms in the preperiod.

IV. Results at the product-supplier level

A. The average effects of the reduction of days payable

Column 1 on Table 2 reports the results of regression (2) when the outcome is “makes sale”, a variable that equals one if the supplier records a sale to the Supermarket during the period (pre or post period as defined above). The coefficient suggests that Treated firms are approximately 9% less likely to sell any given product to the Supermarket following the Agreement, relative to before the Agreement. This evidence implies that the Supermarket chooses to shift purchases away from suppliers who are unable to offer longer days payable relative to suppliers who can offer these terms.

Column 2 studies how the Agreement affects the prices of the products that are sold in the post period. The outcome is $\log(price)$, the natural logarithm of the price of each product sold to the Supermarket every month. On average, after the Agreement is implemented on January 2007, Treated firms sell the same product category at 4.56% lower prices than Control firms relative to 2006. This strongly suggests that the large

and financially unconstrained retailer values trade credit. When suppliers lose the ability to extend it, as under the Agreement, they must adjust through other margins. We document that if suppliers are able to respond, contracts are adjusted through prices. The magnitudes of the price changes appear on the surface to be larger than a reasonable 60-day interest rate for external financing. For example, the 4.65% price reduction is equivalent to an annualized interest rate of 32%. This compares to the Chilean banking sector reported yearly rates for the same period of 7% to 11%.¹⁴

Column 3 looks at an effect on the (log) number of units sold, which conditions on a sale taking place. The regression coefficient shows an insignificant 0.89% reduction in units sold after the Agreement. We modify this variable to include firms that don't sell in the post period, by replacing zero units with one unit. The results are shown on Column 4, and suggest an important and significant reduction in units sold by firms paid 60 days earlier.

We combine the evidence on prices and units to form a variable that measures revenues for each product-supplier sold to the Supermarket per month, *amount*. Unsurprisingly, the unconditional treatment effect on $\log(\text{amount})$ is -5.6%, the sum of the effects of the logarithms of prices and units. This effect is relatively large but insignificant (the coefficient's p-value is 23.6%). To include the effect of observations with zero units sold we adjust the variable in the same fashion as the logarithm of units above, by replacing zeros with one peso (roughly 0.2 cents), the lowest monetary unit in Chile. The results of running our regression model with this modified logarithm of amount as outcome are shown on Column 6 of Table 2, and show a large and significant decrease in revenues per monthly sale to the Supermarket. Thus, the evidence strongly suggests that the change in days payable caused a change in the prices and quantities of the products sold to the Supermarket, resulting in an overall reduction in revenues, on average.

We interpret the results as evidence that trade credit is important in a non-standard setting where suppliers do not hold an advantage in the access to external financing

¹⁴Figure taken from "Tasa de Interés Corriente y Máxima Convencional" in www.sbif.cl, for "Operaciones No Reajustables" for less than 90 days, as of January 1, 2007.

relative to their clients. This strongly suggests that the ability to extend trade credit allows suppliers to enter into procurement relationships. The Agreement may be understood as imposing a constraint to the set of feasible contracts that parties can enter into. Our results show that once some contracts are no longer available, contractual relationships that were in place originally are broken (as in our results in the extensive margin) or adjusted (as in our results with price as the outcome).

B. Placebo test

We assess whether the effects of the Agreement on the incidence of a sale, prices, and product revenues are driven by the size differential between Treated and Control firms. Under this hypothesis, products sold by Treated firms would have had a smaller probability of being sold, and if sold would have been purchased at lower prices relative to Control products, even in the absence of the Agreement because Treated firms are smaller. To test this concern, we define a “placebo” sample of firms whose 2006 revenues are above the UF 100,000 cutoff. These are firms that were not affected by the Agreement in 2007. We then split this placebo sample using the IRS reported revenues categories: firms with revenues below UF 600,000 (USD24 million) are labelled as Treated-placebo, while firms above that threshold are Control-placebo (this includes firms with total revenues of UF 100,000 to UF 600,000 (USD4.0 million to USD24 million) as Treated-placebo, and UF 600,000 and higher as Control-placebo). The placebo sample has 389 Treated-placebo firms and 230 Control-placebo firms, which correspond to 52,668 Treated-placebo product-firms and 66,540 Control-placebo product-firms sold during 2006. This split and sample selection assures that the placebo test has a similar level of power to our main regression specifications.

Table 3 shows the regression results of this placebo test for the outcomes “makes sale”, “ $\log(\text{price})$ ” and “ $\log(\text{amount} + 1)$ ” in columns 1, 2, and 3, respectively. The coefficient on makes sale is positive and insignificant. That is, Treated-placebo firms have an insignificantly higher probability of making a sale to the Supermaret after 2007 relative to 2006, and relative to Control-placebo firms. We interpret this as evidence

that relatively smaller firms do not naturally reduce the incidence of procurement to the Supermarket. Thus, the reduction in the incidence of a sale to the Supermarket for Treated firms in our main tests seems to be driven entirely by the Agreement that forced suppliers to be paid in 30 days.

Columns 2 and 3 of Table 3 show that this conclusion also applies to product level prices and revenues: Treated-placebo firms do not see a reduction in prices or revenues after the Agreement, relative to Control-placebo firms. The placebo test as a whole suggests our results are not mechanically driven by the difference in the size of the suppliers affected and unaffected by the Agreement.

C. Procurement of affected products and vertical integration

One virtue of our comprehensive dataset is that we can ask both how the overall procurement of inputs as well as the organizational structure of the Supermarket change as a result of the Agreement. The empirical tests are based on equation (3), and results are presented in Table (4). In column 1, we find that after the Agreement, the overall level of procurement falls for those goods that previously had been supplied mostly by Treated firms. The magnitude of the reduction, approximately 14,000 units per month, is economically large, and corresponds to 0.2 within-product standard deviations. We interpret this result in the following way: when unable to procure from Treated suppliers because of the inability to extend trade credit, which was the equilibrium in the preperiod, the Supermarket must pay a cost to either vertically integrate or shift purchases to non-affected suppliers. This cost results in a reduction in the total number of units purchased. This evidence suggests that the firm is unable to replicate the market outcomes and settles with a second-best outcome, consistent with Baker, Gibbons, and Murphy (2001).

We test for whether the restriction in the set of feasible contracts made the Supermarket more willing to vertically integrate. Column 2 shows the regression results when the outcome is a dummy variable that equals one if the Supermarket purchased the product internally through a subsidiary. The positive coefficient suggests that faced with the

restriction in days payable, the Supermarket does indeed choose to procure via internal subsidiaries some goods that were previously sourced by Treated firms. Treated firms, which no longer have the ability to use trade credit to post a bond, are unable to continue to supply to the Supermarket. The Supermarket is partially able to offset this effect by bringing more procurement into the firm via vertical integration, but the *ex ante* level of procurement is not met.

To understand the magnitude of this effect, we split products into “high Treated share” and “low Treated share” based on those with higher and lower market share than the median market share of Treated firms in the preperiod (9.7%). We run the same regression as in equation (3) but replace the *treatmentintensity* variable with $hightreatment = 1(treatmentintensity > p50)$. The results of this regression are shown in Column 3 of Table (4). These suggests that the Supermarket is roughly 3% more likely to shift procurement to an internal subsidiary for products that were mostly sold by Treated firms before the Agreement.

Figure 2 shows the effect of the Agreement on the relative market shares at the product-level for the Supermarket’s internal subsidiaries, Treated firms, and Control firms. The figure shows that for products where Treated firms had a market share lower than the median (approximately 10%), the relative shares of the three types of supplier remain roughly constant. However, for products where Treated firms had a large market share, we find that Treated firms experience a large decrease in their market share, while Control firms and Supermarket-owned subsidiaries increase their own market shares substantially.

Columns 3, 4, and 5 of Table (4) confirm the intuition provided by the figure. Consistent with our results so far, the relative shares of the Supermarket and Control firms increase while the relative share of Treated firms decreases for goods that were supplied mostly by Treated firms in the preperiod. We also find that the magnitude of the increase is larger for Control firms than for Supermarket subsidiaries. This suggests that Control firms are on average more efficient than the Supermarket in procuring goods. If a Control firm is available, the Supermarket would prefer to procure from

them. However, for some products and some price levels, the Supermarket vertically integrates and procures from an internally owned subsidiary.

D. Age as a proxy for transparency and reputation

We show above that the ability to extend trade credit is valuable to the Supermarket and ultimately affects its decision to enter into supplier-client relationships. In this section we study what drives the value of trade credit to the Supermarket.

The Supermarket's suppliers that form our main sample are considerable smaller and presumably more financially constrained than the Supermarket itself. Thus, the value of trade credit in this setting is most likely not linked to a financing advantage. Rather, it may serve a role alleviating a potential agency problem in the client-supplier relationship.

In line with previous literature, we posit that the agency problem in this relationship is borne out of information asymmetry about the product's quality. We consider a general definition of quality that may include variables that only affect the Supermarket's demand for the product, for example, reliability over time. Thus, trade credit may serve as a relationship-specific investment that suppliers of good quality products make in order to separate themselves. Theoretically, this information asymmetry problem should be overcome by repeated interactions over time. For example, the Supermarket could learn whether the supplier responds to purchase orders with top-quality products or is able to supply the good on time, or not.

This analysis suggests that length of the relationship may act as a proxy for the level of information asymmetries. We do not observe direct length of relationship, but we proxy for it with the number of years since the supplier was formally incorporated with the Chilean IRS. We rationalize this approximation by noting that the Supermarket has been an important player in the Chilean Supermarket industry for at least 25 years. Thus, suppliers must have had developed a procurement relationship with the Supermarket early on in their life cycles in order to survive in this industry.

As of 2006 the age of incorporation variable is capped at 13 years because the IRS's website defines the first year as 1993. However, the median firm in our main sample is also 13 years old. Thus, we define the variable $old_i = 1$ ($IRSage_i = 13$). Because of missing data in the IRS's website, we are only able to define this variable for 703 of the 734 firms that form our main sample.¹⁵ We expect older firms to be larger, which would imply that our sorting measure (age) is correlated with the treatment as per the criteria in the Agreement. However, the 2006 cross sectional correlation between old_i and 1 ($i \in Treated$) is only -0.1461, which suggests that there is substantial heterogeneity of age within each treatment group (Control firms are split into 141 young and 225 old; Treated firms are split into 178 young and 159 old). Based on this observation, we study the effects of the Agreement at the firm level, and differentially for old and young firms.

We collapse our data at the supplier-month level. Our empirical specification compares the outcome of Treated relative to Control firms, before and after the Agreement was in place. We control non-parametrically for unobserved heterogeneity by considering the main sample of firms, which focuses on a narrow interval of revenues around the UF 100,000 cutoff, as defined in previous sections. That is, we consider firms whose yearly 2006 as reported by the Chilean IRS are in the range of US\$1 million to US\$24 million. For firm i and month t , our main specification is

$$(4) \quad outcome_{i,t} = \alpha_i + \delta_t + \omega_t \times old_i + \beta treated_i \times post_t + \gamma treated_i \times post_t \times old_i + \epsilon_{i,t},$$

where α_i is a firm fixed effect that captures time-invariant heterogeneity, δ_t are monthly dummies, $\omega_t \times old_i$ allows different time trends for old firms, and $treated_i$ is activated for Treated firms. The coefficients of interest are β , which measures the difference in $outcome_{i,t}$, between Treated and Control firms after the Agreement for young firms, and

¹⁵The 31 firms for which we do not obtain age information correspond to 26 Control firms and 5 Treated firms. In principle this could induce a bias in our results as the observation of a firm's age is correlated with the treatment. We run regression (2) on a sample that does not include the products sold by these firms. We find that the coefficients on $treated$ are -4.33% and -10.5% for " $log(price)$ " and "makes sale" as outcomes, both very similar in magnitude and statistically significant at the same levels as in our main sample.

γ , which measures the differential effect of the Agreement for old firms. We estimate regression (4) at the supplier-month level and we include, as in the product-firm regressions, years 2007, 2008, and 2009 in the post period.

Table 5 presents the results. Columns 1 and 2 show the regression results the natural logarithm of revenues to the Supermarket and the natural logarithm of revenues where zeros have been replaced by one peso, respectively. The coefficient on $treated \times post$ in Panel A suggest that young firms suffer an economically large and statistically significant reduction in revenues following the Agreement. The magnitudes suggest a 19% reduction in the intensive margin (i.e., conditional on a supplier selling to the Supermarket on a given month). Column 3 shows that this effect is present when looking at levels of revenues, corresponding to a 9.5 million pesos reduction (roughly \$19.5k dollars).

Further, the coefficients on $treated \times post \times old$ show that old firms are able to almost completely overcome the negative effect of the Agreement. In effect, although statistically insignificant, the measured effect of 18.27%, almost exactly offsets the coefficient on $treated \times post$ for $\log(amount)$ as the outcome. Indeed, the sum of both coefficients is not statistically significant from zero. The coefficient for *revenues* as the outcome, 11.0 million pesos, is statistically significant at the 10% level and is larger in magnitude than the reduction in revenues for young firms.

In summary, this evidence suggests that the Agreement had a large and significant effect on young suppliers' relationship with the Supermarket. Because we do not observe total revenues, we may not test whether this resulted in firms being overall worse off. However, given the Supermarket's large market share, it is unlikely that small, young suppliers were able to offset the reduction in revenues by shifting sales to other clients. Further, the Supermarket's main competitor signed a similar agreement with the government in 2008. This suggests that any benefit obtained from diverting sales to the competitor was short lived.

E. Experience and search goods

Our interpretation of the value of trade credit stems from differences in product quality. Yet, we do not have direct evidence on the quality of the goods being sold to the Supermarket, nor on other measures of quality like reliability, that could potentially impact the suppliers' willingness to extend trade credit. We investigate whether product attributes that make it harder for the Supermarket to ascertain the quality of the good affect the impact of the Agreement. Motivated by the work of Nelson (1970) and Milgrom and Roberts (1986) we exploit the richness of our data and define an ad-hoc measure of whether a good's quality is easily verifiable at the product-level category. We label "experience goods" as those goods where quality may be harder to ascertain. The omitted category will therefore correspond to "search goods", where quality may be easily ascertained. In principle, search goods should not be too different across suppliers based on observable characteristics. Consequently, the Supermarket's ability to detect differences in quality at the product level should be higher for search goods. The effect of the Agreement, in turn, should be higher for experience goods, or for firms that mostly sell experience goods.¹⁶

We investigate this by running a modified version of regression (1) that interacts the right hand side variables with a dummy that equals one for products that are labelled experience goods:

(5)

$$p_{i,j,t} = \beta 1(t = post) \times 1(i \in Treated) + \gamma 1(t = post) \times 1(i \in Treated) \times 1(experience\ good_j) \\ + \lambda 1(t = post) \times 1(experience\ good_j) + \alpha_{j,t} + \omega_{i,j} + \delta_t + \epsilon_{i,j,t}.$$

Table 6 shows the results (to economize space we label $exp = 1(experience\ good_j)$). The first row corresponds to the omitted category in the interaction (search good)s. Columns 1 and 2 show that Treated firms sell search goods at a lower price and with a lower probability after the Agreement relative to Control firms, but this effect is

¹⁶1,063 of the 1,467 products that form our main sample are labelled experience goods. At the supplier-product level, firm age is negatively correlated with product verifiability.

not significantly different from zero. Consistently with our main result, the effects of the Agreement on the price and probability of being sold are larger in magnitude for experience goods. However, the coefficients are not significantly different from the estimates for search goods. We calculate the p-value of the two-sided test that $\beta + \gamma$ differs from zero. This p-value is 0.003 for Column 1 and 0.016 for Column 2, suggesting a significantly negative effect of the Agreement on firms selling experience goods. Notably, these tests lack the statistical power to reject the null that the Agreement has the same effect on search and experience goods. Consistent with our intuition, Column 3 shows that the overall effect of the Agreement on product revenues is amplified for experience goods. We view the evidence on experience versus search goods, and on old versus young firms, as in line with our proposed interpretation.

V. Discussion

In the traditional paradigm, trade credit is used as a means through which suppliers can finance buyers. However, it is commonly observed that small firms with higher costs of capital are ostensibly financing their much larger buyers. We present evidence that trade credit may be playing an entirely different role and can be helpful in overcoming information asymmetries between buyers and suppliers. We document that when trade credit terms are restricted for small suppliers, they are forced to either sell their goods at lower prices or to stop selling their goods altogether. As a lower bound, reducing the days payable of procurement contracts from 90 to 30 days causes a decrease in product prices of 4.56%. The implied annualized interest rate earned on accounts payable is 32%. In comparison, the standard yearly market interest rate on bank loans is 7% to 11%. Further the two large Chilean supermarkets pay approximately 5% per year (plus inflation of 5%-10%) on their publicly traded debt. Thus, it appears that the buyer is willing to pay a relatively large premium for its trade credit.

When it becomes harder for small suppliers to do business with the Supermarket, we observe organizational changes within the Supermarket itself. Namely, for those goods

once sold by outside suppliers, restricting the terms of trade causes the Supermarket to integrate vertically and to procure inputs from within the firm.

We find that the effects of the Agreement are quite heterogeneous in both firm and product characteristics and that some types of firms are able to continue to supply goods despite the restriction of contractual terms. The detrimental effects of the Agreement are stronger for relatively young firms and for firms selling products where quality may be harder to ascertain (experience goods). This is consistent with theories in which reputation and observability can offset the need to offer trade credit to verify product quality. Offering trade credit allowed young firms and firms selling goods of hard-to-verify quality to invest in their relationships with the Supermarket, an investment opportunity that the Agreement took away.

Our results suggest that reduction of days payable may harm suppliers, in particular younger firms, and also may pose costs to the Supermarket (evidenced by the reduction in the procurement of goods sold by treated firms). Strikingly, this harmful effect on small firms is precisely the opposite effect of what government intended by the policy change. Our effects are specific and local to firms whose yearly revenues are near the cutoff. But, based on their revenues, these firms are on the verge of becoming relatively large. Thus, the study is relevant to understand the drivers of firm growth in less developed economies.

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Appendix

Appendix A. A simple model of trade credit in the presence of information asymmetries about quality

Suppose that there are a continuum of suppliers and one buyer. The supplier firms can each be of high type or low type $i \in \{H, L\}$ with proportions in the population $\theta_i \in \{\theta_H, 1 - \theta_H\}$. High type suppliers produce high quality goods, and low type suppliers produce low quality goods. Also suppose that each supplier firm knows its own type. The timing is as follows: In period $t = -1$, supplier firms decide whether or not to produce their product at a cost $\underline{p}$. We assume that each firm is able to supply up to 1 unit of the good and therefore chooses $q_i \in \{0, 1\}$. At time $t = 0$, goods change hands between the suppliers and the buyer. At time $t = 1$, the buyer sells the goods to the consumer market. The consumer market observes the quality of the goods and is willing to pay p_H for high quality goods and p_L for low quality goods. WLOG, we assume that $p_H > \theta_H p_H + c > \underline{p} + c > \underline{p} > p_L = 0$.

We focus on different types of information structures and purchase contracts that might exist in the market.

First, we explore the case where quality is observable to all parties at $t = 0$. If the buyer chooses spot market contracts, she sets $p_H^0 = \underline{p}$ and $p_L^0 = 0$. At these prices, suppliers choose $q_H = 1$, $q_L = 0$ at time $t = -1$. This contract dominates any trade credit contract, because suppliers incur a cost c , which would drive up the procurement price. Therefore, in this extreme case of perfect information, there should be no difference between prices and quantities observed in the market under either a regime with trade credit or a regime without trade credit.

Second, we examine cases where the buyer firm does not know anything about each supplier's type at $t = 0$. However, at $t = 1$, the quality is observed by the market.

When trade credit contracts are not available, the buyer firm offers a uniform price at $t = 0$ for the goods. Because the goods are supplied competitively, the buyer can drive supplier profits to 0. The buyer is therefore willing to pay $p_0 = \underline{p}$ to procure the goods from all firms as long as expected profits are weakly positive, i.e. when $\theta_H p_H \geq \underline{p}$. Under this pricing structure, it is a best response for all suppliers to choose $q_i = 1 \ i \in \{H, L\}$.

Now suppose that the supplier firms are able to pay a cost c and offer trade credit to the buyer. Under the trade credit contract, the buyer pays a prespecified amount to each supplier, conditional on the observed quality of the good. There are two types of possible equilibria, separating equilibria and pooling equilibria. In a separating equilibrium, the buyer offers different prices p_i^1 to suppliers at time 1 offering high vs. low quality products. We propose the following strategy: The buyer offers $p_L^1 = 0$, $p_H^1 = \underline{p} + c$. If the supplier chooses not to provide trade credit, a price $p_0 = 0$ is offered. Under these prices, it is only profitable for the high type suppliers to pay the cost and to offer trade credit. Therefore, only the low types will sell on the spot market. At these prices, working backward to $t = -1$, only the high type firms will be willing to pay the cost $\underline{p}$ and produce the good. There is no profitable deviation from this strategy, thus it is an equilibrium.

Next we turn to the possibility of pooling equilibria where trade credit is not offered. Suppose that $p_0 = \underline{p}$. However, high type firms could propose an alternate contract where they invest c in trade credit and offer their goods at a price $p_H^1 = \theta_H p_H + c + \varepsilon$ for arbitrarily small ε in order to separate themselves from the low types. It is profitable for the buyer to accept this alternate contract if

$$p_H - \theta_H p_H - c - \varepsilon \geq 0$$

$$(1 - \theta_H) p_H - c \geq \varepsilon$$

$$(1 - \theta_H) p_H - c > 0$$

which is satisfied by the assumptions. Therefore, this cannot be a pooling equilibrium.

Thus, when trade credit is not possible and buyers cannot observe product quality at $t = 0$, all firms supply their goods to the buyer at a price $\underline{p}$. However, when trade credit is possible, only the high type firms supply their goods to the market at a price $\underline{p} + c$. Thus, there are two testable hypotheses:

- 1) under trade credit contracts, fewer firms are able to sell to the market.
- 2) under trade credit contracts, the procurement price is higher.

Note that in the case with trade credit, low type firms create a negative social surplus of a size $(0 - \underline{p})(1 - \theta_H)$. In the case with trade credit, high type firms pay a social cost $c\theta_H$. Trade credit is socially efficient if

$$c\theta_H + (0 - \underline{p})(1 - \theta_H) > 0$$

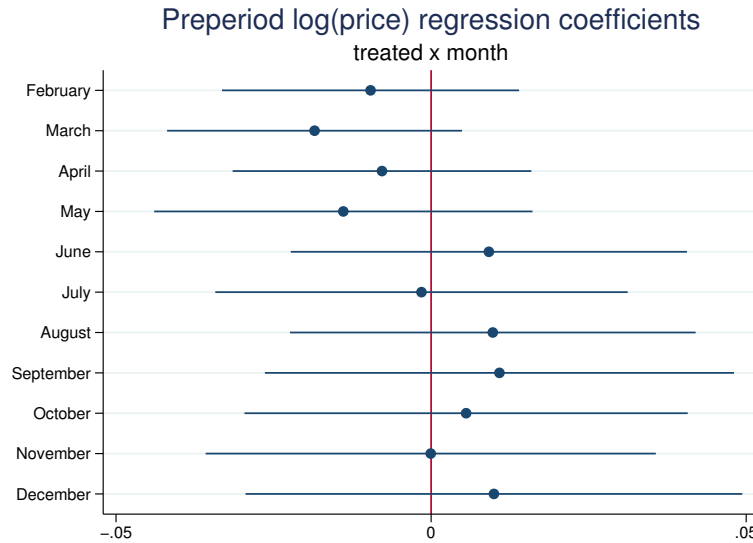
$$\theta_H > \frac{\underline{p}}{c - \underline{p}}$$

Appendix B. Figures and Tables

FIGURE 1. Preperiod trends

This figure shows that there is no statistical difference in the trend of prices and in the propensity to make a sale during 2006 for products sold by Treated and Control suppliers. The plots are based on the regression of $\log(\text{price})$ and a dummy for whether the supplier records a sale to the Supermarket during the period in Panels A and B respectively, on a set of month dummies and product $\times$ firm fixed effects, and on the interaction $\text{treated} \times \text{month}$, which equals one for Treated firms on each month. Treated firms are those with total 2006 revenues below 100k UF and total 2006 sales to the Supermarket below 60k UF. UF (“Unidad de Fomento” is an inflation linked currency unit updated daily, whose value is published by the Banco Central de Chile. We restrict the sample to those firms with total 2006 revenues between 25k UF and 600k UF. The plots show the coefficients on the interaction term, as well as the 95% confidence interval for the estimated coefficient. The regressions are ran on a sample of firms with total 2006 sales between 25k UF and 600k UF, and on those products that have at least one Treated and one Control supplier during 2006.

Panel A: preperiod differential trends in $\log(\text{prices})$ for Treated versus Control firms



Panel B: preperiod differential trends in propensity to sell each month for Treated versus Control firms

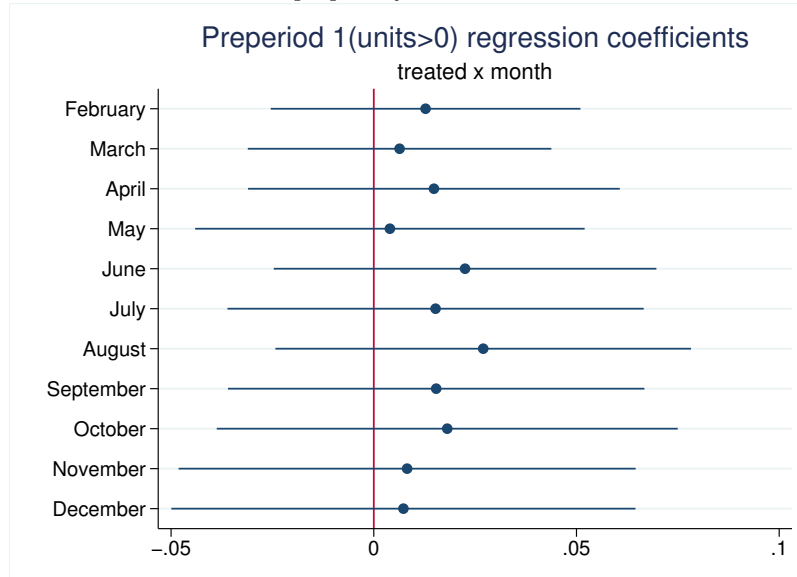


FIGURE 2. Procurement product-level market shares, pre and post period

This figure shows the average market share for sales to the Supertmarket at the product level for Treated, Control, and Supermarket-owned subsidiaries, for two periods: Pre, before the Agreement (2006), and Post, after the Agreement, (2007, 2008 and 2009). Treated market share is calculated over all firms whose 2006 revenues are below 100k UF (\$4 million). We restrict the sample to those firms with total 2006 revenues between 25k UF and 600k UF. We exclude products that were not sold during 2006.

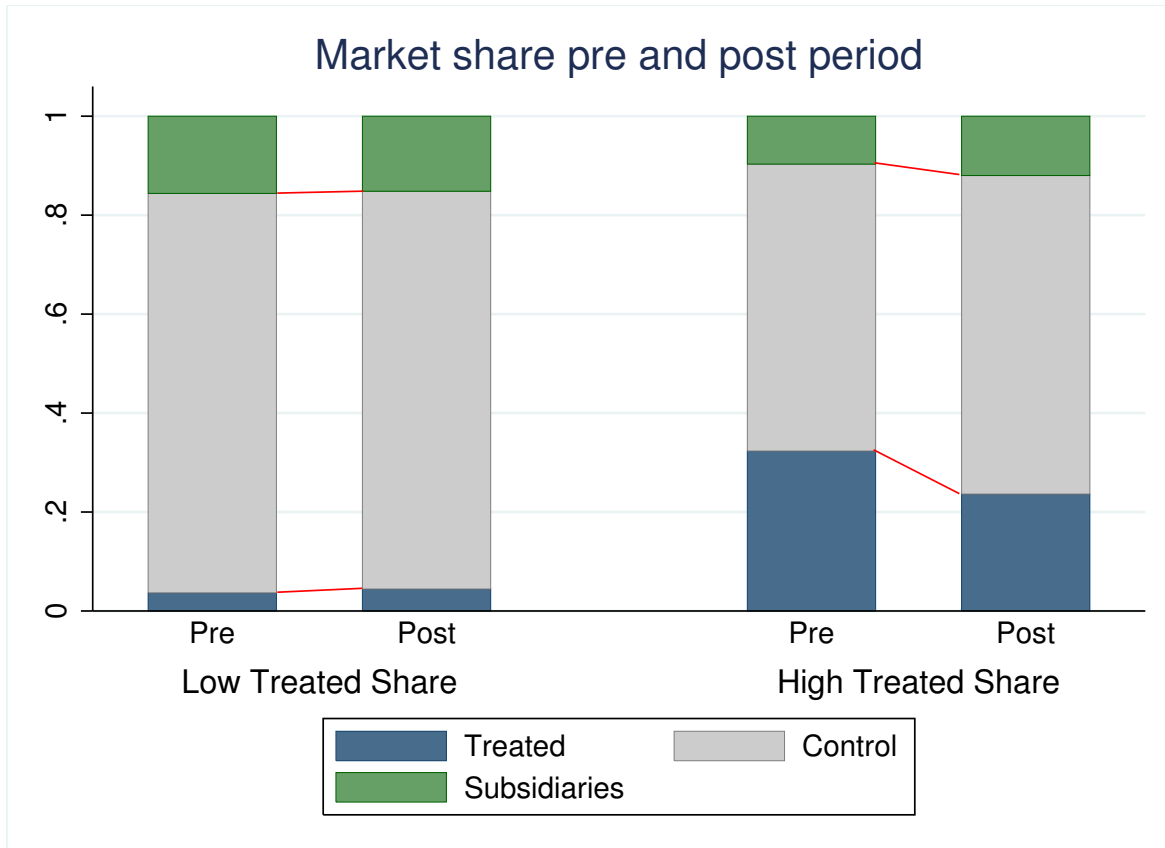


TABLE 1. Summary statistics main sample

This table shows the mean, standard deviation and median of variables for Treated and Control firms as defined above. Panel A shows variables at the firm-level, while Panel B shows variables at the product-firm level. Panel C shows the number of product-firm observations for each of the 16 Departments defined by the Supermarket.

Panel A: Firm level average monthly variables									
	All (N=734)			Treated (N=342)			Control (N=392)		
	Mean	St. Dev.	p50	Mean	St. Dev.	p50	Mean	St. Dev.	p50
Sales ('000,000 pesos)	375.91	714.06	130.98	164.08	217.40	73.29	560.73	917.14	233.98
# Departments	1.540	0.917	1.0	1.442	0.651	1.0	1.625	1.092	1.0
# Lines	1.926	1.582	1.0	1.693	0.964	1.0	2.130	1.947	2.0
# Sections	3.322	4.748	2.0	2.655	2.600	2.0	3.903	5.969	2.0
# Products	6.518	11.099	3.0	4.842	5.595	3.0	7.980	14.108	3.0
Age*	9.861	4.174	13.0	9.056	4.491	11.0	10.601	3.713	13.0
Age>=13 years	0.523	0.500	1.0	0.465	0.499	0.0	0.574	0.495	1.0

Panel B: Product-firm level 2006 monthly average									
	All (N=6,232)			Treated (N=2,092)			Control (N=4,140)		
	Mean	sd	p50	Mean	sd	p50	Mean	sd	p50
Units ('000)	6.51	55.48	0.22	3.39	13.38	0.20	8.09	67.35	0.24
Sales (pesos '000,000)	3.35	11.00	0.31	2.03	5.80	0.24	4.02	12.80	0.35
log(price)	6.978	1.572	6.908	6.941	1.757	6.961	6.997	1.464	6.888
log(units)	6.843	2.195	6.845	6.566	2.158	6.624	6.990	2.201	6.982
log(amount)	13.821	2.159	14.023	13.507	2.219	13.710	13.987	2.108	13.987
1 (<i>experience</i>)	0.6302	0.4827	1.000	0.5611	0.4963	1.000	0.6653	0.4719	1.000

Panel C: Industry distribution of product transactions			
	All (N=6,232)	Treated (N=2,092)	Control (N=4,140)
CLOTHING	521	157	364
BABY	252	37	215
HOME	328	92	236
PETS	51	17	34
GENERAL FOOD	1,286	362	924
PERISHABLES	581	96	485
ENTERTAINMENT	657	217	440
HARDLINES	682	290	392
IMPULSIVE SHOPPING	41	3	38
MEAT AND FISH	263	75	188
DELI	342	146	196
FRUITS & VEGETABLES	616	348	268
BREAD & BAKING	250	103	147
BUSINESS	222	110	112
RESTAURANT	101	24	77
HEALTH & WELLBEING	39	15	24

TABLE 2. Estimates of the effect of the reduction of days payable on firm-product contractual outcomes

This table presents the estimated coefficient of interest of regression (2), β , which measures the relative change in the outcome of a product sold to the Supermarket by Treated firms relative to Control firms, before and after the reduction in days payable as per the Agreement. Treated firms are those with total 2006 revenues below 100k UF and total 2006 sales to the Supermarket below 60k UF. UF (“Unidad de Fomento” is an inflation linked currency unit updated daily, whose value is published by the Banco Central de Chile. We restrict the sample to those firms with total 2006 revenues between 25k UF and 600k UF. We exclude products that were not sold during 2006. The outcomes are “makes sale”: a dummy that equals one if a sale is recorded during the period (pre- or post-Agreement), “ $\log(\text{price})$ ”: natural logarithm of the transfer price; “ $\log(\text{units})$ ”: the natural logarithm of units sold to the Supermarket; “ $\log(\text{amount})$ ” the natural logarithm of monthly sales to the Supermarket in pesos; and “amount”: monthly sales to the Supermarket in pesos. In columns 4 and 6, observations with 0 unit sales are replaced with the log of 1 unit and the log of 1 peso, respectively. The data is a balanced panel at the monthly-firm-month level, as described above. Data is collapsed at the pre and post-Agreement periods, where pre represents the year 2006 and post are the years 2007, 2008, and 2009. Standard errors are clustered at the firm level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	makes sale	$\log(\text{price})$	$\log(\text{units})$	$\log(\text{units} + 1)$	$\log(\text{amount})$	$\log(\text{amount} + 1)$	amount
<i>post</i> $\times$ <i>treated</i>	-0.0885*** (0.029)	-0.0456** (0.018)	-0.0089 (0.056)	-0.3852** (0.183)	-0.0563 (0.055)	-1.0346*** (0.361)	-0.4322 (0.387)
R^2	0.336	0.409	0.136	0.291	0.155	0.351	0.171
Obs.	4,784	3,707	3,707	4,784	3,707	4,784	4,784
Firms	734	597	597	734	597	734	734
Products	1,467	1,174	1,174	1,467	1,174	1,467	1,467
Product-time FE	YES	YES	YES	YES	YES	YES	YES

TABLE 3. Placebo

This table presents the results of a placebo test that shows that our results are not driven by a size difference between firms. The table shows the estimated coefficient of interest of regression (2), β , which measures the relative change in the outcome for the same product for Treated-placebo firms relative to Control-placebo firms, before and after the reduction in days payable for Treated firms. The placebo sample consists of firms with total 2006 revenues of 100k UF or higher; within this Placebo sample, Treated-placebo firms are those with 2006 revenues of 600k UF or lower. UF (“Unidad de Fomento” is an inflation linked currency unit updated daily, whose value is published by the Banco Central de Chile. The outcomes are “makes sale”: a dummy that equals one if the supplier sells the product during the period; “ $\log(price)$ ”: natural logarithm of the transfer price; “ $\log(units)$ ” the natural logarithm of monthly sales to the Supermarket in pesos, where observations with 0 unit sales are replaced with 1 peso. The data is a balanced panel at the monthly-firm-month level, as described above. Data is collapsed at the pre- and post-Agreement periods, where pre represents the year 2006 and post are the years 2007, 2008, and 2009. Standard errors are clustered at the firm level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)
	makes sale	$\log(price)$	$\log(amount + 1)$
$post \times treated_{placebo}$	0.0852 (0.071)	0.0112 (0.014)	1.1086 (1.105)
R^2	0.354	0.340	0.387
Obs.	7,427	5,396	7,427
Firms	805	644	805
Products	1,578	1,292	1,578
Product $\times$ time FE	YES	YES	YES

TABLE 4. Supplier procurement and vertical integration

This table presents the estimated coefficient of interest of regression (3), β , which measures the relative change in the outcome for products with high exposure to the Agreement relative to products with low exposure, before and after the reduction in days payable for Treated firms, measured by the fraction of 2006 sales to the Supermarket made by Treated firms. Treated firms are those with total 2006 revenues below 100k UF and total 2006 sales to the Supermarket below 60k UF. UF (“Unidad de Fomento” is an inflation linked currency unit updated daily, whose value is published by the Banco Central de Chile. We restrict the sample to those firms with total 2006 revenues between 25k UF and 600k UF. The outcomes are “*unitsprocured*” the overall number of units procured of good j in month t winsorized at the 99th percentile; “*subsidiary*”: incidence of procurement from a Supermarket subsidiary; “*subsidiaryshare*”, the unit share procured by internal subsidiaries of the Supermarket; “*treatedshare*”, the unit share procured by Treated firms, and “*controlshare*”, the unit share procured by Control firms. Column 3 corresponds to regression (3) with the interaction variable replaced to *highshare*, a dummy that equals one if during 2006 Treated firms had a market share higher than the cross-sectional median (0.0906%). Data is at the product $\times$ month frequency. Post covers the years 2007, 2008, and 2009. Standard errors are clustered at the product level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>unitsprocured</i>	<i>subsidiary</i>	<i>subsidiary</i>	<i>subsidiaryshare</i>	<i>treatedshare</i>	<i>controlshare</i>
<i>post</i> $\times$ <i>treatmentintensity</i>	-13,882.97*** (4,668.03)	0.0527*** (0.015)		0.0365*** (0.009)	-0.2724*** (0.022)	0.2359*** (0.022)
<i>post</i> $\times$ <i>highshare</i>			0.0304*** (0.010)			
R^2	0.944	0.510	0.510	0.685	0.681	0.731
Obs.	68,612	68,612	68,612	59,247	59,247	59,247
Products	1,009	1,009	1,009	1,009	1,009	1,009
Product and Month FEs	YES	YES	YES	YES	YES	YES

TABLE 5. Firm-level regression for older and younger firms

This table presents the results of the firm-level regressions using specification (4). We present the coefficients on $treated \times post$, which measures the effect of the Agreement on each outcome for young Treated firms in the post period, $treated \times post \times old$, which measures the differential effect of the Agreement for old Treated firms in the post period. Treated firms are those with total 2006 revenues below 100k UF and total 2006 sales to the Supermarket below 60k UF. Old firms are defined as firms with age greater than or equal to the cross sectional median (13 years) of the distribution of suppliers as of 2006. UF (“Unidad de Fomento” is an inflation linked currency unit updated daily, whose value is published by the Banco Central de Chile. The outcomes are “ $\log(amount)$ ”: the natural logarithm of monthly sales to the Supermarket in pesos; “ $\log(amount + 1)$ ”, the natural logarithm of monthly sales to the Supermarket in pesos where zeros have been replaced by one peso; and “amount”: monthly sales to the Supermarket in pesos. The data is a balanced panel at the monthly-firm-month level, as described above. The data includes year 2006 in the pre period, and post are the years 2007, 2008, and 2009. Standard errors are clustered at the firm level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)
VARIABLES	$\log(amount)$	$\log(amount + 1)$	amount
$treated \times post$	-0.1968* (0.108)	-1.0733* (0.596)	-9.55* (4.926)
$treated \times post \times old$	0.1827 (0.143)	1.1570 (0.802)	11.01* (5.742)
R^2	0.013	0.056	0.010
Obs.	21,898	33,744	33,744
Firms	703	703	703

TABLE 6. Product-level regression for experience and search goods

This table reports how the estimated effects of the change in days payable on transaction prices and the revenues to the Supermarket varies for products defined as non-commodities. Experience goods ($exp = 1$) are defined as products where quality may be easier to ascertain. Standard errors are clustered at the firm level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively. We present the p-value of the sum of the two coefficients on $post \times treated$ and $post \times treated \times exp$, which corresponds to the two-sided test of the null hypothesis that the effect of the Agreement on experience goods is different from zero.

	(1)	(2)	(3)
	makes sale	$\log(price)$	$\log(amount + 1)$
$post \times treated$	-0.0568 (0.038)	-0.0246 (0.021)	-0.8103* (0.491)
$post \times treated \times exp$	-0.0680 (0.055)	-0.0450 (0.035)	-0.4811 (0.654)
R^2	0.337	0.409	0.351
Obs.	4,784	3,707	4,784
Firms	734	617	734
Products	1,467	1,023	1,467
p-value of sum	0.003	0.016	0.007
Product×time FE	YES	YES	YES