FINANCIAL CONTRACTING AND ORGANIZATIONAL FORM: EVIDENCE FROM THE REGULATION OF TRADE CREDIT*

EMILY BREZA† AND ANDRES LIBERMAN‡

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 differences-in-differences identification strategy, we find that the restriction reduces the likelihood of trade by 11 percentage points. The large retailer also responds by internalizing procurement to its own subsidiaries and reducing overall purchases. Finally, we find evidence that relational contracts can help mitigate the inability to extend long trade credit terms.

Keywords: Financial Contracting, Trade Credit, Organizational Economics, Vertical Integration
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† Columbia University. Email: ebreza@gsb.columbia.edu.
‡ New York University: Email: aliberma@stern.nyu.edu.

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I. Introduction

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 (1975), Grossman and Hart (1986)). In particular, when contracts are not feasible or enforceable, vertical integration, i.e., an expansion in the vertical boundary of the firm, may replace arm's-length market 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 (2012); 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 prevalent financial contracts in procurement relationships, and estimates suggest that it finances roughly two-thirds of global trade.[1] A large literature examines the determinants of trade credit terms between buyers and suppliers.[2] First and foremost, trade credit is characterized as an efficient financing arrangement whereby credit flows from relatively unconstrained buyers to more financially constrained suppliers. Accordingly, 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

[1]See Bank for International Settlements (2014).

[2]See Petersen and Rajan (1997), Biais and Gollier (1997), Ng, Smith, and Smith (2002), Fisman and Raturi (2004), Fabbri and Klapper (2008), Cuñat and Garcia-Appendini (2011), Giannetti, Burkart, and Ellingsen (2011), Klapper, Laeven, and Rajan (2012), Costello (2014), Antras and Foley (2014), among others.

credit flowing from small, constrained suppliers to large corporations with access to international capital markets.[3] 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 one thousand of its small suppliers. In this specific context, it is unlikely that trade credit reflects a relative advantage of suppliers in external financing terms.[4] 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.[5] The government chose to impose the regulation only for firms with sales below an arbitrary cutoff of UF 100,000[6]–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

[3]See Wilson and Summers (2003), Fabbri and Klapper (2008), and Klapper, Laeven, and Rajan (2012). See also recent coverage in The New York Times (Strom (2015)).

[4]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 (2008)).

[5]The government struck a similar accord with the other large retailer in mid-2008.

[6]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.

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.[7] We find that the probability that a Treated supplier sells a product to the Superstore falls by 11 percentage points 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 percentage points 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, Gibbons, and Murphy (2001)).[8]

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, Gibbons, and Murphy (2002)) predict that relationships are more resilient (i.e., can be more easily sustained by

[7]Due to data restrictions from the Chilean tax authority, we do not observe total revenues to all clients. Thus, it is impossible to implement a fully non-parametric regression discontinuity design.
[8]Of course, it is also likely that the Superstore adjusts by shifting procurement to unaffected suppliers, including Control firms and also even larger suppliers.

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 suppliers that have a large product market share (both of which we measure using pre-reform data).[9] In these cases the relationship is valuable for the supplier and the Superstore, respectively (as in McMillan and Woodruff (1999) and Giannetti, Burkart, and Ellingsen (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, Malitz, and Ravid (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[10], non-perishable products, and for firms that did not have access to factoring. Quality-driven theories also help to 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, Hoberg, and Phillips (2014) and Seru (2014), who link vertical integration to innovation, and Chen, Hong, Jiang, and Kubik (2013) who investigate organizational

[9]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.

[10]We define durables as products sold by the Superstore that are not tracked in the Nielsen consumer panel data.

form in the mutual fund industry. Acemoglu, Johnson, and Mitton (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.[11] 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 II. We study the effects of the Agreement on trade with external suppliers in Section III. Section IV studies the effects of the Agreement on the Superstore's propensity to vertically integrate and on total procurement. Section V shows how relational

[11]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.

contracts may overcome the inability to extend long payment terms. Section VI discusses evidence in support of trade credit as an incentive mechanism. Section VII concludes.

II. 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 this, we focus on the Chilean discount retail industry, which shares many characteristics in common with the US 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 are able to 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.[12] Thus, changes in the procurement decisions of either of these firms are likely to have large impacts 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

[12]Information taken from Chilean pro-competition agency website, www.fne.cl.

(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.[13] 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.[14] 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 fall under the purview of the Agreement:

(1) Total sales to all clients in the last 12 months of no more than UF 100,000, and

(2) total sales to the Superstore in the last 12 months of no more than UF 60,000.

The Agreement had wide-ranging applicability: 67% of the Superstore's suppliers from 2006 (by number) satisfied both of the criteria and became subject to the Agreement.[15] Throughout we refer to the set of all firms satisfying these two criteria as *affected* firms.

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 the

[13]See "Requerimiento contra Cencosud y D&S", www.fne.cl.

[14]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.

[15]However, because the rule was targeted at the smallest firms, this corresponds to only 6.4% of 2006 sales of suppliers to the Superstore.

large suppliers (e.g., Nestlè or CocaCola) needed its protection, and therefore chose to implement selectively the trade credit restriction.

Nominally, the specification of the cutoffs was the result of a 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 were therefore not affected by the Agreement). Second, there is no bunching of firms around the UF 60,000 cutoff of sales to the Superstore.[16]

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.[17] 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).[18] We could find no national policies that were 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 that were 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

[16]See Supplemental Appendix Figure B.1.
[17]Micro and small enterprises in Chile are those firms that have sales below UF 25,000.
[18]See http://www.leychile.cl/Consulta/listado_n_sel?_grupo_aporte=&sub=843&agr=2&comp=.

taxable income. In July 2009, a government loan guarantee program was put in place that affected all firms in our sample equally.

Two laws were passed after 2006 that did affect differentially firms with revenues below UF 100,000. 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 put in place to help the relationship of micro-enterprises (i.e., firms well below UF 25,000 in sales) and their suppliers, and one designed 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 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 to thrive and should obscure any negative consequences of the Agreement on firms that were directly affected by it. As a robustness test, we show in the Supplemental Appendix Table B.3 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 III. The figure shows that there were similar changes in the number of firms, number of employees, total sales and total wage bills of both groups of firms in Chile between 2005 and 2006. After the Agreement is in place, 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. Also, the figure suggests that the effects of the Agreement were not large enough to affect aggregates at the country-level.[19]

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.[20] 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 impacts of the Agreement.

Enforcement

The Chilean government has actively monitored the Agreement's implementation since it was put in place. The Superstore did indeed file with the FNE to make at least four acquisitions between 2007 and 2010, giving it strong incentives to comply with the

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

[20]As we've shown 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.

terms of the Agreement.[21] Private discussions with FNE personnel along with publicly available reports confirm that the Superstore has indeed 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 the Supplemental Appendix Figure B.2 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.[22] The data show a slight but noticeable decrease in the Superstore's accounts payable.[23] This small effect is consistent with the fact that suppliers that were exposed to 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, by which the Superstore reacted to the Agreement by trading less with suppliers that had to be paid earlier.

C. Data

We obtain from the Superstore a proprietary dataset that summarizes all the transactions with its suppliers, including subsidiaries, between January 2006 and August 2011, and contains observations at the supplier-product-month level. The data does 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

[21]Source: 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.

[22]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.

[23]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.

test directly the first stage of the Agreement at the firm-product level.[24] Using each firm's individual tax ID, we match our data to IRS records to obtain information on treatment status. The IRS data allows us to determine whether firms were affected by the Agreement as per the restriction on total revenues to all customers. We defer an analysis of selected summary statistics to after we've introduced our empirical strategy in Section III.

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 how exclusive the relationship was for both parties in the pre-period, interacts with the ability to extend trade credit.[25]

III. 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.[26] Thus, identification requires that the outcomes of both groups of firms would have evolved in a parallel fashion in its absence. In order 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

[24]Recall however, that we do observe a small decrease in the accounts payable of the Superstore. See Supplemental Appendix Figure B.2.

[25]We discuss additional margins of adjustment below, including changes in prices and shifts in procurement from affected to unaffected firms.

[26]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. Indeed, 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.

600,000 (roughly $1.0 million to $24 million). The choice of this 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).[27]

As discussed in subsection II.B, the sample of firms regulated by the Agreement likely did change after 2006, perhaps endogenously. Firms may have tried to expand (shrink) their revenues in order to avoid (fall under) the Agreement's jurisdiction. Therefore, we define our sample of Treated and Control firms based on predetermined 2006 revenues. We further explore the parallel trends assumption below.

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. The median firm in the sample had 1 department, the broadest product categorization used by the Superstore, and sold 6.5 product categories on average. The table also shows the same statistics 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 then the median Treated firm during 2006. About one in four suppliers had access to factoring at some point in the sample, and the difference in this dimension between Treated and Control firms is not statistically significant.

Table 1 (Panel B) provides sample statistics at the firm-product level during 2006 for our sample of firms. The table shows unsurprisingly that Treated firms sell less (in $ and units) of each 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

[27]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. Results are unchanged if we modify the treatment status of these three firms or if we drop them.

different in both cases). The general picture that emerges from these 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, i.e., $trade_{i,j,t} = 1\left(units_{i,j,t} > 0\right)$. The resulting differences-in-differences regression specification is:

$$trade_{i,j,t} = \omega_{i,j} + \omega_{j,t} + \beta post_t \times treated_i + \varepsilon_{i,j,t}. \tag{1}$$

The coefficient of interest β 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 sizes sell different product mixes. Thus, 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). Therefore, the treatment effects are 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.[28] We estimate regression (1) using all product-firms 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 1 year after the Agreement (i.e., 2007. See Supplemental Appendix Table B.3).

[28]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).

A. Graphical evidence

The identification assumption for Equation (1) is that, in the absence of the Agreement, the probability of making a sale of the same product would have evolved in parallel for Treated and Control firms. We provide evidence that supports this assumption in Figure 2. The figure shows 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. There are 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.[29] Further, the graph shows that there are no differential pre-trends unconditionally. We note that the identification assumption we make in our regressions is weaker, as it only requires that the pre-trends do not differ conditional on the product times time and firm times product fixed effects.

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 any 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 2 reports causal effects on the main outcome *trade* (for brevity we omit indices of variables). Consistent with the graphical evidence, the coefficient on the interaction $post \times treated$ shows that Treated firms are approximately 11 percentage points less likely to sell any given product to the Superstore following the Agreement. Thus, the Superstore chooses to shift purchases away from suppliers when payment days are capped at thirty.

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

[29]This is also true statistically speaking. Table B.3 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.

is likely that the Agreement had an effect on Control firms as well, as the Superstore may have chosen to shift more 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 2 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 lead to an underestimate of 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. The magnitude of the price change appears on the surface 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%.[30] However, this discount is comparable to typical estimates of the "cost of trade credit" in the US based on early payment discounts (e.g., Petersen and Rajan (1997) and Cuñat and Garcia-Appendini (2011)).

We combine the evidence on the extensive margin and prices to examine effects on $log\,(revenues)$. 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. Results are presented in Column 3 and 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

[30]Figure 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.

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 4-6 of Table 2. Our "placebo" sample is composed entirely of firms whose 2006 revenues are above the UF 100,000 cutoff and were thus 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.[31]

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 differs from 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 test as a whole suggests our results are not mechanically driven by the difference in size between the suppliers that were affected and unaffected by the Agreement.

One might still worry that the smaller Treated firms targeted by the Agreement may be different 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 propose one additional robustness

[31]Sample statistics for Treated-placebo and Control-placebo firms are available in the Supplemental Appendix Table B.5.

check that allows us to control for *time-varying* firm fixed effects. Doing so 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 where 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 model on a sample restricted to Treated firms, as defined above, that sold to the Superstore during 2006,

$$y_{i,j,t} = \beta post_t \times exposure_j + \omega_{i,t} + \omega_{i,j} + \epsilon_{i,j,t}.$$

We define $exposure_j$ as the share of total procurement of product j that was sold by firms affected by the Agreement. The coefficient on β 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×time fixed effects $\omega_{i,t}$ absorb any differential trend of (slightly) smaller versus (slightly) larger Treated firms. The baseline effect for products with a higher exposure ($exposure_j$) is absorbed by the $\omega_{i,j}$ fixed effects.

The results are presented in Table 3. Column 1 documents that *within Treated firms*, the effect of the Agreement on the propensity to procure to the Superstore is mitigated 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 sizes. 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 diffs-in-diffs in our main results. Column 2 of Table 3 shows a similar although not statistically significant effect on prices, and 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 $highexposure_j$, which equals one for products where the fraction of procurement from affected firms is higher than the cross sectional mean among Treated firms (40%).[32] The results are essentially unchanged. These results suggest that the causal effect of the Agreement presented in Table 2 is not likely to be driven by time-varying differences among firms of heterogeneous sizes.

IV. Vertical integration and reduced procurement

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

To test for vertical integration we run the following regression:

$$subsidiary_{j,t} = \omega_j + \omega_t + \beta post_t \times exposure_j + \epsilon_{j,t}. \tag{2}$$

In this specification, we compare products that were affected differentially by the Agreement before and after 2006. As in the previous section, we define the variable *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 a higher $exposure_j$ is absorbed by the ω_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 - UF 100,000 and UF 100,000 - UF 600,000, respectively).[33]

The coefficient of interest β of regression (2) measures the difference in the probability that the Superstore is its own supplier–is vertically integrated–for products with high

[32]Results are quantitatively similar if instead we use the median to split the sample.

[33]In Supplemental Appendix Table (B.6) we show that results are unchanged if we use alternate sample restrictions.

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 where the Superstore is its own supplier would have evolved in parallel for products with varying degrees of exposure.

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, i.e., products sold mostly by firms affected by the Agreement relative to products sold mostly by firms that were 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 done 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., anti-competitive) purposes.

This setup also allows us to measure of 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 comparability between products, we normalize this variable by its mean and standard deviation. We run the same regression model we use to test for vertical integration as in equation (2). We include all years in our dataset 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 (2), which study the margins of vertical integration and total procurement in response to the Agreement, are presented in Table 4. Column 1 shows 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 continue to buy 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 (2) but replace the $exposure_j$ variable with $highexposure_j$, which is defined as one if the product has an exposure above the mean.[34] The results of this regression are shown in Column 2 of Table 4, 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.

Next, we study whether total procurement was differentially affected for products more exposed to the Agreement. In Column 3 of Table 4 we show the regression output 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 are shown in Column 4 of Table 4 and suggest that products in which firms affected by the Agreement have a market share above the median experience a reduction in

[34]Results are quantitatively similar if instead we use the median to split the sample.

procurement of four percent of a standard deviation. One interpretation of this result is that the Superstore 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. Thus, the firm is unable to replicate the market outcomes and settles with a second-best outcome, which is consistent with Baker, Gibbons, and Murphy (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 the variable, $grossprofit$, by averaging revenues to final customers minus purchases from suppliers for each product on each year. The results of using $grossprofit$ as the outcome variable for regression model (2) are shown in Table 4, 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 suggests that the Superstore is not necessarily choosing to expand into the supply of relatively more profitable products. Rather, it is consistent with the idea that some of the costs of adjustment to the Agreement are borne by the Superstore in the form of lower profits.

V. 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 also respond by adjusting procurement and vertically integrating. Baker, Gibbons, and Murphy (2002) argue that relational contracts, which are prevalent along supply chains, may help the 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, Gibbons, and Murphy (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 these situations, the relationships are more exclusive and termination is more costly. Formally, we use our data at the firm×product×year level and augment regression (1) with an interaction variable, $exclusivity_{i,j}$, that varies at the firm i and product j level,

$$
\begin{aligned}
(3) \qquad trade_{i,j,t} = {} & \beta post_t \times treated_i \\
& + \gamma post_t \times treated_i \times exclusivity_{i,j} \\
& + \delta post_t \times exclusivity_{i,j} + \omega_{j,t} + \omega_{i,j} + \epsilon_{i,j,t}.
\end{aligned}
$$

The coefficients of interest are β, which measures the baseline effect of the Agreement when relationships are not exclusive, and γ, 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 of exclusivity 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 1 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. Then, we define $exclusivity_{i,j}$ as the pre-period market share for product j and firm i. A

positive value of the coefficient γ 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 γ 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 in payments should have no effect.

Table 5 presents the coefficients β and γ from regression (3) using both measures of exclusivity. Columns 1 and 2 show the results using concentration of sales as the interaction variable in regression (3), while columns 3 and 4 use market share as the interaction variable. We find that the impacts 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 patterns are similar for revenues, though the coefficients are not statistically significant. Further in all four columns, we cannot reject 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 to explain why the Agreement was able to persist.

VI. 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 impacts 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 put forth 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; 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 Section I, 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, 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 (2015)). The Chilean regulator had a similar rationale in mind when drafting the Agreement, and on the surface, bargaining power seems like a reasonable theory to explain the use of trade credit in this setting.

However, as Petersen, Williamson, and Chopra (2013) highlights, trade credit is an inefficient way for firms to extract surplus from small, financially constrained suppliers.[35] The buyer can increase its own profits and keep the supplier's profits constant by simply paying a lower procurement price in the spot market and by 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 have argued that the bargaining power theory is still valid, but only when firms are not able to engage in price discrimination.[36] We show in Table

[35]Also see Schwartz (1974).

[36]E.g., see Mian and Smith (1992) and the Robinson-Patman Act in the US.

2 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 V also provide evidence that is inconsistent with the bargaining power hypothesis. In Table 5, 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, Malitz, and Ravid (1993)).[37] We provide a simple model in the spirit of Kim and Shin (2012) in Supplemental Appendix Section C 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, Gibbons, and Murphy (2002)). Indeed in Section V, we show that when the trading relationships are more resilient, the negative effects of the Agreement on the trading relationship are mitigated. 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 where relational contracts are not sufficient to provide incentives.[38]

Under a quality-based model of trade credit, the Agreement should have larger impacts 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

[37]Also see Giannetti, Burkart, and Ellingsen (2011) for suggestive empirical evidence.
[38]We further develop this idea in the Supplemental Appendix model.

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 affects 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 our 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 non-food 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.[39] 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, all items which are sold by the Superstore, are excluded by the panel.

In Panel A of Table 6, we estimate heterogeneous effects regressions to test whether the Agreement affected differentially the likelihood of trade for perishable or durable goods. We focus on heterogeneous effects for the non-exclusive relationships, which we show in Table (5) were the ones most affected by the Agreement. In particular, we restrict the sample to non-exclusive relationships, as defined in Section V, and we estimate a version of the heterogeneous effects regression (3) where we include an interaction term "$interaction_j$" for perishable or for durable goods:

$$(4)\quad trade_{i,j,t} = \beta post_t \times treated_i + \delta post_t \times treated_i \times interaction_j + \omega_{j,t} + \omega_{i,j} + \epsilon_{i,j,t}.$$

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 for both definitions

[39]See https://research.chicagobooth.edu/nielsen/

of the interaction (perishability and durability) the coefficients β and δ. We restrict the perishable and durable regressions to only food and non-food products, respectively. In columns 1-2, we define exclusivity using sales concentration, and in columns 4-5, we define exclusivity using market share. We find that the extensive margin trading effects are much larger in magnitude for durable products relative to non-durable 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.

In Panel B of Table 6, we estimate heterogeneous effects regressions to test whether the Agreement caused differential vertical integration for perishable or durable goods:[40]

$$(5)\quad subsidiary_{j,t} = \omega_j + \omega_t + \beta post_t \times highexposure_j + \gamma post_t \times highexposure_j \times interaction_j + \omega_t \times interaction_j + \epsilon_{j,t}$$

In general, we find little evidence of a vertical integration effect on the food products, both perishable and non-perishable. However, among the non-food 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.[41] Here, by making sure suppliers receive cash upfront, factoring unwinds any incentive effect of trade credit. Thus, we hypothesize that incentive problems should be most severe for firms that cannot factor their receivables. We implement this test by running regression (4) replacing the interaction variable for a dummy that indicates whether firms factored their receivables. Because the indicator variable $factoring_i$ is

[40]Given that the resilience variables are defined at the supplier x product level, and the vertical integration regressions are run at the product level, we cannot focus only on less resilient relationships.

[41]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 treatment and control firms in Table 1.

defined at the firm level, we include interaction of $post_t \times factoring_i$ in the regression. We find evidence consistent with this hypothesis in Columns 3 and 6 of Panel A of Table (6), which show the output of regression (4) where 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 mitigated for perishable food products, for non-food 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 incentives to suppliers 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 mitigated 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.

VII. Conclusion

We show 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 the suppliers (providers of credit) relative to the conglomerate (the debtor). In this

context, 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 document 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 performs unobserved investments that directly affect the value of the product (e.g., quality).

The results highlight one 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 decrease their demand for certain products. However, previous authors have documented 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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Appendix

Appendix A. Figures and Tables

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 is publicly available and was obtained from the website of the Chilean IRS (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.

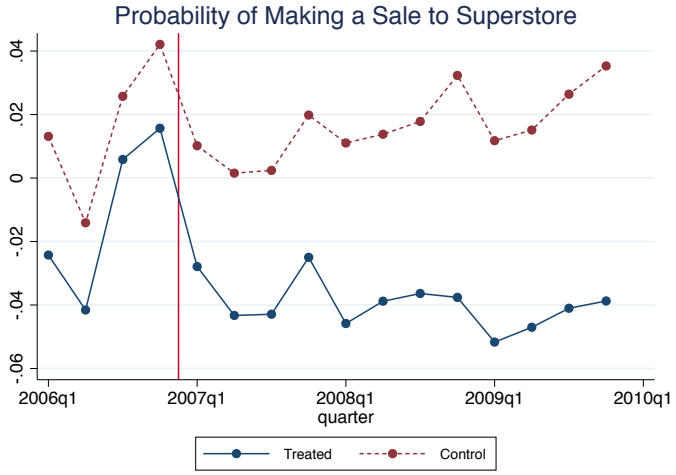


FIGURE 3. Graphical evidence: vertical integration

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

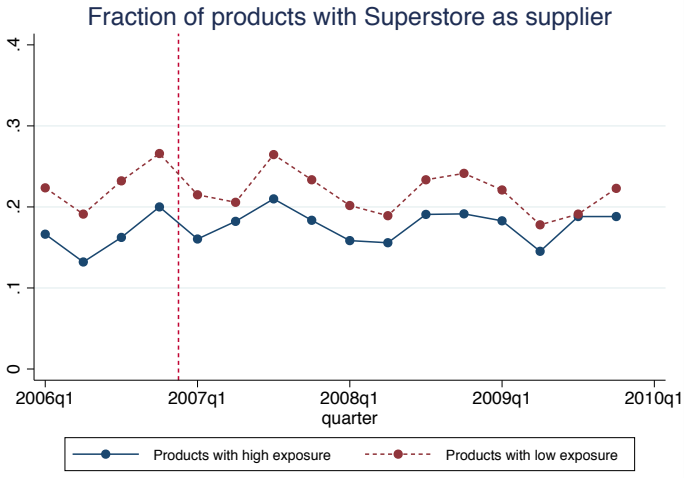


TABLE 1. Summary statistics: main sample

This table shows the mean, standard deviation and median of variables measured in the pre period for Treated and Control firms as defined above. Panel A shows variables at the firm-level, while Panel B shows variables at the product-firm level. 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. We restrict the sample to those firms with total 2006 revenues between UF 25,000 and UF 600,000. * denotes 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 2. The effect of the reduction of 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 β of 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 ("Unidad de Fomento" is an inflation linked currency unit updated daily, whose value is published by the Banco Central de Chile. 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 Placebo sample, Treated-placebo firms ($treatedplacebo = 1$) 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 0 replaced with the log of 1 peso; "$log(units)$", the natural logarithm of the number of monthly product units sold to the Superstore. The dataset is a panel at the product-firm-year level. $post = 0$ represents the year 2006 and $post = 1$ 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.

	Main sample			
	(1)	(2)	(3)	(4)
Dependent variable	trade	$log\,(price)$	$log\,(revenues + 1)$	$log(units)$
$post \times treated$	-0.1086***	-0.0381*	-1.0346***	-0.0078
	(0.034)	(0.022)	(0.361)	(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
	Placebo sample			
	(5)	(6)	(7)	(8)
Dependent variable	trade	$log\,(price)$	$log\,(revenues + 1)$	$log(units)$
$post \times treatedplacebo$	-0.0097	0.0036	0.2012	-0.0331
	(0.085)	(0.017)	(1.322)	(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 3. 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 by the fraction of 2006 sales to the Superstore made by affected firms. The table show the coefficient β of regression:

$$outcome_{i,j,t} = \beta post_t \times exposure_j + \omega_{i,t} + \omega_{i,j} + \epsilon_{i,j,t},$$

where *exposure* is defined as the 2006 product market share of affected firms, defined as 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); "$log\,(price)$": natural logarithm of the transfer price; "$log\,(revenues + 1)$" the natural logarithm of monthly product sales to the Superstore in pesos, with 0 replaced with the log of 1 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 dataset is a panel at the product × year level. $post = 0$ represents the year 2006 and $post = 1$ 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)
Dependent variable	trade	$log\,(price)$	$log\,(revenues + 1)$	trade	$log\,(price)$	$log\,(revenues + 1)$
$post \times exposure$	0.1089**	0.0922	1.0194*			
	(0.043)	(0.058)	(0.557)			
$post \times highexposure$				0.0843***	0.0732*	0.7720**
				(0.026)	(0.044)	(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 4. 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 the average product-level Superstore profits. The table presents the estimated coefficient of interest β of 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 by 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*": 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 dataset is a panel at the product × year level. $post = 0$ represents the year 2006 and $post = 1$ includes all years up to 2011. Standard errors are clustered at the product level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	*subsidiary*	*subsidiary*	*unitsprocured*	*unitsprocured*	$grossprofits$	$grossprofits$
$post \times exposure$	0.0436**		-0.0877***		-137.53**	
	(0.022)		(0.033)		(55.06)	
$post \times highexposure$		0.0388***		-0.0398**		-73.67**
		(0.013)		(0.017)		(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 5. Exclusivity

This table shows that the effects of the restriction to the contracting space are mitigated when trading relationships are valuable. The table reports whether the estimated effects of the change in days payable from 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 ($trade_{i,j,t}$) and the supplier revenues outcome ($log\,(revenues + 1)$) vary with the whether the supplier's total sales to the Superstore during 2006 are more than the median by treatment status–(Concentration) (Columns 1-2), and with the supplier's product market share (Columns 3-4), both definitions of the *exclusivity* interaction variable. 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 dataset is a panel at the product-firm-year level. $post = 0$ represents the year 2006 and $post = 1$ 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)
Interaction var.	Concentration		Market share	
Dependent var.	trade	$log\,(revenues + 1)$	trade	$log\,(revenues + 1)$
$post \times treated$	-0.1504***	-1.5664***	-0.0979***	-1.0627**
	(0.048)	(0.603)	(0.038)	(0.475)
$post \times treated \times exclusivity$	0.1127*	1.0482	0.2686*	1.4589
	(0.067)	(0.854)	(0.162)	(2.068)
Sum of coefficients	-0.0377	-0.5182	0.1707	0.3962
p-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 6. Heterogeneous effects

This table shows that, among non-exclusive relationships, the effects of the Agreement were strongest for non-perishable products, for durable goods, and for firms that did not have access to factoring, consistent with trade credit serving a role as a bond for product quality. Panel A reports the output 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 non-exclusive relationships with the Superstore . Exclusivity is defined as "Concentration", whether supplier's total sales to the Superstore during 2006 are more than the median by treatment status (Columns 1-3), and by "Market share", a dummy that equals 1 if the firm's 2006 average product market share is higher than the mean (13%) (Columns 4-6). 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 non-food products sold in 2006); "no factoring", defined as a dummy for whether the supplier never factored its accounts; "perishable" is defined only for products in Food departments: department 5 (General Food), 10 (Meat and Fish), 11 (Deli), 12 (Fruits and Vegetables), and 13 (Bread and Bakery), and we define "durable" only for non-food products. Panel B reports the output of 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, data is at the product × firm × year level. In panel B, data is at the product×year level. We drop products included in Department 14 (business procurement for the Superstore, e.g., cleaning services) and 15 (catering for internal operations) of the Superstore data, which are hard to match to the Nielsen data and to define as perishable. Post covers the years after 2006 until 2009. Standard errors are clustered at the firm level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

Panel A: probability of trade at the firm-product level

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	trade					
Exclusivity	Concentration			Market share		
Interaction variable	perishable	durable	factoring	perishable	durable	factoring
$post \times treated$	-0.3163***	-0.0061	-0.1941***	-0.1609**	0.0369	-0.1430***
	(0.102)	(0.097)	(0.063)	(0.064)	(0.072)	(0.044)
$post \times treated \times interaction$	0.2333*	-0.2833**	0.1886**	0.0775	-0.2123**	0.1862***
	(0.138)	(0.132)	(0.081)	(0.084)	(0.101)	(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	subsidiary	
Interaction variable	perishable	durable
$post \times highexposure$	-0.0004	0.0117
	(0.047)	(0.015)
$post \times highexposure \times interaction$	0.0430	0.0439*
	(0.053)	(0.026)
Sample	Food	Not Food
R^2	0.877	0.829
Obs.	2,136	2,718
Products	356	453

Appendix B. Supplemental Appendix

A. Supplemental Figures

FIGURE B.1. Absence of bunching in sales to superstore

This graph shows the histogram of firms that sold to the Superstore in 2006 by sales to the Superstore during 2006 in UF. The vertical line represents UF 60,000, the cutoff for the Agreement.

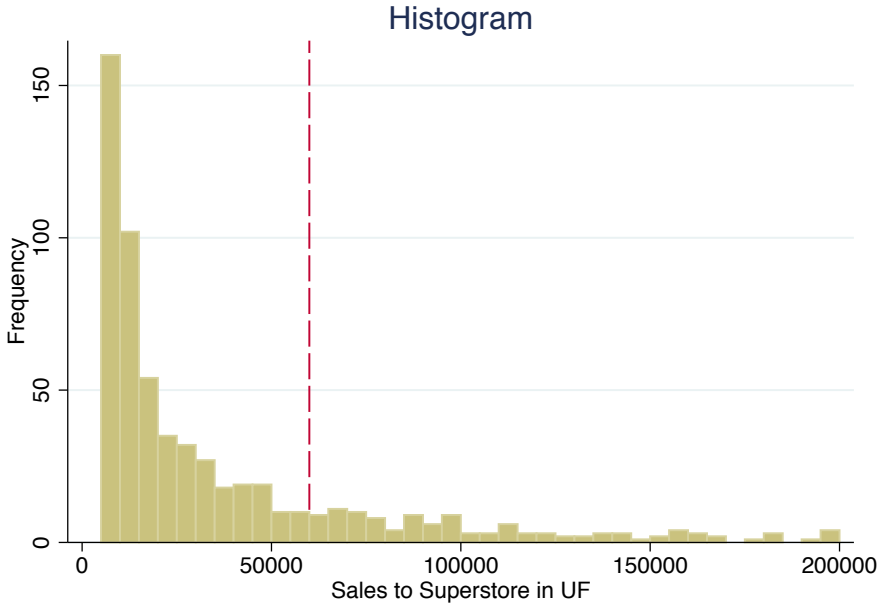
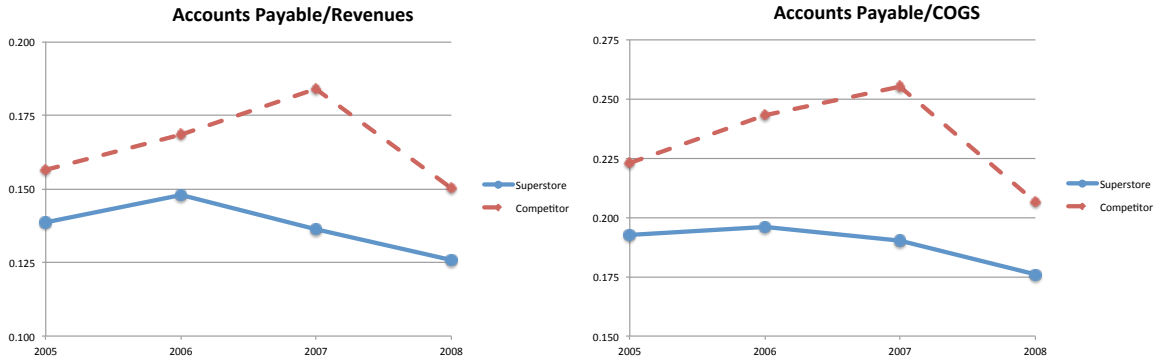


FIGURE B.2. Accounts payable for Superstore and competitor

This graph shows the end-of-year Accounts Payable divided by yearly revenues (left) and Accounts Payable divided by COGS (right) for the Superstore (solid line) and its main competitor (dashed line). Source of accounting data: Superintendencia de Valores y Seguros, FECU Consolidada, and own calculations.



B. Supplemental Tables

TABLE B.1. Industry distribution of transactions

This table shows the number of product-firm observations in 2006 for each of the 16 Departments, the broadest product category defined by the Superstore. 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. We restrict the sample to those firms with total 2006 revenues between UF 25,000 and UF 600,000.

	All (N=4,784)	Treated (N=1,656)	Control (N=3,128)
CLOTHING	295	84	211
BABY	180	31	149
HOME	219	68	151
PETS	37	13	24
GENERAL FOOD	1,060	321	739
PERISHABLES	499	83	416
ENTERTAINMENT	453	158	295
HARDLINES	526	212	314
IMPULSIVE SHOPPING	24	2	22
MEAT AND FISH	233	68	165
DELI	300	136	164
FRUITS & VEGETABLES	508	290	218
BREAD & BAKING	193	73	120
BUSINESS	178	93	85
RESTAURANT	53	12	41
HEALTH & WELLBEING	26	12	14

TABLE B.2. Supplemental summary statistics: main sample

This table provides information on the variation underlying the identification of the causal effects of trade credit on supplier outcomes. 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. We restrict the sample to those firms with total 2006 revenues between UF 25,000 and UF 600,000.

Analysis Sample Description	
Number of Treated Firms	342
Number of Control Firms	392
Number of Products Sold by Both T AND C Firms in 2006	618
Median # Obs Per Product Conditional on T AND C Making Sale	4
Observations	19,136
Firms	734

TABLE B.3. Robustness: timing of effects

This table shows that the effects of the Agreement on the probability that Treated firms sell to the Superstore after the Agreement is in place do not manifest untill the Agreement is in place in 2007. We run the regression model:

$$trade_{i,j,t} = \omega_{i,j} + \omega_{j,t} + \sum_t \beta_t month_t \times treated_i + \varepsilon_{i,j,t}.$$

at the product×firm×month level, and present the coefficients β_t of interaction of the treatment dummy and November 2006, December 2006, and all months in 2007 (Panel A). In panel B we replace the $month_t$ by $quarter_t$ dummies, and present the interaction coefficients between the third quarter of 2006 and 2009 (Panel B). The excluded category for the interaction corresponds to products sold between January and October 2006 (Panel A) and between 2006 Q1 and 2006 Q2 (Panel B). Treated 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 exclude products that were not sold during 2006. We show interacted coefficients from November 2006 to June 2007 for the monthly regression and from 2006 Q3 to 2008 Q4 for the quarterly one. The data is at the product×firm×month level. Standard errors are clustered at the firm level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

Panel A: monthly coefficients

Variable	trade		trade
$treated \times Nov06$	-0.0001	$treated \times Mar07$	-0.0426**
	(0.017)		(0.017)
$treated \times Dec06$	0.0045	$treated \times Apr07$	-0.0134
	(0.018)		(0.017)
$treated \times Jan07$	0.0103	$treated \times May07$	-0.0274
	(0.019)		(0.019)
$treated \times Feb07$	-0.0250	$treated \times Jun07$	-0.0367*
	(0.018)		(0.019)
R^2		0.669	
Obs.		161,016	
Firms		734	

Panel B: quarterly coefficients

Variables	trade		trade
$treated \times 2006Q3$	0.0126	$treated \times 2007Q4$	-0.0216
	(0.014)		(0.024)
$treated \times 2006Q4$	0.0060	$treated \times 2008Q1$	-0.0368
	(0.018)		(0.023)
$treated \times 2007Q1$	-0.0148	$treated \times 2008Q2$	-0.0326
	(0.018)		(0.024)
$treated \times 2007Q2$	-0.0216	$treated \times 2008Q3$	-0.0341
	(0.019)		(0.025)
$treated \times 2007Q3$	-0.0221	$treated \times 2008Q4$	-0.0499*
	(0.021)		(0.027)
R^2		0.635	
Obs.		322,032	
Firms		734	

TABLE B.4. Robustness: dynamic effect

This table documents the dynamic effects of the Agreement on the probability that Treated firms sell to the Superstore after the Agreement is in place. We run the regression model:

$$trade_{i,j,t} = \omega_{i,j} + \omega_{j,t} + \sum_t \beta_t month_t \times treated_i + \varepsilon_{i,j,t}.$$

at the product×firm×month level, and present the coefficients β_t of interaction of the treatment dummy and all months in 2007 (Panel A). In panel B we replace the $month_t$ by $quarter_t$ dummies, and present the interaction coefficients between 2007 and 2009 (Panel B). The excluded category for the interaction corresponds to products sold in 2006. Treated 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 exclude products that were not sold during 2006. The data is at the product×firm×month level. Standard errors are clustered at the firm level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

Panel A: monthly coefficients

Variable	trade		trade
$treated \times Jan07$	0.0100	$treated \times Jul07$	-0.0204
	(0.018)		(0.020)
$treated \times Feb07$	-0.0254	$treated \times Aug07$	-0.0293
	(0.017)		(0.019)
$treated \times Mar07$	-0.0429***	$treated \times Sep07$	-0.0304
	(0.015)		(0.020)
$treated \times Apr07$	-0.0138	$treated \times Oct07$	-0.0428**
	(0.017)		(0.020)
$treated \times May07$	-0.0278	$treated \times Nov07$	-0.0232
	(0.018)		(0.021)
$treated \times Jun07$	-0.0371**	$treated \times Dec07$	-0.0126
	(0.018)		(0.022)
R^2	0.669		
Obs.	161,016		
Firms	734		

Panel B: quarterly coefficients

Variables	trade		trade
$treated \times 2007Q1$	-0.0195	$treated \times 2008Q3$	-0.0388*
	(0.014)		(0.022)
$treated \times 2007Q2$	-0.0262*	$treated \times 2008Q4$	-0.0545**
	(0.015)		(0.024)
$treated \times 2007Q3$	-0.0267	$treated \times 2009Q1$	-0.0340
	(0.018)		(0.026)
$treated \times 2007Q4$	-0.0262	$treated \times 2009Q2$	-0.0327
	(0.020)		(0.025)
$treated \times 2008Q1$	-0.0415**	$treated \times 2009Q3$	-0.0380
	(0.020)		(0.028)
$treated \times 2008Q2$	-0.0372*	$treated \times 2009Q4$	-0.0445
	(0.021)		(0.030)
R^2	0.635		
Obs.	322,032		
Firms	734		

TABLE B.5. Summary statistics: placebo sample

This table shows the mean, standard deviation and median of variables for Treated-Placebo and Control-Placebo firms. Panel A shows variables at the firm-level, while Panel B shows variables at the product-firm level. Treated-placebo firms are those with total 2006 revenues between UF 100,000 and UF 600,000. Control-placebo firms are those with total 2006 revenues above UF 600,000. We restrict the sample to those firms with total 2006 revenues above UF 100,000. * denotes 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=619)			Placebo Treated (N=389)			Placebo Control (N=230)		
	Mean	p50	St. Dev.	Mean	p50	St. Dev.	Mean	p50	St. Dev.
log(revenues)*	19.13	19.58	2.71	18.62	19.18	2.32	19.98	20.82	3.09
# Departments*	1.73	1.00	1.20	1.62	1.00	1.09	1.92	1.50	1.35
# Products*	10.55	4.00	28.51	7.93	3.00	14.13	14.98	7.00	42.71
factoring (%)*	20.52	0.00	40.42	23.39	0.00	42.39	15.65	0.00	36.41

Panel B: Product-firm level 2006 monthly average

	All (N=6,531)			Placebo Treated (N=3,085)			Placebo Control (N=3,446)		
	Mean	p50	St. Dev.	Mean	p50	St. Dev.	Mean	p50	St. Dev.
log(price)	7.05	6.90	1.50	7.02	6.91	1.45	7.08	6.88	1.55
log(units)*	7.11	7.19	2.77	6.49	6.48	2.56	7.67	7.83	2.83
log(revenues)*	14.16	14.37	2.51	13.50	13.78	2.38	14.75	14.96	2.47
mark-up (%)	31.42	28.33	17.69	31.41	29.13	15.42	31.43	27.73	19.48
supplier market share (%)*	20.65	6.79	28.73	13.87	3.30	23.79	26.72	12.52	31.31

TABLE B.6. Supplier procurement and vertical integration: alternative specifications

This table shows two margins of adjustment to the Agreement, vertical integration by the Superstore and reduced total procurement, as well as Superstore (log) gross profits. The table presents the estimated coefficient of interest β of regression:

$$outcome_{j,t} = \alpha_j + \delta_t + \beta post_t \times exposure_j + \epsilon_{j,t},$$

which measures the relative change in the outcome for products with varying exposure to the Agreement, measured by 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. In columns 1 through 6 we place no further restriction on the sample. In Columns 7 through 12 we restrict to products sold either by a Treated firm or by a Control firm, as defined in the text. The outcomes are "*subsidiary*": 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, 6, 8, 10, and 12 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. Data is a panel at the product × year level. $post = 0$ represents the year 2006 and $post = 1$ includes all years up to 2011. Standard errors are clustered at the product level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Sample selection	All products		All products		All products	
Dependent variable	*subsidiary*	*subsidiary*	*unitsprocured*	*unitsprocured*	$grossprofits$	$grossprofits$
$post \times exposure$	0.0665***		-0.0492***		-48.58	
	(0.015)		(0.019)		(29.99)	
$post \times highexposure$		0.0474***		-0.0305**		-33.87
		(0.011)		(0.013)		(23.75)
R^2	0.837	0.837	0.969	0.969	0.948	0.948
Obs.	7,068	7,068	7,068	7,068	7,068	7,068
Products	1,178	1,178	1,178	1,178	1,178	1,178

	(7)	(8)	(9)	(10)	(11)	(12)
Sample selection	Products sold by T or C firms		Products sold by T or C firms		Products sold by T or C firms	
Dependent variable	*subsidiary*	*subsidiary*	*unitsprocured*	*unitsprocured*	$grossprofits$	$grossprofits$
$post \times exposure$	0.0539***		-0.0549**		-81.91**	
	(0.017)		(0.021)		(37.29)	
$post \times highexposure$		0.0428***		-0.0321**		-54.45*
		(0.011)		(0.014)		(27.75)
R^2	0.845	0.846	0.969	0.969	0.948	0.948
Obs.	6,045	6,045	6,045	6,045	6,045	6,045
Products	1,009	1,009	1,009	1,009	1,009	1,009

TABLE B.7. Heterogeneous effects: large goods

The table reports how the estimated effects of the change in days payable on the extensive margin and supplier revenues (Columns 1 and 2, respectively), as well as vertical integration at the product level (Column 3), vary whether a product is *large*, defined as products whose size attribute, as per the Nielsen consumer data, is larger than the median. The table reports the coefficients β and γ of regression:

$$outcome_{i,j,t} = \beta post_t \times treated_i + \gamma post_t \times treated_i \times large_j + \omega post_t \times interaction_{i,j} + \alpha_{j,t} + \omega_{i,j} + \epsilon_{i,j,t},$$

and the vertical integration result at the product level,

$$subsidiary_{j,t} = \alpha_j + \delta_t + \beta post_t \times highexposure_j + \gamma post \times highexposure_j \times large_j + \delta post_t \times large_j + \epsilon_{j,t}.$$

Treated 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. Post covers the years after 2006 until 2009 for columns 1 and 2, and all years up to 2011 for column 3. Standard errors are clustered at the firm level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively. The sample is restricted to product categories included in the Nielsen consumer dataset. Post covers the years after 2006. Standard errors are clustered at the firm level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)
Interaction var.		large goods	
Dependent var.	trade	$log\,(revenues + 1)$	subsidiary
$post \times treated$	-0.0615	-0.7938	
	(0.045)	(0.566)	
$post \times treated \times large$	-0.0137	0.2585	
	(0.057)	(0.711)	
$post \times highexposure$			-0.0063
			(0.025)
$post \times highexposure \times large$			0.0350
			(0.031)
R^2	0.649	0.735	0.863
Obs.	15,100	15,100	3,702
Firms / Products	573	573	617

Appendix C. Framework

We present a simple framework that illustrates how trade credit may help provide incentives to suppliers to increase the value of the good procured.

A. Set-Up

We consider the market for a good g. In this market there are two risk neutral firms: a supplier, which produces the good, and a buyer, which sells the good to end consumers.

We first consider cases where the buyer does not produce the good in-house and instead procures from an independent supplier. The good may be of high or low value, depending on an unobservable investment e made by the supplier. A good of high value sells in the consumer market for V. However, with probability $q_g(e)$, the good is of low value and is worth $V - L$. The key friction in our model is that the value of the product is not observable at the time the supplier sells the good to the buyer. To obtain closed form solutions, we let $q_g(e) = \bar{q}_g - e$, where $e \leq \bar{q}_g$ and $\bar{q}_g > 0$. The supplier bears the cost $c(e) = \frac{1}{2}e^2$ of investment. We characterize the first best solution by the choice of investment that maximizes total surplus:

$$\max_{0 \leq e \leq \bar{q}_g} V - q_g(e) L - c(e).$$

The first-best choice of investment derived from the first order condition is $e^{FB} = \min\{L, \bar{q}_g\}$.

We study the competitive equilibria obtained under three contracting regimes: (1) Trade Credit contracts, (2) No Trade Credit Spot contracts, and (3) No Trade Credit Relational contracts. We also relax the assumption that the buyer cannot produce the good itself and explore when vertical integration may be optimal.

B. Trade Credit Spot Contracts

The buyer can only verify the good's value with a time lag. For example, the buyer can observe demand for the good some time after the supplier delivers it, or it can

monitor the incidence of returns from the end customer. Further, the supplier's unobserved investment could consist of marketing research, or a marketing campaign or of supply-chain management activities, all of which affect the value of the good and are not perfectly observed by the buyer. We model trade credit, which delays payment, as a contracting technology that allows buyers and suppliers to condition payments upon value.

Trade credit contracts have two parts, (τ_N, ρ). τ_N is the price the buyer pays to the supplier for a good of standard quality, and ρ represents the discount for a low quality good. We assume that the parties can agree to share the surplus through Nash Bargaining, where λ represents the supplier's bargaining power and b_S is the supplier's outside option. We normalize the buyer's outside option to 0.[42] The timing is as follows: (1) the buyer offers the supplier a contract, (2) given that contract, the supplier chooses its optimal level of investment e and produces the good, (3) the buyer receives the good, (4) the good's quality is revealed and the buyer pays the supplier. We assume throughout that buyers cannot renege on their trade credit contracts ex post by paying only the reduced price.

To characterize the equilibrium, note that the supplier will have the incentive (at an interior solution) to make the first best investment, e^{FB}, if $\rho = L$. Then, under Nash bargaining, τ_N will be chosen optimally such that the expected payoff of the supplier (S) under trade credit contracts (TC), Π_S^{TC}, equals a share λ of the net surplus,[43]

$$\Pi_S^{TC} = \lambda \left(V - \bar{q}_g L + \frac{L^2}{2} - b_S \right) + b_S. \tag{6}$$

C. No Trade Credit Spot Contracts

We assume that in the absence of trade credit, payments are made before product value becomes observable. Because the parties cannot contract on quality, the buyer

[42] This assumption can be relaxed without altering the results.

[43] The below expression holds for an interior solution where $L \leq \bar{q}_g$. If $L > \bar{q}_g$ then $\Pi_S^{TC} = \lambda \left(V - \frac{1}{2}\bar{q}_g^2 - b_S \right) + b_S$.

cannot provide the supplier with incentives, and no investment will be made.[44] Total surplus equals $V - \bar{q}_g L - b_S$, and payment to the supplier (S) in the No Trade Credit Spot contract (NT, S) equals:

$$\Pi_S^{NT,S} = \max\left\{\lambda\left(V - \bar{q}_g L - b_S\right) + b_S, b_S\right\}. \tag{7}$$

Equations (6) and (7) imply that buyers (and suppliers) are strictly worse off in the No Trade Credit Spot market equilibrium than in the Trade Credit Spot market equilibrium. In this equilibrium, buyers will pay a lower price to suppliers. If the value of the surplus is sufficiently low (i.e. $V - \bar{q}_g L < b_S$), no trade may be a preferred choice by the contracting parties. Note that trade is more likely for goods with V large and for suppliers with b_S small.

D. No Trade Credit Relational Contracts

In reality, buyers and suppliers may engage in long-term relationships, which may strengthen supplier incentives. Following Baker, Gibbons, and Murphy (2002), we explore the degree to which relational contracts, can substitute for trade credit. We follow the literature and look for equilibrium contracts $\{\tau_N, \rho\}$ paid each period that are sustained by grim-trigger punishment threats. We assume that the buyer pays τ_N to the supplier upon receipt of the goods, expecting a high value good. However, if the value is later revealed to be low, the seller is requested (but not contractually obligated) to refund a portion ρ of the procurement price.

The timing of the model is as follows: (1) the buyer offers the supplier a contract, (2) given the contract terms, the supplier chooses its optimal level of investment e and produces the good, (3) the buyer receives the good and pays τ_N, and (4) the good's value is revealed and the supplier refunds ρ. Between contracting periods, supplier firms discount the future at an interest rate r.

[44] Alternatively, a contracting scheme where the buyer pays a high price up front and the supplier reimburses the buyer in case the good is of low value is, again, not enforceable ex post (see the No Trade Credit Relational contract below). In the same spirit, third-party insurance is infeasible due to moral hazard.

In an equilibrium, if the supplier ever defaults on a punishment payment ρ, then the supplier is forced to contract in the spot market at every period in the future. Then, under a grim-trigger punishment threat, the supplier will be willing to make a positive punishment payment in case the good is of low value as long as this payment is sufficiently small.[45] As discussed above, the maximal punishment value in the spot market is $\rho = 0$. Hence, as long as the relational contract is more valuable to the supplier than the spot market contract, the supplier will be able to commit to a strictly positive level of investment. Moreover, the first best level of investment (at an interior solution, $\bar{q}_g > L$) is achievable under relational contracting when setting $\rho = L$ is incentive-compatible. This occurs when:

$$r \leq \frac{L\lambda}{2}. \tag{8}$$

Condition (8) characterizes the parameter space where first best investment can be sustained by the value of the future relationship even when the ability to extend trade credit is taken away.

If investment is not first best, then the buyer will choose ρ so that it is not profitable to deviate to the No Trade Credit Spot contract. Given supplier's optimal choice of investment $e = \rho$, total net surplus is split according to Nash bargaining.[46] Thus, when $(V - \bar{q}_g L - b_S) > 0$, the optimal effort level satisfies$\rho^* = 2\left(L - \frac{r}{\lambda}\right)$. This $\rho^\star$ will only be an equilibrium if investment is both positive and strictly less than first best. These conditions are jointly satisfied if:

$$\frac{L\lambda}{2} < r < L\lambda \tag{9}$$

When condition (9) holds, relational contracting is better than spot contracting but strictly worse than trade credit contracts. However, for firms with $r \geq L\lambda$, the buyer is unable to use the threat of terminating the relationship to incentivize the firm to

[45]In particular, if Π_S^R denotes the per period expected profits to the supplier (S) from the relational contract (R), then feasible punishments satisfy $\rho \leq \frac{\Pi_S^R - \Pi_S^{NT,S}}{r}$.

[46]From equation (7), there is a set of parameters such that there is no trade in the No Trade Credit Spot contract. Relational contracts may sustain trade in the absence of trade credit whenever the value of the relationship is sufficiently high (high V or low b_S).

produce any non-zero level of investment. This leads to the No Trade Credit Spot contract equilibrium as long as the parties have an incentive to trade.

If $(V - \bar{q}_g L - b_S) < 0$, then under no trade credit spot contracting, trade breaks down and $\Pi_S^{NT,S} = b_S$. This changes the payoff in the punishment phase of the grim trigger equilibrium. It can be shown that in this case $\rho^* = \left(L - \frac{r}{\lambda}\right) + \sqrt{\left(L - \frac{r}{\lambda}\right)^2 + 2\left(V - \bar{q}L - b_S\right)} < 2\left(L - \frac{r}{\lambda}\right)$. Also, note that here, ρ^* is increasing in V and decreasing in b_S. When a relational contract is unable to sustain positive levels of effort, then trade will again break down.[47]

E. Vertical Integration

We end our examination of the equilibria induced by the different contractual regimes by relaxing the restriction that the buyer cannot produce the good in-house. Many authors have discussed the costs of vertical integration (e.g., see Bresnahan and Levin (2012)). Our goal is not to provide a new theory of vertical integration, but rather to provide simple intuition in a reduced form fashion for why a firm may choose to vertically integrate.

We follow Williamson (1975) in assuming that firms are not able to provide very strong incentives to workers (e.g., because of ex post hold-up by the buyer as in Grossman and Hart (1986)). However, as suggested by Holmstrom (1999), we assume that control over productive assets gives firms the ability to monitor workers or incentivize them in a manner that internalizes some of the contractual externalities present in market based relationships. In particular, we assume that the firm has a monitoring technology such that a strictly positive minimum investment level e^{VI}, where $L > e^{VI} > 0$, can be guaranteed if the firm pays a monitoring cost C_m (note that if $e^{VI} > \bar{q}_g$, then the firm will only enforce an investment of $\bar{q}_g$). If the firm does not pay for the monitoring technology, then the workers do not invest.

Under this contract when $e^{VI} < \bar{q}_g$, the buyer's profits are:

[47] This occurs when $r < \left(L - \sqrt{-2\left(V - \bar{q}L - b_S\right)}\right)\lambda$

$$\Pi_B^{VI} = \max\left\{\Pi_B^{NT,S}, \Pi_B^{NT,S} + (1-\lambda)\left(e^{VI}L - \frac{1}{2}\left(e^{VI}\right)^2 - C_m\right)\right\}$$

Hence, the buyer will prefer to vertically integrate in the No Trade Credit Spot equilibrium if the cost of monitoring is sufficiently low relative to the employee's level of investment. This may also happen when the parties can enter into relational contracts where the level of investment is below first best and below e^{VI}.