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

EMILY BREZA[†] AND ANDRES LIBERMAN[‡]

ABSTRACT. We present empirical 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 change that restricted the maturity of the trade credit contracts that a large retailer could sign with its small suppliers, defined by an arbitrary sales cutoff, to at most 30 days. Using a within-product differences-in-differences identification strategy, we find that the restriction to the set of feasible contracts reduces the likelihood that trade takes place by 11%. The large retailer responds by internalizing the procurement of some products previously sold by affected firms to its own subsidiaries but reduces the volume of purchases, consistent with the fact that vertical integration is costly. Thus, financial contracts like trade credit may help overcome contracting frictions and enable trade.

Keywords: Organizational Form, Trade Credit, Vertical Integration
JEL codes: D23, G30, L14, L15, L22

I. Introduction

Contracting problems between suppliers and buyers motivate a vast literature on the boundaries of the firm (e.g. Coase (1937), Williamson (1975), Grossman and Hart (1986)).[1] Buyers may enter into spot or relational contracts with suppliers or vertically integrate and bring procurement within the boundary of the firm. This choice depends on the nature of the contracting frictions that may limit trade as well as the contracts available to mitigate those frictions.

In this paper we study the effects of changes to the set of feasible financial contracts on the bilateral procurement relationships of a large buyer and the decision of that buyer to vertically integrate. These effects are generally difficult to measure. In the cross section, counterfactual contractual terms are not available for a vertically integrated firm. Further, within trading relationships, contractual terms are an equilibrium outcome that may depend on observable and unobservable characteristics such as product quality, length of the relationship, and cost of capital. Hence, any correlation between contracting terms and the prevalence of trade may be caused by these characteristics rather than by the set of available contracts.

We overcome these empirical problems by exploiting a quasi-exogenous regulatory change in the terms of trade credit that a large buyer can demand from thousands of its suppliers, and by collecting proprietary panel data on *all* of a large retailer's procurement relationships both from external suppliers and from internal, wholly-owned subsidiaries. Trade credit, or delayed payment of intermediate goods, is one of the most prevalent financial contracting features in procurement relationships (Petersen and Rajan (1997); Cuñat and Garcia-Appendini (2011)) and global trade (Antras and Foley (2014)). A large literature is devoted to understanding observed patterns in the terms of trade credit conditional on observing a sale (i.e., the intensive margin).[2]

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

[2]See Ng, Smith, and Smith (2002), Fisman and Raturi (2004), Fabbri and Klapper (2008), Giannetti, Burkart, and Ellingsen (2011), Klapper, Laeven, and Rajan (2012), Murfin and Njoroge (2012), Costello (2014), among others

But due to the empirical difficulties listed above, much less is known about the role trade credit and other financial contracts may play to facilitate the *existence* of those relationships (i.e., the extensive margin).[3] To our knowledge, we are the first to provide causal evidence that financial contracts affect the organization of the supply chain and the boundaries of the firm.

The large buyer in our empirical analysis is one of two large supermarket chains that operate in Chile (the "Supermarket"). In December 2006, the Chilean government entered into an accord with the Supermarket (the "Agreement") that restricted the maturity of the trade credit contracts with its small suppliers to at most 30 days–previously, typical terms were 90 days. Small suppliers, which per the Agreement were defined by a yearly revenues cutoff of 100,000 UF[4]–roughly $4.0 million–represented 1,280 of the Supermarket's 1,902 suppliers. According to press reports, the Agreement was enacted in response to government fears that large retailers were exerting monopsonistic power over small firms. The Supermarket agreed to this change because the government threatened to deny regulatory approval on its future acquisitions. A similar regulatory change was imposed on the other large supermarket chain one year later.

We use proprietary data obtained from the Supermarket and the Chilean tax authority to study the effects of this change on the Supermarket's supply chain. The Supermarket data details all purchases made between 2006 and 2011 at the month - product - supplier level (e.g., soccer balls sold by firm A in July 2007), while the tax authority data provides information on total firm revenues and regulatory status (i.e., affected or unaffected by the Agreement). In our baseline empirical strategy, we compare changes in the procurement of each product sold by Treated (affected) firms before and after the Agreement relative to the *same* product category–as defined by the Supermarket–but sold by Control (unaffected) firms. We control non-parametrically for firm size by

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[3]An exception is Barrot (2013), who studies firm entry into the trucking industry following a restriction to the terms of trade credit in France.

[4]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 US$40.

focusing on firms whose 2006 yearly revenues were within a relatively tight range above and below the cutoff.[5]

We find that the restriction to the set of feasible contracts makes trade less likely. The probability that an affected supplier sells a product to the Supermarket falls by 11 percentage points relative to the same product sold by an unaffected supplier after the Agreement. Further, the Agreement makes vertical integration more likely. The probability that the Supermarket procures from a wholly-owned subsidiary increases by 3 percentage points (from a baseline of 17%) for products that were mostly procured from Treated firms (above-median levels) before the Agreement. Interestingly, the total procurement quantity of products that were mostly purchased from affected firms is reduced after the Agreement. We interpret this as evidence that by shifting procurement to its subsidiaries, the Supermarket is not fully able to replicate the pre-period market equilibrium. That is, the vertical integration stemming from the Agreement is costly (consistent with Baker, Gibbons, and Murphy (2001)).

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 nor 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 within each Treated firm off of differential exposure to the Agreement by product type. In this specification, the likelihood of observing trade is lower for products that compete mostly with Control firms. 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.

Our results suggest that trade credit is used as a lever to overcome contracting frictions rather than as a manifestation of a financing advantage for intermediation. We find support for two potential sources of such contracting frictions. First, by delaying payment, trade credit may give buyers enough time to verify product value. In turn,

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[5]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.

this gives borrowers incentives to perform an unobserved (to the buyer) investment that increases the value of the product (e.g., to provide effort to improve the good's quality as in Smith (1987), Long, Malitz, and Ravid (1993), Chen, Hong, Jiang, and Kubik (2013), or to do value-enhancing, relationship-specific investments as in Cunat (2007)).[6] The critical friction is that contracting on ex post value is not feasible, for example, because it is hard to demand compensation for low quality goods once the supplier has been paid. Consistent with this hypothesis, we find that the likelihood of purchasing from Treated firms after the Agreement is lower for "marginal" products, which are those held in inventory by the Supermarket between 30 and 90 days (1 to 3 months). For these products, in the pre-period the Supermarket could delay payment until all of the inventory was sold and information about the good was fully revealed. After the Agreement, such an arrangement is no longer feasible. Also consistent with this hypothesis, we document that the effects of the Agreement are mitigated for firms that had access to factoring of accounts receivables at some point during our sample period. Because these firms were able to obtain up-front payment for the goods sold, delayed payment provides no incentives to suppliers.[7]

The second friction that trade credit may overcome is that the Supermarket may be unable to pay different prices to suppliers of the same product. For example, Article 3 in the Chilean Competition Act prohibits "unfair" prices seen as a manifestation of dominant market positions.[8] Then, the Supermarket may be able to extract more of the relationship surplus by demanding that suppliers bear the financing cost required from extending longer payment terms (e.g., Fabbri and Klapper (2008), Barrot (2013)). Consistent with this hypothesis, we find that the effects of the Agreement are most negative for products in which different suppliers charge similar prices for the same product. Note that while trade credit does allow the Supermarket an additional means

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[6]Note that alternative contracts, such as a commitment from the supplier to return money if the good's value is low, are harder to enforce ex post. We reviewed the Supermarket's boilerplate procurement contract used with suppliers. It does not establish explicit payments or warranties, but it does stress the Supermarket's concern over the supplier's unobserved actions.

[7]We interpret the factoring results with caution, as we are unable to observe exactly when the firm factored its receivables. Ideally, we would have a predetermined measure of factoring.

[8]E.g., see http://www.oecd.org/daf/competition/47950954.pdf.

of paying different prices to different suppliers, it is likely to be inefficient. If suppliers have a higher cost of financing than the Supermarket (which is almost surely the case), then the most efficient way to distribute the surplus is by bargaining over the price alone.

Finally, Baker, Gibbons, and Murphy (2002) point out that the value of the future relationship may help sustain trading relationships when formal contracts are restricted. Consistent with this idea, we find that the negative effects of the Agreement on the likelihood of observing trade are significantly mitigated for suppliers who sell mostly to the Supermarket and for suppliers who have a large product market share. In these cases the relationship is valuable for the supplier and the Supermarket, respectively (as in McMillan and Woodruff (1999) and Giannetti, Burkart, and Ellingsen (2011)). These results are also consistent with anecdotal evidence that the Supermarket and its competitor often require customized packaging, which increases the value of the relationship for the parties. The effects of the Agreement are also attenuated for high-surplus products for which the Supermarket charges a large mark-up.[9] These results suggest relational contracts help mitigate the absence of contracting levers ex post. However, they are also consistent with the fact that suppliers with valuable relationships with the Supermarket might already benefit from shorter payment terms ex ante (e.g., Antras and Foley (2014)). In that case, the acceleration in payments should not affect them.

Our paper is closely related to the literature on contracting and the boundaries of the firm. Related papers include Baker and Hubbard (2004), who study how the introduction of a monitoring technology, which expanded the contracting space, influences the decision to vertically integrate, Fresard, Hoberg, and Phillips (2014) and Seru (2014), who empirically link vertical integration to innovation, and Chen, Hong, Jiang, and Kubik (2013) who study organizational form in the mutual fund industry. Acemoglu, Johnson, and Mitton (2009) and Macchiavello (2012) study the relationship between the contracting environment and vertical integration in a large cross section

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[9]We do not observe the length of the relationship, which is a commonly-used proxy for its value.

of firms of 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 paper is also related to an empirical literature that tests competing theories of trade credit. In particular, our work is complementary to the evidence provided by Barrot (2013), who exploits a natural experiment to show that trade credit can act as a barrier to entry for financially constrained suppliers. We extend this work by studying the implications of removing this contracting lever on procurement relationships and the boundaries of the firm. Further, our work is related to Murfin and Njoroge (2012), who investigate the effects on the capital structure and investment policies of small firms when their buyers have different payment policies. We extend this work by exploiting an exogenous source of variation in the large client's available payment policies stemming from external regulation, and by focusing on the extensive margin (i.e., whether a relationship is actually observed) rather than on the intensive margin (i.e., firm investment conditional on supplying to the buyer).

We continue with a description of the data and the empirical setting in Section II. We present our identification strategy in Section III. Section IV presents our main results, and Section V shows heterogeneous treatment effects to study the underlying frictions and relational contracts. Section VI concludes.

II. Empirical and Conceptual Setting

A. The supermarket 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 the Chilean supermarket 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 (2012)). 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 supermarket retailers, which we denote as the "Supermarket." Through a series of aggressive acquisitions and organic growth, these two supermarket chains accounted for 63% of total supermarket revenues in 2006.[10] 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 investigated their business practices. In August of that year, the agency issued a report that articulated these concerns and prompted the two large supermarket chains to modify the terms of their relationships with small suppliers.[11] At the time, it was standard practice for the two large buyers to demand trade credit terms of 90 days from their small suppliers, and the agency feared that this was one symptom of monopsonistic market power.[12] 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 supermarkets agreed to modifications in their contracting practices, the Supermarket in December 2006 and its large competitor in July 2008. The Supermarket implemented this change beginning in January 2007.

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

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

[12]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.

Under the Agreement, the Supermarket could not enter into trade credit contracts with its small suppliers with a maturity beyond 30 days. 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. For existing suppliers:

(1) Total sales to all clients in the last 12 months of no more than 100,000 UF.[13]

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

In practice, firm eligibility is almost entirely driven by total sales to all clients: there is only one supplier with total sales below 100,000 UF who sold more than 60,000 UF to the Supermarket.

The Agreement had wide-ranging applicability: 67% of the Supermarket's suppliers from 2006 (by number) satisfied both of the criteria and became subject to the Agreement.[14] Further, the Chilean government has actively monitored the Agreement's implementation since it was put in place. Publicly available reports suggest the Supermarket has indeed complied with the shorter payment period for small firms, and the Supermarket has explicitly avoided any actions that could be construed as forcing suppliers to extend longer days payable.[15]

We use the implementation of the Agreement as a source of quasi-exogenous variation in the set of available financial contracts between the Supermarket and its suppliers. We compare firms whose 2006 sales were just above and just below the threshold, before and after the Agreement was in place. It is therefore important for identification purposes that the passing of the Agreement did not coincide with other policies or events affecting exactly the same set of firms. Similarly, identification requires that

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[13]Recall that the "UF" is an inflation-linked currency unit updated daily. Total sales were accredited by sales tax information.

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

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

the regulator did not choose the cutoff based on differential business trends for firms with sales above and below the 100,000 UF cutoff.

The exact timing of the Agreement with the Supermarket was largely a surprise. Recall that the Supermarket's main competitor took similar steps to modify its payments practices, but only in July 2008. For transparency and ease of monitoring, the regulator chose fixed sales cutoffs to determine eligibility. However, in the 2008 agreement with the competitor, the eligibility thresholds were different, suggesting no precise patterns immediately around the chosen cutoffs.

Further, there is no evidence that there were other laws or regulations put into place at the same time affecting firms at exactly the 100,000 UF sales cutoff. Figure 1 shows trends in the universe of Chilean firms based on levels of 2006 sales just below ("Treated") and above ("Control") the 100,000 UF cutoff.[16] Treated and Control firms correspond to 1.9% and 5.3% of the total universe of firms of the same size in Chile in 2006 by number. The figure shows that there were similar changes in the number of firms, number of employees, total sales and total wage bill 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, for example, other regulatory changes. Also, the effects of the Agreement were not large enough to affect aggregates at the country-level.

C. Why should the Agreement affect the organizational form of the Supermarket and its supply chain?

Publicly available press reports suggest that, at the time the Agreement was put in place, the regulator was concerned about the Supermarket's size relative to its suppliers. According to these reports, the regulator hoped that, by reducing the maturity of trade credit contracts, the Agreement would strengthen the bargaining

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[16]We define firms using total revenues intervals, 25k UF to 100k UF for Treated firms and 100k UF to 600k UF for Control firms. This definition is consistent with our empirical strategy, which we present in Section III.

positions of small suppliers. This suggests that the impacts of the Agreement were expected to be found at the *intensive* margin, for example, increased revenues for existing clients. Further, the increase in revenues would allow smaller firms to invest more. However, these positive effects are predicated on the idea that there would be no compensating change in the *extensive* margin of the relationships. Which effect dominates, more revenues and investment or less trade, depends on the nature of the underlying frictions that lead to the use of this contracting lever–trade credit– in the first place. Here, we discuss what the implications of the Agreement might look like under alternative theories for those frictions. Importantly, we are not proposing a new theory of trade credit. Rather, we discipline our empirical analysis by developing testable implications of various existing theories.

Asymmetric information plagues trading relationships across many markets. In supply chain relationships, asymmetric information may stem from unobserved actions required by the buyer to ensure product quality or some other dimension of value. If ex post penalty payments are not contractible ex ante, trade credit may allow suppliers to "post a bond" to ensure high quality.[17]

If long payable terms are indeed acting as an incentive mechanism, then restricting the set of feasible contracts along this dimension should make both buyers and suppliers weakly worse off. In particular, the Agreement should be associated with less trade on the extensive margin, as well as lower quality inputs and prices. Further evidence for this particular friction would be related to the time until a good's unobserved value is revealed. We assume that some fraction of a product's unobserved value is revealed when it is sold. Thus, given that the Agreement shortened the time to verify from 90 days to 30 days, the effects should be more pronounced precisely for goods that sit in inventory until they are sold for an average of 30 to 90 days. Also, if the quality of production by subsidiaries versus external suppliers is easier to observe, then we should expect to observe an increase in vertical integration for affected products (Baker,

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[17]See Smith (1987), Long, Malitz, and Ravid (1993), Lee and Stowe (1993), Cunat (2007), Kim and Shin (2012), Chen, Hong, Jiang, and Kubik (2013) for the theory, and Murfin and Njoroge (2012) and Antras and Foley (2014) for consistent empirical evidence.

Gibbons, and Murphy (2002)). In the Online Appendix (available upon request) we provide a simple model to illustrate these predictions. Finally, if suppliers were able to factor their accounts receivables before the Agreement, then the incentives provided by trade credit would have been weaker. Thus we hypothesize that firms with prior access to factoring should be less affected by the Agreement.

A second source of contracting frictions may stem from an inability of the Supermarket to pay different prices to different suppliers (Petersen and Rajan (1997) and Giannetti, Burkart, and Ellingsen (2011)). A large buyer with relatively high bargaining power may want to lower spot prices paid to its smaller suppliers, but there may be regulatory constraints to actually doing so. For example, some clauses of the Chilean Competition Act may prevent large buyers from exerting their bargaining power through anti-competitive prices.[18] In that case, the large buyer may use other contracting levers, such as trade credit, to reduce the effective price paid to its suppliers.

Note that in our setting the easiest way for the Supermarket to exploit its market strength is through negotiating on price alone. The Supermarket 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 Supermarket. 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)). Given this, asking suppliers to provide financing is likely to be inefficient.[19]

If trade credit allows the Supermarket to exert its bargaining power, the effects of the Agreement are ambiguous and depend on whether trading relationships are maintained (i.e., on the extensive margin). If affected firms are able to sell to the Supermarket after the Agreement, then being paid earlier would mean they should be better off. That is, conditional on observing trade, which is an endogenous outcome, affected

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[18]In fact, this was the type of friction that the regulator had in mind when implementing the Agreement.

[19]The Supermarket could be better off reducing the price paid to the supplier, reducing the amount of trade credit taken from the supplier, and instead borrowing directly from the financial market.

firms would be better off than unaffected firms. Note that this may have been the regulator's objective. However, the Agreement also means that the Supermarket must pay a higher effective price to small firms. For some relationships, this may simply be too much and result in severing the relationship. In some cases, the Supermarket may prefer to vertically integrate.

We conclude this section by noting that regardless of what friction drives the use of this contracting lever, the value of the relationship may help mitigate the negative effects of the Agreement at the extensive margin. One commonly used proxy for value of the relationship is its length, which we don't observe. However, relationships are more valuable to the seller when most of its sales are concentrated with the buyer, and more valuable to the buyer when finding another vendor is harder and when the final price to consumers has a large mark-up over the transfer price. In these cases, we expect to see that the negative effects of the Agreement are mitigated.

III. Empirical Strategy

Our goal is to understand what happens to trading relationships and to the boundary of the buyer firm once financial contracting tools are no longer available. Measuring the causal effects of contracting tools on real firm outcomes is difficult, especially using cross-sectional data. Contractual terms between buyers and suppliers are the result of an endogenous bargaining process. For example, firms with high-quality products may be both less financially constrained and more sought-after by buyers than poor quality firms, leading to more trade credit and more trade. However, there are many reasons why this relationship may not be causal. Other characteristics such as length of trading relationship and cost of capital may also be co-determined with trade credit and real outcomes. Even worse, it is often difficult to observe counterfactual contract terms in standard cross-sectional or panel data sets. For example, within a vertically-integrated firm, it is impossible to see the latent contract offer that the firm would be willing to give to an external supplier.

A. Identification Strategy

It is rare to observe quasi-exogenous changes to the set of feasible contracts. We propose to use the Agreement as such a source identifying variation. Given that the Agreement occurred by surprise and affected only a subset of firms, it is an ideal candidate for a Differences-in-Differences identification strategy.

We are interested in how the Agreement may have affected bilateral trading relationships and also the decision of the supermarket itself to vertically integrate. To examine changes in the product sale margin with external suppliers, we run regressions at the firm×product×year level. The chief outcome of interest, $y_{i,j,t}$ is whether firm i sells product j in year t. However, we also consider revenues and prices paid to firm i for selling product j in year t. The causal treatment variable $A_{i.t} = 1(t > 2006)*1(treated_i)$ is formed by interacting firm eligibility for the Agreement $1(treated_i)$ with an indicator for whether the Agreement is in place in year t. The resulting differences-in-differences specification is:

$$(1) \qquad y_{i,j,t} = \omega_{i,j} + \omega_{j,t} + \beta A_{i,t} + \varepsilon_{i,j,t},$$

The coefficient of interest β measures the causal effect of the Agreement on our outcomes $y_{i,j,t}$.

The exogenous variation from the Agreement is at the firm×time level. Therefore, identification requires that Treatment and Control firms would have evolved in a parallel fashion in absence of the Agreement. In order to make the Treatment and Control samples as comparable as possible, we limit our analysis to firms falling in a narrow range of the 100,000 UF 2006 total revenues cutoff. In particular, we define our "main sample" as firms with 2006 revenues between 25,000 UF and 600,000 UF (roughly US\$1.0 million to US\$24 million). Treated firms are those with total 2006 revenues between 25,001 UF and 100,000 UF (US\$1.0 million to US\$4.0 million), and Control firms as those with total 2006 revenues between 100,001 UF and 600,000 UF

(US\$4.0 million and US\$24 million–except for one firm that sold less than 100,000 UF but sold more than 60,000 UF to the Supermarket. Results are unchanged by the treatment status of this particular firm). We further explore the parallel trends assumption in Section C below.

One might worry that firms of different sizes sell different product mixes. Thus, any differential effect would be explained by heterogeneous trends across products. To remove this composition effects, we include firm×product ($\omega_{i,j}$) and product×time ($\omega_{j,t}$) fixed effects in all specifications of Equation 1. Therefore all of our treatment effects are identified using within-product variation, that is, comparing the same product sold by both Treated and Control firms before and after the Agreement.[20]

Finally, note that after 2006, the sample of firms affected by the Agreement did change, perhaps endogenously, based on total sales in prior 12 months. 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.[21]

In order to estimate the effects of the Agreement on the decision for the Supermarket to vertically integrate, we focus on two main outcomes at the product-year level, $y_{j,t}$: whether the Supermarket procured good j from one of its own subsidiaries in year t, and the overall number units procured of good j in year t. The first outcome, procurement from a subsidiary, measures vertical integration, while the second, units sold, allows us to estimate the effect of the restriction of trade credit on the overall procurement of the product.

We run the following regression:

$$(2) \qquad y_{j,t} = \alpha_j + \delta_t + \beta 1\,(t = post) \times treatment\,intensity_j + \epsilon_{j,t}$$

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[20]The full set of fixed effects is quite large. We use the methodology in Gormley and Matsa (2014) for regressions with two high-dimensional fixed effects, implemented using the REG2HDFE Stata command (see Guimaraes and Portugal (2010)).

[21]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.

In these specifications, we compare outcomes for products that were more or less affected by the Agreement before and after the Agreement was put into place. To do this, we construct the variable $treatmentintensity_j$, which is a measure of the fraction of procurement of good j made by the Supermarket from all Treated firms in 2006. The coefficient of interest, β, measures the relative effect of the Agreement on product-level outcomes for products that were procured entirely by Treated firm in the pre-period, relative to products that were procured entirely by Control firms in the pre-period.

Finally, we exploit our data to estimate heterogeneous effects of the Agreement. We use this framework to study the source of the contracting friction for trade credit, as well as the extent to which relational contracting may mitigate the lack of contracting levers. Formally, we augment regression (1) with an interaction variable, $interaction_{i,j}$, that varies at product i and firm j level,

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\begin{aligned}
(3) \qquad y_{i,j,t} = {} & \beta 1\left(t = post\right) \times 1\left(i \in Treated\right) \\
& + \gamma 1\left(t = post\right) \times 1\left(i \in Treated\right) \times interaction_{i,j} \\
& + \omega 1\left(t = post\right) \times interaction_{i,j} + \alpha_{j,t} + \omega_{i,j} + \epsilon_{i,j,t}.
\end{aligned}
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The coefficient γ on the variable $1\left(t = post\right) \times 1\left(i \in Treated\right) \times interaction_{i,j}$ captures the differential effect of the Agreement for Treated firms with different value of $interaction_{i,j}$. In section V we show the results of this regression using different definitions of the $interaction_{i,j}$ variable.

B. Data

We obtain from the Supermarket a proprietary data set that summarizes all the transactions with its suppliers, including fully-owned subsidiaries, between January 2006 and August 2011, and contains observations at the supplier-product-month level. This data allows us to see whether a supplier maintained the trading relationship with the Supermarket and whether prices and revenues changed as a result of the Agreement.

The data does not contain days payable for each transaction or other terms of the trade credit contracts. Hence, we are not able to test directly the first stage of the Agreement. We rely on discussions with managers at the Supermarket, who stated the Agreement was fully implemented, as well as contemporaneous press and government reports stating that the 30 days payment policy was found to be effective. The product-level information also provides scope to compare the heterogeneous effects of restricting trade credit terms over different types of products or supplier-Supermarket relationships.

Our data comes entirely from the Supermarket. Thus, we do not directly observe the balance sheets of the suppliers and we cannot measure their financial constraints. However, because the Supermarket is able to observe which suppliers factor their accounts receivable we do observe this variable in our data set, in particular because the Supermarket has to pay the factoring agent rather than the supplier. Finally, 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 where affected by the Agreement's as per the restriction on total revenues to all customers.

We provide some summary statistics. Table 1 shows descriptive statistics at the firm level for our main sample of suppliers during 2006, before the Agreement. The sample includes 734 firms, 342 Treated and 392 Control. Panel A shows yearly statistics at the firm level. On average, firms in our sample had yearly revenues to the Supermarket of approximately US$753 thousand. The median firm in the sample has 1 department, the broadest product categorization used by the Supermarket, 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. However, the median Control firm sold the same number of products as the median Treated firm during 2006 (three). About one in four suppliers, both Treated and Control, had access to factoring at some point in the sample (the difference is not statistically significant.

Table V (Panel B) provides sample statistics at the firm-product level during 2006 for our sample of firms. The table shows that Treated firms sell less (in $ and units) of each of their products than Control firms. Interestingly, the average price and Supermarket margin of products sold to final customers, measured as a percentage markup over the price paid to the supplier, is similar across both groups (and, based on a simple hypothesis test, not statistically different in both cases). Finally, products sold by both Treated and Control firms were held in inventory for at most 2.8 months on average.

C. Graphical evidence

The identification assumption for regression (1) is that absent the Agreement and conditional on the product times time and firm times product fixed effects, the probability of making a sale of the same product of Treated and Control firms would have evolved in parallel fashion. We provide evidence that supports our identification assumption and present our main results graphically in Figure 2. The figure shows the quarterly average of a dummy that equals one if there is a sale for Treated and Control firms. The figure is de-trended with one common linear trend across Treated and Control firms. There are no noticeable differences in the trends of Treated and Control firms during 2006, before the Agreement was put in place.[22] Further, the graph shows that there are no differential pre-trends unconditionally. The identification assumption we make 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 main result: after 2006 Treated firms exhibit a lower probability of procuring to the Supermarket. 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 only suggests a causal effect of the Agreement.

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[22]This is also true statistically speaking. Tables available upon request.

In Figure 4 we provide similar evidence, but for our investigation of changes in vertical integration and procurement of the Supermarket. The identification assumption required by Equation 2 is that products with a relatively low and high 2006 market share of Treated firms would have evolved in a similar fashion absent the Agreement. The patterns in Figure 4 are consistent with this assumption. Splitting the sample of products by the median 2006 market share of Treated firms suggests no differential pre-trends. Further, the figure also hints at our second result: after the Agreement, the relative incidence of *ownbrand* (i.e., sourcing from a subsidiary) seems to increase for products sold mostly by Treated firms relative to products sold mostly by Control firms.

IV. Results

A. Bilateral Relationships

We first present results on bilateral trading relationships, estimated using Equation 1. Column 1 of Table 2 report causal effects on the outcome "makes sale", a variable that equals one if the supplier records a sale to the Supermarket during the period (pre- or post- period as defined above). The coefficient on the interaction $post \times treated$ shows that Treated firms are approximately 11% less likely to sell any given product to the Supermarket following the Agreement, relative to before the Agreement. Thus, the Supermarket chooses to shift purchases away from suppliers when payment days are capped at 30 under the Agreement.

Our focus is on the effects of the availability of contracting levers on the extensive margin of trade. However, suppliers could also adjust through other margins, namely prices. Column 2 shows that the log of the procurement price decreases 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. Note that the magnitudes of the price changes appear on the surface to be larger than a reasonable 60-day interest rate for external financing for the Supermarket. For example, the 3.8% price reduction is equivalent to an annualized interest rate of 23% from the point of view of the Supermarket. In comparison, the Chilean banking sector's reported yearly rates for the same period are 7% to 11%.[23]

We combine the evidence on the extensive margin and prices to examine effects on $log\left(revenues\right)$. 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.

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 (USD24 million) are labeled as Treated-placebo, while firms above that threshold are Control-placebo (this includes firms with total revenues of UF 100,000 to UF 600,000 (USD4.0 million to USD24 million) as Treated-placebo, and UF 600,000 and higher as Control-placebo). The placebo sample has 389 Treated-placebo firms and 230 Control-placebo firms, which correspond to 52,668 Treated-placebo product-firms and 66,540 Control-placebo product-firms sold during 2006. This split and sample selection assures that the placebo test has a similar level of power as our main regression specifications.[24]

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[23]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.

[24]Further statistics om the sample of placebo firms is available upon request.

We find that the coefficient on makes sale (Column 4) is slightly negative but insignificant. 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 Supermarket 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 (though we could find no evidence of such events) 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 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 did affect the Supermarket's likelihood of purchasing from an external supplier, its effects on Treated firms should be more pronounced for those products where Control suppliers had a higher market share. That is, the likelihood that a Treated firm loses orders from the Supermarket after the Agreement should be higher if the firm's competitors were largely unaffected Control firms. Thus, we estimate the following regression model on a sample restricted to all Treated firms that sold to the Supermarket during 2006:

$$y_{i,j,t} = \beta 1 \left(t = post_t\right) \times treatment intensity_j + \alpha_{i,t} + \omega_{i,j} + \epsilon_{i,j,t},$$

where $treatment intensity_j$ is defined as in regression (2). The coefficient on $1\left(t = post_t\right) \times treatment intensity_j$ represents the average effect of the Agreement depending on the treatment status of the competitors. Under our hypothesis, $\beta < 0$. The firm×time fixed effect, $\alpha_{i,t}$, absorbs any differential trend of small versus large firms.

The results are presented in Table 3. Column 1 documents that within Treated firms, the effects of the Agreement at the extensive margin *within Treated firms* are mitigated for products that compete mostly with Treated firms. This suggests that our results are not simply capturing heterogeneous survival probabilities for firms of different sizes. Column 2 shows a similar effect in prices, and Column 3 reveals a positive, but insignificant effect on revenues. Note that to estimate this effect, we only identify off of those firms selling both a low and a high treatment-intensity product. Columns 4 through 6 run the same tests but replace $treatmentintensity_j$ with a dummy that equals one for products with a higher than median fraction of procurement from Treated firms. The results are essentially unchanged. While this strains the power of the test, 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.

B. Vertical integration and procurement of affected products

One virtue of our comprehensive data set is that we can ask how both the external and internal procurement of inputs change because of the Agreement. The empirical tests based on equation (2) are presented in Table 4. In column 1, 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. 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 median market share of Treated firms in the pre-period (9.7%). We run the same regression as in equation (2) but replace the $treatmentintensity_j$ variable with $hightreatment_j = 1\,(treatmentintensity_j > p50)$. The results are shown in Column 2 and suggest that products in which Treated firms have a market share above the median see a reduction in procurement of 4.8% of a standard deviation, or approximately 260 units fewer than the average normalized mean of 5,442 for products sold by firms in our sample during 2006.

We interpret this result as follows: the Supermarket must pay a cost to either vertically integrate or shift purchases to non-affected suppliers. This cost results in a reduction in the total number of units purchased. 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 test for whether the restriction in the set of feasible contracts made the Supermarket more willing to vertically integrate. Column 3 shows the regression results when the outcome is an indicator of a purchase from a subsidiary. The positive coefficient suggests that faced with the restriction in days payable, the Supermarket does indeed choose to procure via internal subsidiaries rather than continue to buy from some Treated firms. We repeat the regression but change the interaction variable to $hightreatment_j$ as defined above. The results of this regression are shown in Column 4 of Table (4), and show that the Supermarket is roughly 3% more likely to shift procurement to an internal subsidiary for products that were mostly sold by Treated firms before the Agreement.

V. Heterogeneous Treatment Effects and Frictions

The evidence presented so far suggests that the availability of financial contracts enables trade. When faced with a restriction of the contracting space, firms prefer to shift procurement or vertically integrate. As argued above, suppliers in our sample are most likely more financially constrained and face higher marginal borrowing costs than the Supermarket. Thus, we interpret these results as implying that trade credit is used as a lever to overcome contracting frictions between suppliers and the Supermarket. Here we use the framework presented in section III to study the underlying friction that trade credit addresses.

A. Information Asymmetries and Limited Commitment

Recall that information problems may be partly mitigated by longer trade credit terms. For example, trade credit can allow suppliers to post a bond to guarantee the quality of their products. We posit that removing access to financial contracting tools should matter more for products whose unobserved value is revealed after the allowable 30-day payment under the Agreement. Thus, because the Agreement changed payment days from 90 to 30, we would expect to see the strongest effects of the Agreement among products that are sold within 30 to 90 days after being delivered.

In the absence of actual information on inventory holdings, we construct a proxy variable called "inventory months" that tracks the maximum number of months that a product purchased in 2006 was held in inventory by the Supermarket before it was sold. This variable is constructed by calculating, for each product-firm, how many months of sales to final customers does each purchase to suppliers during 2006 represent. We then define inventory months at the product-firm level as the maximum of this variable across all months of 2006.

We define three subsamples by "inventory months": products that are held in inventory by 1 month or less, products that are held between 1 and 3 months, and products that are held for more than 3 months. We first run our main regression (1) separately on each subsample. Figure 3 plots the coefficients of the interaction of $1\left(t = post_t\right) \times 1\left(i \in Treated\right)$ for each subsample. Consistent with our model, we see that the largest effect of the Agreement is on products with inventory months between 1 and 3 months, the "marginal" products.

Columns 1 and 2 in Table 5 present the results when the $interaction_{i,j}$ variable from regression (3) is a dummy that equals one for products with inventory months between 1 and 3, $inventory\,months\,1-3_{i,j}$. Column 1 presents β (the "base" effect on non-marginal products) and γ (the additional effect on "marginal" products) when the outcome is "makes sale". As in our main results, there is a strong negative effect for Treated firms after the Agreement, and this effect is larger for firms selling marginal products. The combined effect for marginal products is a decrease of 16%, which is highly significant.

Note that the coefficient γ on $1\left(t = post_t\right) \times 1\left(i \in Treated\right) \times interaction_{i,j}$ is not significant in Column 1, but neither is the main $1\left(t = post_t\right) \times 1\left(i \in Treated\right)$ effect for the non-marginal products. Column 2 shows that the effect of the Agreement is significantly different for the outcome $log\left(revenues + 1\right)$. That is, overall revenues for Treated firms fall more for firms selling products with inventory months between 1 and 3 months, relative to products with 1 or less inventory months and more than 3 inventory months. Again, the p-value of the sum suggests the sum of both coefficients is significantly negative.

We provide more indirect evidence of this mechanism by studying the factoring behavior of suppliers. We posit that the incentive effect of delayed payment would disappear if firms could factor (securitize) their receivables. In our data we observe whether a supplier factored its account receivables at any point during our sample period. Interestingly, only one in four suppliers did factor their receivables at any point in time, and this number is almost equivalent (and not statistically different) for Treated and Control firms. This suggests that there are relatively high costs (implicit or explicit) to adopting this source of financing in Chile (Klapper (2006)). Columns 3 and 4 of Table 5 show the formal regression tests. We use as interaction the dummy *no factoring*, which equals one if the firm never factored its account receivables. Consistent with our proposed mechanism, the table shows that the effects of the Agreement are exacerbated for firms that never factored their receivables. This is confirmed with the p-value of the sum of both coefficients, which is negative and highly significant.[25] Overall, we interpret these results as suggestive evidence that trade credit is used by the Supermarket as a contracting tool to delay payment until value becomes observed when ex post penalties for low value goods are not enforcible.

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[25]We cannot rule out the possibility that this result is driven by unobservable firm quality that is correlated both with access to factoring and preferential terms with the Supermarket. We would ideally obtain a panel of factoring data throughout time, but the Supermarket is unable to share such information with us.

B. Price discrimination

We study whether trade credit is used as a lever to overcome the Supermarket's inability to charge different prices within the framework of regression (3). We define $price\ dispersion_j$ as the within-product standard deviation of prices across suppliers, and we define the interaction variable as a dummy that equals 1 if the product's $pricedispersion_j$ is lower than the sample median. Summary statistics for $pricedispersion_j$ are shown in Table 1. Thus, the coefficient on $1\,(t = post) \times 1\,(i \in Treated) \times interaction_{i,j}$ measures the differential effect of the Agreement for products with similar prices across suppliers. Columns 5 and 6 of Table 1 show the regression results. Column 5 shows that the probability that a Treated firm sells a product to the Supermarket is significantly lower among products with low price dispersion. Total revenues are also lower, as shown in Column 6, but not significantly so. However, in both cases the p-values of the sums of $\beta + \gamma$ are negative and highly significant, suggesting that the effects of the Agreement were most negative among products where the Supermarket paid similar prices to all suppliers.

C. Relational contracting

We also use the framework presented in section III to study whether relational contracts may substitute for the availability of long maturity trade credit contracts. Table 6 presents the coefficients β and γ from regression (3) along categories that relate to the value of the relationship. We study three sources of firm and product heterogeneity. First, we posit that the value of the relationship is higher for products where the Supermarket is able to charge a higher mark-up over the price paid to its suppliers. Column 1 shows that the effect of the Agreement is indeed mitigated for products where the Supermarket charges a relatively high mark-up over the suppliers' prices. The coefficient on $1\,(t = post) \times 1\,(i \in Treated) \times meanmargin_j$, where $meanmargin_j$ is defined as the average percentage margin across all suppliers that sold product j to the Supermarket in 2006, is positive and significant at the 10% level. Summary stats

for this variable are shown in Table 1 as "mark-up (%). Column 2 shows that the coefficient is also positive but insignificant when the outcome is $log\,(revenues + 1)$.

Second, suppliers whose sales are concentrated with the Supermarket have a large stake in the survival of their relationship. Columns 3 and 4 of Table 6 show the results of regression (3) when the interaction variable is a dummy for whether the supplier's sales to the Supermarket are higher than the median of its treatment status. Given that treatment is assigned by total revenues to all suppliers, this provides a measure of concentration: firms with sales above the median are firms whose revenues are more concentrated with the Supermarket.[26] The coefficient of the interaction is positive and statistically significant for the extensive margin dummy "makes sale" (Column 4), and positive but insignificant for $log\,(revenues + 1)$ (Column 5).

Finally, the Supermarket has a larger incentive in keeping relationships where the supplier has a relatively large market share because the cost of finding another supplier is higher. Columns 5 and 6 of table 6 show the results of regression (3) when the interaction variable is the supplier's product market share in the pre-period. Both coefficients are positive, and the coefficients on the interaction variable for "makes sale" is significant. Thus, these suppliers are more likely to continue procuring after the Agreement is in place. These results suggest that the value of the future relationship may help sustain trade when the set of feasible contracts is restricted.

VI. Discussion

We show that restrictions to the set of feasible contracts reduce the probability of observing trade between buyers and sellers and increase the propensity of the parties to vertically integrate. In our setting, buyers and suppliers are restricted in their ability to use long payment days in their contracts. We document that when trade credit terms are restricted for small suppliers, they are forced to stop selling their goods altogether,

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[26]i.e. These concentrated firms have low or negligible sales to other buyers.

or sell them at a lower price. Thus, our results shows that financial contracts may play an important alternative role in enabling trade by alleviating frictions.

In our setting, these frictions may arise as a consequence of buyers performing some unobserved investment that affects the value of the good, or because prices cannot be lowered for different suppliers. We also find that firms with valuable relationships are able to continue to supply goods despite the restriction of contractual terms. This evidence is consistent both with the fact that contractual terms may be different for firms with valuable relationships ex ante, and with the fact that these firms are able to overcome the contracting friction ex post. We cannot distinguish these alternatives with our data.

Our results suggest that the reduction in days payable may harm some suppliers while helping others. Given the high degree of concentration in the Chilean market for diversified retail goods, our finding that Treated firms stopped supplying to the Supermarket after the Agreement is likely to reflect decreases in total revenues and thus profits for those firms. On the other hand, firms that were involved in valuable procurement relationships were enabled by the Agreement to continue supplying and receive payment 60 days earlier. If those firms were already incentivized by the value of the relationship, early payment is most likely to be beneficial to them without reducing the value of the good. Further, as suggested by Murfin and Njoroge (2012), firms that are paid earlier may be able to invest more.[27] Thus, the overall welfare effects of the Agreement are unclear.

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[27]In the Appendix we show that those firms that were less affected by the Agreement were able to increase their product offering to the Supermarket.

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Appendix

Appendix A. Figures and Tables

FIGURE 1. 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 2006 revenues between 25k UF and 100k UF, Control firms are those with total 2006 revenues between 100k UF and 600k UF UF. UF ("Unidad de Fomento" is an inflation linked currency unit updated daily, whose value is published by the Banco Central de Chile. The data is publicly available and was obtained from the website of the Chilean IRS (www.sii.cl).
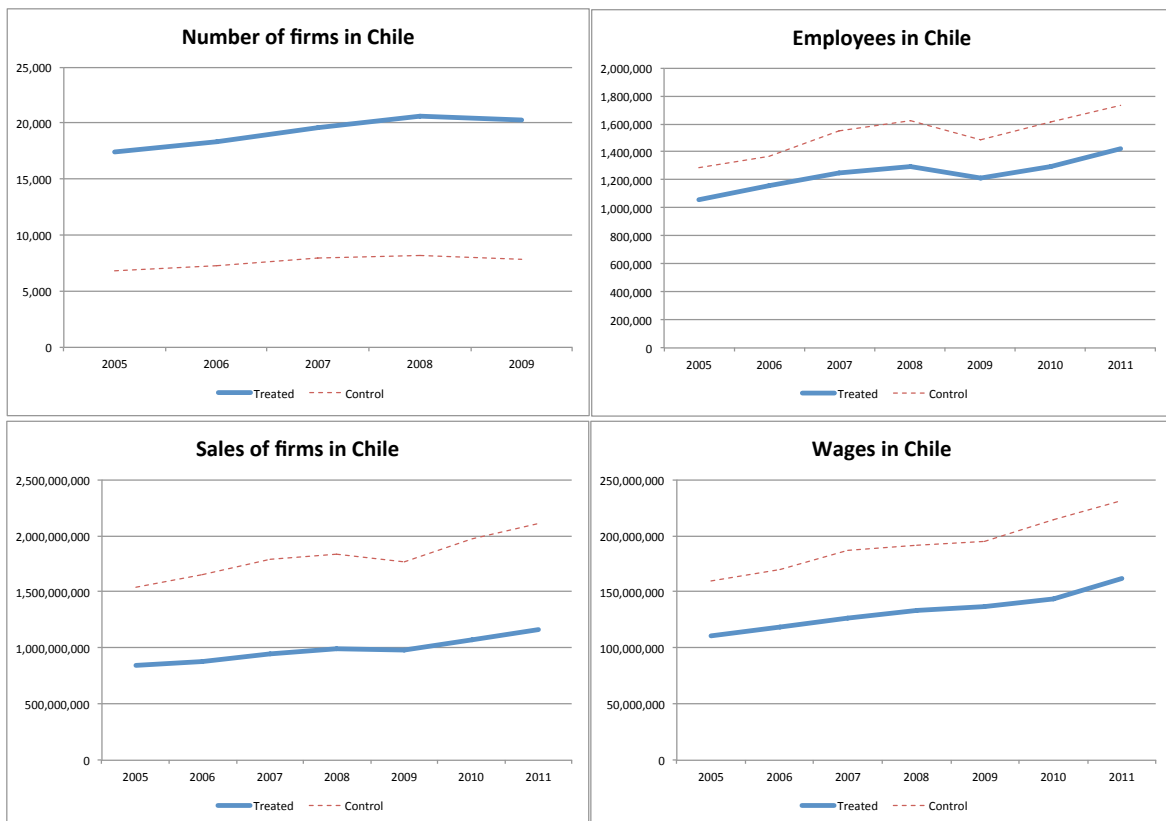
FIGURE 2. Pre-period trends

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 below 100k UF and total 2006 sales to the Supermarket below 60k UF. UF ("Unidad de Fomento" is an inflation linked currency unit updated daily, whose value is published by the Banco Central de Chile. We restrict the sample to those firms with total 2006 revenues between 25k UF and 600k UF.
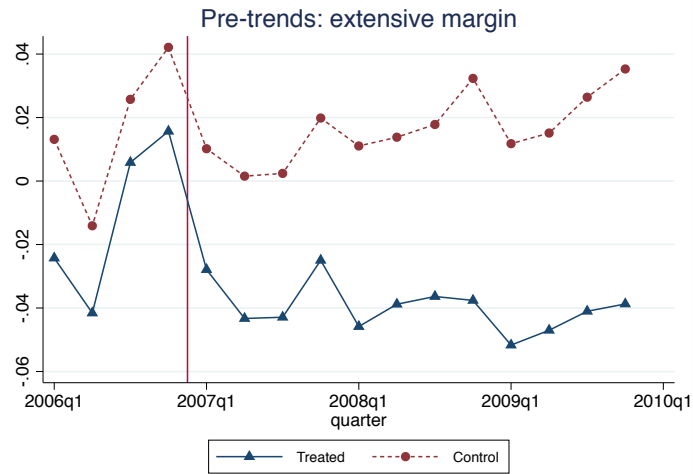
FIGURE 3. Differential effects by inventory months

This figure shows that the effects of the Agreement are strongest for products that during 2006 were held in inventory by the Supermarket between 1 and 3 months, that is, products that are "marginal" with respect to the Agreement's modification of payment terms. The bars correspond to the coefficient on $post \times treated$ of regression (1) when the outcome is "makes sale". Thus, each bar measure the difference between the likelihood that a Treated firm sells a product in the post period relative to Control firms and relative to the pre period for thre subsamples. Each bar groups products based on the maximum number of months, across all months in 2006, that total units purchased to suppliers were ultimately sold to final customers (i.e., maximum number of inventory months): 1 or less months (6,384 observations), more than 1 and up to 3 months (4,468 observations) and more than 3 months of inventory (8,284). All coefficients are significantly different from zero.

Effects by inventory months
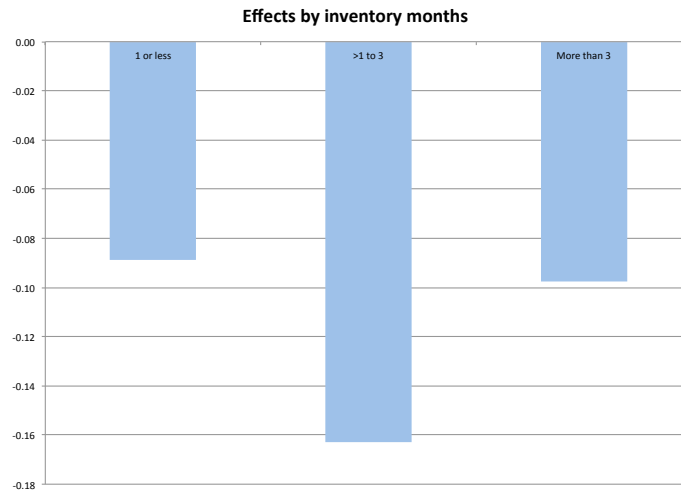
FIGURE 4. Own supplier pre-period trends

This figure shows the pre- and post-Agreement trends of the monthly average fraction of products where the Supermarket was its own supplier. The sample of products is restricted to products sold by firms whose 2006 revenues where between 25k and 600k. The red-X line (blue-circle) corresponds to products in which Treated firms had a market share below (above) the cross section median.

Fraction of products with Supermarket as supplier
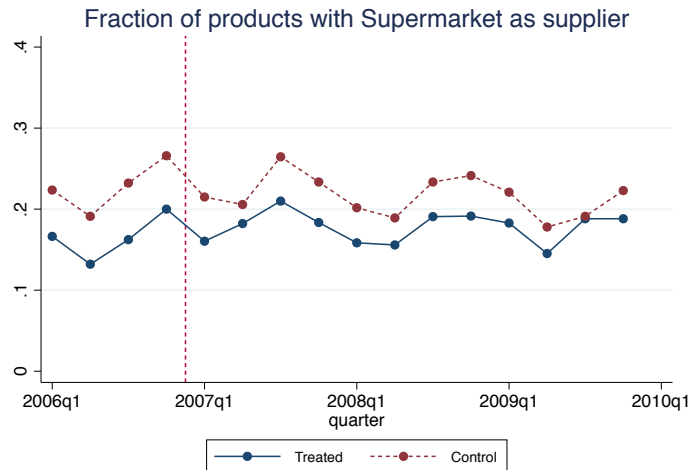
TABLE 1. Summary statistics main sample

This table shows the mean, standard deviation and median of variables for Treated and Control firms as defined above. Panel A shows variables at the firm-level, while Panel B shows variables at the product-firm level.

Panel 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)	13.48	12.42	14.12	12.65	11.84	12.93	13.88	13.00	14.37
# Departments	1.540	1.0	0.917	1.442	1.0	0.651	1.625	1.0	1.092
# Products	6.518	3.0	11.099	4.842	3.0	5.595	7.980	3.0	14.108
factoring (%)	24.17	0.0	42.84	24.93	0.0	43.32	23.50	0.0	42.45

Panel B: Product-firm level 2006 monthly average								
	All (N=6,232)			Treated (N=2,092)			Control (N=4,140)		
	Mean	p50	St. Dev.	Mean	p50	St. Dev.	Mean	p50	St. Dev.
log(price)	6.978	6.908	1.572	6.941	6.961	1.757	6.997	6.888	1.464
log(units)	6.843	6.845	2.195	6.566	6.624	2.158	6.990	6.982	2.201
log(revenues)	13.821	14.023	2.159	13.507	13.710	2.219	13.987	13.987	2.108
mark-up(%)	33.18	30.41	17.24	34.35	33.07	17.62	32.59	29.74	17.00
inventory months	2.85	2.00	2.76	2.86	2.00	2.83	2.84	2.00	2.73
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 contractual outcomes

This table presents the estimated coefficient of interest of regression (1), β, which measures the relative change in the outcome of a product sold to the Supermarket by Treated firms relative to Control firms, before and after the reduction in days payable as per the Agreement. Treated firms are those with total 2006 revenues below 100k UF and total 2006 sales to the Supermarket below 60k UF. UF ("Unidad de Fomento" is an inflation linked currency unit updated daily, whose value is published by the Banco Central de Chile. We restrict the sample to those firms with total 2006 revenues between 25k UF and 600k UF. We exclude products that were not sold during 2006. The placebo sample consists of firms with total 2006 revenues of 100k UF or higher; within this Placebo sample, Treated-placebo firms ($treatedplacebo = 1$) are those with 2006 revenues of 600k UF or lower. UF ("Unidad de Fomento" is an inflation linked currency unit updated daily, whose value is published by the Banco Central de Chile. The outcomes are "makes sale": a dummy that equals one if 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 Supermarket in pesos, with 0 replaced with the log of 1 peso. The data is a balanced panel at the monthly-firm-month level, as described above. Data is collapsed at the yearly level. Pre represents the year 2006 and post are the years 2007, 2008, and 2009. Standard errors are clustered at the firm level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	Core sample			Placebo sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	makes sale	$log\,(price)$	$log\,(revenues + 1)$	makes sale	$log\,(price)$	$log\,(revenues + 1)$
$post \times treated$	-0.1086***	-0.0381*	-1.0346***			
	(0.034)	(0.022)	(0.361)			
$post \times treatedplacebo$				-0.0097	0.0036	0.2012
				(0.085)	(0.017)	(1.322)
R^2	0.750	0.990	0.807	0.764	0.988	0.823
Obs.	19,136	13,825	19,136	26,124	19,327	26,124
Firms	734	734	734	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 Supermarket made by Treated firms. Sample is restricted to all Treated firms. Treated firms are those with total 2006 revenues below 100k UF and total 2006 sales to the Supermarket below 60k UF (note, these are all Treated firms, not restricted to firms with total sales above 25k UF). UF ("Unidad de Fomento" is an inflation linked currency unit updated daily, whose value is published by the Banco Central de Chile. The outcomes are "makes sale": a dummy that equals one if 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 Supermarket in pesos, with 0 replaced with the log of 1 peso. Columns 4, 5, and 6 correspond to regressions with the interaction variable replaced to $highshare$, a dummy that equals one if during 2006 Treated firms had a market share higher than the cross-sectional median (for Treated firms only). 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)	(4)	(5)	(6)
Dependent variable	makes sale	$log\,(price)$	$log\,(revenues + 1)$	makes sale	$log\,(price)$	$log\,(revenues + 1)$
$post \times treatmentintensity$	0.0510**	0.1047***	0.0771			
	(0.025)	(0.038)	(0.326)			
$post \times highshare$				0.0279*	0.0517*	-0.1168
				(0.016)	(0.027)	(0.212)
R^2	0.865	0.994	0.889	0.865	0.994	0.889
Obs.	29,064	14,336	29,064	29,064	14,336	29,064
Firms	1280	1280	1280	1280	1280	1280

TABLE 4. Supplier procurement and vertical integration

This table presents the estimated coefficient of interest of regression (2), β, which measures the relative change in the outcome for products with high exposure to the Agreement relative to products with low exposure, before and after the reduction in days payable for Treated firms, measured by the fraction of 2006 sales to the Supermarket made by Treated firms. Treated firms are those with total 2006 revenues below 100k UF and total 2006 sales to the Supermarket below 60k UF. UF ("Unidad de Fomento" is an inflation linked currency unit updated daily, whose value is published by the Banco Central de Chile. We restrict the sample to those firms with total 2006 revenues between 25k UF and 600k UF. The outcomes are "*subsidiary*": incidence of procurement from a Supermarket subsidiary, and "*unitsprocured*" the overall number of units procured of good j in month t, standardized by the sample mean and standard deviation. Column 3 corresponds to regression (2) with the interaction variable replaced to *highshare*, a dummy that equals one if during 2006 Treated firms had a market share higher than the cross-sectional median (0.0906). Data is collapsed at the product × year frequency. Post covers the years after 2006. Standard errors are clustered at the product level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)
Dependent variable	*unitsprocured*	*unitsprocured*	*subsidiary*	*subsidiary*
$post \times treatmentintensity$	-0.0549**		0.0539***	
	(0.021)		(0.017)	
$post \times highshare$		-0.0477***		0.0307***
		(0.018)		(0.011)
R^2	0.969	0.969	0.845	0.845
Obs.	6,045	6,045	6,045	6,045
Products	1,009	1,009	1,009	1,009

TABLE 5. Mechanisms

This table shows evidence for two contracting frictions that trade credit may alleviate. The table reports how the estimated effects of the change in days payable on the extensive margin and supplier revenues vary with whether a product is in inventory between more than 1 and 3 months, relative to 1 month or less and to more than 3 months (Columns 1 and 2), with whether the never factored its accounts (Columns 3-4), and with whether the price dispersion across suppliers is lower than the median during the pre-period (Columns 5-6). 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)	(4)	(5)	(6)
Interaction var.	Inventory months 1-3		No factoring		Low price dispersion	
Dependent var.	makes sale	$log\,(revenues+1)$	makes sale	$log\,(revenues+1)$	makes sale	$log\,(revenues+1)$
$post \times treated$	-0.0873	-0.5241	-0.0375	-0.5326	-0.0827**	-0.9243**
	(0.061)	(0.604)	(0.051)	(0.708)	(0.036)	(0.460)
$post \times treated \times var.$	-0.0758	-1.7805**	-0.1067*	-1.0059	-0.0643*	-0.6551
	(0.078)	(0.874)	(0.064)	(0.865)	(0.039)	(0.480)
Sum	-0.1631***	-2.3046***	-0.1442***	-1.5385***	-0.1470***	-1.5794***
p-value of sum	0.0048	0.0020	0.0004	0.0022	0.0005	0.0026
R^2	0.789	0.837	0.752	0.808	0.746	0.804
Obs.	19,136	19,136	19,136	19,136	18,736	18,736
Firms	734	734	734	734	731	731
Mean interaction	0.1665		0.3158		0.5000	

TABLE 6. Value of the relationship

This table shows that the effects of the restriction to the contracting space are mitigated when trading relationships are valuable. The table reports how the estimated effects of the change in days payable on the extensive margin and supplier revenues vary with the Supermarket's average mark-up by product (Columns 1-2), if the supplier's total sales to the Supermarket during 2006 are more than the median by treatment status–(Concentration of sales) (Columns 3-4), and with the supplier's product market share (Columns 5-6). Standard errors are clustered at the firm level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Interaction var.	Supermarket mark-up		Concentration of sales		Market share	
Dependent var.	makes sale	$log\,(revenues+1)$	makes sale	$log\,(revenues+1)$	makes sale	$log\,(revenues+1)$
$post \times treated$	-0.2229***	-2.3834***	-0.1504***	-1.5664***	-0.0979***	-1.0627**
	(0.066)	(0.843)	(0.048)	(0.603)	(0.038)	(0.475)
$post \times treated \times var.$	0.3659*	3.8284	0.1127*	1.0482	0.2686*	1.4589
	(0.192)	(2.462)	(0.067)	(0.854)	(0.162)	(2.068)
Sum	0.1430	1.4450	-0.0377	-0.5182	0.1707	0.3962
p-value of sum	0.3071	0.4186	0.4104	0.3841	0.2567	0.8379
R^2	0.751	0.808	0.755	0.809	0.757	0.8089
Obs.	18,992	18,992	19,136	19,136	19,136	19,136
Firms	729	729	734	734	734	734
Mean interaction		0.3193		0.4695		0.1285

Appendix B. Supplemental Appendix (not for publication)

A. Contract Terms

We study a general contract that the Supermarket uses as the basis for the procurement contracts it signs with each individual supplier. In order to preserve the anonimity of our data provider, we translate and summarize some points in that general contract that pertain directly to the importance that the Supermarket assigns to the quality and other unobserved features that affect the value of the good being sold.

- A procurement order is considered fulfilled after delivery only after: (i) the product corresponds exactly to the one detailed in the purchase order; (ii) the number of units delivered corresponds exactly to the one detailed in the purchase order; (iii) the product's quality is exactly as detailed in the purchase order, or in case the purchase order does not mention the quality of the good, the product's quality is as detailed in this General Procurement contract; (iv) the product is delivered in the place indicated in the purchase order; (v) the products are well preserved, have been correctly manipulated and are within their expiration date; (vi) the delivery guide correctly indicates the purchased and delivered products, their prices and quantities; (vii) all other conditions stipulated in the purchase order have been satisfied.
- Any cost of not fulfilling the order are the supplier's responsibility, unless it is shown that the Supermarket did not handle the product appropriately.
- The order is only fulfilled if the good is delivered at the time and place specifically agreed upon through the purchase order.
- Suppliers are responsible and liable for any defective packaging.

TABLE 7. Industry distribution of transactions

This table shows the number of product-firm observations for each of the 16 Departments defined by the Supermarket.

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

TABLE 8. Investment in procurement relationships

This table reports how the change in days payable affected investment in the suppliers' relationship with the Supermarket, and differentially so by average Supermarket mark-up averaged across all products sold by each supplier (Columns 1-2), by whether the supplier's total sales to the Supermarket during 2006 are more than the median by treatment status–(Concentration) (Columns 3-4), and with the supplier's average product market share across all products (Columns 5-6). Standard errors are clustered at the firm level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)
Interaction variable	Supermarket mark-up	Concentration	Market share
Dependent variable	# of departments	# of departments	# of departments
$post \times treated$	-0.2421***	-0.2481***	-0.1431***
	(0.057)	(0.052)	(0.051)
$post \times treated \times var.$	0.7158***	0.3722***	0.2011
	(0.198)	(0.071)	(0.201)
T-test of sum	3.26***	3.04*	0.09
R^2	2,920	2,936	2,936
Obs.	0.132	0.139	0.127
Firms	730	734	734

TABLE 9. 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.

Analysis Sample Description	
Total Number of Firms	1902
Total Number of Products	1871
Definition of Treatment Firms (UF)	25,000 - 100,000
Definition of Control Firms (UF)	100,000-600,000
Number of Treatment Firms	345
Number of Control Firms	389
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
Obs.	33,744
Firms	734

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.

A. Set-Up

We consider the market for 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 goods 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 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 ex ante 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.[28] 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. That is, we assume that trade credit contracts are enforceable by courts.

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

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

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,[29]

$$(4) \qquad \Pi_S^{TC} = \lambda \left(V - \bar{q}_g L + \frac{L^2}{2} - b_S \right) + b_S.$$

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 cannot provide the supplier with incentives, and no investment will be made.[30] 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:

$$(5) \qquad \Pi_S^{NT,S} = \max \left\{ \lambda \left(V - \bar{q}_g L - b_S \right) + b_S, b_S \right\}.$$

Equations (4) and (5) 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

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

[30]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.

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.

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.[31] 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:

$$(6) \qquad\qquad r \leq \frac{L\lambda}{2}.$$

Condition (6) 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

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[31]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 $\rho \leq \frac{\Pi_S^R - \Pi_S^{NT,S}}{r}$.

investment $e = \rho$, total net surplus is split according to Nash bargaining.[32] 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:

$$(7) \qquad \frac{L\lambda}{2} < r < L\lambda$$

When condition (7) 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 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.[33]

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.

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[32]From equation (5), 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).

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

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:

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

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}.