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**Stock Market Listing and the Use of Trade Credit:
Evidence from Public and Private Firms⁺**

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Abstract

This paper examines differences in the use of trade credit by publicly listed firms and their privately held counterparts. We show that public firms maintain a significantly lower level of trade credit than private firms. This finding is consistent with the argument that public firms rely less on supplier financing because of their greater access to cheaper and less risky sources of external capital. We further find that while public and private firms actively seek to adjust toward their optimal trade credit levels, the former firms experience faster adjustment. The recent financial crisis had differential effects on the trade credit ratios of public and private firms.

JEL Classification: G30, G31, G32, G01.

Keywords: Trade credit; Accounts payable; Public firms; Private firms; Speed of adjustment; Financial crisis.

1. Introduction

Trade credit is one of the most important sources of short-term financing for a company. The aggregate volume of trade credit is three times as large as that of bank credit and fifteen times as large as that of commercial papers (Barrot, 2016).¹ Just before the onset of the recent financial crisis of 2007–2009, trade credit funded almost 90% of global merchandise trade, worth US\$25 trillion (Klapper et al., 2012). Trade credit also carries economic importance, acting as a potential substitute for bank credit during monetary contractions or financial crises (e.g., Garcia-Appendini and Montoriol-Garriga, 2013). In this paper, we empirically study how trade credit policies depend on stock market listings.

Theory provides several motives for the use of trade credit. First, supplier financing is often considered to be an important form of credit for firms facing asymmetric information and financial constraints and with limited access to traditional financial intermediaries. Suppliers are willing to provide trade credit to these firms because they have a comparative advantage over financial institutions in acquiring information, evaluating the creditworthiness of buyers, and enforcing credit contracts (e.g., Burkart and Ellingsen, 2004; Fabbri and Menichini, 2010). In a similar vein, trade credit is a useful source of finance for firms facing liquidity shocks or distress risk because, in the case of default, they may be granted renegotiation concessions by suppliers (Wilner, 2000; Cuñat, 2007). Second, using trade credit reduces transaction costs by separating payment from delivery, thus alleviating the need to hold inventories of both money and goods (Ferris, 1981; Emery, 1987). By allowing buyers to use a product before paying for it, trade

¹ Trade credit is also a major component of working capital as nearly 40% of inventories and accounts receivable in US firms are financed with trade credit (Aktas et al., 2015). In our sample, the trade credit ratio, measured as accounts payable scaled by total assets, for public (private) firms is 9.5% (16.1%), while the ratio of short-term debt to total assets is 6.4% (12.9%).

credit also helps reduce the costs of verifying product quality (e.g., Lee and Stowe, 1993). Third, trade credit can be used as a means for risky customers to obtain favorable price discrimination from suppliers (Brennan et al., 1988).

Consistent with the theoretical predictions above, prior empirical research has examined various determinants of trade credit (e.g., Petersen and Rajan, 1997). Much of this literature has focused on publicly listed firms and only a few studies have investigated small privately held companies. To the best of our knowledge, however, no research has studied potential differences in trade credit policies between public and private firms. This is an important omission because despite the growing evidence for the impact of stock market listing on firms' financial policies (e.g., Brav, 2009), prior studies have not examined whether listing status plays a role in determining firms' use of supplier financing. Specifically, little is known about whether private firms use more or less trade credit than comparable public firms and whether private firms' trade credit policies change when these firms go public and gain access to potentially cheaper and longer-term external capital. The latter question is of relevance to an ongoing debate in the literature on the pros and cons of the decision to go public. To the extent that trade credit is a costly source of capital,² it is important to understand whether stock market listing enables firms to rely less on this type of financing.

Our paper aims to address this important gap in the literature. We ask two main interrelated questions regarding the use of trade credit by publicly listed and privately held firms. First, does stock market listing have an impact on the use of supplier financing? Second, do firms

² Existing studies show that many firms do not take advantage of early payment discounts and thus end up borrowing from their suppliers at high interest rates (Petersen and Rajan, 1997; Ng et al., 1999; Burkart and Ellingsen, 2004). For example, in a common "2–10 net 30" trade credit contract, the customer qualifies for a 2% payment discount during the first 10-day period after delivery. This implies a credit at an interest rate of 2% for the remaining 20 days, which is equivalent to an interest rate of 44% per annum (Cuñat, 2007).

have a target level of trade credit and does public listing affect the speed with which they adjust toward this target?

Regarding the primary question of how the listing decision affects the use of trade credit, we examine two alternative views. The first view predicts that private firms rely more on trade credit than their public counterparts. This prediction is based on the argument that trade credit plays a more important role for the former firms, which tend to have higher degrees of distress risk and financial constraints and hence more limited access to other sources of financing. The alternative view, however, suggests that due to private firms' lower credit quality, their demand for trade credit may not be matched by suppliers' willingness to lend. Put differently, this view contends that private firms maintain a lower level of trade credit than their publicly listed counterparts.

We test these alternative hypotheses using a sample of US public and private firms collected from the S&P Capital IQ database for the period 1995–2012. We first document that stock market listing status has a significant and negative impact on the use of trade credit. The level of accounts payable in public firms is 23% lower than that in their private counterparts. This result is both statistically and economically significant and supports the first view that privately held firms rely more on trade credit than publicly listed companies.

We subject our finding to a battery of robustness checks. To address potential sample selection and endogeneity concerns, we follow three identification strategies. First, we confirm our baseline regression results using propensity score matching that controls for observed differences in public and private firms. Second, we analyze a transition sample of firms that were private and subsequently went public. Consistent with our baseline regression results, we document a significant decline in these firms' reliance on supplier financing post-listing, which

is most pronounced in the first three years after their IPO (initial public offering). Third, we run a treatment regression using the maximum likelihood estimation that accounts for the endogeneity of the listing decision. Our treatment regression results continue to show a negative and significant impact of stock market listing on the use of trade credit. We further find that our results are insensitive to including additional control variables, using alternative measures of trade credit, and adopting different estimation methods (e.g., Tobit model).

We next examine potential cross-sectional variations in the relation between stock market listing status and trade credit. To the extent that public firms rely less on supplier financing due to their lower degrees of financial constraints and distress risk, we expect the negative impact of stock market listing status on the use of trade credit to be stronger (weaker) among those public firms that are less (more) financially constrained and distressed. Consistent with this conjecture, our analysis shows that the negative effect of public listing on trade credit is more pronounced among larger firms and those with lower size and age index scores, lower leverage, higher coverage ratios, and fewer losses; that is, public firms that are less constrained and less likely to be in distress. These results provide further evidence that our main finding is driven by financial constraint and distress risk channels.

We next address the question of the optimality and dynamics of trade credit policy (e.g., Nadiri, 1969; Emery, 1984). We examine whether public and private firms attempt to adjust toward target trade credit and, if so, whether the speed with which these firms adjust their trade credit varies according to their listing status. We find evidence to support the prediction that public firms move faster toward target trade credit than their private counterparts, which is consistent with the notion that the former firms face lower adjustment costs. This result suggests

that, although private firms rely more on trade credit than their public counterparts, they may find it more difficult to adjust their trade credit and operate close to the optimal level.

In additional analysis, we investigate the impacts of macro-financial shocks on the use of trade credit by public and private firms. While recent research has acknowledged the importance of trade credit during crisis periods, it has primarily focused on public companies (e.g., Love et al., 2007; Garcia-Appendini and Montoriol-Garriga, 2013), which tend to be less vulnerable to adverse macroeconomic conditions than their private counterparts. We find that, during the recent financial crisis of 2007–2009, public firms on average experienced an economically insignificant change in their use of trade credit. In contrast, private firms were granted significantly less trade credit during the crisis, possibly because their suppliers, which were also hit by the crisis, were less willing to accommodate these firms' demand for supplier financing. Overall, this finding shows how vulnerable private firms are during a credit crunch when the supply of trade credit, a potential substitute for bank credit, may also dry up.

Our paper contributes to a growing debate on the impacts of the going-public decision on several important corporate financial policies. Existing studies show that public and dispersed ownership may exacerbate the agency conflicts between managers and shareholders, leading to distortions in cash management (Gao et al., 2013) and investment policies (Asker et al., 2015), as well as suboptimal CEO turnover decisions (Gao et al., 2017). Likewise, going public may disincentivize or affect the quality of innovation (Ferreira et al., 2014; Bernstein 2015). On the other hand, public listing and access to cheaper external financing may allow public firms to use more equity and less debt (Brav, 2009), distribute more cash flows as dividends (Michaeli and Roberts, 2012), engage in more mergers and acquisitions during merger waves (Maksimovic et al., 2013), take better advantage of investment opportunities (Mortal and Reisel, 2013), and

respond more quickly to those opportunities (Gilje and Taillard, 2016). Stock market listing may also facilitate R&D spending and innovation among firms that are more dependent on external finance (Acharya and Xu, 2017). Our results highlight another potential benefit of stock market listing: publicly listed firms with better access to relatively cheaper and longer-term external capital can rely less on trade credit, a potentially costly source of finance.

Our study also complements earlier studies of trade credit policies in small businesses based on the National Survey of Small Business Finance (NSSBF) (e.g., Petersen and Rajan, 1997; Berger and Udell, 1998; Giannetti et al., 2011). However, we note that the NSSBF data are mainly restricted to a pooled cross-section, as opposed to a panel, of small firms with fewer than 500 employees and are only available over a limited number of time periods (i.e., 1987, 1993, 1998, and 2003). These inherent data limitations do not permit an investigation of either the impact of listing status on trade credit or the dynamic nature of this policy, especially under extreme scenarios such as financial crises.

Our finding regarding the difference in the speed of adjustment between public and private firms contributes to the nascent literature studying the optimality and dynamics of various components of firms' working capital (e.g., García-Teruel and Martínez-Solano, 2010a; 2010b). It further adds to evidence of differences in the target adjustment behavior of public and private firms documented by recent research on other corporate policies, such as capital structure (Brav, 2009) and cash holdings (Gao et al., 2013).

Finally, our additional analysis of the effects of the recent financial crisis on trade credit in public and private firms extends existing studies that focus solely on the former type of firms. We provide the first systematic evidence of the heterogeneous effects of the crisis on the use of trade credit by public and private firms and relevant policy implications for the latter group.

The remainder of the paper is organized as follows. We review the literature and develop our hypotheses in Section 2. We discuss our data and the methodology in Section 3. We present our empirical results in Section 4 and conclude the paper in Section 5.

2. Related Literature and Hypothesis Development

2.1. Use of Trade Credit in Public and Private Firms

The literature offers two alternative views on the impact of listing status on the use of trade credit. The first view predicts that publicly listed firms use less trade credit than their privately held counterparts, for at least two reasons. First, public firms have lower degrees of asymmetric information and financial constraints than private firms (e.g., Brav, 2009; Schenone, 2010). In particular, Farre-Mensa and Ljungqvist (2016) show that public listing status is one of the most relevant measures of financial constraints, as privately held firms appear to face an inelastic supply of external capital. Furthermore, public firms have a lower cost of capital (Campello et al., 2011; Gao et al., 2013), stronger bargaining power with banks (Saunders and Steffen, 2011), greater liquidity (Pagano et al., 1998), and greater access to external sources of liquidity (Faulkender and Petersen, 2006; Lins et al., 2010). Given those important differences between public and private firms, we argue that the former firms are financially less constrained and hence have a lower demand for trade credit as an alternative source of external capital than the latter firms.³

³ Firms with greater agency problems may have an incentive to use less trade credit to avoid the monitoring of suppliers. However, while public firms are more susceptible to agency problems than private firms due to their diffuse ownership (e.g., Gao et al., 2013; Asker et al., 2015), they also have greater bargaining power, thus weakening suppliers' monitoring effect (Wilner, 2000; Klapper et al., 2012). Hence, the argument based on the monitoring role of trade credit is less relevant in the context of studying public versus private firms.

Similarly, because private firms face a higher cost of equity than public firms, they tend to rely more on financial leverage and therefore hold debt that is riskier (Brav, 2009). While firms with a high distress risk may find it difficult and costly to borrow from banks, they may still be able to obtain trade credit from suppliers (Cuñat, 2007; Molina and Preve, 2012; Boissay and Gropp, 2013). Trade credit is also more flexible than bank loans because it is easier to renegotiate due to its revolving nature, and delaying repayments is less costly. Even in the event of financial distress, buyers may be given concessions if suppliers wish to maintain their product market relationship with them (Wilner, 2000). Petersen and Rajan (1997) argue that the implicit equity stake of suppliers in buyers consists of the present value of both current and future sales. As a result, suppliers that are concerned about continuing a relationship with their customers tend to be lenient toward cases of financial distress. Overall, these features of trade credit seem to be more valuable to private and risky firms, suggesting that these firms should have a greater demand for trade credit than their public counterparts. These arguments lead to our first hypothesis:

H1a: Public firms have a lower level of trade credit than private firms.

Although the demand for trade credit from private firms is expected to be greater than that from public firms, it may not be matched by the willingness of suppliers to lend. Public firms may be granted more trade credit than private firms because they have higher credit quality and greater market power. Petersen and Rajan (1997) argue that suppliers can apply quantity restrictions when it comes to lower-quality buyers because, given the industry-standard credit terms, they cannot charge them a higher price. Empirically, the authors find that firms with higher credit quality, such as large firms or firms with high profitability, receive more trade credit than those with lower quality. To the extent that public firms are larger and have better

credit quality than their private counterparts, they may be granted more credit. In addition, public firms tend to have greater bargaining power than private firms because they have higher sales volumes. By exercising their market power, public firms can potentially obtain more trade credit with more favorable terms, which may help reduce their overall borrowing costs. Indeed, Klapper et al. (2012) show that large and trustworthy buyers are offered longer credit terms by small suppliers, consistent with a market power explanation; see also Murfin and Njoroge (2015). Based on these arguments, we develop the following alternative hypothesis:

H1b: Public firms have a higher level of trade credit than private firms.

2.2. Speed of Adjustment toward Target Trade Credit

In line with Nadiri (1969) and Emery (1984), we predict that a firm will have an optimal level of trade credit that balances its benefits and costs, while maximizing the firm's value. As reviewed above, trade credit brings about several benefits because it acts as an alternative source of financing for firms facing credit constraints, liquidity shocks, or financial distress. Trade credit also reduces transaction costs and allows for favorable price discrimination or product quality assurances. However, trade credit has several disadvantages because it can be a more expensive form of credit (Petersen and Rajan, 1997; Ng et al., 1999; Burkart and Ellingsen, 2004; Cuñat, 2007); refer to an example in Footnote 2. As a short-term source of financing, trade credit may also expose firms to refinancing risk. Further, buyers may face opportunity costs when using supplier financing due to a loss of the discount for early payment (Ng et al., 1999), or an increase in future costs of credit due to default risk (Nadiri, 1969).

In Nadiri's (1969) theoretical model, an optimal trade credit policy exists whereby firms trade off the benefits against the costs of trade credit. However, his model shows that the observed level of trade credit may deviate from the optimal level due to firms' inaccurate

estimates of sales, purchases, and the opportunity costs of trade credit, as well as disequilibrium in other assets such as inventories. An important implication of this theory is that firms should attempt to reduce any deviation from the optimal level of trade credit by making adjustments over time. We thus expect firms to gradually adjust toward optimal (target) trade credit levels by taking on more trade credit when they are below the target level and reducing the volume of their accounts payable when they are above the target level.

The speed with which firms adjust toward optimal trade credit depends on the costs of adjustment. Thus, they should be different for public and private firms. There are two alternative views on the impact of being public on these adjustment costs and, ultimately, the speed of adjusting trade credit. One view suggests that public firms have lower adjustment costs than private firms because the former firms have greater bargaining power, thus renegotiating with suppliers more easily to adjust the amount of trade credit taken and the credit contract terms (Klapper et al., 2012). As previously mentioned, public firms also face a lower cost of capital and have greater access to external sources of liquidity, thus enabling them to adjust their overall capital structure more quickly by switching to other forms of credit. In sum, this argument leads to the following hypothesis:

H2a: Public firms adjust toward target trade credit more quickly than private firms.

The alternative view suggests that public firms have less incentive to operate at, or close to, target trade credit, especially when the costs of deviating from such targets are unlikely to be material to them. In theory, firms maintaining more trade credit than is optimal face the expected costs of default and higher costs of future credit due to the deterioration of their credit reputations. However, to the extent that public firms are less prone to bankruptcy and have greater bargaining power, the probability of default and the associated costs of distress may be

small for them. As a result, the incentive to revert toward the optimal level of trade credit may be weaker for public firms than for private firms. This argument enables us to develop the following alternative hypothesis:

H2b: Public firms adjust toward target trade credit more slowly than private firms.

3. Data and Methodology

3.1. Sample Description

We collect our data from the S&P Capital IQ database for the period 1995–2012. S&P Capital IQ provides data on public and private US firms;⁴ however, its coverage of trade credit includes more private than public firms. Colla et al. (2013) compare the quality of data on public firms provided by Compustat against that provided by S&P Capital IQ. Examining several corporate variables such as leverage, size, profitability, cash holdings, tangibility, and asset maturity, they conclude that the quality of S&P Capital IQ data is comparable with that of Compustat data. In Table I.A.1 of our online appendix, we find that Colla et al.’s (2013) conclusion can be extended to data on trade credit, our variable of interest. Specifically, the summary statistics of the trade credit variable for public firms collected from S&P Capital IQ and Compustat are comparable. Hence, for consistency, and in line with prior studies (e.g., Gao et al., 2013), both our sets of data on public and private firms come from S&P Capital IQ.

⁴ S&P Capital IQ provides data on private firms that file forms 10-K (annual reports), 10-Q (quarterly reports), or S-1 (securities registration) with the Securities Exchange Commission (SEC). According to the SEC regulations, firms with total assets of \$10 million or above, and with 500 or more shareholders, are required to file 10-K and 10-Q reports, while firms with public debt are required to file the S-1 form. S&P Capital IQ states that it also covers other private firms from the third-party Private Company Financials provider, which obtains data by directly contacting the company, from CPAs, from courts and recording offices regarding suits, liens, judgments and bankruptcy filings, and from top news providers. Although the database does not provide detailed information about the fraction of firms whose data come from those different sources, the use of various sources of data on private firms helps increase coverage and alleviates sample selection concerns.

Following previous research, we exclude financials and utilities, IPO firms, firms that went private during our sample period, and firms with a cash flow to total assets ratio of less than -50%. Next, we remove observations with missing variables, negative equity, and negative total assets. We further winsorize the continuous variables used in our study at the 1st and 99th percentiles to alleviate the impact of outliers. Our final sample consists of 27,300 private firms with 70,011 firm-year observations and 3,340 public firms with 33,766 firm-year observations.⁵

3.2. Empirical Models

To examine the effect of public listing on the trade credit ratio (*Hypotheses 1a* and *1b*), we estimate the following model:

$$TC_{it} = \beta_0 + \beta_1 Public_{it} + \theta' X_{it} + \varepsilon_{it}, \quad (1)$$

where the dependent variable, trade credit (TC_{it}), is measured as the ratio of accounts payable to total assets (Petersen and Rajan, 1997; Fisman and Love, 2003; Cuñat, 2007; Giannetti et al., 2011). In our robustness checks in Section 4.3, we also consider various alternative measures of trade credit. In Model (1), *Public* is a dummy variable that is equal to 1 for public firms and 0 for private firms. Following prior research (Petersen and Rajan, 1997; Love et al., 2007; Garcia-Appendini and Montoriol-Garriga, 2013), our control variables (X_{it}) include $\ln(1+age)$, $\ln(1+age)^2$, cash flow, cash holdings, current assets, negative growth, positive growth, short-term debt, and firm size. Note that, although we include the contemporaneous values of the controls as in several existing studies (e.g., Love et al., 2007; Molina and Preve, 2009, 2012; Cull et al., 2009), our results remain qualitatively the same when we use their lagged values.

⁵ Consistent with previous research on public and private firms (e.g., Asker et al., 2015), our sample includes more (firm-year) observations for private firms than for public firms.

Next, to test *Hypotheses 2a* and *2b*, we compare how quickly public and private firms adjust toward their target trade credit levels. We do so by estimating the following partial adjustment model for public and private firms separately:

$$\Delta TC_{it} = \beta_0 + \delta(TC_{it}^* - TC_{i,t-1}) + \varepsilon_{it}, \quad (2)$$

where the dependent variable, ΔTC_{it} , is the change in trade credit from year $t - 1$ to t . $TC_{i,t-1}$ is the lagged value of trade credit. TC_{it}^* is the target trade credit ratio, which we estimate from a regression of trade credit on the control variables listed above, separately for public and private firms, as follows:

$$TC_{it} = \beta_0 + \gamma' X_{it} + \varepsilon_{it}. \quad (3)$$

Our estimation strategy involves estimating Models (3) and (2) in sequence, which is similar to the two-stage approach used by several recent studies of capital structure or cash adjustments (e.g., Byoun, 2008; Faulkender et al., 2012; Gao et al., 2013). This approach further allows for the possibility that public and private firms may maintain heterogeneous trade credit targets, which is in line with our argument that these firms have different trade credit demands. In Model (2), the coefficient of interest δ measures the speed of adjustment toward the target level of trade credit; it takes values from zero to one. We compare the speed of adjustment between public and private firms by testing whether the difference in the estimates of δ is significant.

4. Empirical Results

4.1. Impact of Listing Status on the Use of Trade Credit

4.1.1. Descriptive Statistics and Univariate Analysis

Panel A of Table 1 reports summary statistics for the full sample of public and private firms. The mean trade credit ratio is 13.6%, which is much higher than the figure for short-term

debt (mean of 3%); this finding highlights the importance of trade credit among alternative sources of short-term financing. The summary statistics for other variables are generally consistent with prior research and are thus not discussed to preserve space.^{6,7} Panel B presents the results from our univariate analysis. Public firms have a mean trade credit of 8.9% (median of 6.6%), which is about half of the figure for private firms (mean of 15.8% and median of 11.1%). The difference between the trade credit ratios of the two groups is 6.9 percentage points (median of 4.5 percentage points) and is significant according to our statistical tests. This finding is in line with *Hypothesis 1a* that public firms rely significantly less on trade credit than their private counterparts.

[Insert Table 1 here]

Table 2 shows how trade credit varies across the 12 Fama-French industries. Our analysis is motivated by previous evidence that the use of trade credit is uniform within an industry but varies across industries (Ng et al., 1999; Fisman and Love, 2003). In Panel A, we find that the firms relying most heavily on trade credit operate in the retail and wholesale industry, with a mean trade credit ratio of 18.8% (median of 14.7%). This finding is similar to García-Teruel and Martínez-Solano's (2010c) evidence for European firms. The firms with the lowest level of trade credit (mean of 6.5% and median of 4.1%) are in the health sector (including healthcare, medical equipment, and drugs). This finding is consistent with Fisman and Love (2003), who argue that trade credit is unpopular for drug companies because it is difficult for suppliers to resell these specific products in the event of a customer default.

⁶ By definition, negative (positive) growth, which is sales growth times the negative (positive) growth dummy, has a maximum (minimum) value of 0. We note that the negative growth variable also has a median of zero because the majority of firms do not have negative growth.

⁷ In Table I.A.2 of the online Appendix, we report the summary statistics for public and private firms, respectively.

Panel B of Table 2 presents the summary statistics for public and private firms' trade credit ratios across industries. The health sector still exhibits the lowest level of trade credit, and also the smallest difference between the levels of trade credit of public and private firms. We observe the largest difference between public and private firms' trade credit ratios in the energy industry (difference in means of 8 percentage points and in medians of 3.6 percentage points). The difference between the trade credit of public and private firms in other industries ranges from 3.2 to 7.5 percentage points. More importantly, the test statistics for the differences in the means and medians are all statistically significant. Overall, we find that public and private firms maintain significantly different levels of trade credit and that this finding holds across industries.

[Insert Table 2 here]

4.1.2. *Multivariate Analysis: Baseline Regression Results*

Table 3 reports the regression results regarding the effect of public listing on the use of trade credit. In Column (1), we simply regress trade credit on the *Public* dummy without controlling for firm-specific characteristics. However, in this model we include industry and year effects to account for unobserved industry-level heterogeneity and common time trends.⁸ The results show that the difference between the trade credit of public and private firms (4.4 percentage points) is significantly negative, consistent with the univariate analysis. In Column (2), we include the control variables while retaining the industry and year fixed effects.⁹ The results show that, after controlling for those variables, the *Public* dummy remains significant and

⁸ In unreported analysis, we also examine the evolution of trade credit for public and private firms. However, unlike recent evidence of a secular increase in cash holdings (Bates et al., 2009) and in short-maturity debt (Custódio et al., 2013), we observe no clear pattern in trade credit over time.

⁹ Since the firms in our sample remain public or private throughout the sample period, we do not include firm-fixed effects because the *Public* dummy variable, our main regressor, is subsumed by those effects. In Section 4.2.2, we examine a transition sample of IPO firms and are able to employ the fixed-effects estimator.

negative. Overall, the results in Columns (1)–(2) show that the difference between the levels of trade credit for public and private firms varies between 4.4 and 3.6 percentage points and is economically significant. For example, using the latter estimate in our baseline model in Column (2), the trade credit ratio of public firms is 23% lower than the mean trade credit ratio of private firms (0.158). These findings strongly support *Hypothesis 1a* that public listing enhances firms’ access to cheaper and less risky sources of external capital, thus reducing their reliance on supplier financing.

[Insert Table 3 here]

The results regarding the control variables are broadly consistent with theoretical predictions and existing evidence in the literature (e.g., Petersen and Rajan, 1997). $\ln(1+\text{age})$ has a significantly negative coefficient, indicating that young and potentially constrained firms use more trade credit. However, we find that $\ln(1+\text{age})^2$ is insignificant. Consistent with the pecking order theory, firms with large amounts of cash flow rely less on trade credit. Current assets and cash holdings have a positive impact on trade credit, which is in line with the maturity-matching principle. Positive growth has a positive effect on trade credit, consistent with a financial constraint argument. However, negative growth is also significant and positive. A possible explanation for this finding is that firms that have no growth opportunities but are large and less constrained can borrow more. Firms with high short-term debt have less trade credit, which reflects a substitution effect. Finally, on average, large firms are granted more trade credit, supporting the argument that suppliers are willing to provide more trade payables, with more favorable and longer credit terms, to firms that have potentially better quality and greater market power (Petersen and Rajan, 1997).

4.2. Identification Strategies

4.2.1. Propensity Score Matching

One concern with the interpretation of our results thus far, or indeed research examining public versus private firms in general, is the potential endogeneity of a firm's listing status. For instance, firms may self-select themselves to go public and such a choice may be driven by factors that also affect their trade credit policies. We address this concern by adopting three identification strategies. Our first test involves the use of propensity score matching, which controls for sample selection based on observed differences between public and private firms. Since we have more observations for private firms, we consider public firms to be the treated group and private firms the control group. We implement *one-to-one* matching to the nearest neighborhood, without replacement. Specifically, we match each public firm-year observation with a private firm-year observation using the propensity score for being public, taken from a probit regression based on certain firm characteristics. In the probit model, we use two specifications to capture the status of stock market listing. In Specification (1), our matching is based on firm size, industry, and year. We note that, by matching on industry, we can further control for unobserved industry heterogeneity. In Specification (2), the matching is based on all the control variables used in our baseline regression, as well as industry and year effects. In unreported analysis, we confirm that, in our propensity score matched samples, the public (control) firms are broadly similar to the private (treated) firms along the matching dimensions.

Panel A of Table 4 presents the pairwise differences between the mean trade credit of public and private firms in the propensity score matched samples, together with the bootstrapped standard errors estimated based on 50 replications. We find that, in both specifications, there is a statistical and economic difference between the trade credit levels of the propensity score

matched public and private firms. Specifically, the level of trade credit for public firms is between 7.6 and 5.1 percentage points lower than that for private firms. In Panel B, we re-estimate the baseline regression model using the matched samples. The results confirm our earlier baseline findings that public listing has a negative effect on the use of trade credit.

[Insert Table 4 here]

4.2.2. *Transition Sample*

To further mitigate the sample selection bias and endogeneity concern, we next focus our analysis on a transition sample of the same firms that were private and subsequently conducted an IPO during the sample period. In this transition sample, the post-IPO firm-year observations can be considered the treatment group, with the data pre-IPO as the control group. The advantage of this approach is that it controls for time-invariant unobservable firm characteristics that might influence the going-public decision; see, for instance, Michaely and Roberts (2012), Gao et al. (2013), and Gao and Li (2015) for a similar approach.

Figure 1 demonstrates graphically the evolution of the trade credit policy of IPO firms around the IPO event, specifically from the year IPO−4 to the year IPO+5. The peak mean trade credit is in the years IPO−2 and IPO−1, in both of which it is 9.1% (medians 5.1% and 5.8%, respectively). In the IPO year, there is a small decline in the mean trade credit, which remains stable at this relatively low level, compared with the pre-IPO level of about 7%, until the year IPO+5. Overall, this graphical evidence is in line with the multivariate analysis described above.

[Insert Figure 1 here]

Next, we perform a regression analysis to investigate the post-IPO change in trade credit using this transition sample. We estimate the effect of the listing decision on post-IPO trade

credit, as well as the temporary effects in the first few years following the IPO. The models we estimate are as follows:

$$TC_{it} = \beta_0 + \beta_1 D_{Post_IPO} + \theta' X_{it} + \varepsilon_{it}. \quad (4)$$

$$TC_{it} = \beta_0 + \beta_1 D_{IPO} + \beta_2 D_{IPO+1} + \beta_3 D_{IPO+2} + \beta_4 D_{IPO+3} + \beta_5 D_{IPOp4} + \theta' X_{it} + \varepsilon_{it}. \quad (5)$$

In Model (4), D_{Post_IPO} is a dummy variable that is equal to 1 in the IPO year and the subsequent years and 0 otherwise. In Model (5), D_{IPO} is a dummy variable that is equal to 1 in the IPO year and 0 otherwise; D_{IPO+i} with $i=1\dots3$ is a dummy variable that is equal to 1 in the year $IPO+i$ and 0 otherwise. D_{IPOp4} is a dummy variable that is equal to 1 for the period at least four years after the listing event. X_{it} is a vector comprising the control variables, as discussed.

Table 5 presents the regression results for the transition sample of 1,282 IPO firms that went public during our sample period. Column (1) reports the results for Model (5), with industry and year effects.¹⁰ In Column (2), we further control for firm fixed effects. We note that including industry and, in particular, firm fixed effects allows us to control for (unobserved) firm heterogeneity bias and further alleviate endogeneity concerns. The D_{Post_IPO} dummy is significantly negative, which lends support to the notion that, when the firm goes public, its reliance on trade credit decreases. The magnitude of the decline in trade credit is between 1.5 and 1.1 percentage points, which remains economically significant. In Columns (3)–(4), we report the results regarding the temporary effects of the IPO decision on trade credit, with (i) industry and year effects and (ii) industry, year, and firm effects, respectively. The results suggest that IPO firms rely significantly less on trade credit in the first few years following an IPO. Specifically, compared with the pre-IPO level, firms reduce their trade credit level by 0.96–1.5 percentage points between the years IPO and IPO+3. Overall, our analysis using the transition

¹⁰ Using year dummies controls for time trends and changes in market conditions that may affect IPO timing.

sample reveals a significant decrease in firms' reliance on trade credit after going public, which lends further support to *Hypothesis 1a*.

[Insert Table 5 here]

In further analysis (untabulated), we employ two alternative approaches to constructing transition samples. First, we examine a matched sample of IPO (transition) firms and non-IPO (private) firms. Specifically, we match each firm-year observation in the treatment group (IPO firms) with firm-year observations in the control group (non-IPO firms) that are in the same industry and the IPO−1 year, as well as similar in size (allowing for a deviation of 10%). Regression results show that IPO firms experience a decrease in their trade credit ratio following the IPO event, compared with their matched non-IPO firms. Second, we study a transition sample of public firms going private. Building on recent research on going-private transactions (e.g., Bharath and Dittmar, 2010), we argue that theories of why firms go public can be reversible when applying to firms going private. Specifically, we expect that post-going private, firms will use more trade credit due to a potential increase in their degree of financial constraint and distress risk. Using a sample of going-private transactions, we document some moderate evidence that, in the long term after opting out of the public market, firms experience an increase in their use of trade credit. Taken together, these results further strengthen our main inferences on the negative impact of public listing on trade credit policy.

4.2.3. *Treatment-effects Model Regression*

As a third test to deal with the endogeneity concern, we adopt an instrumental variable approach and estimate a treatment-effects model as follows:

$$TC_{it} = \beta_0 + \beta_1 Public_{it} + \boldsymbol{\theta}' \mathbf{X}_{it} + \varepsilon_{it} . \quad (6)$$

$$Public_{it}^* = \gamma_0 + \delta' \mathbf{X}_{it} + \gamma' z_{it} + \omega_{it}, \quad (7)$$

$$Public_{it} = 1 \text{ if } Public_{it}^* > 0; = 0 \text{ otherwise.}$$

In the latent-variable Model (7), we model the response probability of the endogenous *Public* dummy as a probit function of the controls ($\mathbf{X}_{it}$) and an instrumental variable (z_{it}), i.e., $P(Public_{it} = 1 | \mathbf{X}_{it}, z_{it}) = \Phi(\mathbf{X}_{it}, z_{it})$, with $\Phi(\cdot)$ being the cumulative distribution function of the standard normal distribution.

Following Liu and Ritter (2011) and Gao et al. (2013), we use industry-level underwriter concentration as an instrument for the *Public* dummy. We define underwriter concentration as the number of IPOs underwritten by the top-five underwriters, divided by the number of IPOs in that industry. The top-five underwriters are determined using the number of IPOs underwritten in the last five years (Liu and Ritter, 2011). We next evaluate whether underwriter concentration satisfies the relevance and exclusion restrictions of a valid instrumental variable. First, our proposed instrument should have a negative impact on the *Public* dummy because a higher degree of concentration of underwriters is related to the higher costs of doing an IPO and a lower likelihood that firms in the industry will go public. Second, industry-level underwriter concentration appears to be plausibly exogenous because, controlling for industry fixed effects, it is unlikely to be correlated with firms' trade credit policies, unless via the IPO channel.¹¹ Overall, our choice of instrument seems to a reasonable one.

¹¹ It is possible that underwriter concentration in a given industry is related to the degree of asymmetric information and financial constraint facing firms in the industry. This is because in industries with lower information problems and financial constraints, there may be a larger number of active underwriters. While we cannot completely rule out this possibility, by controlling for industry fixed effects as well as firm size and age in Model (7), and to the extent that the latter variables may proxy for financial constraints (Hadlock and Pierce, 2010), we mitigate to an extent the concern that underwriter concentration may be correlated with accounts payable through unobserved industry heterogeneity or an omitted variable (e.g., information problems and financial constraints) other than through the IPO channel.

In estimating Model (6) given (7), we follow prior research and employ the conventional maximum likelihood estimator (MLE) (Wooldridge, 2010; Greene, 2012). Table 6 reports the treatment regression results from using the (nonlinear) maximum likelihood estimation. In Column (1), the regression results of Model (6) show that the *Public* dummy remains negative and significant. This finding again suggests that public firms maintain a significantly lower level of trade credit than their private counterparts. The difference of 7 percentage points between these firms' trade credit levels is economically stronger than in our baseline result, suggesting that the estimated impact of listing status that we reported in earlier sections was a conservative one. In Column (2), the results of Model (7) indicate that the underwriter concentration variable is statistically significant and has the expected negative sign. This is consistent with our conjecture that, the higher the underwriter concentration, the higher the costs of doing an IPO, and the less likely it will be that firms are listed. We also note that the endogeneity test (*rho*) statistic in Column (1) is significant, supporting the argument that the going-public decision should be treated as endogenous, and validating our treatment regression approach using MLE.

[Insert Table 6 here]

In further robustness checks, we adopt two alternative methods for estimating Models (6) and (7). First, according to Maddala (1983, pp. 120–122), we re-estimate these models using the two-step estimation approach (as opposed to the linear two-stage least squares (2SLS) approach). In the first step, we estimate the probit model of the probability of listing and the hazard function (i.e., the inverse Mills ratio). In the second step, we regress trade credit on the endogenous *Public* dummy, the controls, and the estimated hazard function. As in our preferred approach using the MLE, this approach has an important advantage of explicitly accounting for the binary nature of the *Public* dummy. Angrist and Pischke (2008), however, suggest a simpler 2SLS

approach be used to yield consistent estimates of the average treatment effects. Hence, we also employ this method and treat the latent-variable Model (7) in the first stage as a linear probability model rather than a (nonlinear) probit model. We obtain qualitatively similar results (untabulated) when using these two alternative methods.

4.3. *Robustness Checks*

We perform several additional robustness checks and report the results in Table 7. In Columns (1)–(4), we include four additional control variables that may be correlated with both trade credit and the public dummy variable: namely, the cost of external finance, the annual rate of GDP growth, finished goods inventory, and accounts receivable. Specifically, we control for the cost of external finance in Column (1), consistent with previous studies examining small firms (e.g., Rodriguez-Rodriguez, 2006). We measure the cost of external finance as interest expense divided by the sum of total debt minus trade credit. The results show that this variable is positively related to trade credit, consistent with the argument that the higher the cost of external finance, the higher the demand for trade credit. In Column (2), we include the annual rate of GDP growth to proxy for macroeconomic conditions (e.g., Niskanen and Niskanen, 2006). During adverse conditions, trade credit is an alternative source of funds and thus demand for it may increase.¹² Consistent with this argument, we find a positive relation between the GDP growth rate and trade credit. In Column (3), we include the ratio of finished goods to inventory, to account for a possible correlation between inventory and accounts payable (Petersen and Rajan, 1997; Choi and Kim, 2005). Suppliers tend to provide more trade credit to firms with more inventory because they can repossess and resell the goods in the case of a customer default. However, suppliers have less of an advantage in liquidating assets and face higher liquidation

¹² GDP data come from the Federal Reserve Bank of St. Louis website.

costs if customers have transformed their inputs into outputs. Consequently, firms with a larger fraction of finished goods in their inventory may be granted less trade credit (Petersen and Rajan, 1997). The results show that the coefficient on finished goods inventory is positive and insignificant, which seems inconsistent with this conjecture. In Column (4), we include accounts receivable, to control for the interaction between trade credit demand and supply. We expect accounts receivable to be positively related to accounts payable due to the maturity-matching principle. The results, however, indicate that the coefficient on this variable is negative and insignificant. Overall, although the results across Columns (1)–(4) regarding some of the additional controls are mixed, we find that the coefficient on the *Public* dummy variable, our main variable of interest, continues to be significant and negative, which is consistent with our baseline finding.

[Insert Table 7 here]

Next, we examine the robustness of our results to alternative measures of the dependent variable, trade credit. In Column (5), we follow recent research (Love et al., 2007; Garcia-Appendini and Montoriol-Garriga, 2013) and scale accounts payable by the cost of goods sold.¹³ In Column (6), we scale accounts payable by total liabilities to control for differences in the capital structure between public and private firms (e.g., Fisman and Love, 2003). In both columns, we obtain qualitatively similar results that public listing has a negative and significant impact on the accounts payable ratio.¹⁴ In the remainder of the table, we address the concern that

¹³ In unreported analysis, we measure trade credit using days payable, which is accounts payable over cost of goods sold multiplied by 365. We also find that the *Public* dummy is significantly positive.

¹⁴ Another common measure of trade credit is the net trade credit ratio, often measured as the difference between accounts payable and accounts receivable, all scaled by total assets (Ge and Qiu, 2007). However, we refrain from using this measure because accounts receivable may be related to public listing. Thus, a significant effect of listing

the accounts payable ratio is, by definition, censored at 0 and 1. In Column (7), we take the natural logarithm of our original measure of trade credit, which is accounts payable scaled by total assets. In Column (8), we re-estimate our baseline model using the Tobit estimator. In both columns, we find that the coefficient on *Public* remains negative and significant, suggesting that our main finding is not affected by censoring.

Overall, the results from our identification strategies and various robustness checks all point to a negative relation between firms' stock market listing status and their use of supplier financing, which is consistent with *Hypothesis 1a* and inconsistent with *Hypothesis 1b*.¹⁵ Although it is difficult to completely rule out endogeneity concerns, taken together, those consistent and robust results suggest that our finding is unlikely to be simply driven by a spurious correlation between public listing and trade credit policy.

4.4. Cross-sectional Variations in the Impact of Listing Status on the Use of Trade Credit

We next examine whether the difference between public and private firms' trade credit levels varies according to certain firm-specific characteristics that proxy for the degrees of credit constraints and financial distress, and are related to motives for using trade credit. Thus far, our results suggest that stock market listing has a negative impact on the use of trade credit, consistent with the argument that public (private) firms have more (less) access to cheaper and

status on net trade credit may be due to its mechanical association with accounts receivable or driven by factors other than those examined in this paper.

¹⁵ Although *Hypotheses 1a* and *1b* are in line with the financial constraint and distress arguments, they provide contrasting predictions regarding the relation between stock market listing and trade credit, since they capture demand and supply factors affecting the use of trade credit by private firms, respectively. While it is difficult to distinguish between the demand and supply for trade credit, to the extent that our results document strong evidence of a negative impact of public listing on trade credit consistent with *Hypothesis 1a*, they suggest that, on average and under normal market conditions, demand effects may dominate supply effects in shaping private firms' trade credit policies.

less risky sources of external capital, and hence have a lower (higher) demand for supplier financing. To the extent that such results are driven by the financial constraint and distress risk mechanisms, we expect the negative impact of listing status on the use of trade credit to be more pronounced among those public firms that are less financially constrained and distressed.

To test this conjecture, we consider subsamples of firms with different degrees of financial constraints and distress risk. Following prior research, we measure financial constraints using firm size (Denis and Sibilkov, 2010) and the size and age (SA) index (Hadlock and Pierce, 2010). Since small firms are plausibly more constrained than their large counterparts, we define our first measure of financial constraint as *Small*, which is equal to 1 for firms with below-median size and 0 otherwise. Our second financial constraint measure is the *SA* dummy, which is equal to 1 for firms with above-median *SA* scores and 0 otherwise. Based on recent studies of the role of financial distress in shaping firms' trade credit policies (Molina and Preve, 2009; 2012; Hill et al., 2010), we employ two measures of distress risk. First, as in Molina and Preve (2009) we define a firm to be in financial distress if (i) its coverage ratio is less than 1 in two consecutive years or less than 0.8 in any given year (Asquith et al., 1994), and (ii) the firm's leverage is in the top two deciles of its industry in a given year. We use a dummy variable *FDLEV* to capture this definition of financial distress. Second, we consider a firm to be distressed if it has three consecutive years of losses (DeAngelo and DeAngelo, 1990; Molina and Preve, 2009). We construct a dummy variable *LOSSFD* to capture this definition.

In Table 8, we report the results from several augmented regression models that include *Small*, *SA*, *FDLEV*, and *LOSSFD*, and their interactions with the *Public* dummy, respectively. Based on the above conjecture, we expect the interaction terms between those dummies and *Public* to be significant and positive. Columns (1) and (2) present the regression results when

Small and *SA* are used as the measure of financial constraint, respectively. In Column (1), we exclude firm size to alleviate potential multicollinearity with *Small*. Similarly, in Column (2), we exclude the size and age variables, which are used to construct the *SA* dummy. The results show that the coefficients on $Small \times Public$ and $SA \times Public$ are significantly positive, suggesting that those public firms that are more constrained use more trade credit than those public firms that are less constrained. Put differently, the negative impact of public listing on the level of trade credit is more pronounced among those public firms that are less constrained, which is consistent with our expectation and the financial constraint argument.

[Insert Table 8 here]

Columns (3) and (4) of Table 8 reveal that the coefficient on $FDLEV \times Public$ and that on $LOSSFD \times Public$ are both significant and positive. Consistent with our conjecture, these results suggest that the negative impact of public listing on the use of trade credit is stronger among those public firms that are not in financial distress. On the other hand, distressed public firms seem to rely relatively more on supplier financing as a substitute for other forms of external capital than non-distressed public firms. This latter finding is consistent with Molina and Preve's (2012) evidence on the impact of financial distress on public firms' use of accounts payable.

Overall, our results suggest that the observed difference between public and private firms' use of trade credit varies, depending on these firms' degrees of financial constraints and distress. Our analysis thus provides further evidence in support of the financial constraint and distress risk channels through which stock market listing affects trade credit policy.

4.5. *Speed of Adjustment to Target Trade Credit*

We now turn to examining whether public and private firms adjust toward target trade credit levels and whether they do so with different adjustment speeds. We first estimate the target

levels of trade credit separately for public and private firms, using Model (3), and tabulate the results in Table I.A.3 of our online appendix. Our approach accounts for a difference between the target trade credit for public and private firms; indeed, we find evidence of significantly different coefficients on the determinants of those target trade credit levels.¹⁶

Panel A of Table 9 presents the regression results for the partial adjustment model of trade credit, i.e., Model (2). The results indicate that public and private firms adjust toward their target levels of trade credit at moderate rates. This result is consistent with the argument that firms have optimal trade credit (Nadiri, 1969) and that they seek to adjust toward this target. Empirically, our estimated speeds of adjustment are statistically significant but much lower in magnitude than the speed of adjustment estimated using a sample of UK SMEs in previous research (García-Teruel and Martínez-Solano, 2010b). More importantly, we find that private firms adjust at a lower rate (23%) than public firms (29%); the difference of 6 percentage points between these estimates is economically important as well as statistically significant according to the Chow test. This result supports *Hypothesis 2a* that public firms have a higher adjustment speed due to having lower adjustment costs. The finding that public firms adjust their trade credit more quickly than their private counterparts is in line with the earlier evidence on leverage adjustment (Brav, 2009).

[Insert Table 9 here]

¹⁶ In untabulated results, we estimate the target level of trade credit using a pooled sample of public and private firms. This approach is based on the assumption that public and private firms maintain the same target level; that is, the underlying relations between firm characteristics and the target level are homogeneous for both firm types. We obtain qualitatively similar results regarding the adjustment speeds.

To examine the robustness of this finding, in untabulated analysis we adopt an alternative estimation approach whereby, instead of estimating Models (3) and (2) in a two-step sequence, we substitute (3) into (2) to yield the following partial adjustment model:

$$TC_{it} = \beta_0 + (1 - \delta)TC_{i,t-1} + \gamma'X_{it} + \varepsilon_{it}. \quad (8)$$

We note that this approach is based on several studies in the capital structure literature (e.g., Flannery and Rangan, 2006; Öztekin and Flannery, 2012) and involves estimating Model (8) in a single step. Since this model specification is a dynamic panel data model, we follow recent research and estimate it using Blundell and Bond's (1998) system generalized method of moments (SYSGMM) estimator (e.g., Flannery and Hankins, 2013). The results obtained using SYSGMM for the subsamples of public and private firms continue to show that the former firms experience a higher speed of adjustment than the latter. They are qualitatively similar when we use lagged values of the controls. In short, our main finding regarding public and private firms' adjustment toward target trade credit policies is insensitive to an alternative estimation method.

In Panels B and C of Table 9, we conduct additional analysis to investigate whether the difference in the speed of trade credit adjustment between public and private firms varies depending on firms' deviation from target trade credit. To be consistent with the regressions in Panel A, we use the estimation approach detailed in Section 3.2. We find that conditional on being above the target level of trade credit, public firms have a speed of adjustment of approximately 40%, which is significantly higher than the speed of 26% for private firms. In contrast, conditional on being below the target level, private firms have a higher speed of adjustment than public firms (20% versus 11%). The former finding is again in line with our argument that public firms have lower adjustment costs, enabling those with above-target trade credit to revert quickly toward the target level and helping them mitigate the costs of distress. On

the other hand, private firms adjust their trade credit relatively quickly when they are below the target, possibly due to the importance of trade credit as a major source of short-term financing for these firms, and hence the need for them to avoid a shortage of this form of credit.

4.6. Additional Analysis: Use of Trade Credit during a Crisis

In this section, we perform additional analysis to examine the potential differential impacts of credit conditions on public and private firms' trade credit policies. We focus on the recent financial crisis of 2007–2009 because it provides an ideal scenario in which to study the impact of a supply-driven credit contraction on the use of trade credit. Recent research argues that the financial crisis represents a severe shock to the supply of external finance and is not caused by business fundamentals in the real sector (e.g., Duchin et al., 2010; Almeida et al., 2011; Garcia-Appendini and Montoriol-Garriga, 2013).

We expect that public and private firms responded to the 2007–2009 credit crunch in different ways. First, compared with publicly listed firms, privately held companies might have had a greater demand for trade credit during the financial crisis, given that they are already more constrained and have more limited access to alternative forms of credit. This prediction is based on the redistribution view of trade credit provision, which argues that bank credit can be redistributed from large, liquid firms to financially less secure firms via trade credit (Meltzer, 1960; Biais and Gollier, 1997; Nilsen, 2002). It is further motivated by prior results regarding the effect of monetary tightening on public constrained firms' trade credit ratios (Atanasova, 2007; Garcia-Appendini and Montoriol-Garriga, 2013). Overall, we conjecture that both public and private firms used more trade credit during the recent financial crisis and, moreover, that the increase in private firms' level of trade credit was higher than that for public firms.

Although firms' demand for trade credit may increase during crisis periods, it may not be accommodated by suppliers. During a severe credit crunch such as the recent financial crisis, suppliers may face high costs of funds and be credit constrained themselves, thus becoming even more cautious in their trade credit provision. Examining the impact of international currency crises on trade credit policies, Love et al. (2007) document an increase in the level of trade credit in the crisis year, followed by a sharp and prolonged contraction post-crisis, as suppliers vulnerable to the crises extended much less trade credit due to a shortage of funds. Similarly, Garcia-Appendini and Montoriol-Garriga (2013) show that the increase in trade credit provision observed during the financial stage of the recent financial crisis became weaker over the complete crisis episode, which saw a drop in economic activity and an increase in cash hoarding by suppliers. These arguments and findings lead to an alternative prediction that, on average, public firms and particularly their private counterparts might not have experienced a significant increase in their use of trade credit during the recent financial crisis.

To test the above alternative predictions, we estimate the following model:

$$TC_{it} = \beta_0 + \beta_1 Crisis_{it} + \beta_2 Public_{it} + \beta_3 Crisis_{it} \times Public_{it} + \theta' X_{it} + \varepsilon_{it}. \quad (9)$$

Crisis is a dummy variable that is equal to 1 in the years 2007–2009 and 0 otherwise. *Crisis* $\times$ *Public* is an interaction term between the *Public* and *Crisis* dummy variables. We note that our chosen period of crisis consists of the first stage of the crisis from July 2007 to June 2008, as well as the second stage from just after the bankruptcy of Lehman Brothers in September 2008 to the fourth quarter of 2009; however, our main findings (unreported) are qualitatively similar if we restrict the crisis period to 2007 or 2007–2008. In our estimations, we consider two sample periods. First, to avoid confounding effects due to other periods of macroeconomic fluctuation before 2003, we estimate Model (9) for the period 2004–2009. This choice of sample period is

broadly consistent with research examining the effect of the recent financial crisis (e.g., Garcia-Appendini and Montoriol-Garriga, 2013).¹⁷ To examine the robustness of the results, we further estimate Model (8) for the whole sample period between 1995 and 2012.

Table 10 reports the regression results for several specifications based on Model (9). In the first three columns, the sample period is between 2004 and 2009. Column (1) shows the effects of the financial crisis on the use of trade credit by public and private firms. Specifically, the impact of the crisis on private firms, captured by the coefficient on *Crisis* (-0.0104), is negative and significant. This result suggests that, during the recent financial crisis, private firms experienced a 1 percentage point decrease in their trade credit ratio, or a 6% decrease relative to the mean trade credit of these firms. We obtain similar results (untabulated) for private firms when examining the first phase of the financial crisis before the collapse of Lehman Brothers. These findings are consistent with the prediction that private firms might have demanded more trade credit but found their demand unmatched by suppliers' willingness to lend. During the crisis, suppliers might have been reluctant to lend to private firms because these firms are more constrained and thus are more likely to default on their payments, which in turn may pass on a significant and sizeable fraction of the liquidity shocks to suppliers (Boissay and Gropp, 2013).¹⁸ Our evidence of a decline in private firms' use of trade credit during the crisis is a new and important empirical result, and complements existing US evidence of the crisis's effect on public firms' trade-credit policies.

¹⁷ The results are unchanged when we extend the sample to 2004–2012 and examine a longer post-crisis period.

¹⁸ Alternatively, private firms' demand for trade credit could have decreased to the point that they became less willing to take credit. However, to the extent that the recent financial crisis represents an exogenous and unexpected shock that is supply-driven in nature (Garcia-Appendini and Montoriol-Garriga, 2013), this explanation is likely to be more relevant for the later phase of the crisis, which saw a drop in overall economic activity.

The results in Column (1) also show that the crisis effect on public firms, captured by the coefficient on the interaction term *Crisis* $\times$ *Public* (0.0104), plus that on the stand-alone *Crisis* dummy (-0.0104), is economically insignificant. This finding is consistent with the alternative view that firms might not have experienced an increase in the amount of trade credit used during the crisis, but appears to be inconsistent with existing research (e.g., Garcia-Appendini and Montoriol-Garriga, 2013). However, a closer examination of the finding suggests that it can be reconciled with existing evidence in the literature. First, one possible reason for our insignificant finding is that it captures the combined effects on both constrained and unconstrained public firms. Garcia-Appendini and Montoriol-Garriga (2013) show that during the financial crisis the use of trade credit only increased among the former group. We obtain similar results (untabulated) when focusing on a subsample of public firms that are smaller and thus are potentially more constrained: we find that those firms also experienced an increase in their trade credit. Another potential reason for our insignificant finding is the focus of our study on the longer-term effect of the crisis. While Garcia-Appendini and Montoriol-Garriga (2013) use quarterly data and mainly focus on the first (financial) stage of the crisis, from July 2007 to June 2008 (before the collapse of Lehman Brothers), we analyze annual data and cover a longer crisis period. Our results could reflect the combined effects of the first stage of the crisis and the post-Lehman Brothers phase associated with a full-blown economic recession. In further analysis (untabulated), we find that the effect of the first stage of the crisis on the average public firm is positive and significant.

[Insert Table 10 here]

In Columns (2)–(3) of Table 10, we further investigate potential heterogeneous effects of the crisis on the trade credit ratios of public and private firms, conditional on two measures of

financial vulnerability: namely, short-term debt and cash flow. Love et al. (2007) argue that, pre-crisis, firms with a high level of short-term debt are more vulnerable to credit shocks while those with a high level of cash flow are less susceptible to such shocks. We consider 2006 as the pre-crisis year and use the 2006 values of the aforementioned variables as proxies for financial vulnerability. The results in Column (2) show that for private firms, the impact of the crisis, captured by the sum of the coefficients on *Crisis* (-0.0096) and *Crisis* $\times$ *ST debt_{pre-crisis}* (-0.0833), is significant and always negative as *ST debt_{pre-crisis}* is in the unit interval. Moreover, they indicate that a one standard deviation increase in *ST debt_{pre-crisis}* (0.0828) is associated with a decrease in a private firm's trade credit ratio of 0.69 percentage points (or a decrease of 4.37% relative to the mean trade credit of 0.158). Regarding public firms, the impact of the crisis on the use of trade credit is given by the sum of the coefficients on *Crisis* (-0.0096), *Crisis* $\times$ *Public* (0.0097), *Crisis* $\times$ *ST debt_{pre-crisis}* (-0.0833), and *Crisis* $\times$ *Public* $\times$ *ST debt_{pre-crisis}* (0.0668), which is marginally negative at the mean *ST debt_{pre-crisis}* (0.0225). Moreover, a one standard deviation increase in *ST debt_{pre-crisis}* (0.0828) results in a decrease in a public firm's trade credit ratio of 0.14 percentage points (i.e., $0.0828 \times (-0.0833 + 0.0668) = -0.0014$), or a 1.56% decrease relative to the mean trade credit ratio of 0.0896. Overall, these findings are in line with the results shown in Column (1) and suggest that the more short-term debt public and private firms had, the more vulnerable they were pre-crisis, and the more difficult it was for them to obtain trade credit as a substitute form of credit.

Column (3) reports the impact of the crisis on public and private firms' trade credit conditional on these firms' pre-crisis level of cash flow. The impact of the crisis on the trade credit ratio of private firms, equal to the sum of the coefficients on *Crisis* (-0.0065) and *Crisis* $\times$ *Cash flow_{pre-crisis}* (-0.0334), is negative at the mean pre-crisis level of cash flow (0.0729). Further,

a one standard deviation increase in $Cash\ flow_{pre-crisis}$ (0.1949) leads to a decrease in private firms' trade credit ratio of 0.65 percentage points (or a decrease of 4.1% relative to their mean of 0.158). In terms of public firms, the impact of the crisis on their trade credit ratio is given by the sum of the coefficients on $Crisis$ (-0.0065), $Crisis \times Public$ (0.0065), $Crisis \times Cash\ flow_{pre-crisis}$ (-0.0334), and $Crisis \times Public \times Cash\ flow_{pre-crisis}$ (0.0325), which is marginally negative at the mean pre-crisis level of cash flow (0.0729). Additionally, a one standard deviation increase in $Cash\ flow_{pre-crisis}$ (0.1949) is associated with a decrease in public firms' trade credit ratio of 0.02 percentage points (i.e., $0.1944 \times (-0.0334 + 0.0325) = -0.0002$), or a 0.2% decrease relative to the mean trade credit ratio of 0.0896. Overall, the results for both public and private firms suggest that those firms with a greater cash flow generating capacity pre-crisis required slightly less trade credit during the crisis. They provide modest evidence in support of the argument that firms with higher cash flow levels are cushioned during a credit shock, consistent with the substitution effect.

Finally, in Columns (4)–(6), we examine the robustness of our results regarding the effect of the financial crisis by repeating the above analysis for a longer sample period between 1995 and 2012.¹⁹ We find that the results are broadly similar to those in Columns (1)–(3). The financial crisis negatively affected the amount of trade credit used by private firms but this effect was economically weaker for public firms. Moreover, the impact of the crisis on public and private firms' trade credit policies is conditional on those firms' financial vulnerability, measured by their pre-crisis level of short-term debt and cash flow.

¹⁹ The US economy experienced a recession between March and November 2001, following the collapse of the “dotcom” bubble. Hence, in (unreported) additional analysis, we further control for this brief period of economic downturn by including a recession dummy, which is equal to 1 if the year is 2001 and 0 otherwise, as well as its interaction term with *Public*. We find that our main results regarding the impact of the recent financial crisis on the use of trade credit by public and private firms remain qualitatively similar.

5. Conclusion

In this paper, we empirically examine the impact of the going-public decision on different aspects of trade credit policies. Using data for both public and private firms from the S&P Capital IQ database for the period 1995–2012, we find strong evidence to support the hypothesis that public firms rely less on trade credit because they have greater access to cheaper and less risky sources of external capital. This finding persists in several robustness tests and identification strategies that help mitigate sample selection and endogeneity concerns. We also find that the negative impact of public listing on trade credit is more pronounced in firms that are less financially constrained and have lower distress risk. These results provide additional evidence that our main finding is driven by the financial constraint and distress risk mechanisms.

We further examine firms' optimal levels of trade credit and dynamic adjustments toward those optimal levels. Our findings show that public and private firms adjust quite actively toward their target trade credit levels. However, public firms are able to move faster, which is consistent with the argument that they face lower adjustment costs due to having greater bargaining power with suppliers and greater access to other forms of credit. While our findings show how reliant private firms are on supplier financing, they also indicate how difficult it might be for them to adjust their trade credit in order to remain at, or close to, their optimal level.

In additional analysis, we document heterogeneous effects of a credit crunch on the use of supplier financing by public and private firms. During the recent financial crisis, public firms experienced an insignificant change in their use of trade credit. In contrast, private firms were granted significantly less trade credit. The latter finding indicates that private firms' demand for trade credit may not always be accommodated by suppliers during a credit crunch, when the supply of both bank and trade credit dries up.

In summary, our study highlights another potential benefit of stock market listing: going public provides firms with greater access to relatively cheaper and longer-term external capital than staying private, enabling public firms to rely less on trade credit, a short-term and potentially costly source of financing. Moreover, while our results demonstrate the importance of trade credit to private firms, they highlight the limitations of this form of credit in terms of absorbing credit shocks. Our study thus provides implications for policies aimed at enhancing the flow of lending to private firms during times of extreme financial stress.

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Figure 1. Evolution of Trade Credit and Listing Status

This figure illustrates the evolution of trade credit around the IPO year for a transition sample of 1,282 IPO firms that were private and subsequently went public during the sample period. Trade credit is defined as the ratio of accounts payable to total assets.

Table 1. Summary Statistics and Univariate Analysis

Panel A of this table reports the summary statistics (i.e., the mean, median, standard deviation (std. dev.), minimum (min), and maximum (max)) of the variables under consideration. Trade credit is defined as the ratio of accounts payable to total assets. Firm age is the number of years from incorporation. Cash flow is measured as net profits plus depreciation, scaled by total assets. $Cash\ flow_{pre-crisis}$ is the pre-crisis (2006) level of cash flow. Cash holdings is cash and cash equivalents, scaled by total assets. Current assets is current assets minus cash, scaled by total assets. Negative growth is sales growth times the negative growth dummy variable, which is equal to 1 if sales growth is negative and 0 otherwise. Positive growth is sales growth times the positive growth dummy variable, which is equal to 1 if sales growth is positive and 0 otherwise. Short-term debt is short-term borrowings plus the current portion of long-term debt, scaled by total assets. $ST\ debt_{pre-crisis}$ is the pre-crisis (2006) level of short-term debt. Size is the natural logarithm of total sales, measured in 2012 dollar prices. The sample period is 1995–2012. The total number of firm-year observations in our sample is 103,777. Panel B presents the univariate analysis of the trade credit of the public and private firms in the full sample using the *t*-test for differences in mean and the Wilcoxon-Mann-Whitney test for differences in median.

Panel A: Summary Statistics					
Variable	Mean	Median	Std. dev.	Min	Max
Trade credit (%)	0.1359	0.0898	0.1324	0.0077	0.5612
Age (years)	39.97	31.000	29.49	0.0000	135.00
Cash flow (%)	0.0861	0.0540	0.2001	-0.5000	1.5003
Cash flow _{pre-crisis} (%)	0.0729	0.0335	0.1949	-0.5000	1.5003
Cash holdings (%)	0.1344	0.7742	0.1553	0.0000	1.0000
Current assets (%)	0.5071	0.5098	0.2561	0.0000	0.9819
Negative growth	-0.0523	0.0000	0.1262	-0.9999	0.0000
Positive growth	0.2068	0.0650	0.7545	0.0000	15.7500
Short-term debt (%)	0.0301	0.0000	0.0843	0.0000	1.0000
ST debt _{pre-crisis} (%)	0.0225	0.0000	0.0828	0.0000	1.0000
Size (Ln)	2.7121	2.1207	2.0889	-3.6498	8.2483

Panel B: Univariate Analysis				
Trade credit	Public	Private	Mean test (p-value)	Median test (p-value)
Mean	0.0896	0.1582	0.000	
Median	0.0663	0.1108		0.000
Number of obs.	33,766	70,011		

Table 2. Use of Trade Credit by Public and Private Firms across Industries

This table shows the level of trade credit according to the 12 Fama-French industry classifications. Trade credit is defined as the ratio of accounts payable to total assets. Panel A provides the summary statistics (i.e., the mean, median, standard deviation (std. dev.), minimum (min), and maximum (max)) of the trade credit ratio across industries. Panel B shows how the trade credit ratios of public and private firms vary across industries. It reports the p -values of the t -test for differences in mean and the Wilcoxon-Mann-Whitney test of differences in median.

Panel A: Full Sample							
Industry	Industry description	Mean	Median	Std. dev.	Min	Max	N. obs.
1	Consumer non-durables	0.1064	0.0787	0.0941	0.0109	0.4732	4,794
2	Consumer durables	0.1257	0.0998	0.0979	0.0147	0.5621	1,866
3	Manufacturing	0.1080	0.0863	0.0797	0.0171	0.3829	11,762
4	Energy	0.0819	0.0548	0.0936	0.0019	0.9086	2,536
5	Chemicals	0.1049	0.0880	0.0779	0.0159	0.6777	1,891
6	Business equipment	0.1018	0.0637	0.1140	0.0069	0.7944	9,787
7	Telecommunications	0.0690	0.0337	0.1083	0.0029	0.7876	1,610
9	Retail and wholesale	0.1882	0.1474	0.1473	0.0168	0.5751	26,823
10	Health	0.0654	0.0409	0.0758	0.0077	0.5261	9,098
12	Others	0.1467	0.0938	0.1451	0.0054	0.5507	33,610

Panel B: Public Firms versus Private Firms								
Industry	Industry description	Firm type	Mean	Median	Std. dev.	N. obs.	Mean test	Median test
1	Consumer non-durables	Private	0.1295	0.0915	0.1149	2,383		
		Public	0.0836	0.0715	0.0591	2,411	0.000	0.000
2	Consumer durables	Private	0.1480	0.1182	0.1217	710		
		Public	0.1120	0.0940	0.0767	1,156	0.000	0.000
3	Manufacturing	Private	0.1226	0.0969	0.0933	6,460		
		Public	0.0904	0.0788	0.0539	5,302	0.000	0.000
4	Energy	Private	0.1517	0.0887	0.1717	340		
		Public	0.0711	0.0524	0.0685	2,196	0.000	0.000
5	Chemicals	Private	0.1330	0.1035	0.1132	523		
		Public	0.0942	0.0836	0.0554	1,368	0.000	0.000
6	Business equipment	Private	0.1547	0.0982	0.1604	2,846		
		Public	0.0800	0.0564	0.0784	6,941	0.000	0.000
7	Telecommunications	Private	0.0776	0.0305	0.1282	708		
		Public	0.0622	0.0365	0.0892	902	0.005	0.048
9	Retail and wholesale	Private	0.1949	0.1535	0.1516	22,440		
		Public	0.1541	0.1238	0.1178	4,383	0.000	0.000
10	Health	Private	0.0695	0.0394	0.0861	5,058		
		Public	0.0603	0.0426	0.0602	4,040	0.000	0.370
12	Others	Private	0.1587	0.1076	0.1495	28,543		
		Public	0.0793	0.0479	0.0922	5,067	0.000	0.000

Table 3. Public Listing and Use of Trade Credit by Public and Private Firms

This table reports the regression results for Model (1), which captures the difference between the trade credit levels of public and private firms. Columns (1)–(2) provide the results for the full sample. The dependent variable is trade credit, defined as the ratio of accounts payable to total assets. *Public* is a dummy variable that is equal to 1 for public firms and 0 otherwise. Firm age is the number of years from incorporation. Cash flow is net profits plus depreciation, scaled by total assets. Cash holdings are cash and cash equivalents, scaled by total assets. Current assets are current assets minus cash, scaled by total assets. Negative (positive) growth is sales growth times the negative (positive) growth dummy, which is equal to 1 if sales growth is negative (positive) and 0 otherwise. Short-term debt is short-term borrowing plus the current portion of long-term debt, scaled by total assets. Size is the natural logarithm of total sales, measured in 2012 dollar prices. *T*-statistics are reported in parentheses. Standard errors are clustered at the firm level ^{***}, ^{**}, and ^{*} denote statistical significance at the 1, 5, and 10% levels, respectively.

	(1)	(2)
Public	-0.0435 ^{***} (-18.99)	-0.0362 ^{***} (-14.82)
ln(1+age)		-0.0198 ^{***} (-4.82)
ln(1+age) ²		-0.0002 (-0.28)
Cash flow		-0.0474 ^{***} (-17.24)
Cash holdings		0.0761 ^{***} (16.44)
Current assets		0.1961 ^{***} (54.69)
Negative growth		0.0224 ^{***} (6.56)
Positive growth		0.0039 ^{***} (7.39)
Short-term debt		-0.0707 ^{***} (-11.15)
Size		0.0079 ^{***} (17.04)
Intercept	0.1291 ^{***} (30.26)	0.0817 ^{***} (8.29)
Industry effects	Yes	Yes
Year effects	Yes	Yes
Number of obs.	103,777	103,777
Adjusted R ²	0.176	0.283

Table 4. Propensity Score Matching Analysis

This table reports the results of the propensity score matching. We match each public firm to a private firm using one-to-one propensity score matching to the nearest neighborhood, without replacement. In Specification (1), the matching is based on size, industry, and year. In Specification (2), the matching is based on all control variables, including $\ln(1+\text{age})$, $\ln(1+\text{age})^2$, cash flow, cash holdings, current assets, sales growth, short-term debt, size, industry, and year effects. Panel A presents the pairwise differences between the mean trade credit ratios of the matched samples. Bootstrapped standard errors (Std. error) based on 50 replications are reported in square brackets. Panel B reports the baseline regression results for the matched samples. *T*-statistics are reported in parentheses. Standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the 1, 5, and 10% levels, respectively.

Panel A: Pairwise Differences in the Mean Trade Credit		
	Specification (1)	Specification (2)
Difference	-0.0759***	-0.0514***
Std. error	[0.0015]	[0.0015]

Panel B: Regression Results for the Propensity-score-matched Sample		
	Specification (1)	Specification (2)
Public	-0.0546*** (-21.93)	-0.0553*** (-23.41)
$\ln(1+\text{age})$	-0.0152** (-2.49)	-0.0170*** (-2.64)
$\ln(1+\text{age})^2$	-0.0080 (-0.85)	0.0001 (0.10)
Cash flow	-0.0307*** (-6.97)	-0.0493*** (-7.09)
Cash holdings	0.0372*** (6.62)	0.0717*** (10.51)
Current assets	0.2005*** (42.88)	0.2068*** (40.03)
Negative growth	0.0197*** (3.76)	0.0074 (1.20)
Positive growth	0.0019*** (3.34)	0.0136*** (5.29)
Short-term debt	-0.0232** (-2.08)	-0.0815*** (-8.06)
Size	0.0039*** (6.69)	0.0044*** (6.66)
Intercept	0.6753 (1.52)	-1.0456** (-2.27)
Industry effects	Yes	Yes
Year effects	Yes	Yes
Number of obs.	29,010	25,530
Adjusted R ²	0.260	0.243

Table 5. Regression Results for the Transition Sample

This table presents the regression results for the transition sample of 1,282 IPO firms. The dependent variable is trade credit, defined as the ratio of accounts payable to total assets. $D_{\text{Post_IPO}}$ is a dummy variable that is equal to 1 from the IPO year onward and 0 in the pre-IPO years. $D_{\text{IPO}+i}$ with $i=1\ldots 3$ is a dummy variable that is equal to 1 in the year $\text{IPO}+i$ and 0 otherwise. $D_{\text{IPO}p4}$ is a dummy variable that is equal to 1 for the period at least four years after the listing event. The other independent variables are defined in Table 3. T -statistics are reported in parentheses. Standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the 1, 5, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
$D_{\text{Post_IPO}}$	-0.0148*** (-3.46)	-0.0113*** (-4.21)		
D_{IPO}			-0.0107*** (-2.66)	-0.0096*** (-4.62)
$D_{\text{IPO}+1}$			-0.0149*** (-3.57)	-0.0126*** (-5.83)
$D_{\text{IPO}+2}$			-0.0129*** (-2.94)	-0.0106*** (-4.56)
$D_{\text{IPO}+3}$			-0.0143*** (-3.02)	-0.0143*** (-5.80)
$D_{\text{IPO}p4}$			-0.0073 (-1.35)	-0.0120*** (-4.75)
$\ln(1+\text{age})$	-0.0055 (-0.85)	-0.0099 (-1.18)	-0.0071 (-1.08)	-0.0093** (-2.12)
$\ln(1+\text{age})^2$	0.0014 (1.10)	0.0019 (0.53)	0.0016 (1.25)	0.0019 (1.08)
Cash flow	-0.0265*** (-2.79)	-0.0069 (-1.22)	-0.0265*** (-2.79)	-0.0071* (-1.81)
Cash holdings	0.0408*** (3.65)	0.0215** (2.42)	0.0405*** (3.57)	0.0209*** (4.93)
Current assets	0.1357*** (11.01)	0.0844*** (7.09)	0.1353*** (10.97)	0.0843*** (19.51)
Negative growth	0.0049 (0.48)	0.0166** (2.40)	0.0067 (0.65)	0.0164*** (3.69)
Positive growth	-0.0002 (-0.32)	8.34×10^{-05} (0.20)	-0.0002 (-0.30)	6.80×10^{-05} (0.22)
Short-term debt	0.0086 (0.25)	0.0525*** (3.55)	0.0086 (0.25)	0.0525*** (6.46)
Size	0.0028* (1.77)	-9.84×10^{-05} (-0.06)	0.0024 (1.52)	-3.28×10^{-05} (-0.04)
Intercept	0.0319* (1.67)	0.0894*** (5.81)	0.0366* (1.92)	0.0890*** (10.52)
Industry effects	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes
Firm effects	No	Yes	No	Yes
Number of obs.	9,976	9,976	4,444	4,444
Adjusted R^2	0.302	0.086	0.300	0.087

Table 6. Treatment Regression Results

This table reports the results of the treatment regression specified in Models (6) and (7), in which the *Public* dummy variable, which is equal to 1 for public firms and 0 otherwise, is treated as endogenous. We instrument for this variable using the industrywide underwriter concentration variable, defined as the number of IPOs underwritten by the top 5 underwriters, divided by the number of IPOs in that industry. The top 5 underwriters are determined based on the number of IPOs they have underwritten in the last five years. Columns (1)–(2) report the regression results corresponding to Models (6) and (7), obtained using the maximum likelihood estimator. The other independent variables are defined in Table 3. *Rho* measures the correlation between the errors in Models (6) and (7) and thus is used to test for the endogeneity of the *Public* dummy; the *p*-value of this test is reported in square brackets. *T*-statistics are reported in parentheses. Standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the 1, 5, and 10% levels, respectively.

	(1) Trade credit	(2) Public dummy
Public	-0.0482*** (-7.20)	
ln(1+age)	-0.0201*** (-8.10)	-0.0459 (-1.35)
ln(1+age) ²	-0.0002 (-0.65)	-0.0371*** (-7.03)
Cash flow	-0.0506*** (-16.68)	-2.5466*** (-58.38)
Cash holdings	0.0779*** (28.39)	0.2798*** (7.90)
Current assets	0.1901*** (60.80)	-1.5001*** (-66.74)
Negative growth	0.0186*** (4.97)	-1.1572*** (-27.24)
Positive growth	0.0046*** (6.88)	0.5524*** (32.04)
Short-term debt	-0.0691*** (-12.57)	1.8271*** (31.99)
Size	0.0095*** (11.02)	0.4582*** (147.67)
Underwriter concentration		-0.3786*** (-21.74)
Intercept	0.0839*** (10.78)	-0.4103*** (-7.12)
Rho	0.0675* [0.053]	
Industry effects		Yes
Year effects		Yes
Number of obs.		92,495

Table 7. Additional Robustness Checks

This table presents the regression results from several robustness checks. Columns (1)–(4) report the results after including additional control variables. Specifically, Column (1) includes *Fcost*, which is interest expense divided by total debt minus accounts payable. Column (2) includes the annual rate of GDP growth. Column (3) includes finished goods inventory, measured as the ratio of finished goods to inventory. Column (4) includes accounts receivable, measured as accounts receivable scaled by total sales. Columns (5)–(7) report the regression results for three alternative definitions of the dependent variable, namely, (i) accounts payable scaled by cost of goods sold, (ii) accounts payable scaled by total liabilities, and (iii) the natural logarithm of the ratio of accounts payable to total assets, respectively. Column (8) reports the results from a Tobit regression of the baseline model using the original measure of trade credit (i.e., accounts payable scaled by total assets). The other independent variables are defined in Table 3. *T*-statistics are reported in parentheses. Standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the 1, 5, and 10% levels, respectively.

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	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Public	-0.0383*** (-15.20)	-0.0362*** (-14.82)	-0.0311*** (-10.93)	-0.0360*** (-14.73)	-0.0220** (-2.04)	-0.0129*** (-2.92)	-0.2700*** (-11.96)	-0.0362*** (-29.18)
ln(1+age)	-0.0197*** (-4.81)	-0.0198*** (-4.82)	-0.0196*** (-4.16)	-0.0198*** (-4.84)	-0.0646*** (-5.86)	-0.0117* (-1.77)	-0.1359*** (-4.07)	-0.0198*** (-8.48)
ln(1+age) ²	-0.0002 (-0.27)	-0.0002 (-0.28)	-0.0002 (-0.33)	-0.0002 (-0.27)	0.0083*** (5.26)	-0.0005 (-0.44)	0.0028 (0.52)	-0.0002*** (-0.50)
Cash flow	-0.0469*** (-17.09)	-0.0474*** (-17.24)	-0.0396*** (-12.42)	-0.0475*** (-17.26)	-0.1139*** (-6.94)	0.0081 (1.53)	-0.3207*** (-13.86)	-0.0474*** (-25.73)
Cash holdings	0.0769*** (16.60)	0.0761*** (16.44)	0.0399*** (7.28)	0.0759*** (16.19)	-0.0902*** (-5.13)	0.3521*** (37.82)	0.4135*** (9.94)	0.0761*** (29.39)
Current assets	0.1967*** (54.88)	0.1961*** (54.69)	0.1789*** (43.36)	0.1965*** (53.27)	-0.1558*** (-14.89)	0.3184*** (49.89)	1.5593*** (49.65)	0.1961*** (107.74)
Negative growth	0.0224*** (6.57)	0.0224*** (6.56)	0.0323*** (8.13)	0.0220*** (6.01)	-0.3150*** (-11.19)	0.0127* (1.95)	0.3182*** (10.51)	0.0224*** (7.53)
Positive growth	0.0039*** (7.06)	0.0039*** (7.39)	0.0039*** (5.68)	0.0039*** (7.37)	0.0250*** (8.49)	0.0039*** (3.71)	0.0109** (2.10)	0.0039*** (8.27)
Short-term debt	-0.0724*** (-11.45)	-0.0707*** (-11.15)	-0.0801*** (-11.60)	-0.0708*** (-11.18)	0.0178 (0.76)	-0.4042*** (-47.49)	-0.3271*** (-5.09)	-0.0707*** (-16.62)
Size	0.0081*** (17.28)	0.0079*** (17.04)	0.0063*** (12.02)	0.0079*** (16.96)	-0.0209*** (-19.30)	-0.0071*** (-8.58)	0.0742*** (17.32)	0.0079*** (34.81)
Fcost	0.0564*** (3.22)							
ΔGDP		0.0005* (1.82)						
Finished goods inventory			0.0015 (0.46)					
Accounts receivable				-0.0032 (-0.29)				
Intercept	0.0805*** (13.88)	0.0739*** (7.50)	0.0944*** (8.32)	0.0821*** (8.29)	0.3617*** (13.52)	0.1665*** (9.77)	-2.9713*** (-33.33)	0.0734*** (13.25)
Industry effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of obs.	103,777	103,777	77,705	103,777	33,922	103,625	103,777	103,777
Adjusted R ²	0.283	0.283	0.208	0.283	0.238	0.268	0.298	
LR chi-squared								34,494***

Table 8. Regression Results Conditional on Financial Constraint and Distress

This table presents the effects of financial constraints and financial distress on the difference in the level of trade credit used by public and private firms. We use two measures of financial constraints, namely firm size and the size and age (SA) index. *Small* is a dummy variable that is equal to 1 for observations with below the median firm size and 0 otherwise. *SA* is a dummy variable that is equal to 1 for observations with above the median SA index and 0 otherwise. Based on Hadlock and Pierce (2010), the SA index is constructed as $-0.737\text{size} + 0.043\text{size}^2 - 0.040\text{age}$, where size equals the log of inflation-adjusted total assets in 2004 dollars. We use two measures of financial distress, namely *FDLEV* and *LOSSFD*. *FDLEV* is a dummy variable that is equal to one if (i) the firm's leverage (defined as total debt divided by total assets) is in the top two deciles of its industry in a particular year, and (ii) its coverage ratio (defined as earnings before interest, taxes, depreciation, and amortization [EBITDA] divided by interest expenses) is less than 1 for two consecutive years or less than 0.8 in any given year, and 0 otherwise. *LOSSFD* is a dummy variable that is equal to 1 if the firm has three consecutive years of losses and 0 otherwise. The dependent variable is trade credit, defined as the ratio of accounts payable to total assets. *Public* is a dummy variable that is equal to 1 for public firms and 0 otherwise. The other independent variables are defined in Table 3. *T*-statistics are reported in parentheses. Standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively.

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	(1)	(2)	(3)	(4)
Public	-0.0140*** (-4.40)	-0.0308*** (-11.68)	-0.0122*** (-2.96)	-0.0348*** (-12.04)
Small×Public	0.0157*** (4.80)			
Small	-0.0273*** (15.46)			
SA×Public		0.0209*** (6.36)		
SA		-0.0236*** (-13.28)		
FDLEV×Public			0.0185** (2.07)	
FDLEV			-0.0062 (-0.70)	
LOSSFD×Public				0.0105** (2.02)
LOSSFD				0.0036 (0.77)
ln(1+age)	-0.0235 (-5.74)		-0.0176*** (-6.59)	-0.0200*** (-3.93)
ln(1+age) ²	0.0008 (1.20)		0.0020*** (5.02)	0.0004 (0.57)
Cash flow	-0.0408*** (-14.94)	-0.0367*** (-13.33)	-0.0447*** (-9.60)	-0.0449*** (-12.09)
Cash holdings	0.0654*** (14.32)	0.0729*** (15.86)	0.0136*** (3.45)	0.0546*** (10.07)
Current assets	0.1922*** (54.12)	0.1999*** (55.97)	0.1622*** (47.67)	0.1835*** (39.99)
Negative growth	0.0345 (10.40)	0.0311*** (9.35)	0.0258*** (5.69)	0.0204*** (5.04)
Positive growth	0.0022*** (4.35)	0.0045*** (8.20)	0.0012** (2.17)	0.0027*** (4.86)
Short-term debt	-0.0798*** (-12.82)	-0.0762*** (-12.03)	0.0161** (2.38)	-0.0333*** (-3.92)
Size			0.0021*** (8.21)	0.0071*** (13.07)
Intercept	0.0897*** (9.12)	0.0397*** (5.30)	0.0492*** (6.24)	0.0881*** (6.95)
Industry effects	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes
Number of obs.	103,777	103,777	29,317	68,880
Adjusted R ²	0.281	0.269	0.284	0.289

Table 9. Speed of Adjustment to Target Trade Credit

This table presents the regression results for the partial adjustment model of trade credit. It reports the estimated speed of adjustment, showing how quickly public and private firms adjust toward their respective target levels of trade credit. Panel A presents the results for the full sample of public and private firms. Panel B provides the results for the subsample of public and private firms with above-target trade credit. Panel C reports the results for the subsample of public and private firms with below-target trade credit. The dependent variable, ΔTC_{it} , is the change in trade credit. The independent variable, $TC_{it}^* - TC_{i,t-1}$, is the deviation from target trade credit, where TC_{it}^* is the estimated target trade credit (see Table I.A.3 in the online appendix for more details). *P*-values of the Chow test of differences in the adjustment speed estimates are reported in square brackets. *T*-statistics are reported in parentheses. Standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the 1, 5, and 10% levels, respectively.

Panel A: Full Sample			
	Public Firms	Private Firms	F-stat of Chow test
	ΔTC_{it}	ΔTC_{it}	[p-value]
$TC_{it}^* - TC_{i,t-1}$	0.2919*** (22.86)	0.2316*** (60.44)	38.32 [0.000]
Intercept	-0.0029*** (-8.23)	-0.0018*** (-6.30)	
Number of obs.	33,766	70,011	
Adjusted R ²	0.221	0.129	
Panel B: Firms with Above-Target Trade Credit			
	Public Firms	Private Firms	F-stat of Chow test
	ΔTC_{it}	ΔTC_{it}	[p-value]
$TC_{it}^* - TC_{i,t-1}$	0.3956*** (17.10)	0.2638*** (36.84)	66.53 [0.000]
Intercept	0.0088*** (8.45)	0.0032*** (4.24)	
Number of obs.	13,929	29,459	
Adjusted R ²	0.254	0.087	
Panel C: Firms with Below-Target Trade Credit			
	Public Firms	Private Firms	F-stat of Chow test
	ΔTC_{it}	ΔTC_{it}	[p-value]
$TC_{it}^* - TC_{i,t-1}$	0.1088*** (9.85)	0.2046*** (26.15)	68.65 [0.000]
Intercept	0.0011*** (2.71)	-0.0005 (-0.74)	
Number of obs.	19,837	40,552	
Adjusted R ²	0.015	0.031	

Table 10. Effects of the Financial Crisis on Public and Private Firms' Trade Credit Policies

This table presents the regression results for Model (9), capturing the effects of the recent financial crisis on the trade credit ratios of public and private firms. Columns (1)–(3) report the results for the period 2004–2009, while Columns (4)–(6) report the results for the period 1995–2012. The crisis period is defined as years 2007–2009. The dependent variable is trade credit, defined as the ratio of accounts payable to total assets. *Crisis* is a dummy variable that is equal to 1 for the years 2007–2009 and 0 otherwise. *Public* is a dummy variable that is equal to 1 for public firms and 0 otherwise. *ST debt_{pre-crisis}* is the pre-crisis (2006) level of short-term debt. *Cash flow_{pre-crisis}* is the pre-crisis (2006) level of cash flow. Other controls refer to all the control variables that are listed and defined in Table 3 with the exception of cash flow and short-term debt, for which the coefficients are reported in the current table. *T*-statistics are reported in parentheses. Standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the 1, 5, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Crisis	-0.0104*** (-8.44)	-0.0096*** (-7.63)	-0.0065*** (-4.80)	-0.0063*** (-6.17)	-0.0053*** (-5.07)	-0.0028** (-2.41)
Public	-0.0440*** (-15.57)	-0.0499*** (-16.82)	-0.0437*** (-15.46)	-0.0355*** (-15.10)	-0.0389*** (-16.07)	-0.0355*** (-15.08)
Crisis×Public	0.0104*** (6.09)	0.0097*** (5.39)	0.0065*** (3.61)	0.0058*** (3.86)	0.0033** (2.10)	0.0023 (1.45)
Crisis×ST debt _{pre-crisis}		-0.0833*** (-4.79)			-0.0952*** (-5.49)	
Public×ST debt _{pre-crisis}		0.1278*** (5.48)			0.0681*** (6.78)	
Crisis×Public×ST debt _{pre-crisis}		0.0668** (2.29)			0.1243*** (5.28)	
ST debt		-0.0957*** (-12.40)			-0.0767*** (-12.35)	
Crisis×Cash flow _{pre-crisis}			-0.0334*** (-6.62)			-0.0309*** (-6.14)
Public×Cash flow _{pre-crisis}			0.0001 (0.19)			-0.0005* (-1.89)
Crisis×Public×Cash flow _{pre-crisis}			0.0325*** (6.26)			0.0303*** (5.92)
Cash flow			-0.0425*** (-13.59)			-0.0438*** (-16.32)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry effects	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	No	No	No	No	No	No
Number of obs.	56,283	56,283	56,283	103,777	103,777	103,777
Adjusted R ²	0.272	0.274	0.273	0.281	0.283	0.282

Internet Appendix

Table I.A.1. Comparison of Data on Trade Credit from S&P Capital IQ and Compustat

This table provides a comparison of the summary statistics (i.e., mean, median, and standard deviation (std. dev.)) of the trade credit of the public firms in our sample against the corresponding figures for those firms in the Compustat database. The sample period is 1995–2012. Trade credit is defined as the ratio of accounts payable to total assets.

Trade Credit	Our Sample	Compustat
Mean	0.0895	0.0894
Median	0.0663	0.0642
Std. dev.	0.0836	0.0882
Number of obs.	33,766	114,845

Table I.A.2. Summary Statistics of Public and Private Firms

This table presents the univariate analysis of the all control variables (including firm age, cash flow, cash holdings, current assets, sales growth, short-term debt, and firm size) of the public and private firms in the full sample using the *t*-test for differences in mean and the Wilcoxon-Mann-Whitney test for differences in median.

		Public	Private	Mean test (<i>p</i> -value)	Median test (<i>p</i> -value)
Age (years)	Mean	43.91	38.49	0.000	0.002
	Median	30.00	32.00		
Cash flow (%)	Mean	0.0292	0.1135	0.000	0.000
	Median	0.0533	0.0545		
Cash holdings (%)	Mean	0.1213	0.1407	0.000	0.000
	Median	0.0676	0.0837		
Current assets (%)	Mean	0.3939	0.5617	0.000	0.000
	Median	0.3811	0.5879		
Negative growth	Mean	-0.0490	-0.0539	0.000	0.000
	Median	0.0000	0.0000		
Positive growth	Mean	0.3208	0.1518	0.000	0.000
	Median	0.0839	0.0557		
Short-term debt (%)	Mean	0.0423	0.0242	0.000	0.000
	Median	0.0095	0.0000		
Size (Ln)	Mean	4.3784	1.9084	0.000	0.000
	Median	4.6276	1.6885		

Table I.A.3. Estimating Target Trade Credit Levels of Public and Private Firms

This table reports the regression results for the estimation of the target trade credit level as given by Model (3). Trade credit is defined as the ratio of accounts payable to total assets. The other independent variables are defined in Table 3. *P*-values of the Chow test for differences in the coefficient estimates are reported in square brackets. *T*-statistics are reported in parentheses. Standard errors are clustered at the firm level. ***, **, and * denote statistical significance at the 1, 5, and 10% levels, respectively.

	Public firms	Private firms	F-stat of Chow test [p-value]
ln(1+age)	-0.0176*** (-3.44)	-0.0063 (-1.12)	5.93** [0.015]
ln(1+age) ²	0.0019** (2.26)	-0.0030*** (-3.52)	52.59*** [0.000]
Cash flow	-0.0514*** (-9.02)	-0.0540*** (-17.24)	0.33 [0.564]
Cash holdings	0.0019 (0.32)	0.1133*** (19.81)	562.55*** [0.000]
Current assets	0.1362*** (17.79)	0.2197*** (57.79)	478.70*** [0.000]
Negative growth	0.0177*** (3.78)	0.0146*** (3.18)	0.29 [0.592]
Positive growth	0.0007 (1.49)	0.0315*** (13.63)	193.96*** [0.000]
Short-term debt	0.0596*** (4.04)	-0.1397*** (-21.29)	565.98*** [0.000]
Size	0.0019*** (3.05)	0.0143*** (18.89)	641.06*** [0.000]
Intercept	0.0413** (2.51)	0.0250 (1.38)	
Industry effects	Yes	Yes	
Year effects	Yes	Yes	
Number of obs.	33,766	70,011	
Adjusted R ²	0.262	0.260	