

# Financial Contracting and Organizational Form: Evidence from the Regulation of Trade Credit

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## ABSTRACT

We present evidence that restrictions to the set of feasible financial contracts affect buyer-supplier relationships and the organizational form of the firm. We exploit a regulation that restricted the maturity of the trade credit contracts that a large retailer could sign with some of its small suppliers. Using a within-product difference-in-differences identification strategy, we find that the restriction reduces the likelihood of trade by 11%. The retailer also responds by internalizing procurement to its own subsidiaries and reducing overall purchases. Finally, we find that relational contracts can mitigate the inability to extend long trade credit terms.

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A rich literature in organizational economics studies how the institutional environment affects the boundary of the firm and the scope for trade with external parties (e.g. Coase (1937), Williamson (1973), Grossman and Hart (1986)). In particular, when contracts are not feasible or enforceable, vertical integration, that is, expansion in the vertical boundary of the firm, may replace arm's-length transactions with suppliers. Empirical evidence of a link between the contracting environment and the organizational form of the firm and its supply chain has been largely limited to observational case studies or industry-level analyses (Bresnahan and Levin (2013), Lafontaine and Slade (2013)). In general, the confluence of factors that jointly determine the scope of contracting institutions, financial markets, and the choice between trade and integration renders causal inference quite difficult. Further, it is impossible to observe the latent contract that a vertically integrated firm would offer to an external supplier.

This paper provides the first causal evidence that the contracting environment affects the organization of the supply chain and the boundary of the firm. We focus on the ability of upstream and downstream firms to write trade credit contracts with one another. Trade credit, or delayed payment for intermediate goods, is one of the most common financial contracts in procurement relationships, and estimates suggest that it finances roughly two-thirds of global trade.<sup>1</sup> A large literature examines the determinants of trade credit terms between buyers and suppliers.<sup>2</sup> First and foremost, trade credit is characterized as an efficient financing arrangement whereby credit flows from relatively unconstrained buyers to more financially constrained suppliers. In line with this view, Murfin and Njoroge (2015) document that the smallest decile of Compustat firms use by far the most trade credit. However, it is not uncommon to also observe credit flowing from small, constrained suppliers to large corporations with access to international capital markets.<sup>3</sup> Indeed, Murfin and Njoroge (2015) document that firms in the top two size deciles are also net trade credit borrowers. In these settings, little is known about the role trade credit and other financial

contracts may play to facilitate trading relationships with external suppliers.

We exploit a natural experiment in Chile that limited the trade credit terms that a large buyer (the “Superstore”) could obtain from over 1,000 of its small suppliers. In this specific context, it is unlikely that trade credit reflects a relative advantage of suppliers in external financing terms.<sup>4</sup> Fearing the outsized market power of the country’s two large discount retailers (including the Superstore), in December 2006 the Chilean government entered into an accord with the Superstore (the “Agreement”). The Agreement reduced the maturity of trade credit contracts that the Superstore could write with a subset of suppliers (the “affected” suppliers) to 30 days from the pre-Agreement status quo of 90 days.<sup>5</sup> The government chose to impose the regulation only for firms with sales below an arbitrary cutoff of UF 100,000<sup>6</sup>—roughly \$4.0 million. Both the timing and the eligibility criteria of the Agreement are essential components of our empirical strategy.

We use proprietary product-supplier-level procurement data obtained from the Superstore and regulatory status data from the Chilean tax authority to document three margins of adjustment by the Superstore in response to the Agreement. First, we find that the restriction to the set of feasible contracts makes trade with affected suppliers less likely. In our baseline empirical strategy, we compare changes in the procurement of each product sold by Treated firms, defined as firms with total revenues below the UF 100,000 cutoff, before and after the Agreement relative to the *same* product sold by Control firms, defined as firms with total revenues above the UF 100,000 cutoff. We control non parametrically for firm size by focusing on firms whose 2006 yearly revenues were within a relatively tight range above and below the cutoff.<sup>7</sup> We find that the probability that a Treated supplier sells a product to the Superstore falls by 11% after the Agreement relative to the same product sold by a Control supplier. Second, the Agreement makes vertical integration more likely. The probability that the Superstore procures from a wholly owned subsidiary increases by 4% from a baseline of 21% for products that were mostly procured by affected firms (above-median market share).

Third, total procurement of products that were mostly purchased from affected firms is reduced after the Agreement. We interpret this as evidence that the Superstore is not fully able to replicate the pre-period market equilibrium by shifting procurement to its subsidiaries or to unaffected firms. This result suggests that the vertical integration stemming from the Agreement is costly (consistent with Baker et al. (2001)).<sup>8</sup>

We include several robustness checks to ensure that our results are not simply capturing a differential trend between small and large firms. First, we detect no differential pre-trends in any of our specifications or in the universe of Chilean firms of the same size. Second, a placebo test on firms unaffected by the Agreement does not replicate our main results. Third, our results continue to hold in a specification with time-varying firm fixed effects, where we identify off of differential exposure to the Agreement by product type within Treated firms. In this specification, the likelihood of observing trade is lower for products that compete mostly with firms unaffected by the Agreement. Because the effects vary across products within each Treated firm, they cannot be driven only by a differential exit rate of smaller relative to larger firms.

Many relational contracting models (e.g., Baker et al. (2002)) predict that relationships are more resilient (i.e., can be more easily sustained by the threat of termination) when they are more exclusive, in the sense that the outside options of the parties are low. Consistent with this idea, we find that the negative effects of the Agreement on the likelihood of observing trade are significantly mitigated for suppliers that have more exclusive relationships with the Superstore, that is, suppliers that sell mostly to the Superstore, and for suppliers that have a large product market share (both of which we measure using pre-reform data).<sup>9</sup> In these cases the relationship is valuable for the supplier and the Superstore, respectively (as in McMillan and Woodruff (1999) and Giannetti et al. (2011)).

Finally, we argue that, in our setting, the evidence is most consistent with models of trade credit in which suppliers use long-maturity terms to guarantee product quality (Smith

(1987), Long et al. (1993), and Kim and Shin (2012)). Under such a model, shortening the maturity of trade credit contracts should have the largest effects for goods that require more than 30 days to verify quality or that require the supplier to take more costly actions. The effects should also be mitigated when the supplier can factor its receivables and receive payment close to the time of delivery. We use our detailed product-level data to test these predictions and find that the effects of the Agreement are strongest for durable products<sup>10</sup>, for non perishable products, and for firms that did not have access to factoring. Quality-driven theories also help explain why suppliers with more exclusive relationships may be able to overcome the inability to enter into long-term trade credit contracts, as these relational contracts provide sufficient incentives to produce a high-quality product.

Our paper is related to the literature on contracting and the boundaries of the firm. Related empirical papers include Baker and Hubbard (2004), who study how the introduction of a monitoring technology influences the decision to vertically integrate, Fresard et al. (2014) and Seru (2014), who link vertical integration to innovation, and Chen et al. (2013), who investigate organizational form in the mutual fund industry. Acemoglu et al. (2009) and Macchiavello (2012) examine the relationship between the contracting environment and vertical integration in a large cross-section of firms in different countries. Consistent with our results, they find that vertical integration is less likely when small firms are better able to extend longer payment terms.

Our work is also closely related to two recent empirical studies of the real effects of trade credit. Barrot (2015) examines the effects of a similar regulation that reduced the maturity of trade credit extended by French trucking firms. The author finds that the regulation *increased* entry in the sector and *decreased* the probability of bankruptcy, especially among small, financially constrained suppliers. Similarly, Murfin and Njoroge (2015) show that financially constrained firms reduce investment when forced to extend longer maturity trade credit. In contrast, in our setting the buyer adjusts to a restriction in the maturity of trade

credit on several margins, including quantity procured and vertical integration, leading to a *reduction* in trade with small firms.

These stark differences in policy outcomes likely stem from differences in the industrial organization of the trucking and discount retail industries and from the relative costs of vertical integration.<sup>11</sup> The demand for French trucking services is highly competitive and largely inelastic (Barrot (2015)), while the Chilean discount retail sector is highly concentrated with more elastic demand. Given these differences, the responses to the French and Chilean policies occur largely on different margins. Taken together, these results imply that when the suppliers of trade credit do not possess a financial advantage, trade credit has both costs and benefits. Any welfare analysis of a policy change that targets trade credit must consider both.

We continue with a description of the data and the empirical setting in Section I. We study the effects of the Agreement on trade with external suppliers in Section II. Section III studies the effects of the Agreement on the Superstore’s propensity to vertically integrate and on total procurement. Section IV shows how relational contracts may overcome the inability to extend long payment terms. Section V discusses evidence in support of trade credit as an incentive mechanism. Section VI concludes.

## I. Empirical Setting

### A. *The Discount Retail Industry in Chile*

This paper documents the importance of financial contracting in sustaining trading relationships between buyers and suppliers and in determining the boundary of the firm. To do so, we focus on the Chilean discount retail industry, which has many characteristics in common with the U.S. and other global markets. Market power is concentrated in the hands of a few large firms, those firms procure products from suppliers across the firm

size distribution, and the retailers frequently demand long payment terms from their often small suppliers (Wilson and Summers (2003), Murfin and Njoroge (2015)). Further, the store formats are similar (large superstores), the retailers market their own credit cards and payment systems, and one of the two dominant players in Chile was recently acquired by Walmart. In both markets, small firms frequently complain that the large retailers exert their relative strength to extract as much surplus as possible.

In our analysis, we focus on the procurement decisions of one of the two dominant retailers in Chile, the Superstore. Through a series of aggressive acquisitions and organic growth, these two large retailers accounted for 63% of total industry revenues in 2006.<sup>12</sup> Thus, changes in the procurement decisions of either of these firms are likely to have a large impact on its suppliers, especially on small firms.

### *B. The Agreement*

Given the prevalent view that the large retailers were exerting monopsonistic power over their smaller suppliers, in 2006 the Chilean government’s pro-competition agency (Fiscalia Nacional Economica or “FNE”) investigated their business practices. In August of that year, the agency issued a report that articulated these concerns and prompted the two large retail chains to modify the terms of their contracts with small suppliers.<sup>13</sup> At the time, it was standard practice for the two large buyers to demand trade credit terms of 90 days from their small suppliers, a symptom, the agency feared, of monopsonistic market power.<sup>14</sup> The agency entered into separate negotiations with each firm and announced that it would deny regulatory approval for any new acquisitions until modifications were enacted. Both chains agreed to modifications to their contracting practices, the Superstore in December 2006 and its large competitor in July 2008. The Superstore implemented this change beginning in January 2007.

Under the Agreement, the Superstore could not enter into trade credit contracts with a

maturity greater than 30 days with its small suppliers. Because the standard procurement contract prior to the Agreement stipulated 90 days payable, this represents a shortening in the maturity of these contracts of up to 60 days.

The agency used the following criteria to determine which firms would be categorized as small and thus fall under the purview of the Agreement:

1. total sales to all clients over the last 12 months of no more than UF 100,000, and
2. total sales to the Superstore over the last 12 months of no more than UF 60,000.

The Agreement had wide-ranging applicability: 67% of the Superstore's suppliers in 2006 (by number) satisfied both of these criteria and were subject to the Agreement.<sup>15</sup> Throughout we refer to the set of firms satisfying these two criteria as *affected* firms.

### *B.1. Determination of the Cutoffs and Contemporaneous Legislation*

Given the regulator's concern about asymmetric market power, the explicit aim of the Agreement was to empower smaller suppliers. The regulator did not think that large suppliers (e.g., Nestlè or CocaCola) needed its protection and therefore chose to implement the trade credit restriction selectively.

Nominally, the specification of the cutoffs was the result of negotiation between the regulator and the Superstore. The total sales cutoff (criterion 1) was chosen by the regulator, while the Superstore had some discretion regarding the UF 60,000 cutoff involving its own purchases (criterion 2). While it might appear that by giving the Superstore this discretion the cutoff might have been chosen strategically, two pieces of evidence suggest that this is unlikely to be the case. First, the total sales criterion stipulated by the regulator fully determined eligibility for all but three firms (i.e., three suppliers sold less than UF 100,000 in total but more than UF 60,000 to the Superstore and therefore were not affected by the Agreement). Second, there is no bunching of firms around the UF 60,000 cutoff of sales to the Superstore.<sup>16</sup>



The UF 100,000 total sales cutoff coincides with the threshold used by the Chilean government to define a firm to be a medium-sized enterprise.<sup>17</sup> This cutoff plays a central role in our identification strategy, so it is important that other factors were not differentially affecting medium-sized firms at the time when the Agreement was put in place.

We searched the legal archive of the Chilean Library of Congress for legislation passed between 2000 and 2012 that may have differentially favored SMEs (denoted PyMEs, in Spanish).<sup>18</sup> We could find no national policies in place at the time of the Agreement that directly favored SMEs at or below the UF 100,000 cutoff vis-à-vis larger firms. The flat income tax rate of 20% as well as the VAT rate of 19% applied across the firm size distribution. We did find a few pieces of legislation passed during this period that spoke to a generally favorable policy stance toward SMEs. For example, in 2001 a law was passed making it easier to register a microenterprise (cutoff of UF 2,400), and in 2007, the government simplified the process by which very small firms (again, firms well below UF 25,000 in sales) determined their taxable income. In July 2009, a government loan guarantee program was introduced that affected all firms in our sample equally.

Two laws passed after 2006 did affect firms with revenues below UF 100,000 differentially. First, in late 2008 the government gave SMEs a small and transitory tax credit on fixed investments through the end of 2011. Further, in early 2010 the government passed a law (“Ley 20.416”) that created a national SME advisory council. The specific goal of this council was to advise the Minister of Economics in all matters related to SMEs. The law also included other specific provisions, for example, one to facilitate the relationship between micro-enterprises (i.e., firms well below UF 25,000 in sales) and their suppliers and one to accelerate the administrative tasks related to the creation and dissolution of SMEs. In general, the explicit goal of this law was to create a specific and favorable institutional setting for SMEs without affecting private transactions of SMEs with their clients. In terms of the effects of these institutional changes on the relationship between the Superstore and

its medium-sized suppliers, if anything the favorable political climate should have helped SMEs thrive and hence should obscure any negative consequences of the Agreement on firms that were directly affected by it. In a robustness test, in Internet Appendix Table IAIII, we show that the firm-level effects of the Agreement took effect quite quickly. The impact is even detectable by March of 2007, well before these two laws were passed.

One might be concerned that other economic trends may have differentially impacted firms below and above the cutoff during the study period. Figure 1 shows trends in the universe of Chilean firms based on levels of sales just below (“Treated”) and above (“Control”) the UF 100,000 cutoff. We define a firm’s treatment status using intervals of total revenues—UF 25,000 to UF 100,000 for Treated firms and UF 100,000 to UF 600,000 for Control firms. This definition is consistent with the way we define Treated and Control firms for our empirical strategy, which we present below in Section II. The figure shows that there were similar changes in the number of firms, number of employees, total sales, and total wage bills for both groups of firms in Chile between 2005 and 2006. After the Agreement went into effect, there are no large jumps in the level of either curve. This suggests that there were no other contemporaneous trends that could lead to differences between Treated and Control firms, including other regulatory changes. The figure further suggests that the effects of the Agreement were not large enough to affect aggregates at the country level.<sup>19</sup>

[Insert Figure 1 around here]

## *B.2. Manipulation*

In all of our empirical specifications, we assign exposure to the policy change using pre-Agreement levels of sales (i.e., as of 2006). Our empirical strategy would be invalidated if firms were able to manipulate their total revenues or their revenues to the Superstore in order to fall above or below the threshold.<sup>20</sup> However, the institutional setting and the timing of the Agreement makes this possibility very unlikely. Each year, eligibility is determined

by the revenues filed with the Chilean tax authority. Given that Chile uses a VAT system, any manipulation would require costly collusion between the supplier and its buyers. This is because any taxable revenues of a supplier are also reported as tax deductible expenses by the buyer. Further, revenues are reported to the tax authority on a monthly basis, so the announcement of the Agreement on December 15, 2006 gave firms very little room to maneuver (in particular, VAT forms were due on December 12 for paper forms and on December 20 for online forms). Lastly, we note that in our intent-to-treat framework, if firms were endogenously able to expand their revenues to pass the cutoff and avoid regulation *in subsequent years*, then we would have a harder time detecting any effects of the Agreement.

### B.3. Enforcement

The Chilean government has actively monitored the Agreement’s implementation since it was put in place. The Superstore filed with the FNE to make at least four acquisitions between 2007 and 2010, giving it strong incentives to comply with the terms of the Agreement.<sup>21</sup> Private discussions with FNE personnel along with publicly available reports confirm that the Superstore has complied with the shorter payment period for affected firms. Indeed, the FNE explicitly conditioned approval of the Superstore’s acquisitions on compliance with the terms of the Agreement. Further, based on our conversations with management, the Superstore has explicitly avoided any actions that could be construed as forcing suppliers to extend longer days payable.

Finally, we collect data from the Superstore’s publicly available financial statements to study whether the reduction in days payable is noticeable at the Superstore level. In Internet Appendix Figure IA2 we plot the Superstore’s and its main competitor’s end of year accounts payable divided by yearly revenues and by yearly cost of goods sold from 2005 to 2008.<sup>22</sup> The data show a slight but noticeable decrease in the Superstore’s accounts payable.<sup>23</sup> This small effect is consistent with the fact that suppliers that were covered by the Agreement and

continued purchasing from the Superstore were paid earlier, but these purchases represent a small fraction of total procurement in the pre-period. It is also consistent with our main results, which suggest that the Superstore responded to the Agreement by trading less with suppliers that had to be paid earlier.

### *C. Data*

We obtain from the Superstore a proprietary data set that summarizes all the transactions with its suppliers, including subsidiaries, between January 2006 and August 2011, and that contains observations at the supplier-product-month level. The data do not contain days payable for each transaction or other terms of the trade credit contracts. Further, we do not observe the balance sheets of the suppliers. Hence, we are not able to test directly the first stage of the Agreement at the firm-product level.<sup>24</sup> Using each firm’s individual tax ID, we match our data to IRS records to obtain information on treatment status. The IRS data allow us to determine whether firms were affected by the Agreement as per the restriction on total revenues to all customers. We defer analysis of selected summary statistics to after we’ve introduced our empirical strategy in Section II.

### *D. Margins of Adjustment to the Agreement*

We focus our analysis on three margins of adjustment available to the Superstore in response to the Agreement: (i) trade with external suppliers, (ii) vertical integration, and (iii) reduced overall trade. Further, we explore how the value of the relationship (relational contracts), measured by the exclusivity of the relationship for both parties in the pre-period, interacts with the ability to extend trade credit.<sup>25</sup>

## II. Trade with External Firms

We first document how the Agreement affected the terms of trade between the Superstore and affected suppliers. We focus on whether firms were able to maintain their trading relationships with the Superstore. Our empirical strategy compares firms with 2006 sales just below and just above the threshold, before and after the Agreement was enacted.<sup>26</sup> Identification therefore requires that the outcomes of both groups of firms would have evolved in a parallel fashion in its absence. To make the two groups of firms as comparable as possible, we limit our analysis to firms falling in a relatively narrow range around the UF 100,000 total revenues cutoff. In particular, we limit our “main sample” to firms with 2006 revenues between UF 25,000 and UF 600,000 (roughly \$1.0 million to \$24 million). This choice of interval is driven by the categorization of total revenues provided to us by the IRS. We define Treated firms (i.e.,  $Treated_i = 1$ ) as those with total 2006 revenues between UF 25,000 and UF 100,000 (\$1.0 million to \$4.0 million) and Control firms (i.e.,  $Treated_i = 0$ ) as those with total 2006 revenues between UF 100,000 and UF 600,000 (\$4.0 million and \$24 million).<sup>27</sup>

As we discuss in section I.B.2, the sample of firms regulated by the Agreement likely changed after 2006, perhaps endogenously. In particular, firms may have tried to expand (shrink) their revenues in order to avoid (fall under) the Agreement’s jurisdiction. We therefore define our sample of Treated and Control firms based on predetermined 2006 revenues. We further explore the parallel trends assumption below.

Table I presents descriptive statistics for our main sample of suppliers in 2006, before the Agreement. The sample includes 734 firms, 342 Treated and 392 Control. Panel A reports annual statistics at the firm level. The median firm in the sample had one department, the broadest product categorization used by the Superstore, and sold 6.5 product categories on average. The table also reports summary statistics separately for Treated and Control firms, and confirms that by construction Control firms are larger than Treated firms—their

revenues are higher. However, the median Control firm sold only half a product more than the median Treated firm in 2006. About one in four suppliers had access to factoring at some point in the sample, with the difference between Treated and Control firms not statistically significant.

[Insert Table I around here]

Table I Panel B presents sample statistics at the firm-product level for our sample of firms in 2006. The table shows that, not surprisingly, Treated firms sell less (in \$ and units) of their products than Control firms. However, the average prices paid by the Superstore and the Superstore’s margin on products sold to final customers are similar across both groups (and, based on simple hypothesis tests, not statistically different).

The general picture that emerges from the above statistics is that while Treated and Control firms do differ in size, they are similar across other key product-level dimensions such as number and type of products sold, price, margin, and access to factoring of receivables.

To examine changes in the margin of trade with external suppliers, we run regressions at the firm×product×year level. The chief outcome of interest,  $Trade_{i,j,t}$ , is defined as whether firm  $i$  sells at least one unit of product  $j$  in year  $t$ , that is,  $Trade_{i,j,t} = 1 (Units_{i,j,t} > 0)$ . The resulting difference-in-differences regression specification is

$$Trade_{i,j,t} = \omega_{i,j} + \omega_{j,t} + \beta Post_t \times Treated_i + \varepsilon_{i,j,t}. \quad (1)$$

The coefficient of interest,  $\beta$ , measures the causal effect of the Agreement on whether the Superstore procures more from Treated suppliers relative to Control suppliers, after the Agreement is put in place relative to the pre-period.

One might worry that firms of different size sell different product mixes, in which case any differential effect may be explained by heterogeneous trends across different products. To remove this composition effect, we include firm×product ( $\omega_{i,j}$ ) and product×time ( $\omega_{j,t}$ )

fixed effects in all tests based on regression (1). The treatment effects are therefore identified using within-product variation that compares the same product sold by both Treated and Control firms before and after the Agreement. The  $\omega_{i,j}$  fixed effects absorb the baseline  $Treated_i$  effect, while the  $\omega_{j,t}$  fixed effects absorb the year fixed effects as well as the  $Post_t$  variable. As a result, we do not explicitly include these variables in the model.<sup>28</sup> We estimate regression (1) using all firm-product pairs that were sold at least once during 2006, and we include observations between 2007 and 2009 for the post-period. Our results are robust to alternative definitions of the post-period, including restricting it to only one year after the Agreement (i.e., 2007, see Internet Appendix Table IAIII).

### A. Graphical Evidence

The identification assumption for equation (1) is that, in the absence of the Agreement, the probability of selling a given product would have evolved in parallel for Treated and Control firms. We provide evidence that supports this assumption in Figure 2. The figure plots the quarterly average of the main outcome variable,  $Trade_{i,j,t}$ , which is a dummy that equals one if the product was sold during that period. The figure is detrended with one common linear trend across Treated and Control firms for ease of visualization. We find no noticeable differences in the trends of the probability of making a sale for Treated and Control firms during 2006, before the Agreement was put in place.<sup>29</sup> Further, we find no differential pre-trends unconditionally. We note that the identification assumption that we make in our regressions is weaker, as it only requires that the pre-trends not differ conditional on the product  $\times$  time and firm  $\times$  product fixed effects.

[Insert Figure 2 around here]

The graph also hints at our first result: after 2006, Treated firms exhibit a lower probability of procuring to the Superstore. Importantly, other than the time trend, the graph does not

control for differences in the product mix or in other dimensions between Treated and Control firms, and as such suggests a causal effect of the Agreement.

## B. Results

Column 1 of Table II reports causal effects on the main outcome *Trade* (for brevity we omit subscripts). Consistent with the graphical evidence, the coefficient on the interaction  $Post \times Treated$  shows that Treated firms are approximately 11% less likely to sell any given product to the Superstore following the Agreement. Thus, the Superstore chooses to shift purchases away from suppliers when the number of days payable is capped at thirty.

[Insert Table II around here]

We note that this effect corresponds to the change in the probability that a Treated supplier procures to the Superstore *relative* to the same change for Control firms. It is likely that the Agreement had an effect on Control firms as well, as the Superstore may have chosen to shift more of its procurement to them. Thus, our coefficient captures both the reduction in trade by Treated suppliers and the increase in trade by Control suppliers. Below we exploit a within-firm estimator to provide a lower bound on the absolute effect of the Agreement on the probability of trade by Treated suppliers.

Our focus is on the effects of the availability of contracting levers on the extensive margin of trade between firms. However, suppliers could also adjust through other margins, namely, prices. Column 2 of Table II shows that procurement prices decrease by 3.8% for Treated firms relative to Control firms selling the same product. Note that we only observe the price of transactions that actually take place, so this regression is run on a selected sample. We believe that most plausible sources of bias would underestimate the size of the effect. For example, if firms become unprofitable below a threshold price causing them to exit the market, then the latent prices that we do not observe by running the selected regressions



should be even lower. On the surface the magnitude of the price change appears to be larger than a reasonable 60-day interest rate for external financing for the Superstore. For example, the 3.8% price reduction is equivalent to an annualized interest rate of 23% from the point of view of the Superstore. In comparison, the Chilean banking sector’s reported yearly rates for the same period are 7% to 11%.<sup>30</sup> However, this discount is comparable to typical estimates of the “cost of trade credit” in the U.S. based on early payment discounts (e.g., Petersen and Rajan (1997) and Cuñat and Garcia-Appendini (2012)).

We combine the evidence on the extensive margin and on prices to examine effects on  $\log(\text{revenues})$ . To include the effect of observations with zero units sold, we replace zero revenues with one peso (roughly 0.2 cents), the lowest monetary unit in Chile. The results, presented in column 3, confirm a large and significant decrease in product-level revenues. Given that the Superstore is one of the largest clients of small suppliers in Chile, it is likely that this large decrease in revenues with the Superstore also led to a decrease in total revenues. Finally, in column 4 we show that there is no measurable effect of the Agreement at the intensive margin (i.e., conditional on a sale occurring) of sales, using the natural logarithm of units sold as the outcome.

### *C. Robustness*

To provide further support for our identification assumption, we present a placebo test in columns 4 to 6 of Table II. Our “placebo” sample is composed entirely of firms whose 2006 revenues are above the UF 100,000 cutoff and thus were not directly 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 (\$24 million) are labeled as Treated-placebo, while firms above that threshold are Control-placebo (this placebo sample thus includes firms with total revenues above UF 100,000 (\$4.0 million)). The placebo sample has 389 Treated-placebo firms and 230 Control-placebo firms. This split and sample selection assures

that the placebo test has a similar level of power as our main regression specifications.<sup>31</sup>

We find that the coefficient on *Trade* (column 5) is slightly negative but not significantly different from zero. Even though the large standard errors on this estimate do not allow us to reject the null that the coefficient is equivalent to our main specification, we interpret this as evidence that relatively smaller firms do not naturally reduce the incidence of procurement to the Superstore after 2007. We find similar results on prices and revenues in the placebo sample. The placebo analysis as a whole suggests that our results are not mechanically driven by the difference in size between suppliers that were affected and suppliers that were unaffected by the Agreement.

One might still worry that the smaller Treated firms targeted by the Agreement may differ from the larger Control firms in a time-varying fashion. For example, there may be other concurrent policy changes or differential firm survival rates right around the treatment cutoff size (although the graphs for the universe of firms of this size shown in Figure 1 suggest this is not the case). We thus propose one additional robustness check that allows us to control for *time-varying* firm fixed effects, which removes any differential trends affecting Treated and Control firms differently.

We hypothesize that if the Agreement affected the Superstore’s likelihood of purchasing from an external supplier, its effects on Treated firms should be more pronounced for those products for which all suppliers that were unaffected by it had a higher market share. That is, the likelihood that a Treated firm loses orders from the Superstore after the Agreement should be higher if the firm’s competitors were largely unaffected firms. We test this hypothesis by estimating the following regression on the sample of Treated firms, as defined above, that sold to the Superstore during 2006:

$$y_{i,j,t} = \beta Post_t \times Exposure_j + \omega_{i,t} + \omega_{i,j} + \epsilon_{i,j,t}. \quad (2)$$

We define *Exposure<sub>j</sub>* as the share of total procurement of product *j* sold by firms affected

by the Agreement. The coefficient on  $\beta$  represents the average effect of the Agreement depending on whether the firm’s competitors were mostly affected by the Agreement. Under our hypothesis,  $\beta < 0$ . The firm $\times$ time fixed effects  $\omega_{i,t}$  absorb any differential trend of (slightly) smaller versus (slightly) larger Treated firms. The baseline effect for products with higher exposure ( $Exposure_j$ ) is absorbed by the  $\omega_{i,j}$  fixed effects.

The results are presented in Table III. Column 1 documents that *within* Treated firms, the effect of the Agreement on the propensity to procure to the Superstore is lower for products that compete mostly with other firms affected by the Agreement. This suggests that our results are not simply capturing heterogeneous survival probabilities for firms of different size. The within-firm estimator is also a lower bound on the absolute effect of the Agreement on the probability that Treated suppliers sell to the Superstore, which complements the relative estimates obtained using the difference-in-differences in our main results. Column 2 of Table III shows a similar although not statistically significant effect on prices, while Column 3 reveals a positive and significant effect on revenues. Note that to estimate this effect, we only identify off of those firms selling both a low- and a high-exposure product, limiting power substantially. Columns 4 through 6 run the same tests but replace  $Exposure_j$  with the dummy  $Higheposure_j$ , which equals one for products for which the fraction of procurement from affected firms is higher than the cross-sectional mean among Treated firms (40%).<sup>32</sup> The results are essentially unchanged. These results suggest that the causal effect of the Agreement presented in Table II is not likely to be driven by time-varying differences among firms of heterogeneous size.

[Insert Table III around here]

### III. Vertical Integration and Reduced Procurement

To estimate the effects of the Agreement on the Superstore’s decision to vertically integrate, we define  $Subsidiary_{j,t}$  as a dummy variable that equals one if the Superstore procured good  $j$  from a subsidiary in period  $t$ , and we collapse our data at the product  $\times$  year level.

To test for vertical integration, we run the following regression:

$$Subsidiary_{j,t} = \omega_j + \omega_t + \beta Post_t \times Exposure_j + \epsilon_{j,t}. \quad (3)$$

In this specification, we compare products that were affected differentially by the Agreement before and after 2006. As in the previous section, we define  $Exposure$  as the share of total procurement of product  $j$  that was sold by firms affected by the Agreement. The baseline effect on products with higher  $Exposure_j$  is absorbed by the  $\omega_j$  fixed effects. In our main specification, we use the variation in exposure to the Agreement just below and just above the threshold by restricting the sample to only those products that were sold by at least one Treated and one Control firm (that is, firms with sales from UF 25,000 to UF 100,000 and UF 100,000 to UF 600,000, respectively).<sup>33</sup>

The coefficient of interest in regression (3),  $\beta$ , measures the difference in the probability that the Superstore is its own supplier—that is, is vertically integrated—for products with high and low exposure to the Agreement, before and after the Agreement was in place. The identification assumption is that, in the absence of the Agreement, the fraction of products for which the Superstore is its own supplier would have evolved in parallel for products with varying degrees of exposure.

[Insert Figure 3 around here]

The patterns in Figure 3 are consistent with this assumption. Splitting the sample of products by the median 2006 market share of affected firms suggests no differential pre-trends. Further, the figure also hints at our second result: after the Agreement, the

relative incidence of *Subsidiary* (i.e., sourcing from a subsidiary) seems to increase for more exposed products, that is, products sold mostly by firms affected by the Agreement relative to products sold mostly by firms not affected by it.

Note that our definition of vertical integration is a functional one. We test whether after the Agreement the Superstore performs an action, in this case to supply a product, that was previously performed by external suppliers. In the context of the discount retail industry, vertical integration could result in skipping an intermediary or distributor, or importing products directly. In turn, this could be the result of the Superstore acquiring other firms or of organic expansion of the Superstore’s existing subsidiaries. We cannot test whether this vertical integration is indeed the result of more acquisitions because of data limitations. In particular, acquisitions by the Superstore of any of the suppliers in our sample are not public, most likely because their scale deems them “not material” for reporting or regulatory (e.g., anticompetitive) purposes.

This same regression model (equation (3)) also allows us to measure the third margin of adjustment, reduction in total trade. Here we test whether, after the Agreement is in place, the Superstore reduces the volume of trade in those products that were most exposed to the Agreement. We define our outcome measure as the total number of units sold by all suppliers,  $Totalprocurement_{j,t} = \sum_i Units_{i,j,t}$ . To facilitate comparison across products, we normalize this variable by its mean and standard deviation. We include all years in our data set to account for any (potentially slow-moving) decision to vertically integrate. Our results are qualitatively robust to restricting the length of the post period, although statistical significance is lost for some specifications.

## A. Results

The empirical tests based on equation (3), which study the margins of vertical integration and total procurement in response to the Agreement, are presented in Table IV. Column

1 presents the regression results when the outcome is an indicator of a purchase from an internal subsidiary. The positive coefficient suggests that when faced with the restriction in days payable, the Superstore does indeed choose to procure via internal subsidiaries rather than from some Treated firms. To better interpret the economic magnitude of this effect, we divide the products into “high Treated share” and “low Treated share” (as in the pre-trends graph) based on the mean market share of Treated firms across sample products in the pre-period (24%). We run the same regression as in equation (3) but replace  $Exposure_j$  with  $Highexposure_j$ , which equals one if the product has an exposure above the mean.<sup>34</sup> The results of this regression are reported in column 2 of Table IV, and show that the Superstore is roughly four percentage points (from a pre-period average of 21%) more likely to shift procurement to an internal subsidiary for products that were mostly sold by Treated firms before the Agreement.

[Insert Table IV around here]

Next, we study whether total procurement was differentially affected for products more exposed to the Agreement. In column 3 of Table IV we report the regression results when the outcome is  $Unitsprocured_{j,t}$ , the sum of all units of product  $j$  that were purchased by the Superstore. We find that after the Agreement, the overall level of procurement (standardized by the mean and standard deviation) falls for those goods that had previously been supplied mostly by Treated firms. We repeat the regression but change the interaction variable to  $Highexposure_j$  as defined above. The results reported in column 4 of Table IV, suggest that products in which firms affected by the Agreement have a market share above the mean experience a reduction in procurement of 4% of a standard deviation. One interpretation of this result is that the Superstore must pay a cost either to vertically integrate or to shift purchases to non affected suppliers. This cost results in a reduction in the total number of units purchased. Thus, the firm is unable to replicate the market outcomes and settles with a second-best outcome, consistent with Baker et al. (2001).

We further explore whether the reduction in units procured represents an efficiency loss by studying the effect of the Agreement on product-level Superstore gross profits. We construct *Grossprofit* as average revenues to final customers minus purchases from suppliers for each product-year. The results of using *Grossprofit* as the outcome variable in regression model (3) are reported in Table IV, columns 5 and 6, and suggest that the Superstore’s gross profits are significantly lower for products in which affected suppliers have a higher market share, after the Agreement is passed relative to the pre-period. This finding suggests that the Superstore is not necessarily choosing to expand into the supply of relatively more profitable products, but rather that some of the costs of adjustment to the Agreement are borne by the Superstore in the form of lower profits.

## IV. Exclusive Relationships and the Effects of the Agreement

The evidence presented so far suggests that the availability of long-maturity trade credit enables trade with external suppliers. Further, when the maturity of trade credit is restricted, firms can respond by adjusting procurement and vertically integrating. Baker et al. (2002) argue that relational contracts, which are prevalent along supply chains, may help parties overcome difficulties in formal contracting. We explore in our setting whether relational contracts may indeed substitute for the availability of long-maturity trade credit contracts.

We follow the relational contracting literature (Baker et al. (2002) and McMillan and Woodruff (1999)) and posit that relationships should be more resilient when the outside options of either the supplier or the Superstore are low. In such cases, the relationships are more exclusive and termination is more costly. Formally, we use our data at the firm $\times$ product $\times$ year level and augment regression (1) with the interaction variable *Exclusivity<sub>i,j</sub>*, which varies at the firm  $i$  and product  $j$  level:

$$\begin{aligned}
Trade_{i,j,t} = & \beta Post_t \times Treated_i \\
& + \gamma Post_t \times Treated_i \times Exclusivity_{i,j} \\
& + \delta Post_t \times Exclusivity_{i,j} + \omega_{j,t} + \omega_{i,j} + \epsilon_{i,j,t}.
\end{aligned} \tag{4}$$

The coefficients of interest are  $\beta$ , which measures the baseline effect of the Agreement when relationships are not exclusive, and  $\gamma$ , which measures the differential effects of the Agreement on relationships that are more exclusive. The  $\omega_{i,j}$  fixed effects absorb all interactions of the  $Treated_i$  and  $Exclusivity_{i,j}$  variables, while the  $\omega_{j,t}$  absorb all year fixed effects and the  $Post_t$  variable.

We use two measures of  $Exclusivity_{i,j}$ . First, we define a measure from the point of view of the supplier that captures the relative importance of the Superstore among its clients and proxies for the outside option of the supplier. Because we do not observe total revenues, we cannot calculate the Superstore's share of purchases for each supplier. However, our IRS data broadly categorizes firms into two buckets of total 2006 revenues, UF 25,000 to UF 100,000 and UF 100,000 to UF 600,000. Thus, within each bucket, 2006 sales to the Superstore represent a measure of sales concentration. For our main sample of firms, the interaction variable  $Exclusivity_{i,j}$  is defined as a dummy that equals one if supplier  $i$ 's 2006 sales to the Superstore are higher than the median within Treated and Control firms, respectively. Second, we define a measure of exclusivity that captures the relative importance of each supplier for a given product and proxies for the outside option of the Superstore. We calculate each firm-product's market share of 2006 sales to the Superstore. We then define  $Exclusivity_{i,j}$  as the pre-period market share for product  $j$  and firm  $i$ . A positive value on the coefficient  $\gamma$  using both definitions of  $Exclusivity_{i,j}$  would indicate that when relationships are more exclusive, trade depends less on the ability to enter into long-maturity trade credit contracts. Of course, a positive value of  $\gamma$  might also indicate that suppliers



with more exclusive relationships with the Superstore already benefit from shorter payment terms ex ante (e.g., Antras and Foley (2014)). In that case, the acceleration of payments should have no effect.

Table V presents the coefficients  $\beta$  and  $\gamma$  from regression (4) using both measures of exclusivity. Columns 1 and 2 report the results using sales concentration as the interaction variable in regression (4), while columns 3 and 4 use market share as the interaction variable. We find that the effects of the Agreement are largely offset for suppliers with more exclusive relationships with the Superstore. The interactions are positive and significant in columns 1 and 3, using the likelihood of making a sale as the dependent variable. The results are similar for revenues, though the coefficients are not statistically significant. Further, in all four columns, we cannot reject the hypothesis that the total effect ( $\beta + \gamma$ ) for more exclusive relationships is different from zero. These results demonstrate that while the average Treated firm is likely to lose business with the Superstore after the Agreement, some Treated firms are able to maintain (or increase) their trading relationships. That not all small firms experienced reduced trade may help explain why the Agreement was able to persist.

[Insert Table V around here]

## V. Trade Credit as an Incentive Mechanism

At its core, the use of trade credit is a financing decision, and under the assumptions of Modigliani and Miller (1958), its use should have no real impact on a buyer’s trading relationships. That we find such strong effects of the Agreement implies a departure from the Modigliani and Miller world. In this section, we use our detailed data to explore why access to trade credit alters trade between buyers and suppliers. The finance literature has proposed three key theories for why trade credit might be valuable: first, trade credit as intermediation; second, trade credit as a means of exploiting asymmetric bargaining power;

and third, trade credit as an incentive mechanism. We discuss each theory through the lens of our specific empirical setting and show that our results are most consistent with trade credit as an incentive mechanism.

In a wide range of settings, trade credit plays a valuable intermediation role. As mentioned in the introduction, this occurs when the supplier can provide funds to the buyer more cheaply than the credit market. Such an intermediation advantage may arise due to information revealed through the trading relationship, through the repeated nature of the trading relationship, or through the supplier having a higher valuation of the buyer’s collateral than the outside market (e.g., see Mian and Smith (1992)). However, in settings such as ours where the supplier is much more financially constrained than the buyer, intermediation-based theories do not have much explanatory power.

A second theory holds that trade credit is a manifestation of the asymmetric bargaining power held by buyers over small suppliers (Wilner (2000), Fabbri and Klapper (2008)). Under this theory, the buyer demands trade credit as a way to extract more of the surplus from the trading relationship and thus increase its total profits. An April 2015 article in the New York Times describes long payment terms demanded of small suppliers as “an illustration of the power imbalance with their big customers” (Strom, Stephonie, “Big companies pay later, squeezing their suppliers”, *The New York Times* April 6, 2015). The Chilean regulator had a similar rationale in mind when drafting the Agreement, and intuitively, bargaining power seems like a reasonable theory to explain the use of trade credit in this setting.

However, as Petersen et al. (2013) highlight, trade credit is an inefficient way for firms to extract surplus from small, financially constrained suppliers.<sup>35</sup> The buyer can increase its own profits and keep the supplier’s profits constant simply by paying a lower procurement price in the spot market and borrowing directly from the credit market at a lower interest rate. Thus, it is hard to explain the prevalence of trade credit borrowing by large firms with the simplest model of bargaining power. Some researchers have argued that the bargaining power theory

is still valid, but only when firms are not able to engage in price discrimination.<sup>36</sup> In Table II we show that, in response to the Agreement, the Superstore does indeed lower prices paid to Treated firms relative to Control firms selling the same product. Our results in Section IV also provide evidence that is inconsistent with the bargaining power hypothesis. In Table V, we find similar mitigating effects when either the supplier has a low outside option (low supplier bargaining position) or the buyer has a low outside option (high supplier bargaining position). Such symmetric effects would not be predicted in a model of asymmetric bargaining power. Taken together, it is hard to reconcile our results with bargaining power theories.

Third, trade credit may provide incentives for suppliers. By withholding payment for 90 days, buyers can use that time to verify actions taken by the supplier that may be unobservable at the time of delivery (Kim and Shin (2012) and Long et al. (1993)).<sup>37</sup> We provide a simple model in the spirit of Kim and Shin (2012) in Internet Appendix Section III to illustrate how access to trade credit may be efficiency-enhancing. We note that in such a model, quality is a stand-in for a range of unobservable actions including physical product quality, market research, demand forecasting, and relationship-specific investments.

Surely, buyers can provide incentives to suppliers through other channels aside from trade credit. One way is through relational contracts and the threat of terminating the trading relationship (Baker et al. (2002)). Indeed, in Section IV, we show that when trading relationships are more resilient, the negative effects of the Agreement such relationships are less pronounced. However, when it is harder to write a relational contract, the effects of restricting trade credit are especially large. This is consistent with trade credit being important for incentives in those cases in which relational contracts are not sufficient to provide incentives.<sup>38</sup>

Under a quality-based model of trade credit, the Agreement should have larger effects on the purchases of products that require more than 30 days to verify quality or that require the supplier to take more costly actions. We hypothesize that trade credit should be least

valuable for perishable food items. Given the short product shelf life of perishables, the buyer should be able to assess quality in fewer than 30 days. Conversely, we hypothesize that trade credit is most valuable for durable products, which are purchased less frequently by consumers, tend to be more differentiated across suppliers, and have a longer product life. Thus, under a quality-based model, we predict that the effects of the Agreement are smaller for perishable products and larger for consumer durables.

We use our detailed product data to test these predictions. We focus attention on attributes that can be objectively measured. We categorize a food product as perishable if the production information in our data set contains the word “fresh” or “perishable” or if it is sold by the deli counter or fresh baked goods department. We define a nonfood product line to be a durable product if it does not belong to one of the 119 product groups that Nielsen tracks in its consumer purchases panel.<sup>39</sup> Nielsen focuses on products that are purchased regularly and frequently by consumers and explicitly avoids durable goods such as furniture or electronics. Examples of goods included in the Nielsen panel and sold by the Superstore are printer ink, batteries, and small kitchen supplies. In contrast, computers, kitchen tables, and baby clothing, which are sold by the Superstore, are excluded from the panel.

In Panel A of Table VI, we estimate heterogeneous effects regressions to test whether the Agreement differentially affected the likelihood of trade for perishable or durable goods. We focus on heterogeneous effects for the nonexclusive relationships, which in Table V we show are the ones most affected by the Agreement. In particular, we restrict the sample to nonexclusive relationships as defined in Section IV and we estimate a version of the heterogeneous effects regression (4) in which we include an interaction term,  $Interaction_j$ , for perishable or durable goods:

$$Trade_{i,j,t} = \beta Post_t \times Treated_i + \delta Post_t \times Treated_i \times Interaction_j + \omega_{j,t} + \omega_{i,j} + \epsilon_{i,j,t}. \quad (5)$$

The  $\omega_{j,t}$  fixed effects absorb the  $Post_t \times Interaction_j$  interaction, while the  $\omega_{i,j}$  fixed effects absorb all interactions of  $Treated_i$  and  $Interaction_j$ . We report the coefficients  $\beta$  and  $\delta$  for both definitions of the interaction (perishability and durability). We restrict the perishable and durable regressions to only food and nonfood products, respectively. In columns 1 and 2 we define exclusivity using sales concentration, and in columns 4 and 5 we define exclusivity using market share. We find that the extensive margin trading effects are much larger in magnitude for durable products relative to nondurable products in both columns 2 and 5. Further, we detect no effects of the Agreement for non-durable goods, though the standard errors are quite large in both specifications. We also find evidence that the negative effects of the Agreement are largely mitigated for perishable goods. The differential effect is positive and statistically significant in column 1, but loses significance in column 4.

[Insert Table VI around here]

In Panel B of Table VI, we estimate heterogeneous effects regressions to test whether the Agreement caused differential vertical integration for perishable or durable goods:<sup>40</sup>

$$\begin{aligned} Subsidiary_{j,t} = & \omega_j + \omega_t + \beta Post_t \times Highexposure_j \\ & + \gamma Post_t \times Highexposure_j \times Interaction_j + \omega_t \times Interaction_j + \epsilon_{j,t}. \end{aligned} \tag{6}$$

In general, we find little evidence of a vertical integration effect on food products, both perishable and nonperishable. However, among nonfood products, the effect for durables is quite large and is statistically significant at the 10% level.

Finally, we can observe which firms had access to factoring during the sample period.<sup>41</sup> Here, by making sure suppliers receive cash upfront, factoring unwinds any incentive effect of trade credit. We thus hypothesize that incentive problems should be most severe for firms that cannot factor their receivables. We test this conjecture by running regression (5) after replacing the interaction variable with a dummy that indicates whether firms factored

their receivables. Because the indicator variable  $Factoring_i$  is defined at the firm level, we include the interaction  $Post_t \times Factoring_i$  in the regression. We find evidence consistent with our hypothesis in columns 3 and 6 of Table VI, which report the results of regression (5) when the sample is restricted to non-exclusive relationships defined by sales concentration in column 3 and market share in column 6. Indeed, suppliers that factored their receivables were significantly less affected by the inability to extend long-maturity trade credit under both definitions of exclusivity (columns 3 and 6).

In summary, we find that the effects of the Agreement are less pronounced for perishable food products, for nonfood products that are not durable, and for firms that are able to factor their receivables. While these dimensions of heterogeneity may be correlated with other unobserved product characteristics, we believe that, taken together, our results are most consistent with existing quality-based theories. That is, in the absence of an intermediation advantage, long-maturity trade credit may provide suppliers incentives to perform an action that can only be observed by the Superstore with some delay. Under such information asymmetries, the inability to enforce long-maturity trade credit contracts may result in lower prices and in the termination of trading relationships. These effects are less pronounced for more exclusive relationships, as relational contracts may substitute for other formal contracts such as trade credit. Further, when unable to demand long payment terms, the Superstore may prefer to vertically integrate and bring procurement in-house, which is costly and may result in an efficiency loss as evidenced by the reduced level of procurement.

## VI. Conclusion

We analyze the effects of a policy change that restricted the set of contracts that a large conglomerate and its small suppliers could write with one another. In particular, the policy restricted the maturity of trade credit contracts to no more than 30 days. In this setting, trade credit is likely not a reflection of a relative financing advantage of suppliers (providers of

credit) relative to the conglomerate (the debtor). We document three margins of adjustment to this restriction. First, we show that the large conglomerate shifts procurement away from suppliers affected by the restriction. Second, we show that the Superstore chooses to vertically integrate and become its own supplier in products mostly procured by firms affected by the policy change. Third, we show that total procurement volume is reduced. Thus, the ability to extend long-maturity trade credit enables trade and directly affects the organizational form of the conglomerate and its supply chain. Our evidence suggests that in this setting where suppliers do not have a financial advantage, trade credit may act as a bond to guarantee that suppliers make unobserved investments that directly affect the value of the product (e.g., quality).

The results highlight a particular channel through which the extension of trade credit may be beneficial for firms: it enables trade. In its absence, firms may prefer to vertically integrate or to decrease their demand for certain products. However, previous authors document in other settings that long trade credit contracts may be costly to extend (Murfin and Njoroge (2015), Barrot (2015)). Thus, any welfare analysis of the effects of a regulation that limits the terms of trade credit terms must consider both its costs and benefits. An interesting avenue for future work is to estimate how these costs and benefits vary depending, for example, on local financial conditions and on the industrial organization of the supply chain.

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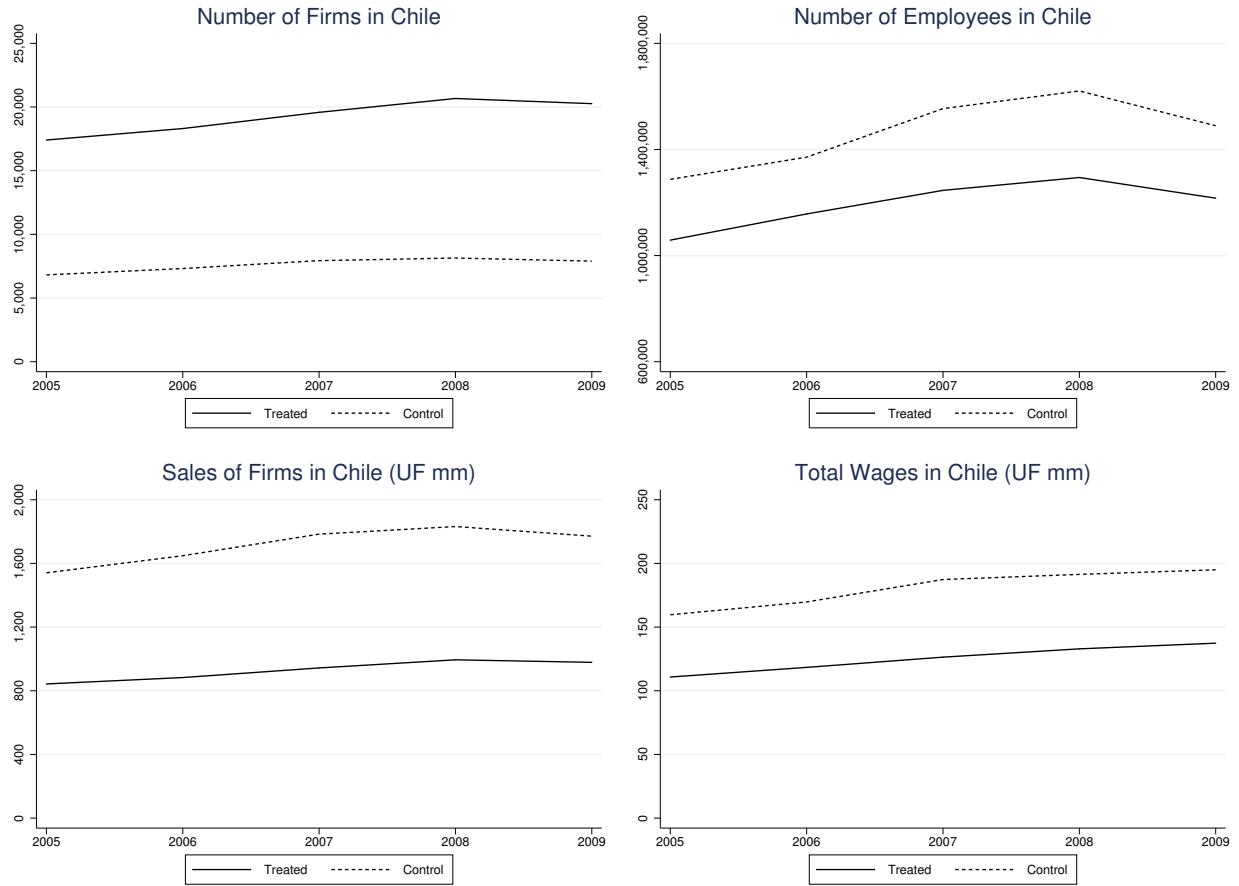
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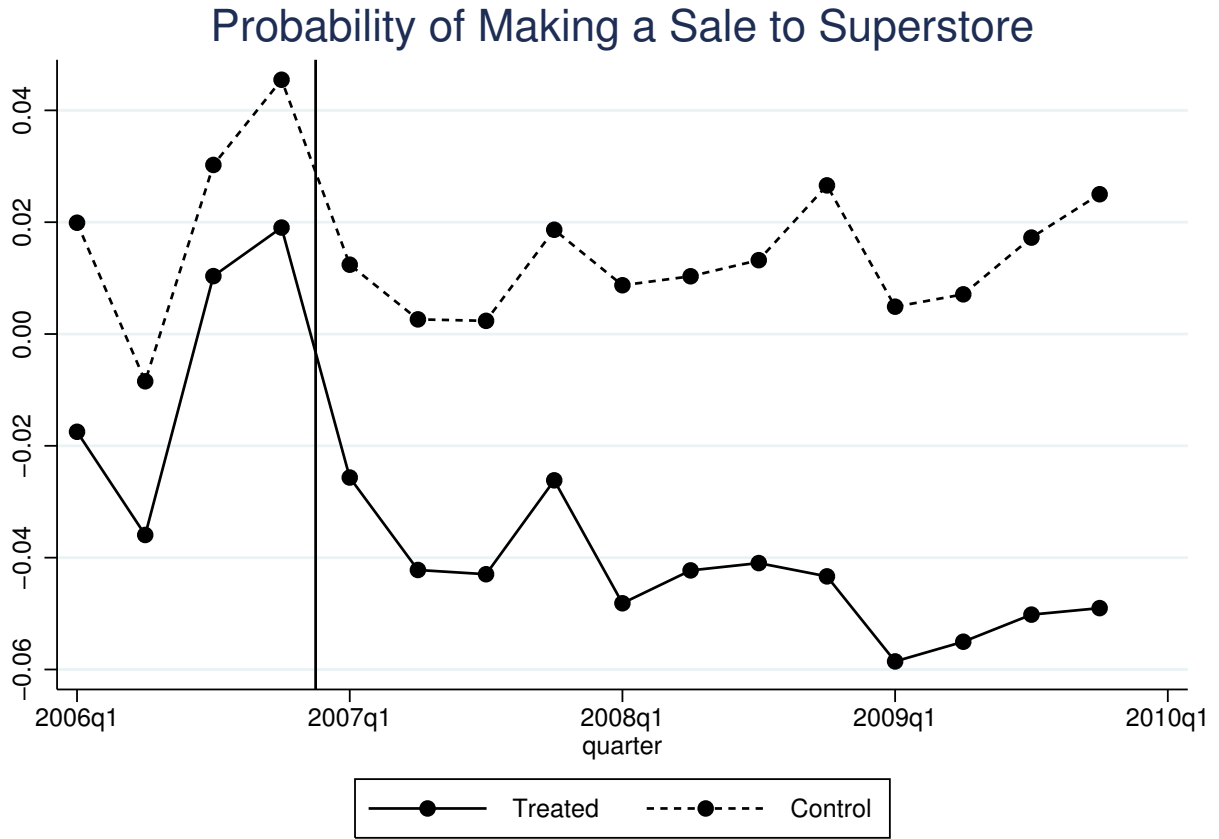
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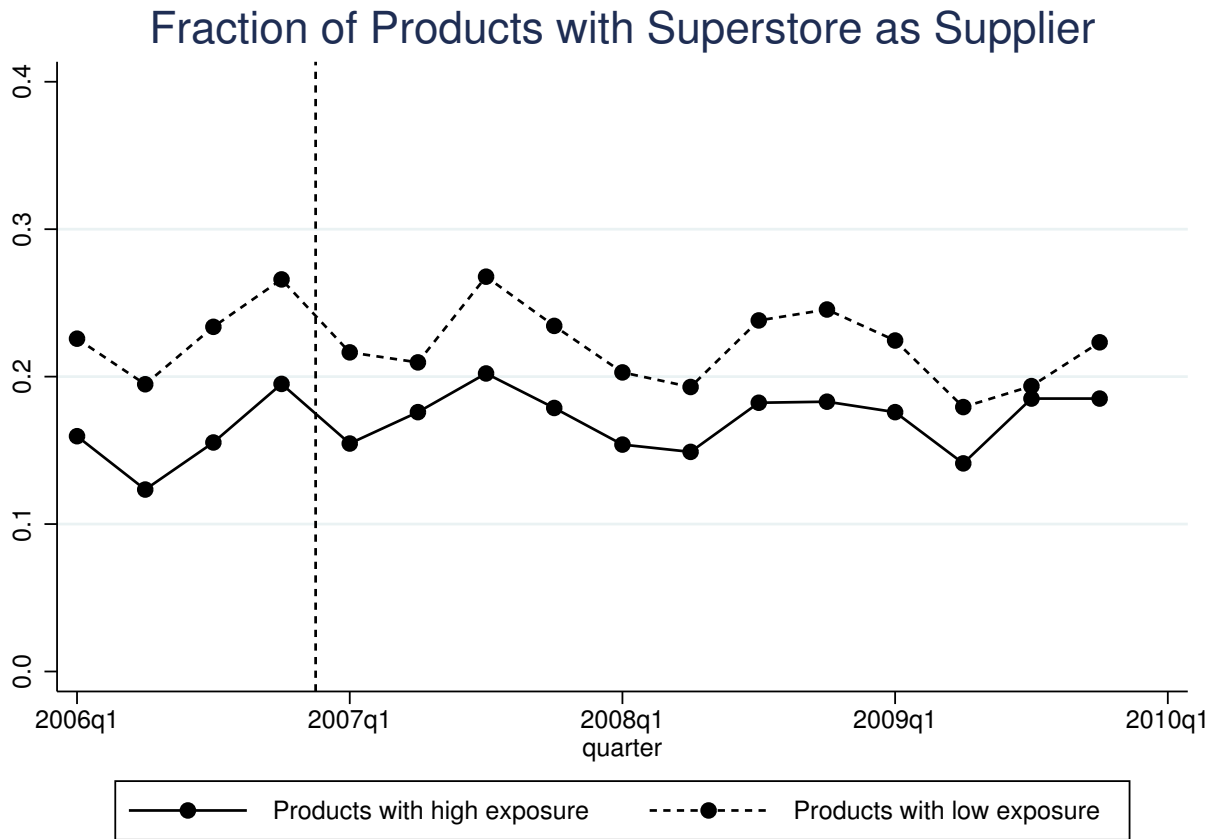
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**Figure 1. Trends for universe of Chilean firms.** This figure shows yearly trends for the universe of Chilean firms with sales equal to Treated and Control firms. The graphs plots the total number of firms, total number of employees, total sales, and total wages paid from 2005 to 2011. Treated firms are those with total yearly revenues between UF 25,000 and UF 100,000, Control firms are those with total yearly revenues between UF 100,000 and UF 600,000. The data are publicly available and come from the website of the Chilean IRS ([www.sii.cl](http://www.sii.cl)).



**Figure 2. Graphical evidence: making a sale.** This figure shows that there is no difference in the pre-period trends of the propensity to make a sale during 2006 for products sold by Treated and Control firms. The graph plots the (detrended mean) of “makes sale” at the quarterly level for Treated and Control firms. Treated firms are those with total 2006 revenues between UF 25,000 and UF 100,000 and 2006 sales to the Superstore below UF 60,000. Control firms are those with total 2006 revenues between UF 100,000 and UF 600,000 or 2006 sales to the Superstore above UF 60,000.



**Figure 3. Graphical evidence: vertical integration.** This figure shows the pre- and post-Agreement trends in the quarterly average fraction of products for which the Superstore was its own supplier. The sample of products is restricted to those products sold by firms whose 2006 revenues were between UF 25,000 and UF 600,000. The dashed (solid) line corresponds to products for which affected firms had a market share below (above) the cross sectional median.

**Table I****Summary Statistics: Main Sample**

This table shows the mean, standard deviation and median of variables measured in the pre period for Treated and Control firms. Treated firms are those with total 2006 revenues between UF 25,000 and UF 100,000 and 2006 sales to the Superstore below UF 60,000, and Control firms are those with total 2006 revenues between UF 100,000 and UF 600,000 or 2006 sales to the Superstore above UF 60,000. We restrict the sample to those firms with total 2006 revenues between UF 25,000 and UF 600,000. Panel A reports results for firm-level variables, while Panel B reports results for product-firm-level variables. \* indicates that the difference in the mean of Treated and Control groups is statistically different from zero at the 10% level.

Panel A: Firm-level average monthly variables									
	All (N=734)			Treated (N=342)			Control (N=392)		
	Mean	p50	St. Dev.	Mean	p50	St. Dev.	Mean	p50	St. Dev.
log(Revenues)*	18.11	18.59	2.37	17.51	18.02	2.29	18.64	19.18	2.32
# Departments*	1.54	1.00	0.92	1.44	1.00	0.65	1.63	1.00	1.09
# Products*	6.52	3.00	11.10	4.84	3.00	5.60	7.98	3.50	14.11
factoring (%)	23.98	0.00	42.72	24.85	0.00	43.28	23.21	0.00	42.27

Panel B: Product-firm-level 2006 monthly average									
	All (N=4,784)			Treated (N=1,656)			Control (N=3,128)		
	Mean	p50	St. Dev.	Mean	p50	St. Dev.	Mean	p50	St. Dev.
log(Price)	6.98	6.91	1.57	6.94	6.96	1.76	7.00	6.89	1.46
log(Units)*	6.30	6.33	2.56	5.91	6.02	2.52	6.51	6.51	2.57
log(Revenues)*	13.28	13.59	2.43	12.86	13.18	2.46	13.51	13.78	2.38
Mark-up(%)	31.93	29.84	15.46	32.74	31.82	15.62	31.50	29.32	15.36
Supplier market share (%)	12.85	2.56	22.99	10.96	1.74	21.34	13.85	3.30	23.75

Table II

**The Effect of the Reduction in Days Payable on Firm-Product Outcomes**

This table shows the effect of the restriction on trade credit contracts on firm-product level outcomes. The table presents the estimated coefficient of interest  $\beta$  for the regression

$$Outcome_{i,j,t} = \omega_{i,j} + \omega_{j,t} + \beta Post_t \times Treated_i + \varepsilon_{i,j,t},$$

which measures the relative change in the outcome of a product sold to the Superstore 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 UF 100,000 and total 2006 sales to the Superstore below UF 60,000. UF The main sample corresponds to firms with total 2006 revenues between UF 25,000 and UF 600,000. We exclude products that were not sold during 2006. The placebo sample consists of firms with total 2006 revenues of UF 100,000 or higher; within this sample, Treated-placebo firms ( $Treated_{placebo} = 1$ ) are those with 2006 revenues of UF 600,000 or lower. The outcomes are *Trade*, a dummy that equals one if a sale is recorded during the period (pre- or post-Agreement);  $\log(Price)$ , natural logarithm of the transfer price;  $\log(Revenues + 1)$ , the natural logarithm of monthly product sales to the Superstore in pesos, with zeros replaced with the log of one peso; and  $\log(Units)$ , the natural logarithm of the number of monthly product units sold to the Superstore. The data set is a panel at the product-firm-year level.  $Post = 0$  corresponds to 2006 and  $Post = 1$  corresponds to 2007, 2008, and 2009. Standard errors are clustered at the firm level. \*, \*\*, and \*\*\* indicate significance at the 10%, 5%, and 1% level, respectively.

Panel A. Main sample				
	(1)	(2)	(3)	(4)
Dependent variable	<i>Trade</i>	$\log(Price)$	$\log(Revenues + 1)$	$\log(Units)$
$Post \times Treated$	-0.1086*** (0.034)	-0.0381* (0.022)	-1.0346*** (0.361)	-0.0078 (0.083)
$R^2$	0.750	0.990	0.807	0.949
Obs.	19,136	13,825	19,136	13,825
Firms	734	734	734	734
Panel B. Placebo sample				
	(5)	(6)	(7)	(8)
Dependent variable	<i>Trade</i>	$\log(Price)$	$\log(Revenues + 1)$	$\log(Units)$
$Post \times Treated_{placebo}$	-0.0097 (0.085)	0.0036 (0.017)	0.2012 (1.322)	-0.0331 (0.088)
$R^2$	0.764	0.988	0.823	0.949
Obs.	26,124	19,327	26,124	19,327
Firms	619	619	619	619



Table III

**Robustness: Regressions Controlling for Differential Firm-Level Trends**

This table reports the differential effect of the Agreement 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 as the fraction of 2006 sales to the Superstore made by affected firms. The table presents the estimated coefficient  $\beta$  for the regression

$$Outcome_{i,j,t} = \beta Post_t \times Exposure_j + \omega_{i,t} + \omega_{i,j} + \epsilon_{i,j,t},$$

where  $Exposure_j$  is defined as the 2006 product market share of affected firms, which are those with total 2006 revenues below UF 100,000 and total 2006 sales to the Superstore below UF 60,000. We restrict the sample to Treated firms as defined in the text (i.e., with total 2006 revenues above UF 25,000). The outcomes are *Trade*, a dummy that equals one if a sale is recorded during the period (pre- or post-Agreement);  $\log(Price)$ , the natural logarithm of the transfer price; and  $\log(Revenues + 1)$ , the natural logarithm of monthly product sales to the Superstore in pesos, with zeros replaced with one peso. Columns 4, 5, and 6 replace the interaction variable  $Exposure_j$  with  $Highexposure_j$ , a dummy that equals one if the 2006 product market share of affected firms is higher than the cross-sectional average market share among Treated firms (0.4). The data set is a panel at the product $\times$ year level.  $Post = 0$  corresponds to 2006 and  $Post = 1$  corresponds to 2007, 2008, and 2009. Standard errors are clustered at the firm level. \*, \*\*, and \*\*\* indicate significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	<i>Trade</i>	$\log(Price)$	$\log(Revenues + 1)$	<i>Trade</i>	$\log(Price)$	$\log(Revenues + 1)$
<i>Post</i> $\times$ <i>Exposure</i>	0.1089** (0.043)	0.0922 (0.058)	1.0194* (0.557)			
<i>Post</i> $\times$ <i>Highexposure</i>				0.0843*** (0.026)	0.0732* (0.044)	0.7720** (0.348)
$R^2$	0.813	0.992	0.854	0.817	0.993	0.857
Obs.	6,624	4,461	6,624	6,624	4,461	6,624
Firms	342	342	342	342	342	342

Table IV

**Vertical Integration and Total Procurement**

This table shows two margins of adjustment to the Agreement, vertical integration by the Superstore and reduced total procurement, as well as the change in average product-level Superstore profits. The table presents the estimated coefficient of interest  $\beta$  for the regression

$$Outcome_{j,t} = \beta Post_t \times Exposure_j + \omega_j + \omega_t + \epsilon_{j,t},$$

which measures the relative change in the outcome for products with varying exposure to the Agreement, measured as the fraction of 2006 sales to the Superstore made by affected firms, before and after the reduction in days payable. Affected firms are those with total 2006 revenues below UF 100,000 and total 2006 sales to the Superstore below UF 60,000. We restrict the sample to firms with total 2006 revenues between UF 25,000 and UF 600,000. We further restrict the sample to products sold by at least one Treated firm and one Control firm. The outcomes are *Subsidiary*, the incidence of procurement from a Superstore subsidiary; *Unitsprocured*, the overall number of units procured of good  $j$  in month  $t$ , standardized by the sample mean and standard deviation; and *Grossprofits*, the product-level average Superstore gross profits, defined as sales to final customers minus purchases from suppliers. In columns 2, 4, and 6 we replace the interaction variable  $Exposure_j$  with  $Highexposure_j$ , a dummy that equals one if the 2006 product market share of affected firms is higher than the cross-sectional average market share across products in the sample (0.24). The data set is a panel at the product $\times$ year level.  $Post = 0$  corresponds to 2006 and  $Post = 1$  corresponds to years 2007 to 2011. Standard errors are clustered at the product level. \*, \*\*, and \*\*\* indicate significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	<i>Subsidiary</i>	<i>Subsidiary</i>	<i>Unitsprocured</i>	<i>Unitsprocured</i>	<i>Grossprofits</i>	<i>Grossprofits</i>
$Post \times Exposure$	0.0436** (0.022)		-0.0877*** (0.033)		-137.53** (55.06)	
$Post \times Highexposure$		0.0388*** (0.013)		-0.0398** (0.017)		-73.67** (33.21)
$R^2$	0.853	0.854	0.969	0.969	0.947	0.947
Obs.	4,914	4,914	4,914	4,914	4,914	4,914
Products	819	819	819	819	819	819

Table V

**Exclusivity**

This table shows that the effects of the restriction to the contracting space are less pronounced when trading relationships are valuable. The table reports whether the estimated effects of the change in days payable from the regression

$$Outcome_{i,j,t} = \beta Post_t \times Treated_i + \gamma Post_t \times Treated_i \times Exclusivity_{i,j} + \delta Post_t \times Exclusivity + \omega_{j,t} + \omega_{i,j} + \epsilon_{i,j,t}$$

using the extensive margin outcome (*Trade*) and the supplier revenues outcome ( $\log(Revenues + 1)$ ) vary with 1) whether the supplier's total sales to the Superstore during 2006 are more than the median by treatment status–(Concentration) (columns 1 and 2), and 2) the supplier's product market share (columns 3-4), both measures of *Exclusivity*. Treated firms are those with total 2006 revenues below UF 100,000 and total 2006 sales to the Superstore below UF 60,000. The sample corresponds to firms with total 2006 revenues between UF 25,000 and UF 600,000. The data set is a panel at the product-firm-year level. *Post* = 0 corresponds to 2006 and *Post* = 1 corresponds to 2007, 2008, and 2009. Standard errors are clustered at the firm level. \*, \*\*, and \*\*\* indicate significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)
Interaction var.	Concentration		Market share	
Dependent var.	<i>Trade</i>	$\log(Revenues + 1)$	<i>Trade</i>	$\log(Revenues + 1)$
<i>Post</i> × <i>Treated</i>	-0.1504*** (0.048)	-1.5664*** (0.603)	-0.0979*** (0.038)	-1.0627** (0.475)
<i>Post</i> × <i>Treated</i> × <i>Exclusivity</i>	0.1127* (0.067)	1.0482 (0.854)	0.2686* (0.162)	1.4589 (2.068)
Sum of coefficients	-0.0377	-0.5182	0.1707	0.3962
<i>p</i> -value of sum	0.4104	0.3841	0.2567	0.8379
$R^2$	0.755	0.809	0.757	0.8089
Obs.	19,136	19,136	19,136	19,136
Firms	734	734	734	734
Mean interaction	0.4695		0.1285	

Table VI

## Heterogeneous Effects

This table shows that, among nonexclusive relationships, the effects of the Agreement were strongest for nonperishable products, for durable goods, and for firms that did not have access to factoring, consistent with trade credit serving as a guarantee of product quality. Panel A reports results of regression

$$Trade_{i,j,t} = \beta Post_t \times Treated_i + \delta Post_t \times Treated_i \times Interaction_j + \omega_{j,t} + \omega_{i,j} + \epsilon_{i,j,t},$$

where we restrict the sample to nonexclusive relationships with the Superstore. *Exclusivity* is defined by *Concentration*, whether the supplier's total sales to the Superstore in 2006 are more than the median by treatment status (columns 1 to 3), and by *Market share*, whether the firm's 2006 average product market share is higher than the mean (13%) (columns 4 to 6). The interaction variables are *Perishable*, defined as products whose names contain the words "fresh" or "perishable", deli products, and bread and bakery products (43% of all food products sold in 2006); *Durable*, defined as a dummy for products not included in the Nielsen database (42% of all nonfood products sold in 2006); and *Factoring*, defined as a dummy for whether the supplier ever factored its accounts receivable. *Perishable* is defined only for products in food departments: General Food, Meat and Fish, Deli, Fruits and Vegetables, and Bread and Bakery, and we define *Durable* only for nonfood products. Panel B reports results for the regression

$$Subsidiary_{j,t} = \omega_j + \omega_t + \beta Post_t \times Highexposure_j + \gamma Post_t \times Highexposure_j \times Interaction_j + \omega_t \times Interaction_j + \epsilon_{j,t},$$

where the interactions are *Perishable* and *Durable*. Treated firms are those with total 2006 revenues below UF 100,000 and total 2006 sales to the Superstore below UF 60,000. The sample corresponds to firms with total 2006 revenues between UF 25,000 and UF 600,000. In Panel A, the data are at the product×firm×year level. In panel B, the data are at the product×year level. We drop products included in Department 14 (Business Procurement for the Superstore, for example, cleaning services) and 15 (Catering for Internal Operations) of the Superstore data, which are hard to match to the Nielsen data and thus to define as perishable. Post corresponds to the years 2007 to 2009. Standard errors are clustered at the firm level. \*, \*\*, and \*\*\* indicate significance at the 10%, 5%, and 1% level, respectively.

Panel A. Probability of trade at the firm-product level						
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	<i>Trade</i>					
Exclusivity	Concentration			Market share		
Interaction variable	<i>Perishable</i>	<i>Durable</i>	<i>Factoring</i>	<i>Perishable</i>	<i>Durable</i>	<i>Factoring</i>
<i>Post</i> × <i>Treated</i>	-0.3163*** (0.102)	-0.0061 (0.097)	-0.1941*** (0.063)	-0.1609** (0.064)	0.0369 (0.072)	-0.1430*** (0.044)
<i>Post</i> × <i>Treated</i> × <i>Interaction</i>	0.2333* (0.138)	-0.2833** (0.132)	0.1886** (0.081)	0.0775 (0.084)	-0.2123** (0.101)	0.1862*** (0.056)
Sample	Food	Not food	All	Food	Not food	All
$R^2$	0.788	0.838	0.816	0.729	0.789	0.761
Obs.	3,884	4,520	8,404	7,688	5,696	13,384
Firms	252	210	451	382	255	616

Panel B. Vertical integration at the product level			
	(1)	(2)	
Dependent variable	<i>Subsidiary</i>		
Interaction variable	<i>Perishable</i>	<i>Durable</i>	
<i>Post</i> × <i>Highexposure</i>	-0.0004 (0.047)	0.0117 (0.015)	
<i>Post</i> × <i>Highexposure</i> × <i>Interaction</i>	0.0430 (0.053)	0.0439* (0.026)	
Sample	44	Food	Not Food
$R^2$	0.877	0.829	
Obs.	2,136	2,718	
Products	356	453	

# Notes

<sup>1</sup>See Bank for International Settlements (2014).

<sup>2</sup>See Petersen and Rajan (1997), Biais and Gollier (1997), Ng et al. (2002), Fisman and Raturi (2004), Fabbri and Klapper (2008), Cuñat and Garcia-Appendini (2012), Giannetti et al. (2011), Klapper et al. (2012), Costello (2014), Antras and Foley (2014), among others.

<sup>3</sup>See Wilson and Summers (2003), Fabbri and Klapper (2008), and Klapper et al. (2012). See also recent coverage in *The New York Times* (Strom, Stephonie, “Big companies pay later, squeezing their suppliers”, *The New York Times* April 6, 2015).

<sup>4</sup>The Superstore is orders of magnitude larger than the privately held suppliers in our sample, and has the ability to raise capital in the public market. In contrast, the suppliers in our sample are all privately held firms with annual sales between \$1 million and \$24 million and most likely face substantially higher borrowing costs than the buyer. Further, small firms in an emerging market like Chile are probably even more financially constrained than small firms in developed markets (e.g., Rajan and Zingales (1998), Banerjee and Duflo (2014)).

<sup>5</sup>The government struck a similar accord with the other large retailer in mid-2008.

<sup>6</sup>UF, which stands for “Unidad de Fomento”, is an inflation-linked currency unit updated daily. Its value is published by the Banco Central de Chile. 1 UF is worth roughly \$40.

<sup>7</sup>Due to data restrictions from the Chilean tax authority, we do not observe total revenues to all clients. It is therefore impossible to implement a fully nonparametric regression discontinuity design.

<sup>8</sup>Of course, it is also likely that the Superstore adjusts by shifting procurement to unaffected suppliers, including Control firms and also even larger suppliers.

<sup>9</sup>The results are also consistent with the fact that suppliers with more exclusive relationships with the Superstore might already benefit from shorter payment terms ex ante (e.g., Antras and Foley (2014)). In that case, the acceleration in payments should have no effect.

<sup>10</sup>We define durables as products sold by the Superstore that are not tracked in the Nielsen consumer panel data.

<sup>11</sup>Another difference between our setting and Barrot (2015) is the way the policies were implemented: the Chilean policy only affected the trade credit terms of small firms, while the French policy affected all firms equally.

<sup>12</sup>This information comes from the Chilean pro-competition agency website, <http://www.fne.cl>.

<sup>13</sup>See “Requerimiento contra Cencosud y D&S,” <http://www.fne.cl>.

<sup>14</sup>In contrast, many of the much larger supplier firms were able to negotiate shorter days payable. Note that these larger firms are not part of our empirical analysis.

<sup>15</sup>However, because the rule was targeted at the smallest firms, this corresponds to only 6.4% of suppliers’ sales to the Superstore in 2006.

<sup>16</sup>See Internet Appendix Figure IA1. The Internet Appendix is available in the online version of this article on The Journal of Finance website.

<sup>17</sup>Micro and small enterprises in Chile are those firms that have sales below UF 25,000.

<sup>18</sup>See [http://www.leychile.cl/Consulta/listado\\_n\\_sel?\\_grupo\\_aporte=&sub=843&agr=2&comp=](http://www.leychile.cl/Consulta/listado_n_sel?_grupo_aporte=&sub=843&agr=2&comp=).

<sup>19</sup>The number of Treated and Control firms in our sample corresponds to 1.9% and 5.3%, respectively, of the total universe of firms of the same size in Chile in 2006.

<sup>20</sup>As we show above, there is no evidence of bunching using the UF 60,000 cutoff for sales to the Superstore. We cannot test directly whether firms bunch on either side of the UF 100,000 total revenues cutoff due to lack of data.

<sup>21</sup>Source: <http://www.fne.cl>. Three of these acquisitions were small regional supermarkets. The fourth acquisition, in 2010, was a large distributor whose clients are mainly small local retailers.

<sup>22</sup>We present revenues as well as the more common cost of goods sold as the denominator, as the latter may be endogenously affected by the Agreement.

<sup>23</sup>The data also show that accounts payable decrease for the Superstore’s competitor in 2008 once it entered into a similar accord with the regulator.

<sup>24</sup>Recall, however, that we do observe a small decrease in the accounts payable of the Superstore. See Internet Appendix Figure IA2.

<sup>25</sup>We discuss additional margins of adjustment below, including changes in prices and shifts in procurement from affected to unaffected firms.

<sup>26</sup>Ideally, we would like to use total revenues in 2006 as the forcing variable in a regression discontinuity design. This is not possible due to data limitations. In particular, the Chilean IRS was not willing to provide us with the actual level of sales by any firm in any year, but instead shared with us the revenue range. These ranges are used for IRS reporting.

<sup>27</sup>We also code the three firms that sold less than UF 100,000 in total but more than UF 60,000 to the Superstore as Control firms. Results are unchanged if we modify the treatment status of these three firms or if we drop them.

<sup>28</sup>The full set of fixed effects is quite large. We use the methodology of Guimaraes and Portugal (2010) for regressions with two high-dimensional fixed effects, implemented using the REG2HDFE Stata command, as suggested by Gormley and Matsa (2014).

<sup>29</sup>This is also true statistically speaking. Table IAI in the Internet Appendix shows that the propensity to trade of Treated firms relative to Control firms only becomes significantly negative in 2007, after the Agreement is in place.

<sup>30</sup>Figure taken from “Tasa de Interés Corriente y Máxima Convencional,” in <http://www.sbif.cl>, for “Operaciones No Reajustables” for less than 90 days, as of January 1, 2007.

<sup>31</sup>Sample statistics for Treated-placebo and Control-placebo firms are available in Internet Appendix Table IAV.

<sup>32</sup>Results are quantitatively similar if we instead use the median to split the sample.

<sup>33</sup>In Internet Appendix Table IAVI we show that the results are unchanged if we use alternate sample restrictions.

<sup>34</sup>The results are quantitatively similar if instead we use the median to split the sample.

<sup>35</sup>Also see Schwartz (1974).

<sup>36</sup>For example, see Mian and Smith (1992) and the Robinson-Patman Act in the U.S.

<sup>37</sup>Also see Giannetti et al. (2011) for suggestive empirical evidence.

<sup>38</sup>We further develop this idea in the Internet Appendix.

<sup>39</sup>See <https://research.chicagobooth.edu/nielsen/>.

<sup>40</sup>Given that the resilience variables are defined at the supplier×product level, and the vertical integration regressions are run at the product level, we cannot focus only on less resilient relationships.

<sup>41</sup>We interpret the factoring results with caution as we only observe whether a firm had access to factoring at any time during our sample period. Ideally we would like to observe whether firms had access to factoring before the Agreement to avoid endogeneity concerns. For example, access to factoring may be correlated with financial health, which may indeed be correlated with the probability of continuing to supply to the Superstore. We do, however, find that access to factoring is balanced across Treated and Control firms in Table I.