

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

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ABSTRACT. We study how restrictions to the set of feasible contracts affect buyer - supplier relationships and the organizational form of the firm. We exploit a regulation change that forced a large Chilean retailer to reduce the time it took to pay some of its small suppliers, defined by an arbitrary sales cutoff, from approximately 90 to no more than 30 days. Using a within-product differences-in-differences identification strategy, we find that firms restricted in their ability to extend trade credit are 11% less likely to make a sale to the retailer and receive a 3.8% price reduction when they do make a sale, an implied yearly interest rate of 23% from the large retailer's point of view. The large retailer responds by internalizing the procurement of some products previously sold by affected firms to its own subsidiaries. We interpret this as evidence that trade credit allows small firms to post a bond to overcome information asymmetries. When this guarantee is no longer possible, the retailer has an incentive to vertically integrate. However, the negative impacts of the regulation are mitigated when the value of the relationship is relatively high.

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I. Introduction

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

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

To study the firm’s decision whether to integrate or procure from a third party, we exploit a natural experiment that restricted the available terms of trade credit. To our knowledge, this is one of the first studies to provide causal estimates of the effect of restrictions to the set of feasible contracts on the organizational form of a firm and its supply chain. In response to fears that large retailers in Chile were exerting monopsonistic power over their suppliers, the government signed an accord (the “Agreement”) in December 2006 with one of the two large Chilean supermarket chains (the “Supermarket”). Under the Agreement, the Supermarket was forced to

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

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

reduce the number of days in which it paid its small suppliers from approximately 90 to no more than 30 days. As per the Agreement, suppliers were categorized as small by an arbitrary yearly revenues cutoff (roughly US\$4.0 million).

We use proprietary data obtained from the Supermarket and the Chilean tax authority. The Supermarket data details purchases made between 2006 and 2011 at the month - product - supplier level (e.g., soccer balls sold by firm A on July 2007), while the tax authority data provides information on total firm revenues and regulatory status. In our empirical strategy, we compare changes in the procurement of each product sold by Treated (regulated) firms before and after the Agreement relative to the *same* product category—as defined by the Supermarket—but sold by Control (unregulated) firms. By using this within- product differences - in- differences strategy, we control for differential product mixes between Treated and Control firms, as well as for differential time trends across products. Our main specification controls for differential trends by firm size by focusing our analysis on firms whose 2006 yearly revenues were within a relatively tight range above and below the cutoff.³

We find that the restriction on trade credit did affect the organizational form of the Supermarket and its supply chain. First, we show that the probability that an affected supplier sells the same product to the Supermarket falls by 11 percentage points relative to an unaffected supplier after the Agreement. Further, for those products that are still sold by Treated firms, the reduction in days payables results in 3.8% lower prices paid to suppliers, on average. These lower prices imply a yearly interest rate on trade credit of 23% from the point of view of the Supermarket, given the 60 day change in days payable.⁴ We document that the Agreement also changed the organizational structure of the Supermarket itself; for products that were mostly procured from Treated firms before the Agreement (above-median levels), the probability that the Supermarket procures from a fully owned subsidiary increases by 3 percentage points (from a baseline of 17%). Interestingly, we find that the total procurement quantities of products that

³Due to date 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.

⁴Calculated as the value r such that $\frac{1}{(1+r/12)^{90/30}} = \frac{1-0.038}{(1+r/12)^{30/30}}$.

were mostly purchased from affected firms is reduced after the Agreement. We interpret this as evidence that by shifting procurement to its subsidiary, the Supermarket is not fully able to replicate the preperiod market equilibrium.⁵

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. 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. We find that, within the set of firms that were affected by the reduction in days payable, the negative effects are larger for products that compete mostly with Control firms. Because the effects of the Agreement vary across products within each Treated firm, they cannot be driven only by a differential exit rate of smaller firms.

One commonly argued reason for the existence of trade credit is that the supplier may face a lower cost of capital than the buyer or may have an information advantage over the banking sector.⁶ In our setting, the buyer is orders of magnitude larger than the privately held supplier firms in our sample and has the ability to raise capital in the public market.⁷ Further, small firms in an emerging market like Chile may face even more difficulties in obtaining external financing (e.g., Rajan and Zingales (1998); Banerjee and Duflo (2008)). Therefore, the rationale for supplying credit (at any price) is most likely to come from mechanisms outside of the relative financing advantage.⁸ Further, because the Supermarket can always offer lower prices via spot contracts, our

⁵This is consistent with vertical integration being costly (Baker, Gibbons, and Murphy (2001)).

⁶Intermediation advantages relative to the financial sector may derive from the information contained in supplier relationships, the value of the buyer's collateral if used by the supplier, or relationship-based incentives for buyers to repay suppliers (Petersen and Rajan (1997); Smith (1987); McMillan and Woodruff (1999); Fisman and Love (2003); Burkart and Ellingsen (2004)).

⁷The suppliers in our sample are generally privately held firms with annual sales between \$1 million and \$24 million, and most likely face higher borrowing costs than the Supermarket.

⁸Smith (1987), Petersen and Rajan (1997), and Fisman and Love (2003) provide evidence for the financing advantage explanation of trade credit. Klapper, Laeven, and Rajan (2012) and Murfin and Njoroge (2012) also study supplier-buyer relationships where the suppliers tend to be substantially smaller than the buyers. They rule out financing constraints as the main driver of the provision of trade credit, as well.

results cannot be explained by the relatively larger market power of the Supermarket (as pointed out by Klapper, Laeven, and Rajan (2012)).

To interpret our findings, we present a model in which suppliers may take a costly action to improve a product's value. For example, suppliers could expend effort to improve the good's quality (Smith (1987); Long, Malitz, and Ravid (1993)) or value-enhancing, relationship-specific investments (Cunat (2007)). In the model, product value is observable only after a waiting period, and thus spot contracts fail to provide incentives for suppliers to invest. By delaying payment, trade credit allows the parties to contract on ex post value, achieving first best outcomes. 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. In the spirit of Baker, Gibbons, and Murphy (2002), intermediate levels of investment may be attained by relational contracts, sustained by the value of the future relationship. Vertical integration is costly (e.g., because incentive schemes are low powered as in Williamson (1975)), but it also provides a mechanism to improve allocations.

Consistent with our interpretation, we find that the effects of the Agreement are significantly mitigated for suppliers who sell mostly to the Supermarket and for suppliers who have a relatively large market share, where the relationship is valuable to the Supermarket and to the supplier, respectively (as in McMillan and Woodruff (1999) and Giannetti, Burkart, and Ellingsen (2011)).⁹ The effects of the Agreement are also attenuated for high-surplus products for which the Supermarket charges a large mark-up. Finally, 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. Because these firms were able to obtain up-front payment for the goods sold, delayed payment provides no incentives to suppliers.¹⁰

⁹These results are also consistent with the anecdotal evidence that the Supermarket and its competitor often require customized packaging, which increases the value of the relationship for the parties.

¹⁰We interpret the factoring results with caution, as we are unable to observe exactly when the firm factored its receivables. This result is also consistent with unobserved heterogeneity that is correlated with access to factoring and with the probability that a firm continues to supply the Supermarket.

Our paper is closely related to the literature on the theory of the firm and its boundaries and the decision to vertically integrate.¹¹ In our setting, the Agreement removed contracting levers previously used by suppliers to overcome an information problem. Once those contractual levers are removed, we observe how firms endogenously manage to find other means to overcome the contractual incompleteness. We add to this literature by providing causal estimates of how firms manage and integrate their supply chain when the set of feasible contracts is constrained. Finally, our paper is related to a vast empirical literature that tests competing theories of trade credit (see Ng, Smith, and Smith (2002), Fisman and Raturi (2004), Fabbri and Klapper (2008), Giannetti, Burkart, and Ellingsen (2011), Klapper, Laeven, and Rajan (2012), Costello (2014), among others). In particular, our work is consistent with evidence provided by Barrot (2013) that trade credit terms can act as a barrier to entry for financially constrained suppliers. Further, our work is complementary to Murfin and Njoroge (2012) who investigate the effects to firms' capital structure and investment policy from changes in the payments policies of their buyers when information about quality is asymmetrically distributed.

We continue with a description of the data and the empirical setting in Section II. We present our identification strategy in Section IV. Section III presents a model of trade credit as a guarantee. Section V presents our results. Finally, Section VI considers other mechanisms, and Section VII concludes.

II. Empirical Setting

A. The supermarket industry in Chile and the Agreement

The Chilean supermarket industry is composed of two large firms (including the Supermarket) and a host of smaller, geographically concentrated firms. The two large Chilean supermarket chains have some characteristics in common with US supermarkets and discount retailers, including store format (large superstores), means of payments

¹¹See Williamson (1979), Grossman and Hart (1986), and Hart and Moore (2007) among many others.

(own credit cards), and relationship with suppliers. Indeed, one of these two large Chilean supermarket chains was recently acquired by Walmart. Through a series of aggressive acquisitions and organic growth, these two large chains accounted for 63% of total supermarket revenues during 2006.¹² As a comparison, the third largest firm accounted for less than 3% of total sales. In contrast, the US supermarket sector exhibits less concentration, with the 4 (8) largest firms representing 32% (46%) of total industry revenues.¹³

In August 2006 the Chilean government's pro-competition agency issued a report that forced the two large supermarket chains to modify the terms of their relationships with small suppliers. In its report, the agency expressed its concern over the industry's concentration as a source of monopsonistic power.¹⁴ The agency's strategy consisted of denying both supermarkets regulatory approval for their new acquisitions until the modifications were put in place. This prompted both supermarkets to agree to modifications, the Supermarket in December 2006 and its large competitor in July 2008. The Supermarket implemented this change beginning in January 2007.

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

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

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

¹³Figure taken from 2007 Economic Census, NAICS code 44511 in factfinder2.census.gov

¹⁴See "Requerimiento contra Cencosud y D&S", www.fne.cl.

- (1) Suppliers who in the last 12 months had sold no more than 60,000 UF¹⁵ to the Supermarket.
- (2) Suppliers who in the last 12 months had sold no more than 100,000 UF to all their clients, as accredited by their sales tax information.
- (3) For new suppliers, maximum monthly revenues no higher than 5,000 UF.

The sales cutoffs were negotiated between the Supermarket and the government. For example, the Supermarket's main competitor signed a similar agreement with the government that applied to firms who in the last 12 months had sold no more than 25,000 UF to the Supermarket and 100,000 UF overall. Firms that were affected by the Agreement represent 64% of the universe of the Supermarket's suppliers during 2006 by number, but only 6.4% of all procurement during that year in terms of revenues.¹⁶ As per the Agreement, firms that met the three selection criteria were to be paid by the Supermarket no later than 30 days after delivery. Hence, the Agreement reduced the accounts payable days to eligible firms by up to 60 days.

The Chilean government has actively monitored the Agreement's implementation since it was put in place. Publicly available reports suggest the Supermarket has indeed complied with the shorter payment period for small firms. Further, the Supermarket has explicitly avoided any actions that could be construed as forcing suppliers to extend longer days payable.¹⁷

III. Framework

We present a simple framework motivated by our empirical setting. The purpose of the model is to show that trade credit allows buyers to provide incentives to suppliers to take actions that affect product value. When trade credit is no longer available, we explore alternative contracting structures that may be available. Namely, the model

¹⁵UF, 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.

¹⁶In practice, we find that all but one existing supplier are categorized as small or not small based on the second criteria only. That is, the first restriction is not active except for one supplier.

¹⁷See, 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

describes the conditions under which the buyer chooses to lower the product's price but maintain a procurement relationship or terminate the relationship and vertically integrate.

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. Throughout, we assume for tractability that the buyer's outside option for the sale of the good has value equal to zero.¹⁸

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 $e^{FB} = \min \{L, \bar{q}_g\}$.

In what follows 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.

¹⁸This assumption can be relaxed without altering the results.

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 on a marketing campaign or on 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 unobserved 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. The timing, shown in Figure 1, 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 expend the first best investment, e^{FB} , if $\rho = L$. Intuitively, suppliers have aligned incentives when they bear the full cost of delivering a low value good. 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 total surplus,¹⁹

$$(1) \quad \Pi_S^{TC} = \lambda \left(V - \bar{q}_g L + \frac{L^2}{2} \right).$$

¹⁹The below expression holds for an interior solution where $L \leq \bar{q}_g$. If $L > \bar{q}_g$ then $\Pi_S^{TC} = \lambda \left(V - \frac{1}{2} \bar{q}_g^2 \right)$.

C. No Trade Credit Spot Contracts

We assume that in the absence of trade credit, payments are made before product value becomes observable. In this setting, the supplier has no way to force the buyer to pay the price for a high value good ex post.²⁰ Hence, suppliers rationally expect that prices will be low and invest nothing. Since total surplus equals $V - \bar{q}_g L$, the payment to the supplier (S) in the No Trade Credit Spot contract (NT, S) equals:

$$(2) \quad \Pi_S^{NT,S} = \lambda (V - \bar{q}_g L).$$

Equations (1) and (2) 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 relationship is sufficiently low, no trade may be a preferred choice by the contracting parties.

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, which include the value of the future relationship, can substitute for trade credit. We follow the relational contracting 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

²⁰Alternatively, 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.

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 (i.e. years), 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.²¹ 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. Moreso, the first best level of investment (at an interior solution, $\bar{q}_g > \frac{L}{k}$) is achievable under relational contracting if the surplus is split like in the first best Trade Credit contract at the first best level of investment. This occurs whenever:

$$(3) \quad r \leq \frac{L\lambda}{2}.$$

Condition (3) 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 $e = \frac{\rho}{k}$, total net surplus is split according to Nash bargaining. Thus, ρ equals $\rho^* = 2 \left(L - \frac{r}{\lambda} \right)$. This ρ^* will only be an equilibrium if reimbursements are both positive and strictly less than first best. These conditions are jointly satisfied if:

$$(4) \quad \frac{L\lambda}{2} < r < L\lambda$$

²¹In particular, if Π_S^R denotes the per period expected profits to the supplier (S) from the relational contract (R), then $\rho \leq \frac{\Pi_S^R - \Pi_S^{NT,S}}{r}$.

When condition (4) 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.

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 point out that it is very likely that firms face a trade off when deciding to vertically integrate. Thus, we choose an extreme setup and simplify our assumptions to obtain a tractable model that illustrates our results. Similar intuition could be reached with a more detailed exploration of these costs.

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 up to a strictly positive minimum investment level e^{VI} , ($\frac{L}{k} > e^{VI} > 0$) 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:

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

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} .

F. Effects of the Agreement

It is clear from the above analysis that outsourcing under trade credit (weakly) dominates all other types of procurement contracting relationships for both the buyer and the external supplier. In the absence of trade credit, the buyer firm must choose whether to engage in a relational outsourcing contract, a spot market outsourcing contract, or in vertical integration. If not all firms are able to sustain first best relational contracts, then the average treatment effect on prices and/or the likelihood of making a sale are expected to decrease. Further, if the relationship is valuable enough, the inability to extend trade credit may not reduce the supplier's incentive to expend first best investment. In the data, we test the main predictions of this model by examining the average effect of the Agreement on the likelihood that a supplier makes a sale and the good's price, as well as on the probability that the Supermarket vertically integrates. We also examine whether the effects of the Agreement are heterogeneous across dimensions that correlate with differences in the value of the future relationship.²²

IV. Empirical Strategy

A. Baseline model: Product by firm by time variation

We obtain from the Supermarket a proprietary data set that contains all the monthly transactions with its suppliers between January 2006 and August 2011 and contains

²²In terms of our model, this is captured by how large is the reduction in value due to lower levels of investment (i.e., $V - L$ or just L). A lower level of positive investment may also be sustained by reputational contracts for intermediate levels of impatience. For these suppliers, the buyer may compensate for the lower value by decreasing prices or by terminating the relationship and vertically integrating. When neither trade credit nor relational contracts are available, the buyer's incentives to terminate the relationship are even stronger.

observations at the supplier-product-month level. We estimate the effect of changes in trade credit on product-level contractual outcomes. The identification concern is that trade credit is an equilibrium outcome that depends, in part, on all other contractual outcomes, including most notably the fact that a procurement relationship exists in the first place. Formally, denote $TC_{i,j,t}$ as the equilibrium trade credit offered by firm i on product j and period t . We assume that $TC_{i,j,t}$ represents Supermarket days payable and may take on one of two values, $TC_{i,j,t} \in \{30, 90\}$. Let $y_{i,j,t}$ denote the outcome of interest for product j sold by firm i on time t . We consider a range of outcomes, $y_{i,j,t}$ including the incidence of a sale, the log of the procurement price, and the total revenue received by the supplier. We are interested in the coefficient δ of the regression,

$$(5) \quad y_{i,j,t} = \alpha_{i,j} + \alpha_{j,t} + \delta TC_{i,j,t} + \epsilon_{i,j,t}.$$

Here, $\alpha_{i,j}$ denotes firm-by-product fixed effects, while $\alpha_{j,t}$ denotes time-varying product fixed effects. Estimating (5) using observational, cross-sectional data would lead to a biased estimate as long as strict exogeneity didn't hold. This is likely to be the case, if, for example firms selling more valuable products are also more likely to extend trade credit. Such a correlation may arise if products with higher value-added were sold by larger, less financially constrained firms. Alternatively, high value-added products may take longer to turn over. If trade credit contracts are written to match the product turnover cycle, that might also explain such a correlation.

We use the Agreement as a source of exogenous variation in days payable in the time series. In particular, there is a direct relationship between the Agreement and the permissible levels of trade credit. Because most small suppliers were expected to offer 90-days payables prior to 2006, the Agreement was binding. We encode the relationship between exposure to the Agreement and the resulting trade credit terms as:

$$(6) \quad TC_{i,j,t} = a_{i,j} + a_{j,t} + bA_{i,t} + e_{i,j,t}.$$

In this implicit first stage regression, the variable $A_{i,j,t}$ is an indicator for whether the Agreement was in effect for firm i in year t . We can write $A_{i,j} = 1(t > 2006) * 1(treated_i)$, where $1(treated_i)$ for firms that meet the sales cutoffs of the Agreement. Because we do not observe the levels of accounts payable on the Supermarket’s balance sheet for each transaction, we cannot separately estimate Equations 5 and 6. We instead estimate the reduced form specification,

$$(7) \quad y_{i,j,t} = \omega_{i,j} + \omega_{j,t} + \beta A_{i,t} + \varepsilon_{i,j,t},$$

which allows us to identify an intent-to-treat effect.²³

It is important to note that firms are affected by the Agreement during a calendar year based on their prior year’s revenues. After 2006, the sample of firms affected by the Agreement did change, perhaps endogenously. Firms may have tried to expand (shrink) their revenues in order to avoid (fall under) the Agreement’s jurisdiction. Because total revenues are endogenous to the Agreement after it was put in place, we define and fix our sample of Treated and Control firms based on 2006 revenues. This means the sample is predetermined as of the time the of treatment.²⁴

A concern with the identification strategy is that the outcomes of products sold by larger Control suppliers would have evolved in a manner different to smaller Treated suppliers, irrespective of the change in trade credit terms, precisely because of the size differential. We control non-parametrically for this possibility by restricting our sample to firms with total yearly revenues during 2006 sufficiently close to the 100,000 UF cutoff, as defined by the IRS categorization. In particular, we define our “main sample” as firms with 2006 revenues between 25,000 UF and 600,000 UF (roughly US\$1.0 million to US\$24 million). Treated firms are firms with total 2006 revenues

²³Note that the Wald estimate for Equation 5 is $\hat{\delta} = \frac{\hat{b}}{\hat{b}}$. Anecdotal evidence suggests that most firms were offering 90-day terms before the Agreement, but we are unable to verify this with our data. Thus, for interpretation, we typically assume that the first stage regression coefficient $\hat{b}$ captures a 60-day reduction of days payable. However, if not all firms were affected by the Agreement by the full drop from 90 to 30 days, then our estimate of δ would be a lower bound.

²⁴We should note that 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. 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.

between 25,001 UF and 100,000 UF (US\$1.0 million to US\$4.0 million), and Control firms as those with total 2006 revenues between 100,001 UF and 600,000 UF (US\$4.0 million and US\$24 million).

Table 1 shows descriptive statistics at the firm level for our main sample of suppliers during 2006, before the Agreement. The sample includes 734 firms, 342 Treated and 392 Control. Panel A shows yearly statistics at the firm level. On average, firms in our sample had yearly revenues to the Supermarket of approximately US\$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).

Table 1 (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, 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, Control firms exhibit a higher probability of making a product sale on any given month during the year.

Treated and Control firms may sell different products.²⁵ To avoid inducing bias in our tests due to this fact, we include a product-time fixed effect, $\alpha_{j,t}$ that controls for differential trends in each product. This also allows us to interpret the coefficient β in regression 5 as the average effect of trade credit terms on outcomes of the same product *over time*. We also include firm-product fixed effects, $\omega_{i,j}$. These account for firm and firm-product specific factors that may influence the estimated coefficient, including, for example, size, relative importance of the product on the firm's portfolio, relative differences in firm focus across products, and access to formal credit markets, among others. Finally, we include overall time trend dummies, δ_t .

²⁵Appendix Table 6 reports the concentration of product-firm transactions each month (by number) at the department level for all months in the sample.

The identification assumption for regression (7) is that absent the Agreement and conditional on the product-times-time and firm-times-product fixed effects, the sales terms for products sold by 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 for ease of exposition, with one common trend for Treated and Control. There are no noticeable differences in the trends of Treated and Control firms during 2006, before the Agreement was put in place. Note that the graph shows that there are no differential pretrends unconditionally. The identification assumption requires weaker evidence that the pretrends 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.

We restrict our sample to the first three years after 2006: 2007, 2008, and 2009. Our results are unchanged if we include the latter years of our sample and if we drop 2009, and are attenuated if we include only one year in the post period (2007). This suggests that suppliers may enter into medium-long term contracts with the Supermarket, and that we are able to capture the timing of the negotiation of a new contract. We estimate regression (7) in the cross section of supplier-products that are at least present in the pre-period ($post = 0$). 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)).

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

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

We are interested in two outcomes: the overall units procured of each good j in each time period t , and the incidence of the Supermarket purchasing each good j from one of its own subsidiaries. The first outcome, units sold, allows us to estimate the effect of the restriction of trade credit on the overall procurement of the product. In turn, the incidence of the Supermarket purchasing the good from its own supplier allows us to measure the Supermarket's decision to bring the procurement of a particular product within the boundaries of the firm.

We denote by $y_{j,t}$ the outcome variable for product j in month t and estimate the following differences-in-differences specification:

$$(8) \quad y_{j,t} = \alpha_j + \delta_t + \beta 1(t = post) \times treatmentintensity_j + \epsilon_{j,t}$$

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

V. Results

A. The average effects of the reduction of days payable

Column 1 on Table 2 reports the results of regression (7) when the outcome is “makes sale”, a variable that equals one if the supplier records a sale to the Supermarket during the period (pre- or post- period as defined above). The coefficient suggests that Treated firms are approximately 11% less likely to sell any given product to the Supermarket following the Agreement, relative to before the Agreement. This evidence implies that the Supermarket chooses to shift purchases away from suppliers who will have to be paid sooner.

Column 2 shows how the Agreement affects the prices of the products that are sold in the post period. The outcome is $\log(\text{price})$, the natural logarithm of the price of each product sold to the Supermarket every month. On average, after the Agreement is implemented on January 2007, Treated firms sell the same product at 3.81% lower prices than Control firms relative to 2006. Because we only observe the price of transactions that occurred, this coefficient is estimated in a selected sample.

These two effects suggest that the large and financially unconstrained retailer values trade credit. When suppliers lose the ability to extend it, as under the Agreement, they must adjust through other margins. We document that if suppliers are able to respond, contracts are adjusted through prices. The magnitudes of the price changes appear on the surface to be larger than a reasonable 60-day interest rate for external financing for the Supermarket. For example, the 3.81% price reduction is equivalent to an annualized interest rate of 23%. This compares to the Chilean banking sector reported yearly rates for the same period of 7% to 11%, and probably even lower for a large corporation like the Supermarket.²⁶

We combine the evidence on the extensive margin and prices to form a variable that measures revenues for each product-supplier sold to the Supermarket per month,

²⁶Figure 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.

$\log(\text{revenues})$. To include the effect of observations with zero units sold we adjust the variable by replacing zero revenues with one peso (roughly 0.2 cents), the lowest monetary unit in Chile. The results of running our regression model with this modified logarithm of amount, $\log(\text{revenues} + 1)$, as outcome are shown on Column 3 of Table 2, and show a large and significant decrease in revenues per monthly sale to the Supermarket.

We interpret these results as evidence that trade credit is important in a non-standard setting where suppliers do not hold an advantage in the access to external financing relative to their clients. Consistent with our theoretical framework, this strongly suggests that the ability to extend trade credit allows suppliers to enter into procurement relationships. The Agreement imposes a constraint to the set of feasible contracts that parties can enter into. Once some contracts are no longer available, contractual relationships that were in place originally are broken (as in our results in the extensive margin) or adjusted (as in our results with price as the outcome).

B. Placebo test

Recall that our main differences-in-differences specification in equation (7) is identified off of firm times time-level variation. Therefore, it is important to show that differential trends between small versus large firms are not driving our results. In addition to the pre-trend analysis in Section IV.(A), we further conduct a placebo test based on a comparison of size categories within the sample of unaffected firms. Our “placebo” sample is composed 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.

Table 2 shows the regression results of this placebo test for the outcomes “makes sale”, “ $\log(\text{price})$ ” and “ $\log(\text{revenues} + 1)$ ” in columns 4, 5, and 6, respectively. The coefficient on makes sale 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.

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

C. Identification controlling for differential firm trends

The results of the placebo test suggest that differences in size do not necessarily correlate with changes in the likelihood of making a sale after the Agreement. However, one might still be concerned that the small firms targeted by the Agreement are nonetheless different from the Control firms in a time-varying fashion. Examples might include other concurrent policy changes (though we could find no evidence of such events) or differential firm survival rates right around the treatment cutoff size. Here, we propose a robustness check that allows for time-varying firm fixed effects and instead identifies off of product-level treatment intensity. This allows us to verify whether the effects of the Agreement vary across products within each firm that was affected by it.

We hypothesize that if the Agreement affected the Supermarket’s incentives to procure from a third-party vendor, its effects on Treated firms should be more pronounced

for those products where Control suppliers had a higher market share. Thus, we estimate the following regression model on a sample restricted to all Treated firms that sold to the Supermarket during 2006:

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

for Treated firm i selling product j during period t , where $treatmentintensity_j$ is a measure of the fraction of procurement of good j made by the Supermarket from Treatment firms in 2006, as defined above. This model identifies only off of Treated firms that sell more than one product concurrently each period, in this case, each year. The coefficient on $1(t = post_t) \times treatmentintensity_j$ represents the average effect of the Agreement on products depending on the ex ante share of product-level procurement from Treated firms. Note that any differential trend of small versus large firm is absorbed by the firm times time fixed effects $\alpha_{i,t}$. As above, we estimate the model using the methodology in Gormley and Matsa (2014).

The results are presented in Table 3. Column 1 documents that within Treated firms, the effects of the Agreement at the extensive margin are mitigated for products that compete mostly with Treated firms. That is, the probability that a Treated firm sells a product to the Supermarket decreases if the firm competes mostly with Control firms in that particular product. Thus, not all products sold by Treated firms see a reduction in their procurement after the Agreement. 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: the price of products sold by Treated firms decreases as the firm competes with Control firms. Column 3 shows that these effects combine into a positive but insignificant effect on revenues. Taken together, 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.

D. Procurement of affected products and vertical integration

One virtue of our comprehensive dataset is that we can ask both how the overall procurement of inputs as well as the organizational structure of the Supermarket change as a result of the Agreement. The empirical tests are based on equation (8). The identification assumption 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. Figure 3 presents evidence 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 shows how after the Agreement, the relative incidence of *ownbrand* seems to increase for products sold mostly by Treated firms relative to products sold mostly by Control firms.

Formal regression results 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 get a better sense of the economic magnitude of this effect, we split products into “high Treated share” and “low Treated share” (as in the pre-trends graph) based on those with higher and lower market share than the median market share of Treated firms in the preperiod (9.7%). We run the same regression as in equation (8) but replace the *treatmentintensity* variable with *hightreatment* = 1 (*treatmentintensity* > *p*50). The results are shown on Column 2, and suggest that products in which Treated firms have a market share above the median see a reduction of 3.7% standard deviations, approximately 200 units less 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. This evidence suggests that the firm is unable to replicate the market outcomes and settles with a second-best outcome, consistent with Baker, Gibbons, and Murphy (2001).

We test for whether the restriction in the set of feasible contracts made the Supermarket more willing to vertically integrate. Column 3 shows the regression results when the outcome is a dummy variable that equals one if the Supermarket purchased the product internally through a subsidiary. The positive coefficient suggests that faced with the restriction in days payable, the Supermarket does indeed choose to procure via internal subsidiaries some goods that were previously sourced by Treated firms. We repeat the regression but change the interaction variable to *hightreatment* 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.

Our results are rationalized by the theoretical framework presented in section III. Because of the Agreement, Treated firms, which would have to be paid earlier, do not continue to supply to the Supermarket. The Supermarket then chooses to offset this effect by bringing more procurement into the firm via vertical integration but the ex ante level of procurement is not met.

E. Relational contracting

Our theoretical framework implies that the value of future relationships may help sustain trade in the absence of trade credit contracts. We exploit the richness of our data to study whether the negative effects of the Agreement are mitigated for relationships that a priori may seem to be more valuable. To do this, we augment regression (7) with an interaction variable, $interaction_{i,j}$, that varies at product i and firm j level,

$$\begin{aligned}
 (9) \quad y_{i,j,t} = & \beta 1(t = post) \times 1(i \in Treated) \\
 & + \gamma 1(t = post) \times 1(i \in Treated) \times interaction_{i,j} \\
 & + \omega 1(t = post) \times interaction_{i,j} + \alpha_{j,t} + \omega_{i,j} + \epsilon_{i,j,t}.
 \end{aligned}$$

The coefficient γ on the variable $1(t = post) \times 1(i \in Treated) \times interaction_{i,j}$ captures the differential effect of the Agreement for Treated firms with different value of $interaction_{i,j}$.

Table 5 presents the coefficients β and γ from regression (9) 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. 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 5 show the results of regression (9) 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.²⁷ 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 show the results of regression (9) when the interaction variable is the supplier's product market share. 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

²⁷i.e. These concentrated firms have low or negligible sales to other buyers.

results are consistent with our framework: a relatively high value of the future relationship may sustain trade when payment must be made 60 days earlier.

In the Appendix, we present regression results where we look for heterogeneity of effects of the Agreement along dimensions that may be correlated directly with the quality of the good: whether the good is perishable, and whether it has large demand fluctuations. The results are suggestive of such effects being present, but they are not statistically significant. We interpret these results to mean that product value is associated with quality but is most likely a more general characteristic of the buyer - supplier relationship. As such, value is affected by investments suppliers can make in other dimensions, such as supply-chain management, marketing campaigns, or investments in logistics.

VI. Other Mechanisms

Our simple model frames trade credit as a contracting lever that allows parties overcome information asymmetries. Here we discuss whether trade credit might also be explained by other mechanisms, including financial intermediation, side-contracting, anti-price discrimination laws, and bargaining power.

A. Intermediation-based Arguments

An intermediation-based story to explain our findings would rely on the Supermarket's marginal external borrowing cost being higher than that of its small suppliers (Demirgüç-Kunt and Maksimovic (2001)). In our setting, this is not likely to be the case. At the time the Agreement was put in place, the Supermarket's shares were publicly traded in Chile, Madrid and the NYSE, and the Supermarket's value was orders of magnitude larger than the privately held suppliers in our sample.²⁸ In 2006, the Supermarket had a \$2.3 billion market cap, 24% net debt to enterprise value, and had access to 9 bank

²⁸The suppliers in our sample are all privately held firms with annual sales between \$1 million and \$24 million.

credit lines in various currencies at rates around 6%.²⁹ We do not have access to the financing costs of the suppliers in our sample, but the average rate paid by comparable firms in Chile in 2006 was approximately 16%.³⁰ Further, the buyer is one of the largest retailers in Chile, so any information problems relating to repayment are likely to be quite small.³¹

B. Side Contracting

If firms could factor their receivables at competitive rates, trade credit would fail to provide incentives to deliver high levels of investment. In that case, we would expect the Agreement to have no noticeable effect on procurement relationships. To test this hypothesis, we obtain from the Supermarket's management a list of its suppliers that had factored their receivables at any point in time. Interestingly, we find that only 23.2% of Control firms and 24.9% of Treated firms did factor their receivables at any point in time.³² This suggests that there are constraints to the adoption of factoring, including for example its relatively high price (Klapper (2006)).³³

Because the data do not distinguish when the factoring was done, we are not able to test whether pre-Agreement access to financing helped firms offset its effect. With this caveat in mind, Columns 7 and 8 of Table 5 show that the effects of the Agreement are indeed mitigated for firms that at any point in time had access to factoring.³⁴ This result suggests that these suppliers were able to contract with the Supermarket to provide high levels of investment via alternative mechanisms, for example, a relational

²⁹Own calculations based on data from Chilean securities regulator SVS and Chile stock exchange. As a comparison, the average D/(D+E) ratio in the US was 24% (source: Aswath Damodaran's website).

³⁰From Chilean banking regulator SBIF for firms with access to bank debt of up to approximately \$200,000.

³¹It is likely that small firms in an emerging market like Chile have even more difficulty credit constrained (Rajan and Zingales (1998); Banerjee and Duflo (2008)).

³²This difference is remarkably small and is not statistically significant.

³³Anecdotally, we learned from discussions with the owner of a very small firm that supplies to the Supermarket that factoring was available to his firm at a rate of 4% for receivables due 30 days later, an implied yearly rate of 48%.

³⁴We 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.

contract, or that products were sold at a relatively low price due to the uncertainty about their value.

Finally, based on press reports, the Supermarket has explicitly avoided any alternative side contracting that could be construed as forcing suppliers to extend longer days payable.³⁵

C. Anti-price discrimination laws

If suppliers were unable to charge different prices to different clients, trade credit could be used as a way to price discriminate (see Petersen and Rajan (1997)). However, Chile does not have an explicit law like the Robinson-Patman Act that prevents price discrimination. Further, the government, through its Pro-Competition agency, has in previous cases upheld covenants in exclusive distribution agreements where price variation cannot be linked directly to differences in cost.³⁶ This suggests that price discrimination is most likely not the main driver for the use of trade credit.

D. Bargaining power

A static model of bargaining power likely cannot explain the prevalence of trade credit in small supplier - large buyer relationships. Indeed, the large advantage that the Supermarket has in accessing external financing together with the fact that small suppliers must finance their extension of trade credit suggests that the Supermarket would be better off obtaining external financing directly (at a lower cost) and paying lower spot prices to their suppliers. More generally, it is unclear why trade credit would be a better contracting lever than price reductions for the Supermarket to exert its bargaining power.

³⁵See, 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

³⁶E.g., see <http://www.oecd.org/daf/competition/47950954.pdf>

VII. Discussion

In the traditional paradigm, trade credit is used as a means through which suppliers can finance buyers. However, it is commonly observed that small firms with higher costs of capital are ostensibly financing their much larger clients. We present evidence that trade credit may be playing an entirely different role as a contracting lever that allows for trade to occur. We document that when trade credit terms are restricted for small suppliers, they are forced to stop selling their goods altogether, or sell them at a lower price. As a lower bound, reducing the days payable of procurement contracts from 90 to 30 days causes a decrease in product prices of 3.8%. When it becomes harder for small suppliers to do business with the Supermarket, we observe organizational changes within the Supermarket itself. Namely, for those goods once sold by outside suppliers, restricting the terms of trade causes the Supermarket to integrate vertically and to procure inputs from within the firm.

We find that some firms are able to continue to supply goods despite the restriction of contractual terms. The detrimental effects of the Agreement are mitigated for suppliers who sell mostly to the Supermarket and for suppliers with a relatively large market share. In these cases, both parties have a large incentive in preserving their relationship. Further, the effects of the Agreement are also attenuated for products in which the Supermarket receives a high margin *ex ante*, in which the surplus of the relationship is relatively high. Finally, firms that were able to factor their receivables were also able to better withstand the negative effects of restricting the contract space.

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.³⁷ Thus, the overall welfare effects of the Agreement are unclear.

³⁷In the Appendix we show that these 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. Model timeline

This figure shows the basic model timeline.

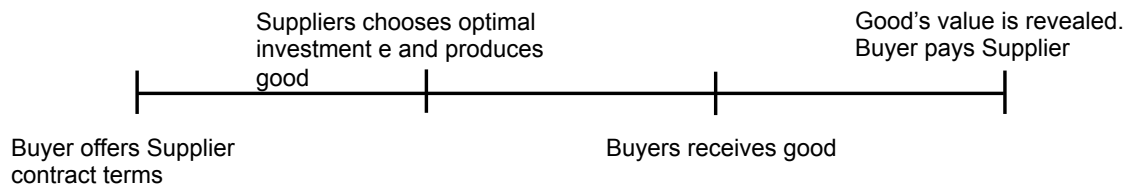


FIGURE 2. Preperiod trends

This figure shows that there is no difference in the preperiod 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.

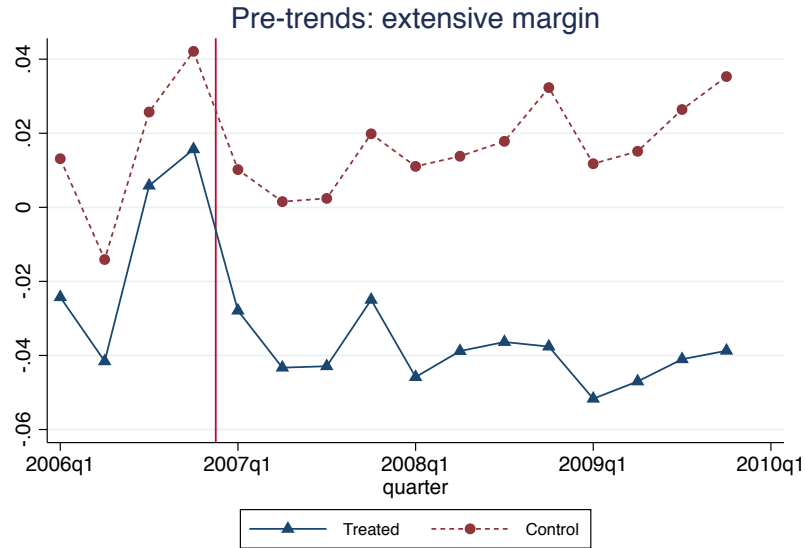


FIGURE 3. Own supplier preperiod trends

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 were 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.

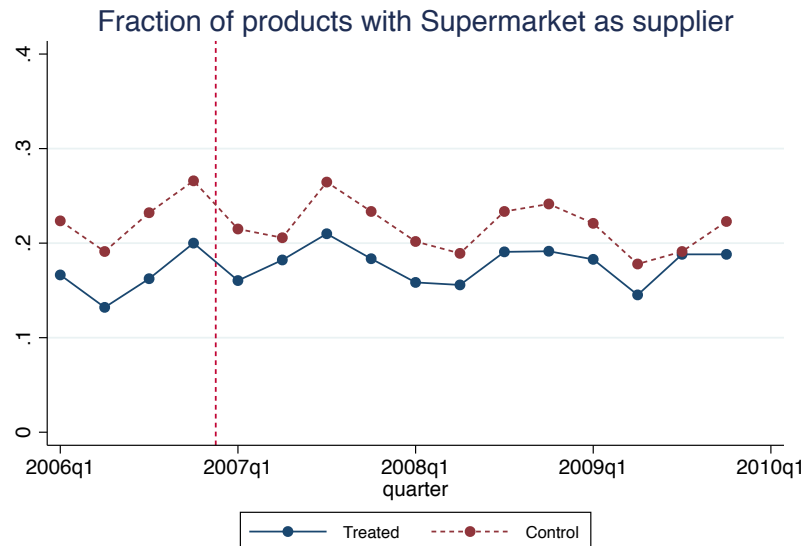


TABLE 1. Summary statistics main sample

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	St. Dev.	p50	Mean	St. Dev.	p50	Mean	St. Dev.	p50
Sales ('000 dollars)	712.53	1,353.46	248.27	311.00	412.07	138.92	1,062.84	1,738.39	443.49
# Departments	1.540	0.917	1.0	1.442	0.651	1.0	1.625	1.092	1.0
# Products	6.518	11.099	3.0	4.842	5.595	3.0	7.980	14.108	3.0

Panel B: Product-firm level 2006 monthly average									
	All (N=6,232)			Treated (N=2,092)			Control (N=4,140)		
	Mean	sd	p50	Mean	sd	p50	Mean	sd	p50
Units ('000)	6.51	55.48	0.22	3.39	13.38	0.20	8.09	67.35	0.24
Sales ('000 dollars)	6.36	20.85	0.58	3.85	10.99	0.46	7.62	24.26	0.66
log(price)	6.978	1.572	6.908	6.941	1.757	6.961	6.997	1.464	6.888
log(units)	6.843	2.195	6.845	6.566	2.158	6.624	6.990	2.201	6.982
log(revenues)	13.821	2.159	14.023	13.507	2.219	13.710	13.987	2.108	13.987
margin (%)	31.93	15.46	29.84	32.74	15.62	31.82	31.50	15.35	29.32

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 (7), 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*** (0.034)	-0.0381* (0.022)	-1.0346*** (0.361)			
$post \times treatedplacebo$				-0.0097 (0.085)	0.0036 (0.017)	0.2012 (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. 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), “ $\log(\text{price})$ ”: natural logarithm of the transfer price; “ $\log(\text{revenues} + 1)$ ” 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). 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	$\log(\text{price})$	$\log(\text{revenues} + 1)$	makes sale	$\log(\text{price})$	$\log(\text{revenues} + 1)$
$\text{post} \times \text{treatmentintensity}$	0.0534* (0.027)	0.0947** (0.038)	0.2161 (0.340)			
$\text{post} \times \text{highshare}$				0.0304* (0.018)	0.0405 (0.029)	0.0153 (0.224)
R^2	0.869	0.995	0.895	0.869	0.995	0.895
Obs.	19,136	11,512	19,376	19,136	11,512	19,376
Firms	1,280	1,280	1,280	1,280	1,280	1,280

TABLE 4. Supplier procurement and vertical integration

This table presents the estimated coefficient of interest of regression (8), 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 (8) with the interaction variable replaced to *highshare*, a dummy that equals one if during 2006 Treated firms had a market share higher than the cross-sectional median (0.0906). Data is at the product $\times$ month frequency. Post covers the years 2007, 2008, and 2009. Standard errors are clustered at the product level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)
Dependent variable	<i>unitsprocured</i>	<i>unitsprocured</i>	<i>subsidiary</i>	<i>subsidiary</i>
$post \times treatmentintensity$	-0.0422*** (0.014)		0.0527*** (0.015)	
$post \times highshare$		-0.0371*** (0.013)		0.0304*** (0.010)
R^2	0.944	0.944	0.510	0.510
Obs.	68,612	68,612	68,612	68,612
Products	1,009	1,009	1,009	1,009

TABLE 5. Differential effects by value of the relationship and switching costs
 This 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), with the supplier's product market share (Columns 5-6), and with whether the supplier ever factored its accounts (Columns 7-8). Standard errors are clustered at the firm level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)
Interaction variable	Supermarket mark-up		Concentration of sales	
Dependent variable	makes sale	$\log(\text{revenues} + 1)$	makes sale	$\log(\text{revenues} + 1)$
$post \times treated$	-0.2229*** (0.066)	-2.3834*** (0.843)	-0.1504*** (0.048)	-1.5664*** (0.603)
$post \times treated \times var.$	0.3659* (0.192)	3.8284 (2.462)	0.1127* (0.067)	1.0482 (0.854)
R^2	0.751	0.808	0.755	0.809
Obs.	18,992	18,992	19,136	19,136
Firms	729	729	734	734
Mean interaction	0.3193		0.4695	
	(5)	(6)	(7)	(8)
Interaction variable	Market share		Factoring	
Dependent variable	makes sale	$\log(\text{revenues} + 1)$	makes sale	$\log(\text{revenues} + 1)$
$post \times treated$	-0.0979*** (0.038)	-1.0627** (0.475)	-0.1442*** (0.041)	-1.5384*** (0.503)
$post \times treated \times var.$	0.2686* (0.162)	1.4589 (2.068)	0.1067* (0.064)	1.0059 (0.865)
R^2	0.757	0.8089	0.752	0.808
Obs.	19,136	19,136	19,136	19,136
Firms	734	734	734	734
Mean interaction	0.1285		0.3158	

Appendix B. Supplemental Figures and Tables

TABLE 6. 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 7. 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*** (0.057)	-0.2481*** (0.052)	-0.1431*** (0.051)
$post \times treated \times var.$	0.7158*** (0.198)	0.3722*** (0.071)	0.2011 (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 8. Additional heterogeneity

The table reports how the estimated effects of the change in days payable on the extensive margin and supplier revenues vary with whether a food product is defined as “perishable” based on whether the product’s description has the words “perishable” or “fresh” (Columns 1-2), with the sum of the squared ratios of 2006 monthly supplier revenues over total 2006 supplier revenues–(Demand fluctuations index) (Columns 3-4), and with whether the good is defined as a commodity (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)
Interaction var.	Perishables		Demand fluctuations index	
	makes sale	$\log(\text{revenues} + 1)$	makes sale	$\log(\text{revenues} + 1)$
$post \times treated$	-0.1554** (0.066)	-1.9482** (0.777)	-0.1174*** (0.041)	-1.3167** (0.512)
$post \times treated \times var.$	0.0208 (0.104)	1.0172 (1.166)	0.0195 (0.061)	0.2851 (0.786)
R^2	0.724	0.984	0.750	0.807
Obs.	7,204	7,204	19,136	19,136
Firms	282	282	734	734
Mean interaction	0.611		0.404	

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