CREDIT MARKET DISRUPTIONS AND LIQUIDITY SPILLOVER EFFECTS IN THE SUPPLY CHAIN*

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Abstract

How do shocks to the banking sector travel through the corporate economy? Using a novel dataset of inter-firm sales, I show that suppliers exposed to a large and exogenous decline in bank financing pass this liquidity shock to their downstream customers. The spillover effect occurs through two channels: a reduction in trade credit offered on each sale, and a reduction in the total supply of goods and services. The trade credit channel explains a large portion of the initial spillover, with the least important customers seeing the largest decline in credit. Subsequently, both trade credit and total sales decline, leading to adverse consequences for downstream firms. After exposure to the spillover, customers show a spike in credit risk and a reduction in employment, suggesting that they can't fully absorb the spillover without adverse consequences. Overall, the paper highlights the importance of financial spillovers in explaining corporate sector outcomes.

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1 Introduction

How did the deterioration of the financial sector during the 2007-08 financial crisis translate into losses in the corporate sector? An important literature examines how, and to what extent, the contraction in bank lending affected corporations (Gan (2007), Almeida, Campello, Laranjeira, and Weisbenner (2012), Chodorow-Reich (2014)). However, the prior literature primarily focuses on the effect of bank health on their directly linked borrowers. Given extensive linkages among firms, it's important to consider whether banking sector shocks can have spillover effects that ripple through the corporate economy. Even if only a subset of firms are exposed to troubled banks, the existence of spillovers could materially impact the scope of aggregate adverse consequences.

I examine an important mechanism that may transmit shocks from the financial sector to the rest of the economy: the trade credit channel. Trade credit, or delayed payment for intermediate goods, plays a substantial and well documented role in financing inter-firm trade. Indeed, the Bank for International Settlements (2014) estimates that two thirds of global trade is supported by trade credit. Moreover, many firms rely on trade credit as an important source of financing (Demirguc-Kunt and Maksimovic (2001)). Given the magnitude and importance of this source of financing, it is reasonable to believe that a liquidity shortage that affects firms' trade credit lending could have adverse consequences for downstream firms. Consistent with the idea that firms are exposed to the credit risk of trading partners, Kiyotaki and Moore (1997) model the propagation of liquidity shocks through networks of financially-linked firms. In light of this literature, the goal of this paper is threefold: to document a liquidity spillover effect from a constrained supplier to his downstream customer, to provide some preliminary evidence on the trade credit channel versus other liquidity spillover channels, and to quantify any adverse consequences for downstream firms.

The role of interconnections in spreading adverse shocks was prominently discussed in the popular press during the recent global recession. In particular, there was concern that auto supplier bankruptcies might exacerbate auto manufacturers' own financial woes; this concern prompted the U.S. government to offer $5 billion in aid to the troubled suppliers. There was also concern that suppliers were tightening their financing terms to retailers and often requiring cash on delivery.[1] Whether these interconnections were an economically significant source of spreading distress in the corporate sector remains an open question.

Studying the link between banking shocks and liquidity spillover effects is challenging for two main reasons. First, identifying the causal effect of the financial crisis on corporate decisions requires the researcher to disentangle the shortage of credit supply from the reduction in demand from the corporate sector. This is particularly problematic during the crisis period when there was a concurrent decline in the demand for liquidity. The second challenge in testing the research question is that identifying the mechanism for the transmission of the banking shock requires granular data on the flow of inter-firm transactions.

I address these challenges in two ways. First, in order to identify a causal channel between bank credit and trading behavior, I follow Almeida et al. (2012) and consider the fraction of long-term debt coming due during the initial phase of the 2007-2008 financial crisis; the novelty of this identification strategy lies in an ability to separate a pure supply-side shock from the effects of a simultaneous reduction in demand. Second, I rely on a proprietary database of detailed transactions between buyers and sellers. The data not only provides the identity of buyer-supplier pairs, but it also identifies the volume of trade credit extended for every transaction. The resulting dataset is a panel of monthly trade credit balances and the aging reports for each seller-buyer pair. Importantly, the pairs transact on a repeated basis and report at high frequency (monthly), enabling me to more tightly identify causal effects of a shock to bank credit on trading behavior.

The empirical approach I take is to exploit ex ante variation in corporate debt maturity. Specifically, I identify a treatment sample of firms that have a significant percentage of their long-term debt with a scheduled maturity date in the initial phase of the credit crisis. During the crisis period, the banking sector faced large write-downs on real estate assets and increases in funding costs. This led to a sharp and unanticipated increase in the cost of credit, making it costly for firms to refinance maturing debt. Those firms in the treatment sample that were scheduled to refinance their debt should therefore face higher liquidity constraints than the control group. The underlying assumption in the identification strategy is that

[1] http://articles.baltimoresun.com/2010-12-11/business/bs-bz-small-retailers-inventory-20101212-1-trade-credit-inventory-holidays

variation in the *ex ante* agreed upon maturity dates of corporate debt is exogenous to the observable and unobservable characteristics that drive trading behavior after the credit crisis ensued. Using a difference-in-differences strategy, I find that suppliers impacted by a bank liquidity shock reduce the volume of credit to downstream customers by about 30% more than suppliers that don't face liquidity constraints. Several additional tests confirm this causal link between banking shocks and trading behavior. First, I use the Khwaja and Mian (2008) within-firm estimator and show that the results are unchanged when I include customer-time fixed effects, mitigating concerns that the results may be driven by downstream demand. Second, a placebo test that shifts the treatment to the pre-crisis period reveals no significant relationship between debt maturity structure and trading behavior. Finally, I show that my results are robust to using an alternative treatment strategy that relies on pre-crisis variation in lender health as an instrument for bank lending shocks (Chodorow-Reich (2014)). Taken together, the evidence suggests that banking shocks have a significant causal link to trading behavior.

Though the focus of this study is on the trade credit channel, it is important to recognize that the effects documented could also be operating through an alternative, but related, channel. Notably, liquidity-constrained suppliers may reduce output or raise prices, leading to a decline in *total* sales, in addition to the sales made on credit (i.e., the 'sales channel'). An examination of the terms of trade reveals that suppliers in the treatment group respond to the liquidity shock first by cutting trade credit, followed later by a cut in total sales. In the first year following treatment, the trade credit channel explains the majority of the financial spillover from the supplier to his downstream customer. In contrast, total sales remain flat until four quarters after treatment, at which point treated suppliers begin to also cut sales more than the control group. Together, the evidence suggests that both channels may be important in spreading the liquidity shortage to downstream customers.

Having established that banking shocks have a causal impact on suppliers' terms of trade, I ask which customers are impacted the most. Suppliers may be apprehensive to cut sales and trade credit to important customers, since they rely on these customers to lock in future rents (Cunat (2007) and Wilner (2000)). On the other hand, more liquid customers may be those that are most equipped to share in the risk of the constrained supplier, and thus may be the most likely to shift to shorter payment terms. Therefore, I test whether the downstream customer's importance and/or financial strength influence the degree to which they bear the spillover. In the cross-section, I find that suppliers reduce trade credit most to their least important customers, suggesting that trade credit is used as an important competitive tool to keep important customers. In addition, while unconstrained customers only face a decline in trade credit, more constrained customers see a decline in *both* trade credit and total sales. These customers may be the least able to pre-pay for goods, suggesting that they may suffer the most from the spillover.

Next, I assess whether the spillover effect is economically important enough to result in negative consequences for downstream firms. I refer to any adverse consequences for downstream customers as the 'liquidity spillover effect' because the supplier's liquidity constraint makes his downstream customer worse off. In the absence of financing frictions, exposure to a constrained supplier may be inconsequential if downstream customers can easily substitute with other sources of financing or shift to other input suppliers. Kiyotaki and Moore (1997) show that the shocks will propagate through the supply chain until they reach "deep pockets" with ample access to external finance. Capturing material downstream consequences thus depends on the intensity of the spillover and the existence of financing frictions.

To test how downstream firms respond to liquidity spillovers, I identify customers that are linked to suppliers in the treatment group ('exposed customers'). I perform a difference-in-differences analysis where the exposed customers are assigned to the treatment group and unexposed customers are assigned to the control group. The evidence reveals that downstream customers exhibit a spike in several measures of credit risk, suggesting that spillovers lead to credit contagion, as discussed in Kiyotaki and Moore (1997), Jorion and Zhang (2009), and Jacobson and von Schedvin (2015). I also show that buyers who are exposed to liquidity spillovers from their supplier reduce employment more after their exposure, relative to a control sample of unexposed buyers. The result is significant for small firms who are vulnerable to these spillover effects and who are likely to face both the trade credit and sales channels.

To put the results into context, a comparison of the impact of the *spillover* effect on employment relative to the effect of a *direct* banking shock on employment reveals that for small firms, the spillover effect on employment is about 60% as large as the direct effect. The large magnitude is consistent with the idea that trade credit is particularly important as a source of financing for small firms (Petersen and Rajan (1997)). It's also consistent with the evidence that small firms faced more substantial spillovers because they were impacted by *both* the sales and trade credit channels. In Section 6 of the Online Appendix, I use a partial equilibrium aggregation exercise in order to assess whether the spillover effects on employment are important to the macroeconomy. The results of this calculation suggest that liquidity spillovers explain about 10% of the overall decline in employment for small downstream customers in the two years following the spillover. Overall, the results suggest that customers likely faced financing frictions that prohibited them from fully absorbing the shock, which resulted in measurable adverse consequences. This is consistent with the narrative that the crisis period amplified the transmission of shocks due to widespread frictions in financial markets.

This paper is broadly related to an extensive literature studying how input-output linkages affect the transmission of idiosyncratic shocks (e.g., Gabaix (2011); Atalay, Hortacsu, Roberts, and Syverson (2011); Raddatz (2010); Acemoglu, Carvalho, Ozdaglar, and Tahbaz-Salehi (2012); Carvalho (2014)). These papers aim to determine whether firm- or sector-specific shocks can become macro-level fluctuations through spillover effects. Despite this important literature, there is relatively little empirical work studying how supply chain links facilitate spillovers. The primary innovation of my paper is to provide detailed evidence on a causal mechanism that transmits financial shocks through members of a supply chain. Specifically, using detailed trade data allows for identification of a channel linking bank liquidity to firms' trade credit behavior and comparing its importance relative to a separate, but related, sales channel. I then illustrate the economic relevance of the spillover by showing that downstream customers suffer adverse consequences. Therefore, I view my paper as a first step in identifying a causal channel for the transmission of idiosyncratic liquidity shocks.

My paper is also closely related to the literature studying the effects of the banking sector crisis on corporate sector outcomes. Ivashina and Scharfstein (2010) find that bank syndicates linked to Lehman Brothers reduced lending by a significant amount after the financial crisis, relative to syndicates unexposed to the Lehman bankruptcy. Chodorow-Reich (2014), Gan (2007), and Almeida et al. (2012) link the contraction in the supply of credit to a reduction in the borrowers' investment and employment levels. To my knowledge, I am one of the first to study the indirect spillover effects linking the financial sector to firms' outcomes. I also quantify the relative importance of direct versus spillover effects of the crisis on corporate sector outcomes. In this way, I contribute to our understanding of the aggregate impact of banking sector shocks on the corporate economy.

The paper proceeds as follows, Section 2 outlines the framework I rely on for my main predictions, Section 3 describes the setting and the identification strategy, Section 4 describes some notable features of the data, Section 5 presents the main results, Section 6 discusses the outcomes of the trade credit channel on buyers, and Section 7 provides a discussion of conclusions, caveats, and aggregate employment effects from the spillover.

2 Conceptual Framework: The Liquidity Spillover

Recent literature documents an important causal link between the supply of credit and borrower outcomes such as employment levels and investment spending (Chodorow-Reich (2014) and Almeida et al. (2012)). This literature focuses on the *direct* link between distressed banks and their borrowers. However, linked production networks with shared financial exposures in the form of trade credit may lead to an *indirect* link between banking sector shocks and corporations – through a liquidity spillover effect in the supply chain. For example, Kiyotaki and Moore (1997) show that credit chains make up a channel through which liquidity shocks can pass from one firm to their linked counterparties.

While the Kiyotaki and Moore (1997) model focuses on the propagation of shocks upstream through default channels, I investigate whether a supplier will pass the liquidity shock downstream through two channels - the trade credit channel and the sales channel. I jointly refer to these as the liquidity spillover channels. First, the liquidity effects may pass through a trade credit channel, whereby a constrained supplier reduces the volume of deferred payment from customers. Trade credit may decline because the banking shock increases the supplier's cost of credit, thereby increasing the opportunity cost of allowing customers to pay late (Murfin and Njoroge (2014)). This would result in a supplier demanding cash in advance, cash on delivery, or simply shorter payment terms.[2] Relatedly, trade credit may decline because it serves a risk-sharing role, whereby a more liquid customer may redistribute his liquidity by *offering* to pay for goods upfront. Both of these motivations lead to the first testable hypothesis: when a supplier faces an exogenous liquidity shock, the trade credit to his customers will decline.

Second, a supplier faced with liquidity shock from the bank may pass this on through a reduction in total sales. Since the supplier now finds his factors of production to be more expensive, this may disrupt the supply of goods and lead to a reduction in *total* sales to downstream customers.[3] This 'sales channel' is still a result of a liquidity spillover effect, though the underlying forces are slightly different from the trade credit channel.

It is reasonable, however, that not all customers are equally impacted by the spillover. In fact, prior work shows that relationship characteristics matter. For instance Cunat (2007) and Wilner (2000) view trade credit as an insurance mechanism between suppliers and buyers and argue that suppliers will be more willing to lend to buyers that they are particularly reliant on for future rents. Therefore, suppliers may use trade credit as a competitive tool to ensure the continued viability of their most important customers. If a supplier strategically uses trade credit to manage important relationships, then liquidity-constrained suppliers should differentially shrink the supply of trade credit to their less important customers. On the other hand, trade credit is often used to redistribute liquidity in the supply chain, thereby allowing trade partners to share in the risk of constrained counterparties. If trade credit is primarily used for risk sharing, we should observe a larger reduction in credit for the most liquid customers or those that are most reliant on their existing suppliers for input goods.

Similarly, the sales channel may differentially impact downstream firms. The customers who are financially constrained are the least able to absorb any of the liquidity shock – they may not be able to agree to shorter payment terms nor higher product prices, thus for these customers we should observe both a decline in trade credit *and* a decline in total sales.

[2]Suppliers can also reduce the liquidity risk of providing trade credit by selling their receivables to a factoring firm. See, for example, Klapper (2006) and Mian and Smith (1992). However, over the crisis period the factoring and trade credit insurance markets dried up.

[3]The supplier may react to their higher cost of capital by either cutting output or increasing price, which would lead to a reduction in sales to customers.

3 Research Setting and Empirical Strategy

3.1 The Credit Crisis

To test the main hypotheses, I explore the response of trade credit behavior to variation in bank lending during the financial crisis of 2007-2008. The credit crisis offers some advantages in capturing a liquidity spillover effect. First, the origins of the crisis began largely outside of the corporate sector, making it particularly useful in studying the causal effects of a banking sector shock on corporate outcomes. Second, the prior literature shows that during the crisis period, there was a sharp decrease in the supply of bank credit and a corresponding increase in the price of new loans (Ivashina and Scharfstein (2010), Santos (2011), Chodorow-Reich (2014)). Therefore, I can be sure that this setting captures a measurable reduction in the supply of bank credit.

The first signs of market distress occurred in early 2007 with a wave of defaults of subprime mortgages. In June 2007, the investment bank Bear Stearns rescued a subsidiary hedge fund that had heavily invested in subprime mortgages. By August 2007, BNP Paribas became one of the first financial institutions to acknowledge the subprime crisis by freezing three investment funds due to an inability to value the underlying assets. Financial markets deteriorated sharply in September 2008 when Lehman Brothers filed bankruptcy. Figure (1) plots the time series of 3-month LIBOR and commercial paper spreads. One can see that there is a sharp increase in both spreads around July of 2007. The period of uncertainty continues through the remainder of 2007 and the beginning of 2008, peaking in September 2008 around the Lehman bankruptcy.

3.2 Identification

Capturing the transmission of a bank lending shock to the trade credit contract between a seller and buyer is challenging from an empirical standpoint, since the demand for liquidity for trading purposes likely changes contemporaneously with the decline in loan supply. This problem is particularly acute in the context of trade credit due to the intrinsic connection with the good underlying the contract. Thus, a demand shock downstream in the supply chain can perturb the seller's demand for bank credit even if he is distant from the buying firm. To help address this challenge, I employ a shock to supplier liquidity that is plausibly exogenous to the trade credit contracting environment.

Following Almeida et al. (2012) I consider the fraction of long-term debt that was *prescheduled* to come due during the initial phase of the 2007-2008 financial crisis as a shock to supplier financing that is exogenous to changes in the buyer-supplier relationship at the onset of the crisis. In this way, I argue this captures exogenous variation in the opportunity cost to a supplier of providing trade credit to buyers. The 2007-08 crisis resulted in an unexpected increase in the cost of external finance. Therefore, firms that were scheduled to refinance expiring debt now faced a higher cost of capital on new loans. Importantly, assignment of this shock is predetermined by the capital structure decisions accumulated over the years leading to the crisis. Therefore firms that, by chance, had a large portion of their debt scheduled to mature in mid-2007 should be faced with larger financing constraints than firms that did not face loan renewal during this period. The key identification assumption is that the maturity date of long-term debt is orthogonal to supplier-customer characteristics that determine changes in demand for trade credit around the crisis.

The assumption that the credit crisis had a significant impact on borrowers is well-supported; the treatment period was marked by sharply increased tension in markets for commercial paper, corporate debt, and equity issuance. Additionally, Ivashina and Scharfstein (2010) show that new bank loans to large borrowers fell by 79 percent between the second quarter of 2007 and the fourth quarter of 2008. Taken together, the evidence suggests that it was costly to switch to alternative sources of liquidity at short notice. Consequently, firms were forced to replace maturing debt largely by adjusting the asset side of the balance sheet. Almeida et al. (2012) show that, following a shock to the supply of bank credit, treated firms cut investments at a significantly higher rate than control firms.

Based on this figure and consistent with Almeida et al. (2012), I measure the effect of the liquidity shortage beginning in July 2007 and lasting through August 2008. This corresponds to the two red vertical lines in Figure (1).

3.3 Main Specification

I use a difference-in-differences (DD) setting to analyze how the trade credit channel responds to a reduction in the supply of bank credit. In the baseline specification, a supplier assigned to the treatment group has more than 20 percent of his long-term debt maturing between July 2007 and August 2008. Control firms include suppliers that have less than 20 percent of their long-term debt due between July 2007 and August 2008.[4] The treatment period corresponds to the onset of volatility in financial markets, when troubles in the banking sector first emerged. However, it largely precedes the widespread deterioration in the corporate sector.[5]

My assumption is that the assignment to the treatment group is exogenous to trade credit outcomes. It is possible, however, that treatment and control firms differ along some dimensions, which might be correlated with the outcome variables and would bias the DD estimation. To address this issue, I control for firms' pretreatment characteristics, as well as their interaction with the *After* indicator. These controls help to alleviate the concern that my results might be driven by pretreatment variation between the treatment and control groups. They also prevent bias if the treatment and control groups respond differently to macroeconomic fluctuations.

The DD approach compares the amount of trade credit offered during the treatment period to the amount offered during a pre-period beginning in June 2006, for both treatment and control firms. The DD is estimated by OLS using the following specification:

$$y_{i,j,t} = \beta Treated_i * After_t + \gamma Controls_i * After_t + \alpha_{i,j} + \alpha_{t,j} + \varepsilon_{i,j,t} \tag{1}$$

where y is the volume of trade credit extended from supplier i to buyer j in month t. *After* is a dummy variable equal to one from the beginning of treatment through August 2008, and *Treated* is a dummy indicating whether the firm has more than 20% of their debt maturing during the initial crisis period. As mentioned above, I include time-invariant control variables, interacted with the *After* indicator.[6] All controls are measured by taking

[4]Both treatment and control firms must have some loans outstanding at the month before treatment.

[5]Extending the sample through the second phase of the crisis (through December 2009) introduces additional shocks on the corporate sector, which should lead to more residual variation in trade credit. Untabulated tests reveal qualitatively similar results, though the standard errors increase.

[6]I elect to use a specification with time-invariant controls in order to avoid confounding inferences of the effect on credit extended. Parallel trends are established during the pre-period on the dependent variable of interest, absent time-varying controls. The primary challenge to identification is therefore the question of selection into the treatment and control groups. Using time invariant controls address this directly by absorbing the treatment effect that would be attributable to factors that are expected to generate a selection problem (i.e., size and age).

their average over the pre-period. Specifically, I control for size (log total assets), liquidity (cash and cash-equivalents over total assets), firm age (the number of quarters since a supplier was first observed in Compustat), and number of buyers (the total number of buyers for a given supplier). The controls follow Petersen and Rajan (1997), and are meant to capture variation in firm-specific observable characteristics that might be correlated with selection into the treatment. I include an exhaustive set of fixed effects to capture time invariant characteristics of the supplier and buyer as well as bilateral supplier-buyer relationships. The control variables, *After* indicator, and *Treated* indicator do not enter into the regression separately because they are absorbed by the fixed effects.

I also follow Khwaja and Mian (2008) and include a specification with buyerXmonth fixed effects. This specification can be interpreted as the change in trade credit extended to a particular customer in a given month from a treated supplier, relative to the change in trade credit extended to the *same customer* in that month from other suppliers. Including these fixed effects requires that the downstream buyer has multiple suppliers, and it should absorb buyer-specific idiosyncratic shocks and the associated changes in the demand for trade credit. Additional fixed effects for the supplier's long-term debt rating are also used in all specifications. Finally, the main specifications include indicators for the pre-period (the five months prior to the beginning of the treatment period) interacted with the *Treatment* indicator in order to assess any pre-treatment trends. The fixed effects, in concert with the identifying assumption of orthogonality of treatment, provide a strong basis to draw causal inference.

4 Data

The primary innovation of the paper is to document the transmission of a liquidity shock in the supply chain. To do so, I rely on a novel and proprietary dataset that contains disaggregated data on accounts receivable for supplier-buyer pairs. These data are obtained from Credit2B, a credit information platform that produces risk scores for millions of customers worldwide, based on trade payment data collected from their member firms and other data including credit bureau information, financial filings, and industry trends. In order to become a member firm, suppliers are required to produce a monthly report disclosing all transactions with customers; the report contains the current receivables and past due balances for their universe of buyers. In other words, all members must make a monthly upload of every transaction made with every customer.[7] The data also includes a receivables aging report for each transaction. Past due balances are reported for each customer broken into 30 day buckets. Credit2B relies on this data to produce time-varying customer risk scores, which are shared with all member firms.[8] The universe of Credit2B transactions covers the years 2001 through 2015 and totals close to ninety-five million trade transactions.[9] Additional details on the Credit2B universe of firms are discussed in Section 1 of the Online Appendix, including industry distributions and summary statistics.

Figure 2 plots the time series of average current receivables for the universe of suppliers in the Credit2B dataset over the period 2005 through 2010. Consistent with a link between external credit markets and trade credit, the sharp decline in trade credit balances corresponds remarkably tightly to the onset of the turmoil in credit markets. To provide additional evidence on the reduction in receivables balances, Figure 3 plots the time series of average current receivables including all customers (labeled extensive margin) and the time series of average current receivables including only customers that remain in the sample from January 2005 through December 2009 (labeled intensive margin). Customers in the balanced panel represent the largest reduction in receivables, though both the intensive and extensive margins decline.[10]

4.1 Sample Construction

I use the transaction-level data to build a panel of matched supplier-buyer pairs over the period June 2006 through August 2008. I begin by applying a number of standard filters to the sample, removing suppliers flagged as factoring or other financial companies in the

[7]Credit2B strictly enforces a rule that all member firms must upload receivables data at a minimum monthly frequency, and it must include all existing customers. If suppliers violate this rule, they lose access to all credit information provided by Credit2B.

[8]Identities of buyer-supplier pairs and their transaction balances are not disclosed to other member firms, which should mitigate suppliers' incentives to underreport due to proprietary concerns.

[9]The author has proprietary access to the Credit2B data through her affiliation as a member of the Advisory Board. Information was shared under a Non-Disclosure Agreement.

[10]One difficulty that arises from the nature of the data reporting is how to handle missing values. When a buyer is omitted between consecutive reports it is difficult to determine whether this should represent a zero value. For the main specification, I use an unbalanced panel and treat missing transactions as missing observations. Per discussions with the Credit2B management, this is a reasonable assumption. However, results are robust to a fully balanced panel.

Credit2B database. Next, I match the supplier firms to the Thomson Reuters' Dealscan database in order to assign treatment. In addition to the company name, suppliers provide their location information including address and zip code. I match the suppliers in the Credit2B dataset to Dealscan by name and location, using string-matching algorithms in several rounds. After collecting exact matches, I first run a fuzzy match, with the set of potential matches restricted to those companies that are headquartered in a matching zip code. Finally, I search for the remaining matches by expanding the set of potential matches to the Dealscan universe. All matches are then verified manually. The preliminary matching procedure yields a sample of about 1,520 Credit2B companies that are successfully matched to Dealscan. Next, I require the firm to have at least one loan outstanding at the onset of the crisis. Finally, I use the Dealscan-Compustat linking file provided by Michael Roberts to obtain the relevant control variables over the period of interest. These additional filters reduce the sample of unique suppliers to 438.[11]

4.2 Descriptive Statistics

Table 1 reports the descriptive statistics for the Credit2B suppliers included in the final sample. The firms are well-established; the average age of firms in the sample is 17 years. The suppliers in the sample have an average of around 7 thousand customers, while the customers in the sample have an average of 8 suppliers. The difference is driven by the structure of the Credit2B business model, which requires all member firms to report their transactions with customers, but they do not report their transactions with their own suppliers.[12] In order to get a sense of potential issues of selection into the sample, Table 1 provides the sample averages for the Compustat universe during the period of the experiment. Of note is that the sample firms appear to have larger balance sheets, on average, than the Compustat universe. Specifically, they report higher assets and long-term debt. Comparison of the mean to the median suggests this is driven by the right tail of the Credit2B sample. Based on this difference, one might take caution in extrapolating the results of this study to the population of firms. On balance, however, the sample firms do not seem wholly different from the Compustat universe.

[11] A detailed discussion on how the Credit2B data are cleaned and matched is provided in the Online Appendix.

[12] The sample is highly skewed, with a few very large suppliers causing the averages to be high.

4.3 Assignment to Treatment

Dealscan collects loan-level information on firms' syndicated loans. The data includes information on the characteristics of the loan contract, including borrower identity, loan amount, loan maturity, collateral requirements, covenants, and other features. Using this information, I calculate the percentage of a firm's total debt coming due in the initial phase of the crisis period. Specifically, I identify loans with a stated maturity date between July 2007 and August 2008. I take two precautions in identifying expiring loans in order to mitigate concerns that the measure may be correlated with borrower characteristics. First, I require the loan to have been initiated *before* July 2006 in order to ensure that I do not sort on firms that may anticipate the effects of the credit crisis. In this way, I am more confident that I am not simply identifying firms with lower quality managers. Second, I require the loan to have a maturity of at least one year, to ensure I do not capture differences in firms' ability to obtain short- versus long-term debt. Since prior literature shows that debt maturity is correlated with firm characteristics such as size, profitability, and credit ratings (e.g., Barclay and Smith (1995), Diamond (1993)), restricting the treatment calculation to only long-term debt helps to mitigate concerns that assignment to treatment is correlated with firm characteristics.[13]

For each firm, I take the sum of all loans maturing during the treatment period and divide it by the sum of all loans outstanding at the onset of the crisis. Scaling by total debt outstanding reduces concerns that I am capturing firms with a preference for higher leverage. Further, it allows me to assign a relative strength of treatment to firms with expiring loans. Following Almeida et al. (2012), I identify treated borrowers as those with greater than 20 percent of their total debt coming due during the crisis period.[14]

I should note that though there are advantages to using Dealscan to assign treatment, there are a few caveats. First, Dealscan reports loans at origination, and thus I don't observe refinancing. If high quality firms can refinance their expiring debt, then treatment is

[13] In sensitivity analyses, I incrementally push the start date of loans to at least three years before the crisis. I also extend the loan maturity requirement to a minimum of two years. Finally, I also control for the firm's average loan maturity, in order to mitigate concerns that I somehow capture credit quality. In all of these sensitivity tests, inferences remain unchanged.

[14] In sensitivity analyses, I vary the threshold used to assign treatment. I find similar results with treatment assigned at 15 percent. The results get stronger as I increase the percentage threshold past 20 percent. I also use a fully continuous treatment variable, and results are robust though decline in statistical significance, in some instances.

assigned with measurement error and my results will suffer from attenuation bias. Second, Dealscan only covers syndicated loans, which should again result in attenuation bias. To help alleviate these concerns, in untabulated analyses I follow Almeida et al. (2012) and re-assign treatment based on debt measures from Compustat. Assignment to treatment using Compustat overlaps with the assignment using Dealscan in 87% of the cases. All results are robust to this alternative treatment assignment.

4.4 Treatment versus Control Firms

I assume that the discontinuity around the debt maturity date sorts firms into two groups, such that the only difference between these groups is the supply of bank credit and not differences in other borrower characteristics that might influence trade credit. To provide some evidence toward this assumption, Table 2 reports the covariate balance in the pre-period. I include balance sheet characteristics as well as qualities of the supplier's loans. It is encouraging to observe no significant differences in loan characteristics and only small differences in borrower characteristics. Treated firms have higher total assets and cash balances than control firms. To ensure that these differences do not drive the results, in the regressions I include a vector of firm controls interacted with the *After* indicator.

The DID estimator relies on the assumption of parallel trends. To assess this assumption, I run the following regression:

$$y_{i,j,t} = \alpha_t + \alpha_t * Treated_i + \epsilon_{i,j,t} \tag{2}$$

,where $y_{i,j,t}$ is the log of current receivables and α_t represents an indicator for month t. A test that the interacted coefficients in equation 2 representing months in the pre-period are jointly equal to zero yields an F statistic of 0.92, failing to reject the null of parallel trends. Figure 4 plots estimated coefficients on the interacted terms over ten months surrounding the treatment date. The estimated difference between treatment and control groups appears to be largely insignificant in the pre-period, with a slight anticipation effect in the month prior to treatment. Since June 2007 marked the first onset of the crisis; it is feasible that trade suppliers anticipated problems and started cutting receivables accordingly.

5 Results: Spillover of a Banking Shock

To test whether suppliers facing a banking liquidity shock pass this to their customer in the form of reduced trade credit, I estimate equation 1 using ordinary least squares, where the dependent variable is the suppliers' balance of accounts receivable. The level of observation is a unique buyer-supplier-transaction. The primary coefficient of interest is the $Treated * After$ term, which captures the effect of the difference-in-differences. Standard errors are clustered at the supplier level.

The results from estimating equation 1 are reported in Table 3. In columns 1 through 4, the dependent variable is the log of the current receivables on a transaction, as reported in the monthly receivables reports collected by Credit2B. Note that the balance of current receivables represents new credit sales made to a particular customer in a given month. Since the receivables data is highly positively skewed, I estimate the regression using logs. In addition to the control variables discussed in 3.3, I also include controls for the supplier's long-term debt rating, which should help absorb any remaining variation in credit quality. Further, in all specifications, I include buyer and supplier fixed effects to control for any time-invariant characteristics of the buyer and supplier that may drive trade credit policy. Month fixed effects are included in all specifications to capture any macroeconomic fluctuations that might correlate with lending decisions.[15]

Moving from columns 1 through 4, the fixed effects get progressively more rigorous. In particular, in column 3 I add a supplierXbuyer fixed effect to account for the effect of relationship-type on changes in trade credit, and in column 4 I add a buyerXmonth fixed effect to account for time varying characteristics of the buyer.[16] In all specifications, I find a negative and significant coefficient on the $After * Treated$ term, suggesting that suppliers experiencing a liquidity shock from their bank had a larger reduction in the trade credit

[15] In an untabulated test, I also include controls for the terms of the trade credit contract, where available. These include the number of days that the buyer has to pay the invoice as well as the implicit interest rate. Because this information is voluntary, I can only control for these terms for a subset of the transactions. All results are robust to these controls and are available upon request.

[16] Results are robust to replacing the buyer and buyerXmonth fixed effects with control variables that capture variation in the buyer's demand for trade credit. Specifically, in untabulated tests I directly control for a buyer's direct exposure to a bank liquidity shock, interacted with the *After* variable. I also include a measure that captures the percentage change in trade credit from all of the buyer's other suppliers. Results are available upon request.

offered to customers during the crisis period, relative to the control group. The result is economically significant; the coefficient in column 4 of Table 3 suggests that treated suppliers have a 31 percent larger decrease in lending to customers.[17]

5.0.1 Robustness of the Primary Result

Though I take several precautions in the research design, one may have lingering concerns that the treatment is functioning through a correlation with an omitted factor that predicts both the timing of the debt contract expiry and changes in trade credit. To help mitigate these concerns, I perform three additional robustness checks.

First, I test whether the spillover effects show up in a placebo period, well before the financial crisis. This falsification test helps to check the validity of my experiment, since debt maturity structure should only matter during periods when refinancing is costly. The analysis is moved back one year such that the treatment date is assigned at July, 2006. Since suppliers assigned to the treatment group should not be differentially financially constrained, there shouldn't be a spillover effect. Results, reported Table A5 of the Online Appendix, show that the coefficient on *After*Treated*, is insignificantly different from zero in all specifications.

Second, in order to validate my findings I use a different identification strategy to capture liquidity constrained suppliers. Specifically, I follow Chodorow-Reich (2014) and Ivashina and Scharfstein (2010), who argue that variation in bank health *before* the crisis influences the bank's ability to lend during the crisis. Following this work, I use the change in lending behavior for all of the supplier's pre-crisis syndicate members as the treatment effect. I also instrument the change in loans with measures of the syndicate's pre-crisis risk, including exposure to mortgage-backed securities and syndicates that share Lehman Brothers as a co-lender. Each of these variables is meant to capture a change in bank lending that is exogenous to borrower characteristics.[18] The results from this alternative experiment are reported in Table A6 of the Online Appendix. In all specifications, I find that suppliers facing a tighter loan supply extend less trade credit to their downstream customers.

Third, one may be concerned that my treatment strategy is sorting firms into those with

[17] In an untabulated test, I restrict the sample to the transactions where the customer has multiple suppliers, but I omit the buyerXmonth fixed effect. The magnitude of the coefficient increases to -0.339, suggesting that time-varying buyer characteristics explain a small portion of the reduction in trade credit.

[18] More detailed descriptions of these variables are available in the Online Appendix.

a lumpy debt structure, where a large portion of debt comes due at once. Therefore, in robustness tests I restrict the control sample to those that have more than 20% of their debt expire in the year *before* the crisis. Results are robust to this restricted control group. Taken together, the placebo test, the alternative treatment, and the restricted control group help to alleviate concerns that the results in Table 3 are an artifact of a flaw in the research design.

5.1 The Trade Credit Channel versus the Sales Channel

To this point, the focus of the paper links a banking shock from suppliers to their customers through the trade credit channel. A plausible additional channel linking supplier liquidity to spillovers on the customer is the sales channel.[19] Consider a supplier in the treatment group, who now faces a higher cost of credit. The higher cost of capital may impact the supplier's ability to pay for factors of production, leading to a decline in output and therefore sales. Relatedly, the supplier may respond to a higher cost of capital by increasing product prices, which would also lead to a decline in sales. Both the sales channel and the trade credit channel are consistent with the spillovers in Kiyotaki and Moore (1997) and relate to the idea of production distortions discussed in Bigio and La'O (2017). However, because *credit* sales are inherently linked to *total* sales, the empirical findings in columns 1-4 of Table 3 may reflect a combination of both channels.

To evaluate the relative importance of the trade credit channel, columns 5-7 of Table 3 replace the dependent variable with receivables scaled by sales. Note that Credit2B evaluates *receivables* risk, thus they don't collect total sales on each transaction. Therefore, I scale by the total sales revenue reported in the Compustat Quarterly File. To account for differences in reporting frequency, I take the three-month sum of receivables for each buyer-supplier pair reported in Credit2B, and I scale by the three-month quarterly sales number reported in Compustat.

Columns 5 through 7 of Table 3 suggest that some portion of the spillover effect is indeed due to the trade credit channel; I continue to find a negative and significant coefficient on $After * Treated$. The magnitude of the effect is somewhat difficult to interpret, given that the measurement of sales is at the firm level rather than the firm-customer level. To aid in interpretability, I aggregate the Credit2B transaction-level data to the supplier-quarter level

[19] This is also referred to as supply disruption risk (Tomlin (2006), Ellis, Henry, and Shockley (2010)).

and then scale by Compustat quarterly sales. Results of this specification are reported in column 8 of Table 3 and indicate that treated suppliers have a 20 percent larger decrease in lending to customers, relative to the control sample.[20] The robustness of the negative coefficient across all specifications suggests that the trade credit channel may represent a significant portion of the spillover effect.

5.1.1 *Further Evidence on the Spillover Channel: A Subsample Analysis*

Though the results in columns 5 through 8 are suggestive that the trade credit channel exists, Compustat-level data limits my ability to assess whether customer- or relationship-specific effects influence the spillover channels. To address these concerns, I collect transaction-level detail of the *total* sales versus the *credit* portion of sales, for a small subset of the suppliers in the sample. All suppliers in the sample were offered incentives to provide this additional data. Requests were made through email, in-person, and through follow-up phone calls. This effort resulted in a response rate of 16%, and covers the period June 2006 through June 2008.[21] The relatively low response rate was a reflection of non-response, inability to find or access the data, and proprietary concerns. Nonetheless, the response was large enough to conduct a subsample analysis in order to further disentangle the sales channel from the trade credit channel (henceforth, I refer to this as the subsample analysis).[22]

Table 4 reports the results of the subsample analysis. In columns 1 and 2, the dependent variable is accounts receivable scaled by sales, and in columns 3 and 4, the dependent variable is the log of total sales. The unit of observation is transaction-level, so I include the most rigorous specification of fixed effects including buyerXmonth. Column 1 reveals that treated suppliers decreased the credit extended per dollar of sales by about 22% more than the control group. This estimate is in line with that reported in column 8 of Table 3 and provides supporting evidence that suppliers' liquidity problems spill over through the trade credit channel. In contrast, I find that on average, in the year following treatment *total* sales for the treatment group do not differ from that of the control group.

Investigating these results in the time-series provides additional insights. Next, for the

[20] I forgo customer-specific fixed effects due to the aggregation.

[21] Of those that responded, some of the data was unusable, and some didn't make the final sample due to the fixed effects structure. The final subsample used in regressions includes 12% of suppliers in the main sample.

[22] Additional details on this subsample data can be found in Section 1 of the Online Appendix.

After period, I split the sample into four quarters and re-run the analyses.[23] Column 2 reports the results of this specification, where the dependent variable is receivables scaled by sales. In each post-treatment quarter, treatment firms show a larger reduction in receivables relative to the control sample. Column 4 reports the quarterly results for the sales channel. Interestingly, the coefficient of interest only becomes significant in the fourth quarter after treatment. Though the coefficients on *[PostQ1-PostQ3]*Treated* are insignificant, one can see a monotonic increase in the point estimates, suggesting that over time, the sales channel may explain a larger portion of the spillover effect from liquidity constrained suppliers to their downstream customers.[24]

Taken together, the results in Tables 3 and 4 provide some valuable and novel insights. The timing effects documented in Table 4 suggest there may be a pecking order of adjustment options, whereby cutting sales is more costly than cutting receivables. This could be because (1) public suppliers want to maintain current-period revenues as long as possible; (2) suppliers may have enough inventory on hand to maintain sales for the first few periods after treatment, and after that they must fund new inventory at which point sales drop; and/or (3) suppliers may face some rigidity in the price of their product but have more flexibility on the trade credit margin.

Some caveats are in order on interpretation of these results. First, though the number of transactions is relatively large, the number of unique suppliers is small. Thus the generalizability of the results in Table 4 may be threatened. Table A4 of the Online Appendix reports a differences in means test for the subsample relative to the larger sample, and indeed, the subsample represents larger suppliers. Results should be interpreted with this caution in mind, and should be viewed as only suggestive of the time trends of the trade credit and sales channels.[25] Second, since the subsample data is restricted to one year post-treatment, I cannot speak to the relative importance of the sales channel versus the receivables channel thereafter. And finally, while I can empirically separate total sales versus receivables, con-

[23]The quarters are not cumulative. Thus the interpretation for each coefficient is relative to the pre-period.

[24]The point estimates in column 2 reveal a monotonic *decline* in the trade credit channel, suggesting a possible 'switch' from the trade credit channel to the sales channel over time.

[25]To provide some guidance on the representativeness of the sample, I re-estimated column (4) from Table 3, using only the subsample. The coefficient of interest is qualitatively similar, though slightly larger, to that reported in Table 3 (-0.357).

ceptually the sales channel and the trade credit channel may not be distinct. Ultimately, spillover effects likely come through each channel, or a combination of the two.[26]

5.2 Heterogeneity in the Main Effect

Trade credit may be adjusted for two reasons. First, liquidity constrained suppliers who are now forced to reduce the asset side of the balance sheet may optimally shrink trade credit more to their less important customers. I refer to this as a supplier-driven reduction in trade credit, since suppliers are using trade credit to retain their important customers (e.g., Cunat (2007) and Wilner (2000)). Trade credit may also serve a risk-sharing role, whereby customers offer to pre-pay their liquidity constrained suppliers in order to smooth the adverse shock. While this still imposes costs on the customer relative to more lenient trade credit terms (i.e., the customer must now finance his inputs upfront), it may be less costly than facing rationed inputs. Indeed, the redistribution view of trade credit posits that unconstrained firms will redistribute credit to their less advantaged counterparties (Meltzer (1960); Petersen and Rajan (1997)). I refer to this as a customer-driven reduction in trade credit, because customers willingly reduce their trade credit to share their supplier's risk.

To test for the supplier-driven motive of trade credit, I develop proxies for the importance of the customer to the supplier. First, I use the number of years that the buyer and seller have been trading. McMillan and Woodruff (1999) and Antras and Foley (2015) show that relationship length is particularly important for trade credit decisions since it proxies for the importance of that customer in the firm's future rent stream. For each customer, the Credit2B database reports the number of years they have transacted with the seller, even if the start year preceded the year the supplier joined Credit2B. *New* is set equal to one if the relationship is below the supplier's sample median, zero otherwise. Second, I count the total number of buyers in an industry in the Credit2B database.[27] The argument I make for using this variable is motivated by the literature on relationship specific investments. Specifically, a supplier with a single customer may face hold up problems since their product is worth less in an alternative use (Klein, Crawford, and Alchian (1978), Williamson (1979), Grossman

[26] For example, a decline in trade credit implicitly increases the price of the product since the buyer now has to finance the purchase ex ante. This would then lead to a decline in demand and therefore lower sales.
[27] One can think of this as a noisy proxy for industry concentration, based on the data available to me in the Credit2B database.

and Hart (1986)). Therefore, the supplier is more reliant on rents from a small potential pool of customers than he would be with many replacement options. *Substitutes* is set equal to one if the total number of buyers in an industry is above the sample median.

To test for the customer-driven motive of trade credit, I develop a proxy for the customer's ability to share in the risk of his supplier. Suppliers may reduce credit most to their most liquid customers, since these are precisely the customers that offer to share in the supplier's risk by paying upfront. The variable, *Constrained* is an indicator variable for whether the customer has access to public debt or equity markets, since these firms have relatively limited access to different sources of financing.[28]

I estimate equation 1 on the subsample, including a triple interaction term based on the cross-sectional variables discussed above. The results are reported in Table 5. The first three columns report the results where the dependent variable is the ratio of receivables to sales on each transaction, and the last three columns report the results where the dependent variable is the log of total sales. The first two columns report that the spillover effect through the trade credit channel is substantially larger for customers that are newer and that compete in industries with a large number of substitutes. This is consistent with the literature that suggests that withholding trade credit is costly, so treated suppliers should differentially pass a larger portion of the spillover to less important customers. Interestingly, column 3 reports no difference in the trade credit adjustment between constrained and unconstrained customers; both face a larger decline in trade credit than the control group. This preliminary evidence is inconsistent with trade credit serving a risk-sharing role, whereby constrained customers should see a relatively smaller decline in trade credit.

Columns 4-6 report the results where the dependent variable is the log of total sales. The results are striking. I fail to find any statistically significant relation between the DD treatment effect and sales volume, except in the subgroup of financially constrained customers. Thus, constrained customers face both a decline in trade credit and a decline in total sales. Perhaps these are the least influential customers, and thus suppliers choose to cut both margins for them. Alternatively, perhaps these customers risk-share to their capacity, at which

[28]This follows Chodorow-Reich (2014). Since my sample of public customers in the sub-sample is relatively small, alternative measures of buyer constraint, particularly those that rely on balance sheet data, are not possible.

point sales decline. Thus, the result in column 6 could be explained by the supplier-driven motive for trade credit, the risk-sharing channel, or both.[29]

6 Buyer Outcomes

6.1 Liquidity Spillovers and Buyer Credit Quality

The results thus far establish that suppliers facing a liquidity shock respond by reducing trade credit and total sales to downstream customers. In the absence of financing frictions, however, these trade credit and sales adjustments should have little adverse effects on the downstream customer. In the context of the Kiyotaki and Moore (1997) model of credit chains, a liquidity shock will propagate through the supply chain until the chain reaches a 'deep pocket,' who can absorb the liquidity shock because they have sufficient access to capital. Therefore, whether the liquidity shock results in measurable downstream spillover effects is an open empirical question.

To capture adverse spillover effects, I first sort customers into two groups: those that are linked to liquidity constrained suppliers (*Exposed* buyers) and those that are not linked to any treated suppliers (*Unexposed* buyers). Note that the identification strategy hinges on the assumption that the treatment is only based on the random assignment of the *supplier's* debt maturity dates, rather than any characteristics related to the supplier or the customer himself. If my assumption holds, sorting into *Exposed* versus *Unexposed* buyers should be exogenous to buyer characteristics. A comparison between the *Exposed* and *Unexposed* buyers reveals that the covariates are balanced along relevant dimensions.[30]

I estimate the following equation, using OLS:

$$y_{j,t} = \beta_1 After_t * Exposed_j + \beta_2 After_t * Treated_j + \gamma Controls_j * After_t + \alpha_j + \alpha_{t,z} + \varepsilon_{i,j,t} \quad (3)$$

where j indexes buyers and z indexes the buyer's industry. In order to maintain the DD assumption of parallel trends, I include time invariant controls, interacted with the *After* indicator. Buyers are matched by name and address to Dealscan and Compustat in a similar

[29]In Table A7 of the Online Appendix, I supplement the risk-sharing tests by investigating proxies for the customer's *willingness* to risk-share. I fail to find evidence consistent with risk-sharing, though empirical constructs may not be precise enough to pick up an effect.

[30]Reported in Table A2 of the Online Appendix.

way as described in Section 4.1. I control for buyer size, buyer liquidity, buyer age, and the number of suppliers, each interacted with the *After* indicator. As in the main specification, I include fixed effects and cluster standard errors at the buyer level. I cannot include the buyerXmonth fixed effect since it would absorb the effect of $After * Exposed$. Instead, I replace it with an industryXtime fixed effect, which captures time-varying characteristics of the buyer's industry. I assign industry based on the Fama-French 48 Industry Portfolios. In the most rigorous specifications I also include time invariant buyer, supplier, and relationship (sellerXbuyer) characteristics. In all specifications, standard errors are clustered at the buyer level. The primary term of interest is $After * Exposed$, which captures the differential spillover effect on the exposed buyer, relative to the unexposed buyer. Importantly, I also include $After * Treated$ to capture the *direct* effect of a bank liquidity shock on a buyer. Direct treatment is assigned using the same methodology as in Section 4.1.[31] Estimating the spillover effect versus the direct effect provides a useful comparison to gauge the relative importance of the liquidity spillover channel.[32]

In Table A8 of the Online Appendix, I test whether exposed buyers are more likely to draw on other sources of liquidity to try to absorb the shock, as predicted in Kiyotaki and Moore (1997). Indeed, exposed buyers adjust by drawing on cash balances, drawing down existing lines of credit, and further delaying the invoices that are due to other, more liquid, suppliers. This suggests that they attempt to absorb some of the sales and trade credit channels by drawing on other sources of liquidity. Yet, this may not be sufficient to completely mitigate a spillover effect.

I begin by testing whether the spillover effects have any impact on the buyer's credit quality. Prior literature suggests that buyer-supplier exposures can lead to credit contagion (e.g., Kiyotaki and Moore (1997); Jorion and Zhang (2009); Jacobson and von Schedvin (2015)). For example, since financial distress at an upstream supplier leads to supply disruptions, less liquidity, and a limited ability to pay bills, this may lead to an increase in the distress of the customer and an increase in his credit risk. Therefore I estimate equation 3,

[31] To retain a large enough sample, buyers that do not have loans recorded in Dealscan during this time period are assigned to the untreated group, instead of omitted.

[32] The control variables, *After* indicator, *Exposed* indicator, and *Treated* indicator do not enter into the regression separately because they are absorbed by the fixed effects.

where y includes three measures of credit quality. The first is the credit score, developed by Credit2B and available on a monthly basis (known as the 'RScore'). It uses a combination of financial, trade, industry, and other information to develop a score that ranges from 1 to 100, where a higher score indicates better quality. The final two proxies that I use to capture credit quality are (1) an indicator variable for whether the buyer's payable is transferred to a collection agency and (2) an indicator variable for whether the buyer violates a covenant in their contracts with outside lenders. I obtain both of these proxies from annual Experian and Equifax reports, which provide information on credit risk from the entity's various sources of borrowing.[33]

Table 6 reports the results of the spillover effect on customer credit quality. Consistent with the prior tests, controls include the buyer's cash and number of suppliers. Therefore, the results in this panel can be interpreted as the effect of the spillover on the buyer's credit risk, *after* controlling for his access to other forms of liquidity.[34] In column 1, I estimate equation 3 using OLS at the buyer-month level, where the dependent variable is the credit score. The results reveal that customers exposed to spillovers see a reduction in their credit score of 3.6 points more than the control group. In columns 2 and 3, I estimate equation 3 using OLS at the buyer-year level, where the dependent variable is an indicator for the customer's default on their trade and bank debt, respectively. In both cases, I find that the spillover increases the exposed customer's chance of default, relative to customers that are not exposed to the spillover. The impact of the spillover on the customer's credit risk represents about 60 to 70% of the magnitude of the direct effect of a banking liquidity shock, suggesting a substantial amount of credit contagion.

The results in Table 6 indicate that exposed customers must face some financing frictions in absorbing the spillover effect, and ultimately suffer a decline in their credit quality. I should note that effects documented in Table 6 could be due to *both* the trade credit and the sales channels. The limited data on customers in the subsample described in Section 5.1.1 do not allow me to separately quantify the impact of each channel. However, in Table

[33]Credit2B has data sharing agreements with Experian and Equifax, and they have a linking table between the Credit2B firm-level identifiers and the Experian and Equifax firm-level identifiers.

[34]I also control for the volume of the buyer's unused line of credit reported from Capital IQ. This number is set to zero for buyers not observed in Capital IQ.

A9 of the Online Appendix, I analyze the timing of the effects documented in Table 6, and the results are suggestive. For those outcomes that I can measure at a monthly or quarterly frequency, I split the *After* period into quarters. I find that the borrower's credit score goes up starting the first quarter after exposure. Since sales remained flat until the fourth quarter but trade credit declined immediately, this preliminary evidence is suggestive that the trade credit channel played a role in the buyer's decline in credit quality.

6.2 Liquidity Spillovers and Real Effects

As discussed previously, in frictionless markets exposed buyers should be able to easily substitute with other sources of financing. This would result in little measurable adverse consequences. Thus far, the results in Table 6 provide some evidence to the contrary: exposed buyers, though they draw on other sources of funding, face additional costs in the form of an increase in credit risk. However, it remains an open question whether liquidity spillovers influence real activities of the downstream buyer. To provide evidence on this front, I explore whether the spillover has measurable effects on an exposed buyer's employment levels. Chodorow-Reich (2014) shows that firms that are *directly* linked to the financial sector shock are more likely to cut employment. Having established that direct borrowers of troubled banks cut employment, I ask whether the effect can also manifest through the spillover channel.

I estimate equation 3 , where y is the log of the number of employees, collected from Dun and Bradstreet (D&B). D&B collects employment for establishments on an annual frequency by drawing on numerous sources including surveys sent direct to companies, web searches, and other data. D&B conducted a name and address match for the buyers in my sample to the DUNS number in their database and provided employment data for 2005 through 2009. I include buyer and year fixed effects, as well as industry-year fixed effects to account for time-varying unobservable effects of the buyer's industry. Consistent with the prior tests, controls include the buyer's cash and number of suppliers. Therefore, the results in this panel can be interpreted as the effect of the spillover on the buyer's employment, *after* controlling for his access to other forms of liquidity. The results are tabulated in column 1 of Table 7. I fail to find evidence that exposed buyers have a greater decline in employment than unexposed buyers. However, as documented by prior literature, a larger majority of employment losses came from small firms (Chodorow-Reich (2014)). Therefore, I split the sample at the median of the buyers' size, and I estimate the regression for each subsample.[35] Column 3 of Table 7 reveals that there is a statistically significant decline in employment for the smaller firms in the sample.[36] The interpretation of the results is that the spillover effect resulted in an 11% greater decline in employment for small firms, relative to unexposed buyers. This is relative to the *direct* effect of a bank liquidity shock of a 17.8% decline. Overall, the results in Table 7 highlight an important and not previously studied channel through which the banking sector shocks spread to non-financial firms.

7 Discussion and Aggregate Implications

In this paper, I provide evidence on the transmission of financial shocks through spillover effects in the supply chain. Downstream customers who are exposed to liquidity constrained suppliers suffer adverse consequences: their credit quality spikes and they cut employment. It's important to note that these downstream adverse effects come through two channels – the trade credit channel and the sales channel. I am not able to separately quantify the relative contribution of these channels on the adverse downstream effects. Rather, my paper is a first step in showing that spillover effects are significant, and that they can occur through both of these channels. In Section 5 of the Online Appendix, I provide some discussions and preliminary tests to separately evaluate the trade credit channel versus the sales channel, and the results suggest that no single channel dominates the adverse downstream effects.

While the primary objective of my paper is to illustrate a liquidity spillover effect at the microeconomic level, one may be interested in whether these effects are important in the aggregate. In Section 6 of the Online Appendix, I provide some evidence that the effect of spillovers on aggregate employment are significant. A partial equilibrium exercise suggests that spillover effects account for 10% of the reduction in employment levels for small firms in the aggregate. In contrast, a direct liquidity shock to a firm accounts for about 14% of

[35] Buyer size is defined as the average number of employees in the pre-period. This assigns a 'small' classification to firms with less than 1,200 employees.

[36] I should note that though the result in column 2 is statistically insignificant, the magnitude of the coefficient is similar to that in column 3. However, when I split the sample into quartiles, I find a statistically significant difference in spillover effects between the smallest and largest firms. Thus, there does appear to be a firm size effect on the spillover.

the reduction in employment levels for small firms. Thus, spillovers appear to represent a substantial portion of unemployment for small firms during the crisis period. In Section 7 of the Online Appendix, I expand this exercise using a static general equilibrium model, and show that the full network effect of a banking shock on labor decisions is potentially substantial.

The findings are important for understanding the full impact of the 2007 financial crisis on corporations. While the prior literature shows that the contraction in the supply of bank credit caused a reduction in borrowers' investment and employment levels (Chodorow-Reich (2014), Gan (2007), and Almeida et al. (2012)), my study shows that the effects of the banking crisis were also indirect due to inter-firm spillovers. Many point out that the magnitude and the persistence of the economic downturn cannot be fully explained by the direct effect of credit market disruptions on corporations Hall (2010). My paper suggests that a possible indirect effect - the liquidity spillover effect - may aid in explaining the magnitude and persistence of the corporate slump.

The paper is important in light of the burgeoning literature on the transmission of idiosyncratic shocks (e.g., Gabaix (2011); Raddatz (2010); Acemoglu et al. (2012); Carvalho (2014)). This literature aims to understand how firm- or sector-specific shocks may impact the macroeconomy. Though it is beyond the scope of my paper to estimate network effects of the spillover channel, I view my paper as a first step in identifying a causal channel for the transmission of idiosyncratic liquidity shocks.

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Table 1: Sample Summary Statistics versus Compustat Universe
This table presents the sample summary statistics. *Total Assets, Long-Term Debt, Cash, Sales, and Age* are all measured using the Compustat quarterly file. *Current Receivable, Past Due Receivable, Days Slow, nBuyers, nSuppliers, Relationship Age, and Receivables/Sales* are all measured using the Credit2B dataset. The last column provides sample means for the population of Compustat firms. *** , **, and * represent significant differences in the Credit2B sample mean and the Compustat sample mean at the 1%, 5%, and 10% level, respectively.

	Mean	*Q1*	*Median*	*Q3*	*Compustat Mean*
Total Assets ($M)	12,948.18	1,837.87	1,973.72	9,844.89	12,858.28*
Long-Term Debt ($M)	2,504.99	104.50	491.38	1,440.25	2,486.89**
Cash ($M)	934.21	98.58	128.23	787.37	937.67
Sales ($M)	3,763.94	498.43	1,629.75	3,798.84	3,731.26
Age	16.75	6.23	9.28	38.25	13.15
Current Receivable ($ Per Transaction)	56,474.89	357	1725	8,932	
Past Due Receivable ($ Per Relationship)	10,501.19	0.00	0.00	1,100	
Days Slow	10.75	0.00	0.00	15.00	
nBuyers	7,236	113.5	1,322	16,371	
nSuppliers	8.24	1.00	2.78	13.44	
Relationship Age	11.67	3.00	10.00	17.00	
Receivables/Sales (Per Transaction)	0.84	0.51	0.70	0.97	

Table 2: Covariate Balance in the Pre-Period
This table reports the descriptive statistics for both the treated and the control firms in the pre-period. The variables are broken out into *Loan Characteristics* and *Borrower Characteristics*. *Maturity* is the total length of the loan in months, *All in drawn* is the interest spread in basis points, *Log Amount* is the log of total amount of the loan facility, and *N Lenders* is the total number of lenders in the lending syndicate. All of the loan characteristics are measured from Dealscan and are averaged at the supplier level. The *Difference* column calculates the difference in means for the treatment and the control group. *** , **, and * represent significant differences in the treatment and control groups at the 1%, 5%, and 10% level, respectively

		Treated	*Control*	*Difference*
Loan Characteristics				
	Maturity	35.574	37.338	-1.764
	All in drawn	178.551	187.814	-9.263
	Log Amount	18.390	19.101	-0.771
	N Lenders	4.279	5.128	-0.849
Borrower Characteristics				
	Log Total Assets	9.528	8.157	1.371**
	Age	16.4	16.8	-0.4
	Cash/Total Assets	0.11	0.07	0.04*
	Current Receivables	57,229	52,127	5,102
	Relationship Age	9.018	10.161	-1.143

Table 3: The Effect of Lender Credit Supply on Trade Credit

This table reports the results from estimating equation 1, where the dependent variable in columns 1-4 is the log of the balance of current receivables, the dependent variable in columns 5-7 is the sum of three month's current receivables for a seller-buyer pair scaled by Compustat quarterly sales revenue, and the dependent variable in column 8 is the sum of three month's current receivables, aggregated at the seller-level, scaled by Compustat quarterly sales revenue. There are 438 unique suppliers in the sample. The regression is run at the transaction level, and the sample is an unbalanced panel. The variable *Treated* is equal to one if at least 20 percent of the supplier's bank debt has a scheduled maturity date between July 2007 and August 2008. *After* is set equal to one for all months between the same period. The pre-trend period picks up the observations in the 5 months preceding treatment. Control variables include: (1) size, defined as the log of quarterly total assets; (2) cash, defined as cash and cash equivalents scaled by assets; (3) age, defined as the current date less the first date the supplier appeared in Compustat; and (3) nBuyer, defined as average number of unique buyers for each supplier. Control variables are taken as averages in the pre-period and are interacted with *After*. Standard errors are clustered at the supplier level and are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
After*Treated	-0.327***	-0.345***	-0.338***	-0.313**	-0.003**	-0.002**	-0.001***	-0.204**
	(0.086)	(0.078)	(0.071)	(0.153)	(0.001)	(0.001)	(0.000)	(0.083)
Pre-Trend*Treated	0.004	0.007	0.013	0.007	0.000	-0.000	0.000	-0.028
	(0.073)	(0.072)	(0.066)	(0.008)	(0.000)	(0.000)	(0.000)	(0.085)
After*Size		-0.015	-0.005	-0.033	-0.000**	-0.000**	-0.003***	0.009
		(0.015)	(0.014)	(0.034)	(0.000)	(0.000)	(0.000)	(0.030)
After*Cash		0.496	0.687	-1.601*	-0.002	0.003	-0.060***	1.126*
		(0.492)	(0.458)	(0.918)	(0.008)	(0.006)	(0.014)	(0.649)
After*Age		0.057	0.038	0.072	0.000	0.000	0.000	-0.027
		(0.060)	(0.055)	(0.061)	(0.002)	(0.001)	(0.001)	(0.101)
After*nBuyer		-0.011	-0.012	-0.008	-0.000	-0.000	-0.000	-0.048*
		(0.008)	(0.007)	(0.008)	(0.000)	(0.000)	(0.000)	(0.028)
Observations	988,290	988,290	988,290	411,403	207,720	207,720	77,128	3,618
Adjusted R^2	0.842	0.842	0.849	0.985	0.749	0.899	0.955	0.927
LTD Rating Dummies	Y	Y	Y	Y	Y	Y	Y	Y
Buyer FE	Y	Y	Y	Y	Y	Y	Y	N
Seller FE	Y	Y	Y	Y	Y	Y	Y	Y
SellerXBuyer FE	N	N	Y	Y	N	Y	Y	N
Month FE	Y	Y	Y	Y	Y	Y	Y	Y
BuyerXMonth FE	N	N	N	Y	N	N	Y	N

Table 4: Liquidity Spillover Channels

This table reports the results of testing the spillover effect through two channels: the trade credit channel and the sales channel. Equation 1 is estimated on the subsample of transactions where suppliers opted to provide sales data, as discussed in Section 5.1.1. The dependent variable in columns 1 and 2 equals receivables scaled by sales and the dependent variable in columns 3 and 4 equals the log of total sales. Regressions are run at the transaction level. *PostQ1* equals one if the transaction occurred between July and September, 2007, *PostQ2* equals one if the transaction occurred between October and December 2007, *PostQ3* equals one if the transaction occurred between January and March 2008, and *PostQ4* equals one if the transaction occurred between April and July 2008. All other variables are as previously defined. Standard errors are clustered at the supplier level and are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

	(1)	(2)	(3)	(4)
After*Treated	-0.223***		-0.147	
	(0.045)		(0.153)	
PostQ1*Treated		-0.343***		-0.025
		(0.071)		(0.058)
PostQ2*Treated		-0.162**		-0.108
		(0.060)		(0.188)
PostQ3*Treated		-0.128**		-0.111
		(0.035)		(0.165)
PostQ4*Treated		-0.110**		-0.159**
		(0.036)		(0.064)
Observations	139,058	139,058	139,058	139,058
Adjusted R^2	0.686	0.688	0.795	0.796
Pre*Treated	Y	Y	Y	Y
After*Controls	Y	Y	Y	Y
LTD Rating Dummies	Y	Y	Y	Y
Buyer FE	Y	Y	Y	Y
Seller FE	Y	Y	Y	Y
SellerXBuyer FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
BuyerXMonth FE	Y	Y	Y	Y

Table 5: Liquidity Spillover Effects in the Cross-Section
This table reports the results from estimating Equation 1 on the subsample of transactions where suppliers opted to provide sales data, as discussed in Section 5.1.1. The dependent variable in columns 1-3 is receivables scaled by sales, and the dependent variable in columns 4-6 is the log of total sales. Regressions are run at the transaction level. *New* is equal to one if the number of years that the seller has been doing business with the buyer is below the median; *Substitutes* is equal to one if the total number of buyers in an industry is greater than the median; *Buyer Constraint* equals one if the buyer does not have access to the bond or equity markets. Control variables are included and are previously defined. Standard errors are clustered at the supplier level and are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

	RecSales			Log Sales		
	(1)	(2)	(3)	(4)	(5)	(6)
After*Treated	-0.168***	-0.187***	-0.218***	-0.120	-0.113	-0.099
	(0.025)	(0.039)	(0.049)	(0.134)	(0.163)	(0.146)
After*Treated*New	-0.071***			-0.057		
	(0.019)			(0.082)		
After*Treated*Substitutes		-0.033**			-0.049	
		(0.012)			(0.033)	
After*Treated*Constrained			-0.021			-0.107***
			(0.017)			(0.024)
Observations	139,058	139,058	139,058	139,058	139,058	139,058
Adjusted R^2	0.690	0.687	0.686	0.796	0.796	0.802
Pre* Treated	Y	Y	Y	Y	Y	Y
After*Controls	Y	Y	Y	Y	Y	Y
LTD Rating Dummies	Y	Y	Y	Y	Y	Y
Buyer FE	Y	Y	Y	Y	Y	Y
Seller FE	Y	Y	Y	Y	Y	Y
SellerXBuyer FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
BuyerXMonth FE	Y	Y	Y	Y	Y	Y

Table 6: Liquidity Spillover Effect on Buyer Credit Quality

This table reports the results from estimating equation 3, where the dependent variable captures the buyer's credit quality. Observations are limited to the sample of public buyers that are matched to Computstat, in order to include control variables. Regressions are run at the buyer-time level. *Credit Score* is the buyer's risk score assigned by Credit2B. It ranges from 1 to 100, where higher scores indicate better credit quality. For columns 2 and 3, observations are limited to those buyers that are covered by both Credit2B and Experian or Equifax, to obtain data on *Collections* and *Covenant Viol.*. *Collections* is an indicator variable equal to one if one of the buyer's invoice was transferred to a collections agency. *Covenant Viol.* equals one if the customer had a technical default on one of his debt contracts. *Exposed* is set equal to one if the customer is linked to at least one supplier in the treatment group, zero otherwise. *Treated* is set equal to one if the customer is directly exposed to the liquidity shortage (at least 20 percent of the customer's bank debt has a scheduled maturity date between July 2007 and August 2008). All other variables are as previously defined. Standard errors are clustered at the buyer level and are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

	(1) Credit Score	(2) Collections	(3) Covenant Viol.
After*Exposed	-3.564**	0.005***	0.015**
	(1.103)	(0.001)	(0.006)
After*Treated	-5.339***	0.008***	0.020***
	(1.379)	(0.002)	(0.006)
Observations	10,886	3,465	3,465
Adjusted R^2	0.618	0.104	0.102
After*Controls	Y	Y	Y
Access to Credit Line	Y	Y	Y
Buyer FE	Y	Y	Y
Time FE	Y	Y	Y
IndustryXTime FE	Y	Y	Y

Table 7: Liquidity Spillover Effect on Buyer Employment

This table reports the results from estimating equation 3, where the dependent variable is the log of *employment*, measured from Dun & Bradstreet. Observations are limited to the sample of public buyers that are matched to Computstat, in order to include control variables. Regressions are run at the buyer-year level. Column 1 reports results for the full sample, and in columns 2 and 3 I split the buyers into above and below median size, respectively. Size is calculated as the average number of employees in the pre-period. *Exposed* is set equal to one if the customer is linked to at least one supplier in the treatment group, zero otherwise. *Treated* is set equal to one if the customer is directly exposed to the liquidity shortage (at least 20 percent of the customer's bank debt has a scheduled maturity date between July 2007 and August 2008). All other variables are as previously defined. Standard errors are clustered at the buyer level and are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

	(1) Whole Sample	(2) Size>Median	(3) Size<Median
Observations	1,490	731	697
Adjusted R^2	0.787	0.709	0.782
After*Controls	Y	Y	Y
Access to Credit Line	Y	Y	Y
Buyer FE	Y	Y	Y
Year FE	Y	Y	Y
IndustryXYear FE	Y	Y	Y

8 Figures

Figure 1. Indicators of Financial Stress
This figure displays the three-month LIBOR and commercial paper (CP) spreads over treasuries, for the period January 2006 through January 2009. The data is obtained from http://www.federalreserve.gov/datadownload/.

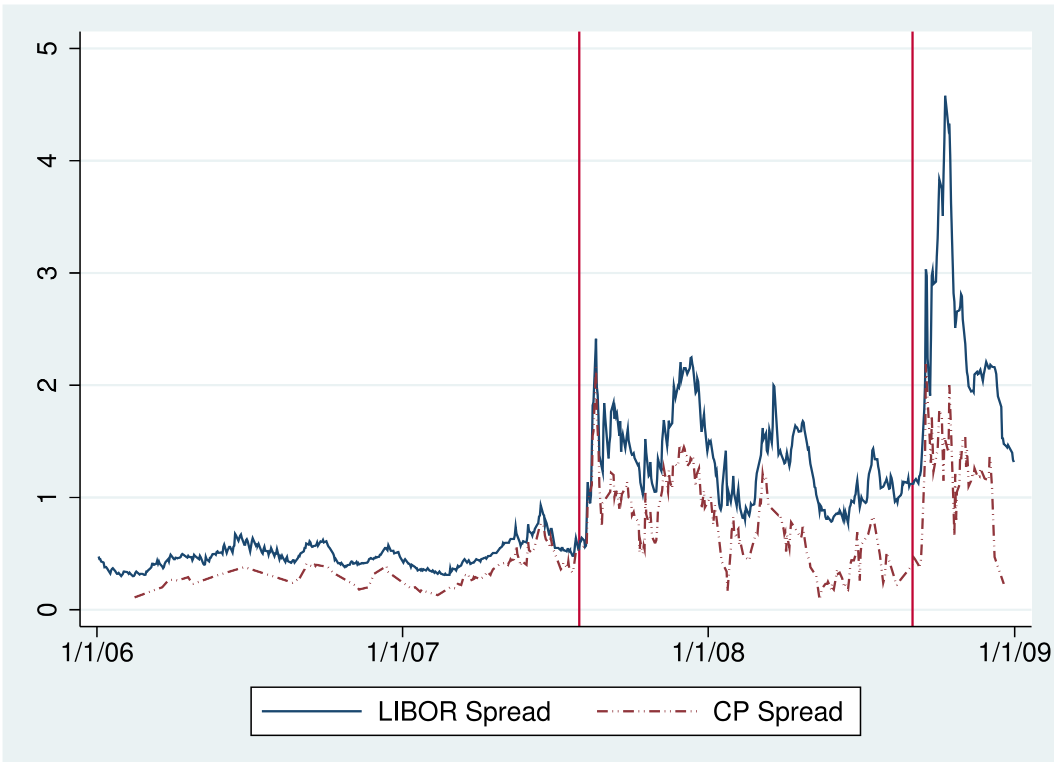


Figure 2. Average Receivables Over Time
This figure plots the average current receivables balance for all transactions recorded with Credit2B over the period January 2005 through December 2009.

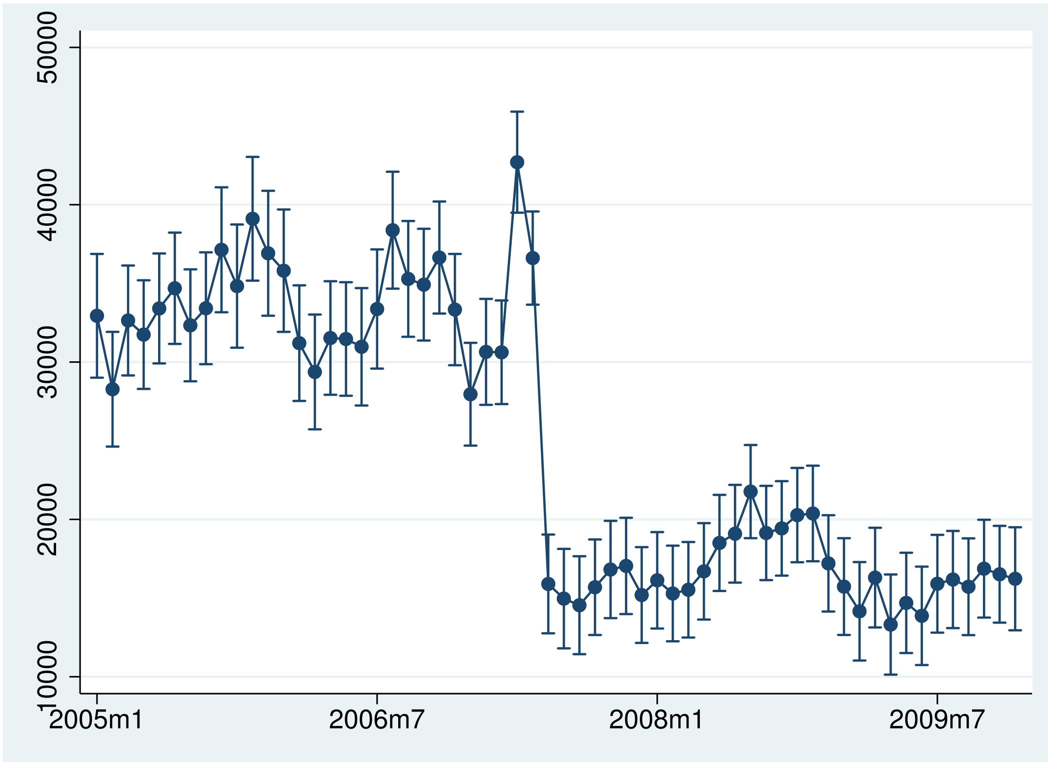


Figure 3. Average Receivables Over Time: Intensive and Extensive Margin
This figure plots the average current receivables balance for all transactions recorded with Credit2B over the period January 2005 through December 2009, split by the extensive versus the intensive margins. The intensive margin requires that the supplier-customer relationship persists for the sample period, while the extensive margin allows customers to drop out.

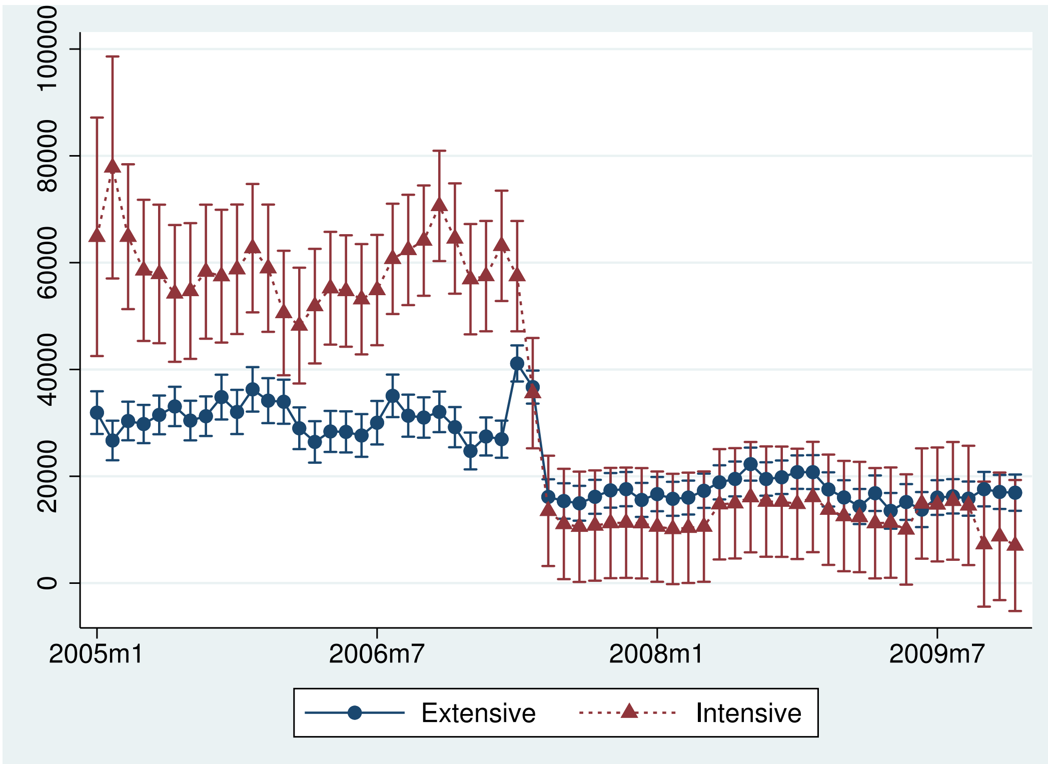
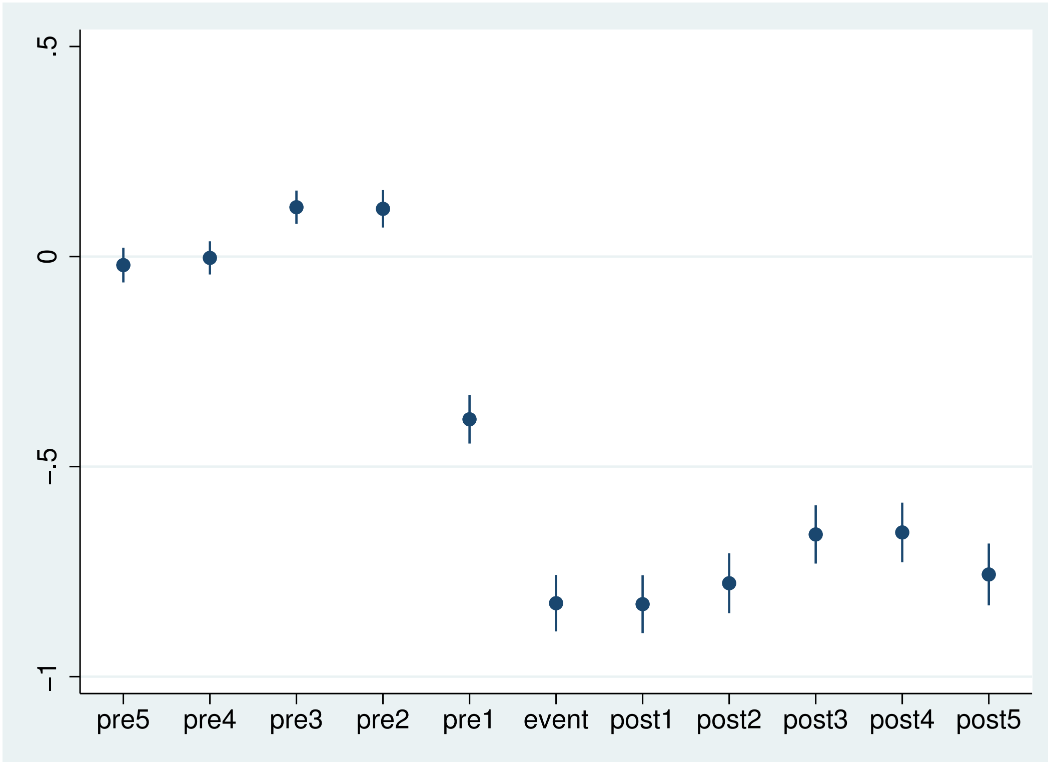


Figure 4. Estimated Coefficients on Pre-Trend Regression
This figure plots the regression coefficients for each month from equation 2. The coefficients represent the estimated difference between the treatment and control groups for each month spanning five months before the crisis through five months after the crisis.



Online Appendix

Credit Market Disruptions and Liquidity Spillover Effects in the Supply Chain

1 Data

Below I provide additional details on data collection, cleaning, and supplementary summary statistics.

1.1 Access

Costello has served in an advisory role to the CEO and management team of Credit2b since 2014, and formally joined their Advisory Board in 2016. Due to this role, she was granted access to the data used in their credit models under a confidentiality agreement, which protects the identities of the debtors and creditors, receivables data, programs, and any other firm-specific information. Non-disclosure agreements were executed between member firms and Credit2B, and between Costello and Credit2B. Under the non-disclosure agreement, Costello is allowed to perform additional analyses on behalf of editors, reviewers, and readers of the Journal. The summarized output of these analyses will be shared subject to Credit2B approval.

1.2 Summary of Credit2B Data

Table A1 reports limited summary statistics for the full Credit2B sample. The universe of Credit2B transactions covers the years 2001 through 2015 and totals close to ninety-five million trade transactions. In their universe of data, there are forty-eight thousand unique members (suppliers), reporting transactions with 7.3 million unique customers. The average dollar value of receivables that are current (i.e., not past due) is $21,032, while the average dollar value of past due receivables is $7,934.[1] It is important to note that there is significant variation in these balances, with some customers representing a large portion of the receivables extended and some customers representing small, one-time transactions.

1.3 Cleaning and Matching the Credit2B Companies

1.3.1 Suppliers

The Credit2B data is collected from member firms' internal data systems. When a supplier firm joins, they provide Credit2B with identifying information that includes company

[1]Note that I assume all missing values are zero, thus the sample averages are much lower than those in the manuscript. Since I take the log of current receivables, zero values will be omitted from my final sample.

name, address, and zipcode. Additional identifying information can be provided, but is not required. If the member is a subsidiary of a larger company, they also provide the company information at the parent level. Credit2B then assigns a unique identifier (called ProviderID), which will link all future transactions to that member. Based on the identifying information, I match supplier firms to Dealscan using exact matches and fuzzy merges. I manually review each match, and then all matches are checked and confirmed by the Credit2B data team.

Next, I match the supplier firms to Compustat. First, I use the Dealscan-Compustat linking file provided by Michael Roberts. To pick up additional observations, I use a fuzzy match to link ProviderIDs to GVKEY by name and location.

1.3.2 Cleaning and Matching the Buyers

For each transaction, the member firms record the name of their customer. Because the data is from individual members' internal systems and is not subject to regulatory rules, there is significant variation in the level of detail. Upon submitting the monthly report, Credit2B has a data team that reviews the report and assigns customer-level identifiers (CompanyID). They do so as follows: first, they obtain location and industry information for each customer from the member firm. Where this is not possible, they attempt to determine the identity of the customer with the name and characteristics of the member firm (i.e., what types of customers are typically downstream from that particular supplier). If that customer already appears in the data, the previously-assigned CompanyID will be assigned for that transaction, so that the customer-level identifier is consistent in the universe of transactions. Credit2B assigns CompanyIDs conservatively, such that a new ID is assigned when the confidence of their matching procedure is low.

Most of the customers are at the establishment-level, and therefore CompanyID is assigned at this level. Based on proprietary sources and web-searches, the Credit2B team then assigns each customer to their parent company (HeadCompanyID). Thus, for each transaction, I observe the supplier ID (ProviderID), the customer-establishment ID (CompanyID), and the customer-parent ID (HeadCompanyID).

I follow a similar procedure to match the customers to Dealscan and Compustat, as reported in Section 1.3.1, above. The data is matched at the HeadCompanyID level, consistent with the structure of the Dealscan and Compustat data.

For all tests of buyer outcomes (Section 6 of the Manuscript), I sort buyers into those that are *Exposed* and those that are *Unexposed*. Table A2 reports the covariate balance across these groups. They are statistically similar along all dimensions.

1.4 Industry Comparison

Descriptive Statistics on the sample used in the main analysis are included in the manuscript. To further understand potential selection into the sample, I tabulate the dispersion in supplier's SIC codes. Table A3 compares the two digit SIC dispersion for the sample as compared to the dispersion for Compustat. The sample firms seem to be a close representation of the industry dispersion in Compustat. The notable differences are the financial services industry (SIC 6067) and the service industry (SIC 7089). While the population of suppliers in Credit2B includes financial firms (factors), I exclude these firms from the analyses since their behavior during the crisis is likely to differ from that of non-financial corporations.

1.5 Subsample Data

As described in the manuscript, the data used to investigate the sales channel (the subsample) was collected through direct request. Specifically, to split out the receivables from the sales channels, I had to obtain transaction-level detail of the *total* sales versus the *credit* portion of sales. All suppliers in the sample were offered incentives to provide this additional data. Specifically, they were offered a \$50 Amazon gift card from Credit2B, early access to the new credit score produced by Credit2B, and one hour of free consulting time by the author on a topic of their choice. Requests were made via email, in-person, and through follow-up phone calls. The emails were sent from Credit2B; in person requests were made by the author and by the CEO (Shyarsh Desai); phone calls were made by the Credit2B data team and by the author. In total, the effort resulted in a response rate of 16% of the main sample, and covers the period June 2006 through June 2008.

I provide evidence on the representativeness of the subsample by comparing it to the full sample in Table A4. The *Full Sample* includes all observations used in the main analysis, and the *Sub Sample* includes only the observations for which I obtained sales and receivables on each transaction. The subsample has a larger balance sheet, on average, than the full sample; they exhibit both higher total assets and higher long-term debt. This is not surprising for a few reasons: (1) larger companies attend the annual trade meetings, where in-person requests were made; (2) larger companies are likely to have better internal accounting systems to retain and organize old data; and (3) larger companies have personal relationships with the management team at Credit2B, incentivizing them to participate.

The size differential may threaten the external validity of the subsample results, so results should be interpreted with caution.

2 Robustness of Primary Results

The primary concern with my identification strategy is that the treatment is functioning through a correlation with an omitted factor that predicts both the timing of debt contract expiry and changes in trade credit. While the claim of randomness of maturity profile is quite plausible, I include a number of robustness checks to explore this possibility, as discussed in the manuscript. I run the analyses with the treatment assigned up to three years in advance to mitigate anticipation effects. I also require loans to have a maturity greater than one year to mitigate concerns that poor quality firms get shorter term loans and may be more likely to fall into the treatment group. I include a control for the average maturity of the firm's outstanding debt agreements. All results are robust to these additional controls.

Two additional tests are performed to provide further support for my primary results. First, I perform a falsification test wherein the analysis is moved backward by one year. This places the treatment date at July, 2006. The results of this falsification test are reported in Table A5. Reassuringly, the coefficient of interest, $After * Treated$, is insignificant in all specifications when pulling the treatment effect back one year.

Second, to validate the identification strategy and to increase the external validity of the tests beyond the 2007-2008 period, I reassign treatment following Chodorow-Reich (2014) and Ivashina and Scharfstein (2010). In particular, the authors predict that variation in bank health before the crisis influences the bank's ability to lend during the crisis. Chodorow-Reich (2014) argues that since the origins of the financial crisis started in the banking sector rather than the corporate sector, variation in the bank's ability to lend during the crisis is plausibly orthogonal to borrower characteristics. Therefore, the author argues that these exogenous differences in bank health have a causal effect on borrower employment levels.

To measure credit availability to borrower b during the crisis period, I follow Chodorow-

Reich (2014) and calculate the change in lending behavior for all of borrower b's precrisis syndicate members. The precrisis period is defined as October 2005 through June 2007, and the crisis period is defined as October 2008 through June 2009. Note that the sample period, and therefore the sample of suppliers, is different from that in the main specification of the Manuscript. The change in loans to borrower b is calculated as a weighted average of the change in loans for each of the borrower's syndicate members, weighted by the share of the loan retained by each lender.[2]

Chodorow-Reich (2014) further acknowledges that unobserved characteristics of borrowers that affect loan demand might be correlated at the lender level, causing the change-in-lending variable to be biased. Therefore, the change in loans is instrumented with three sets of variables. *Lehman Exposure* is calculated as the fraction of the bank's syndication portfolio where Lehman Brothers had a lead role (Ivashina and Scharfstein (2010)); the second instrument uses the bank's exposure to mortgage-backed securities (ABX); the third set of instruments uses bank balance sheet and income statement items including *trading revenue/Assets*, *real estate chargeoffs/assets*, and *bank deposits/assets*. Each of these instruments is purported to influence borrower outcomes only through their effect on the bank's change in lending behavior.

To measure the effect of a change in bank health on borrower outcomes, I follow the prior literature and regress the percentage change in current receivables (measured between March 2008 and March 2009) on the change in loans from the borrower's syndicate members. Regressions are run at the supplier level, and Table A6 reports the results. Column 1 reports the results from an OLS regression where the change in loans is the independent variable of interest, while columns 2 through 5 report the results where the change in loans is instrumented with each of the variables discussed above. The results are strongly significant in all specifications, and they indicate that borrowers with a tighter loan supply lend less trade credit to their downstream customers. Though preliminary, the results in Table A6 lend credence to the main findings relating the supply of bank liquidity to trade credit.

[2] Since banking relationships are sticky, the author assumes that the borrower cannot easily switch to other lenders during the crisis period.

2.1 Continuous Treatment and Exposure-to-Treatment

In principle, the treatment should have an impact on spillovers in a continuous fashion. Therefore, I re-estimate all specifications from Table 3 of the manuscript using a continuous treatment variable, and I re-estimate the employment effects from Table 7 of the manuscript using a continuous exposure-to-treatment variable. The results are reported in Table A7. The mean value of *Treated_ Continuous* is 0.109, with a standard deviation of 0.273. Thus, moving one standard deviation in the continuous treatment decreases trade credit by about 8.2%. I do note that the specifications where the dependent variable is aggregated and/or scaled by sales (columns 5-8) lose some statistical significance. The dependent variables in these specifications have more noise (since the numerator and denominator are from different data sources and potentially calculated differently), so this is not entirely surprising. This highlights that the binary treatment may be most appropriate for the main tests included in the Manuscript.

Table A8 re-estimates Table 7 using a continuous exposure-to-treatment variable. Specifically, *Exposed_ Continuous* is the buyer's weighted-average exposure to their suppliers' liquidity shocks, where weights are calculated as the percentage of the buyer's total pre-period purchases (accounts payable) from each supplier in the pre-period, and exposure is calculated as the percentage of the supplier's debt maturing between July 2007 and August 2008. I continue to find that among the smaller buyers, those with a more intense exposure-to-treatment see larger declines in employment. This is reassuring and suggests my results are not an artifact of the dichotomous assignment.

3 Cross-Sectional Variation in the Spillover: Further Evidence on the Risk-Sharing Motive

As discussed in the Manuscript, one motivation for the trade credit adjustment is that customers may be willing to share in the risk of their liquidity-constrained supplier. An important literature highlights the roll of trade credit in filling the financing gap when there are frictions in external financing markets.[3] This is also consistent with the logic in Wilner (2000) and Cunat (2007), where trade credit is used as an insurance mechanism to retain important partners.

To further assess the risk sharing channel, I develop two proxies for the customer's *willingness* to share in the risk of his supplier. First, for each customer in the pre-period, I count the number of suppliers he is connected to and split this at the sample median. Customers with relatively few alternative suppliers should be more willing to risk-share with existing suppliers because there may be fewer outside options. Second, for each customer in the pre-period, I calculate the median age of his relationships with all of his suppliers and split the sample into above and below the median age. Following similar logic, customers should be more willing to share in the risk of their more established suppliers than with their younger ones.[4] I report the results inTable A9. The dependent variable in columns 1 and 2 is receivables scaled by sales, and the dependent variable in columns 3 and 4 is the log of total sales. I fail to find any differential impact of a customer's willingness to risk share on the observed trade credit behavior, based on these proxies.

I caveat that these non-results do not necessarily indicate that the risk-sharing channel does not exist. Perhaps these proxies are not precise enough to capture the effect. Alternatively, the non-result may be a function of the time period. In good times the customers may be more willing to risk-share, but the onset of the crisis may magnify the customer's desire to hold liquidity.[5]

4 Buyer Liquidity Adjustments

In the absence of financing frictions, spillovers should have little adverse effect on the downstream customer. In the context of the Kiyotaki and Moore (1997) model of credit chains, a liquidity shock will propagate through the supply chain until the chain reaches a 'deep pocket,' who can absorb the liquidity shock because they have sufficient access to

[3]The prior literature focuses on a relatively less constrained supplier financing his downstream customer, but the reverse logic also applies.

[4]Note that this split is different from that in Table 5 of the manuscript, as age was calculated according to the median age of the *supplier's* portfolio rather than the *customer's* portfolio.

[5]Strong, liquid buyers may have the power to delay both the sales and the trade credit spillovers the longest.

capital. Based on this literature, we should observe that some buyers respond to the trade credit channel and the sales channel by shifting to other sources.

To test this theory, I estimate equation 3 from the Manuscript, where the dependent variables are the buyer's alternative sources of liquidity. Specifically, I include: *Cash*, defined as cash scaled by assets; *New Bank*, defined as the log volume of new loans entered into on a quarterly basis, obtained from Dealscan; *Drawn Bank*, defined as the percentage of the total line of credit that is drawn in a given year, obtained from Capital IQ[6]; *New Trade*, defined as the log volume of new trade credit from the customer's non-treated suppliers; and *Days Slow*, defined as the number of days past the agreed upon due date that the buyer pays his bills to non-treated suppliers. Together, these dependent variables represent sources of liquidity that may be on-hand at the onset of the spillover (i.e., cash reserves, existing lines of credit, and delayed payment on already extended receivables) versus *new* loans (i.e., new bank debt, and new trade debt).

Results are reported in Table A10 and tell a compelling story. Downstream buyers respond to the liquidity spillover from exposure to treated suppliers by drawing on cash balances, drawing down existing lines of credit, and further delaying the invoices that are due to other suppliers. There is no evidence that they obtain new bank debt, consistent with the narrative that lending markets tightened during this time period, making it difficult to obtain new loans. I also find that buyers do not increase trade debt, which could indicate that trade credit markets also tightened; the non-result could also be due to the fact that buyers are now operating at a smaller scale, thus purchasing fewer inputs from other suppliers.

It should be noted that the spillover effects documented are incremental to any *direct* exposure to a banking liquidity shock. The spillover effect on cash holdings represents about 25% of the direct effect of a liquidity shock, and the spillover effect on the number of days late on existing trade credit represents about 75% of the direct effect of a liquidity shock. In either case, the spillover channel appears to be substantial, and is likely a result of a combination of the trade credit channel and the sales channel.

[6]Note that the sample of loans covered by Capital IQ is relatively small. I only include the firms with data in Capital IQ and assume that if a buyer is not in the data, he doesn't have access to a line of credit during this period

5 Preliminary Tests to Separately Assess the Trade Credit and the Sales Channels

As stated in the Manuscript, the purpose of the paper is not to separately quantify the relative contribution of the sales and trade credit channels on the adverse downstream effects. Rather, my paper is a first step in showing that spillover effects are significant, and that they can occur through both of these channels. However, below I provide some preliminary and suggestive evidence that at least some of the spillover effect is due to the trade credit channel.

5.1 The Timing of Buyer Outcomes: Liquidity Adjustments and Buyer Credit Quality

Table A10 and Table 6 of the manuscript test whether buyers respond to the liquidity spillover. Specifically, Table A10 captures any shift to alternative sources of liquidity after exposure to the spillover, and Table 6 tests whether the buyers who are exposed to the spillover show a change in credit quality. I find that buyers exposed to the spillover show a substitution to other forms of credit and a concurrent increase in their credit risk. However, as noted in the manuscript, the effects could be due to *both* the trade credit and the sales channels.

In an attempt to provide some evidence on whether the liquidity and credit quality effects are due to the trade credit channel or the sales channel, I investigate the timing of these responses. The logic is as follows. In Table 4 of the manuscript, I find that the trade credit channel explains much of the spillover in the first three quarters following the treatment. The sales channel only comes in effect in the fourth quarter following treatment. In other words, suppliers cut credit first, and wait to cut sales until quarter 4. Therefore, most of the liquidity and credit quality response in the period before quarter 4 can be largely attributed to the trade credit channel.

I can only investigate timing for the outcomes that are recorded at a monthly or quarterly level. This is due to the fact that the subsample only spans one year after treatment, so I don't know which channel dominates thereafter. Cash reserves are recorded at a quarterly level, and Days Late and Credit Score are recorded at a monthly level. Therefore, I include these three measures as my dependent variables. Results are reported in Table A11. Cash and Days Late spike in the first quarter after exposure to the spillover, suggesting that the buyer's adjustment in this period is in response to the trade credit channel. Further, the buyer's credit risk increases immediately after exposure, consistent with the trade credit channel explaining at least a portion of the contagion in credit risk. I should note, however, that in theory cash drawdowns and rating declines can occur through both the sales and trade credit channels. These timing effects are suggestive and should be interpreted with caution.

5.2 *Controlling for Purchases*

The effect of the sales channel on the downstream buyer should manifest in adjustments the customer's inventory purchases. Purchases should adjust if the supplier changes the price or quantity, as long as demand is not at or near unit elasticity. Therefore, directly controlling for purchases should absorb much of the sales channel effect on the adverse downstream consequences. This analysis is subject to the caveat that I am controlling for an endogenous variable, but is in the spirit of attempting to isolate the two channels.

For each customer in the sample, I calculate their purchases as the change in inventory balance plus the cost of goods sold.[7] I re-estimate Tables 6-7 while controlling for buyer purchases and report results in Tables A12 and A13. The results from estimating the spillover effect on buyer credit quality are virtually unchanged, and the employment effects show some minor attenuation when controlling for purchases. Though imperfect, this suggests that the sales channel does not subsume the adverse downstream effects.

5.3 *Employment Effects for Small, Constrained Customers versus Small, Unconstrained Customers*

In Table 5 of the Manuscript, I show that 'Constrained' customers (those without access to debt or equity markets) suffer from both the sales channel and the trade credit channel, whereas 'Unconstrained' customers only suffer from the trade credit channel. However, for all of my buyer outcomes, I only investigate adverse effects for the subsample of *unconstrained* customers. This is because I rely on publicly listed firms so that I can include

[7] All customers included in the sample are publicly listed. Under U.S. GAAP, $Inventory_t = Inventory_{t-1} + Puchases_t$ - $COGS_t$. I observe the beginning and ending inventory balances from the consecutive balance sheets and the current period COGS from the income statement. Therefore, current period purchases are calculated as $Purchases_t = \Delta Inventory + COGS_t$. Similarly, for firms using raw materials and work in progress inventory, I calculate purchases as $\Delta RawMaterials + \Delta WorkInProgress + \Delta FinishedGoods +$ COGS.

buyer-level control variables in the regressions. Therefore, to provide a more direct comparison between Tables 5 and 7, I re-estimate the employment effects for the subsample of 'Constrained' buyers that do not have access to the bond or equity markets.[8] The results are reported in Table A14. In column (1), I re-estimate the equation for small, *public*, firms (i.e., those included in column 3 of Table 7 in the manuscript) *without* buyer-level control variables, in order to provide a more appropriate benchmark for column (2), which captures the employment effects of exposure to treatment for the subsample of small private firms. Results show that the impact of the buyer's exposure to treatment is larger for the sample of small, private (constrained) buyers, relative to the sample of small, public (less constrained) buyers, though the impact is significant in both cases. This evidence, though suggestive, points to a combined effect of the sales and trade channels, rather than the sales channel alone explaining the results.

6 Aggregate Importance of Liquidity Spillovers: Partial Equilibrium

Though the results of the paper demonstrate that the transmission of financial sector shocks have important adverse spillover effects on employment at the firm level, this analysis has several limitations. First, I only investigate the spillover effect on one node in the downstream network. It is reasonable to believe that spillovers may result in knock-on effects that continue to propagate downstream. To address this limitation, I add in a second downstream node to the analysis. For exposed and unexposed buyers in the sample, I mapped in the trade connections with *their own* downstream buyers, where possible.[9] Thus, *Exposed* is set equal to one if the first downstream customer is linked to at least one supplier in the treatment group, zero otherwise, and *Exposed2* is set equal to one if a customer two nodes downstream is linked to at least one supplier in the treatment group, zero otherwise. Twenty-four percent of the sample of first-node downstream buyers are *Exposed* and 12.1 percent of the sample of second-node downstream buyers are *Exposed2*. I now estimate the

[8]Dun and Bradstreet collect employment data for both public and private firms, so I'm able to estimate this. Other downstream consequences like cash drawdowns cannot be explored because I cannot obtain this data for private firms.

[9]To obtain this data, the buyers have to join the Credit2B network as a supplier.

following regression, using OLS:

$$
\begin{aligned}
y_{j,t} = \beta_1 After_t * Exposed_j + \beta_2 After_t * Exposed2_j + \beta_3 After_t * Treated_j \\
+\gamma Controls_j * After_t + \alpha_j + \alpha_{t,z} + \varepsilon_{i,j,t}
\end{aligned} \tag{1}
$$

This new specification allows me to assess (i) the employment effect of a direct liquidity shock (β_3); (ii) the employment effect of an indirect liquidity shock on the first node downstream (β_1); and (iii) the knock-on employment effect of an indirect liquidity shock on an additional second node downstream (β_2). I re-estimate the effect of exposure to treatment on downstream employment including all three of these effects. Results are reported in Table A15. Interestingly, there is some dampening in the second node, but the knock-on effects remain significant.

Next, I evaluate whether these spillover effects are important in the aggregate. To provide some insight on this front, I do a simple quantification exercise to gauge the importance of the direct effect, the spillover effect, and the knock-on effect on aggregate employment levels. I rely on the results from my micro-economic estimation in Table A15 to evaluate the importance of each effect on the overall employment decline, relative to the counterfactual of an economy without spillovers. In doing so, I make the simplifying assumption that the total employment effects are equal to the sum of the employment effects at each firm. Using the estimates from column 3, I calculate the fitted decline in employment due to the spillover as follows:

$$
\hat{y}_{i,t} = \beta_x * \sum_i Exposed_i * Employment_{i,t-k} \tag{2}
$$

, where β_x is the coefficient on *After*Exposed* or *After*Exposed2*, and $Employment_{i,t-k}$ is the buyer's average pre-period employment level. Since the estimated effect of the spillover for large firms is not statistically significant I restrict the aggregation to the sub-sample of small firms. The predicted value represents the total number of jobs lost at small firms, due to the buyer's exposure to the spillover, or the second node's exposure to the spillover. This estimate is relative to the counterfactual of no spillover effect on employment.

To obtain the fraction of the sample decline in employment due to the spillover effect, I divide equation 2 by the sum of the employment change over all small firms in the sample. The results of this exercise suggest that the first node liquidity spillover channel explains about 9.98% of the reduction in employment for small firms, and the second node liquidity spillover explains about 3.70% of the reduction in employment for small firms. This estimate implies that the liquidity spillover - both through trade credit and sales channels - explains a significant portion of the employment losses during the crisis. To put the number in context, a similar exercise to estimate the aggregate effects of the direct exposure to bank distress (β_4) explains about 13.7% of the decline in employment at small firms.[10]

A few caveats are in order. First, these calculations ignore general equilibrium concerns. For example, labor may reallocate to unexposed, or less-constrained, firms. Note, however, that the DD estimator should account for the reallocation to less-constrained firms, since these are the firms in the control group. Second, my sample may not extrapolate to the population as a whole. And finally, the aggregate effects I document are relative to the sample of small firms.

7 Aggregate Importance of Liquidity Spillovers: Static GE model

In this section, I present a stylized static model of trade credit that illustrates the basic intuition in the paper: firms that have access to neither bank loans nor trade credit are forced to use the labor margin in responding to demand shocks. The model in this section is a simplification of the model in Boyarchenko and Costello (2019). In spirit, the economy is close to those modeled in Chodorow-Reich (2014) and Luo (2019), but I consider a setting where firms have access to both trade credit and bank loans as sources of bridge liquidity. The trade credit modeling here is also different from Luo (2019): in this economy, firms charge the same price on delivery to both end consumers and trade credit partners but charge a relationship-specific interest rate on deferred payments. Finally, firms face an explicit borrowing constraint, with their total borrowing through both bank loans and trade credit

[10] I also estimate the aggregate effects of the liquidity spillover using a continuous measure of exposure-to-treatment, discussed in Section 2.1 of this Appendix. Based on the estimates in Table A8, I find that the first-node spillover explains about 9.73% of the decline in employment for small firms. This estimate is in line with the estimates using the dichotomous treatment, though slightly muted.

limited by their free cash-flows.

7.1 Timing

The economy exists for two periods, indexed by $t = 0, 1$. Firms enter date 0 having selected their intermediate good inputs from their trading partners. Uncertainty about household demand for the differential good varieties and about the willingness of the bank to provide intraperiod loans to firms is resolved at date 0. Firms make labor and trade credit decisions at date 0 to maximize total profits, given the shock realizations and the pre-determined use of intermediate goods, and subject to a working capital constraint. At date 1, firms use net revenue from the goods produced at date 0 to repay the bank loan and trade credit from its trade network partners.

7.2 Firms

There are J firms in the economy, indexed by $j = 1, \ldots, J$. Each firm j produces a differentiated good according to a Cobb-Douglas production function

$$y_j = n_j^{\alpha} \left(\prod_{j'=1}^{J} m_{jj'}^{\omega_{jj'}} \right)^{1-\alpha}, \tag{1}$$

where n_j is the labor input into production, $m_{jj'}$ are the goods produced by firm j' that are used as intermediate inputs in the production of firm j, and $\omega_{jj'}$ is the intensity of that input, with

$$\sum_{j'=1}^{J} \omega_{jj'} = 1, \quad \omega_{jj} = 0, \quad \forall j = 1, \ldots, J.$$

All production happens at date 0, but firms enter into the period with their intermediate inputs predetermined. The only margin of adjustment in terms of production at date 0 is thus the labor market decision. As is common in the literature (see e.g. Jermann and Quadrini, 2012), I assume that each firm must make payments to workers and supply-network partners before receiving revenue from sales of its own production. Each firm covers these payments with an intraperiod bank loan, so that the total amount of intraperiod loan contracted by firm j at date 0 is

$$l_j = w_j n_j + \sum_{j'=1}^{J} (1 - \theta_{jj'}) \, p_{j'} m_{jj'}, \tag{2}$$

where w_j is the wage paid by firm j, $p_{j'}$ is the equilibrium price of the goods produced by firm j', and $\theta_{jj'}$ is the fraction of goods delivered by supplier j' at date 0 that the supplier accepts deferred payment on.

At date 1, once the revenues from its production are realized, firm j repays the bank loan (at interest rate r_{bj}) and the trade credit extended by its suppliers (at a price markup $\eta_{jj'}$). That is, the interperiod budget constraint of firm j is

$$p_j \left(c_j + \sum_{j'=1}^{J} \left[(1 - \theta_{j'j}) + \theta_{j'j} \, (1 + \eta_{j'j}) \right] m_{j'j} \right) + l_j \geq$$
$$w_j n_j + \sum_{j'=1}^{J} \left[(1 - \theta_{jj'}) + \theta_{jj'} \, (1 + \eta_{jj'}) \right] p_{j'} m_{jj'} + (1 + r_{bj}) \, l_j.$$

Using goods market clearing, the intertemporal budget constraint can be simplified to obtain

$$p_j \left(y_j + \sum_{j'=1}^{J} \theta_{j'j} \eta_{j'j} m_{j'j} \right) + l_j \geq w_j n_j + \sum_{j'=1}^{J} (1 + \theta_{jj'} \eta_{jj'}) \, p_{j'} m_{jj'} + (1 + r_{bj}) \, l_j. \tag{3}$$

Finally, I assume that the ability of the firm to borrow, both from its supply network partners and from the bank, is bounded by the limited enforceability of debt contracts, and that total borrowings are "collateralized" by the gross revenue of the firm at date 0

$$\lambda p_j \left(y_j - \sum_{j'=1}^{J} \theta_{j'j} m_{j'j} \right) \geq (1 + r_{bj}) \, l_j + \sum_{j'=1}^{J} \theta_{jj'} \, (1 + \eta_{jj'}) \, p_{j'} m_{jj'}, \tag{4}$$

where λ is the maximum leverage allowed.

Substituting the liquidity constraint (2) into the budget constraint (3) obtains

$$p_j \left(y_j + \sum_{j'=1}^{J} \theta_{j'j} \eta_{j'j} m_{j'j} \right) \geq (1 + r_{bj}) \, w_j n_j + \sum_{j'=1}^{J} \left[r_{bj} \, (1 - \theta_{jj'}) + (1 + \theta_{jj'} \eta_{jj'}) \right] m_{jj'} p_{j'}. \tag{5}$$

Similarly, the borrowing constraint (4) can be rewritten as

$$\lambda p_j \left(y_j - \sum_{j'=1}^{J} \theta_{j'j} m_{j'j} \right) \geq (1 + r_{bj}) \, w_j n_j + \sum_{j'=1}^{J} \left[(1 + r_{bj}) \, (1 - \theta_{jj'}) + \theta_{jj'} \, (1 + \eta_{jj'}) \right] m_{jj'} p_{j'}. \tag{6}$$

Thus, firm j chooses n_j and $\{\theta_{j'j}\}_{j'=1}^{J}$ to maximize total profits

$$\begin{aligned} \max_{n_j, \{\theta_{j'j}\}_{j'=1}^{J}} t_j = p_j & \left(n_j^{\alpha} \left(\prod_{j'=1}^{J} m_{jj'}^{\omega_{jj'}} \right)^{1-\alpha} + \sum_{j'=1}^{J} \theta_{j'j} \eta_{j'j} m_{j'j} \right) \\ & - (1 + r_{bj}) \, w_j n_j + \sum_{j'=1}^{J} \left[r_{bj} \, (1 - \theta_{jj'}) + (1 + \theta_{jj'} \eta_{jj'}) \right] m_{jj'} p_{j'}, \end{aligned}$$

subject to the borrowing constraint (6) and $\theta_{j'j} \in [0, 1] \; \forall j' = 1, \ldots, J$. The first order conditions are thus

$$[n_j] : \qquad 0 = p_j \alpha n_j^{\alpha - 1} \left(\prod_{j'=1}^{J} m_{jj'}^{\omega_{jj'}} \right)^{1-\alpha} (1 + \gamma_{lj} \lambda) - (1 + r_{bj}) \, w_j \, (1 + \gamma_{lj}) \tag{7}$$

$$[\theta_{j'j}] : \qquad 0 = p_j m_{j'j} \, (\eta_{j'j} - \gamma_{lj} \lambda) + \gamma_{0j'} - \gamma_{1j'}, \tag{8}$$

where γ_{lj} is the Lagrange multiplier on the borrowing constraint, $\gamma_{0j'}$ is the Lagrange multiplier on $\theta_{j'j} \geq 0$, and $\gamma_{1j'}$ is the Lagrange multiplier on $\theta_{j'j} \leq 1$. Solving (7) for n_j obtains

$$n_j = \left(\prod_{j'=1}^{J} m_{jj'}^{\omega_{jj'}} \right) \left(\frac{(1 + r_{bj}) \, w_j \, (1 + \gamma_{lj})}{p_j \alpha \, (1 + \gamma_{lj} \lambda)} \right)^{\frac{1}{1-\alpha}}, \tag{9}$$

so that the equilibrium output of firm j is

$$y_j = \left(\prod_{j'=1}^{J} m_{jj'}^{\omega_{jj'}} \right) \left(\frac{(1 + r_{bj}) \, w_j \, (1 + \gamma_{lj})}{p_j \alpha \, (1 + \gamma_{lj} \lambda)} \right)^{\frac{\alpha}{1-\alpha}}. \tag{10}$$

Next, notice that (8) implies that $\theta_{j'j}$ is always either 1 (if firm j is borrowing unconstrained) or 0 (if firm j is borrowing constrained). Denote by $\mathcal{C}$ the set of firms that, in equilibrium, are borrowing-constrained. Then, if firm j is borrowing-constrained, the borrowing constraint

(6) implies

$$\left(\frac{(1+r_{bj})^{\alpha}\, w_j^{\alpha}}{p_j}\right)^{\frac{1}{\alpha-1}} \left(\prod_{j'=1}^{J} m_{jj'}^{\omega_{jj'}}\right) \left(\lambda \left(\frac{1+\gamma_{lj}}{\alpha\,(1+\lambda\gamma_{lj})}\right)^{\frac{\alpha}{\alpha-1}} - \left(\frac{1+\gamma_{lj}}{\alpha\,(1+\lambda\gamma_{lj})}\right)^{\frac{1}{\alpha-1}}\right) = \sum_{j'\in\mathcal{C}} (1+r_{bj})\, m_{jj'} p_{j'} + \sum_{j'\notin\mathcal{C}} (1+\eta_{jj'})\, m_{jj'} p_{j'}. \tag{11}$$

7.3 Banks

A bank consists of a continuum of banks who supply loans to firms, pooling repayment risk, and take deposits from households to finance the loans. The representative bank chooses loans made to firms to maximize the net payments to the households, $r_b L$, subject to the budget constraint

$$L = d,$$

where d is the total deposits of the household. The firm loan bundle L is a Dixit-Stiglitz constant elasticity of substitution (CES) aggregation of bank loans to the differentiated goods producers

$$L = \left(\sum_{j=1}^{J} \zeta_j^{\frac{1}{\varphi}} l_j^{\frac{\varphi+1}{\varphi}}\right)^{\frac{\varphi}{\varphi+1}},$$

and r_b is the interest earned on extending one dollar of the loan basket L:

$$1+r_b = \left(\sum_{j=1}^{J} \zeta_j^{-1} (1+r_{bj})^{\varphi+1}\right)^{\frac{1}{\varphi+1}}.$$

The representative bank thus chooses the loan allocation to firms to maximize total interest earned on loaning out L

$$\max_{l_j} \sum_{j=1}^{J} (1+r_{bj})\, l_j \quad \text{s.t.} \quad L = \left(\sum_{j=1}^{J} \zeta_j^{\frac{1}{\varphi}} l_j^{\frac{\varphi+1}{\varphi}}\right)^{\frac{\varphi}{\varphi+1}},$$

so that the optimal loan amount to firm j is

$$l_j = \left(\frac{1+r_{bj}}{1+r_b}\right)^{\varphi} \zeta_j^{-1} L. \tag{12}$$

7.4 Households

A household consists of a continuum of individuals who supply labor to firms and pool consumption risk. The representative household choose its consumption bundle, labor allocation, and bank deposits to maximize the expected utility of consumption, labor and deposits at date 0

$$U = u\left(C, N, d\right), \tag{13}$$

subject to the budget constraint

$$PC = wN + r_b d + T,$$

where P is the nominal price index and T is the total payments from productive firms. I assume that the household has Poterba and Rotemberg (1986) preferences over consumption and deposits, and Greenwood, Hercowitz, and Huffman (1988) preferences over consumption and labor, so that

$$u\left(C, N, d\right) = \frac{1}{\vartheta}\left(\left(C - \phi\frac{\epsilon}{1+\epsilon}N^{1+\frac{1}{\epsilon}}\right)^{\beta} d^{1-\beta}\right)^{\vartheta}.$$

This utility function exhibits complementarity between consumption and labor, and substitutability between the consumption-labor aggregate and liquidity, so that more consumption raises the marginal utility of liquidity and vice versa. The consumption bundle C is a Dixit-Stiglitz constant elasticity of substitution (CES) aggregation of the differentiated varieties

$$C = \left(\sum_{j=1}^{J} \xi_j^{\frac{1}{\sigma}} c_j^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}},$$

where σ is the elasticity of substitution across the varieties, and ξ_j is a variety-specific taste shock. The corresponding economy-wide nominal price index P is the cost of purchasing one unit of C:

$$P = \left(\sum_{j=1}^{J} \xi_j p_j^{1-\sigma} \right)^{\frac{1}{1-\sigma}}.$$

Similarly, N is a CES aggregator of the labor supplied to different firms, with elasticity of substitution ν

$$N = \left(\sum_{j=1}^{J} n_j^{\frac{\nu+1}{\nu}} \right)^{\frac{\nu}{\nu+1}},$$

and w is the economy-wide composite wage earned by the household from optimally allocating one unit of labor across firms

$$w = \left(\sum_{j=1}^{J} w_j^{1+\nu} \right)^{\frac{1}{1+\nu}}.$$

Given a choice of total consumption C, the household chooses the consumption basket to minimize total expenditure on acquiring that level of consumption

$$\min_{c_j} \sum p_j c_j \quad \text{s.t.} \quad C = \left(\sum_{j=1}^{J} \xi_j^{\frac{1}{\sigma}} c_j^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}},$$

so that the optimal consumption of the output of firm j is

$$c_j = \left(\frac{p_j}{P} \right)^{-\sigma} \xi_j C. \tag{14}$$

Similarly, the household chooses the labor allocation to maximize the total wage earned by working N hours

$$\max_{n_j} \sum_{j=1}^{J} w_j n_j \quad \text{s.t.} \quad N = \left(\sum_{j=1}^{J} n_j^{\frac{\nu+1}{\nu}} \right)^{\frac{\nu}{\nu+1}},$$

so that the optimal hours worked in firm j is

$$n_j = \left(\frac{w_j}{w}\right)^{\nu} N. \tag{15}$$

The household then chooses C, N, and d to maximize utility subject to its budget constraint, taking the equilibrium price index P, wage index w, interest rate index r_b and net transfers T from firms as given. The first order conditions for the household are thus

$$\begin{aligned}
[C]: \quad & 0 = \vartheta\beta\left(C - \phi\frac{\epsilon}{1+\epsilon}N^{1+\frac{1}{\epsilon}}\right)^{-1} u - \gamma P \\
[N]: \quad & 0 = -\vartheta\beta\phi N^{\frac{1}{\epsilon}}\left(C - \phi\frac{\epsilon}{1+\epsilon}N^{1+\frac{1}{\epsilon}}\right)^{-1} u + \gamma w \\
[L]: \quad & 0 = \vartheta\left(1-\beta\right) d^{-1} u + \gamma\left(1+r_b\right),
\end{aligned}$$

where γ is the Lagrange multiplier on the budget constraint, so that

$$\phi N^{\frac{1}{\epsilon}} = \frac{w}{P}; \quad \frac{\beta-1}{\beta}\left(C - \phi\frac{\epsilon}{1+\epsilon}N^{1+\frac{1}{\epsilon}}\right) = \frac{1+r_b}{P}d.$$

Substituting into the budget constraint, I thus obtain

$$d = \frac{P\left(\beta-1\right)}{\beta\left(1+r_b\right)}\left(C - \phi\frac{\epsilon}{1+\epsilon}\left(\frac{w}{\phi P}\right)^{1+\epsilon}\right) \tag{16}$$

$$N = \left(\frac{w}{\phi P}\right)^{\epsilon} \tag{17}$$

$$C = \left(\frac{w}{P}\right)^{1+\epsilon}\phi^{-\epsilon}\left(\frac{\left(1+r_b\right)\beta + \epsilon\left(\beta+r_b\right)}{\left(1+\epsilon\right)\left(\beta+r_b\right)}\right) + \frac{T}{P}\frac{\beta\left(1+r_b\right)}{\beta+r_b}. \tag{18}$$

7.5 Equilibrium

Given household $\{\xi_j\}_{j=1}^{J}$ and bank $\{\zeta_j\}_{j=1}^{J}$ preference shocks, production network connections $\{\omega_{jj'}\}_{j=1}^{J}$ and the pre-determined production inputs $\{\omega_{jj'}\}_{j,j'=1}^{J}$, an equilibrium is a set of trade credit decisions $\{\theta_{jj'}\}_{j,j'=1}^{J}$, bank loans $\{l_j\}_{j=1}^{J}$, labor allocations $\{n_j\}_{j=1}^{J}$, consumption decisions $\{c_j\}_{j=1}^{J}$, prices $\{p_j\}_{j=1}^{J}$, wages $\{w_j\}_{j=1}^{J}$, and interest rates $\{r_{bj}\}$ such that the firms' decisions satisfy (2) and (7) – (11), household's decisions satisfy (14) – (18), bank decisions satisfy (12) and:

1. Goods markets clear:

$$\left(\prod_{j'=1}^{J} m_{jj'}^{\omega_{jj'}}\right)\left(\frac{(1+r_{bj})\,w_j\,(1+\gamma_{lj})}{p_j\alpha\,(1+\gamma_{lj}\lambda)}\right)^{\frac{\alpha}{1-\alpha}} = \sum_{j'=1}^{J} m_{j'j} + \left(\frac{p_j}{P}\right)^{-\sigma}\xi_j C \tag{19}$$

2. Labor market clears:

$$\left(\prod_{j'=1}^{J} m_{jj'}^{\omega_{jj'}}\right)\left(\frac{(1+r_{bj})\,w_j\,(1+\gamma_{lj})}{p_j\alpha\,(1+\gamma_{lj}\lambda)}\right)^{\frac{1}{1-\alpha}} = \left(\frac{w_j}{w}\right)^{\nu} N \tag{20}$$

3. Bank loan market clears:

$$w_j\left(\prod_{j'=1}^{J} m_{jj'}^{\omega_{jj'}}\right)\left(\frac{(1+r_{bj})\,w_j\,(1+\gamma_{lj})}{p_j\alpha\,(1+\gamma_{lj}\lambda)}\right)^{\frac{1}{1-\alpha}} + \sum_{j'\in\mathcal{C}} p_{j'}m_{jj'} = \left(\frac{1+r_{bj}}{1+r_b}\right)^{\varphi}\zeta_j^{-1}L \tag{21}$$

4. And net transfers from firms equal total firm profits:

$$\sum_{j=1}^{J} t_j = T \tag{22}$$

7.6 Relation to empirical work

Using (17) and (15), the equilibrium wage paid by firm j is

$$w_j = w\left(\frac{n_j}{N}\right)^{\frac{1}{\nu}} = w^{\frac{\nu-\epsilon}{\nu}} n_j^{\frac{1}{\nu}} (\phi P)^{\frac{\epsilon}{\nu}}.$$

Substituting into the goods market clearing condition (19), I thus have

$$\left(\prod_{j'=1}^{J} m_{jj'}^{\omega_{jj'}}\right)^{1-\alpha} n_j^{\alpha} = \sum_{j'=1}^{J} m_{j'j} + \left(\left(\prod_{j'=1}^{J} m_{jj'}^{\omega_{jj'}}\right)^{1-\alpha} n_j^{\alpha-1}\right)^{\sigma}\left((1+r_{bj})\,w^{\frac{\nu-\epsilon}{\nu}}P^{-\frac{\nu-\epsilon}{\nu}} n_j^{\frac{1}{\nu}}\,(1+\gamma_{lj})\,\phi^{\frac{\epsilon}{\nu}}\alpha^{-1}\right)^{-\sigma}\xi_j C.$$

Denote by $\hat{x} \equiv \frac{dx}{\bar{x}}$ the deviation from the steady state value $\bar{x}$ of x. Then the goods market clearing condition can be expressed as

$$\left(\alpha + \kappa\sigma\left(1 - \alpha + \nu^{-1}\right)\right)\hat{n}_j = \kappa\sigma\frac{\nu - \epsilon}{\nu}\left(\hat{P} - \hat{w}\right) - \kappa\sigma(\widehat{1 + r_{bj}}) - \kappa\sigma(\widehat{1 + \gamma_{lj}}) + \hat{C} + \hat{\xi}_j,$$

where

$$\kappa = \frac{\left(\left(\prod_{j'=1}^{J} m_{jj'}^{\omega_{jj'}}\right)^{1-\alpha}\bar{n}_j^{\alpha-1}\right)^{\sigma}\left(\left(1 + \bar{r}_{bj}\right)\bar{w}^{\frac{\nu-\epsilon}{\nu}}\bar{P}^{-\frac{\nu-\epsilon}{\nu}}\bar{n}_j^{\frac{1}{\nu}}\left(1 + \bar{\gamma}_{lj}\right)\phi^{\frac{\epsilon}{\nu}}\alpha^{-1}\right)^{-\sigma}\bar{\xi}_j\bar{C}}{\left(\left(\prod_{j'=1}^{J} m_{jj'}^{\omega_{jj'}}\right)^{1-\alpha}\bar{n}_j^{\alpha-1}\right)^{\sigma}\left(\left(1 + \bar{r}_{bj}\right)\bar{w}^{\frac{\nu-\epsilon}{\nu}}\bar{P}^{-\frac{\nu-\epsilon}{\nu}}\bar{n}_j^{\frac{1}{\nu}}\left(1 + \bar{\gamma}_{lj}\right)\phi^{\frac{\epsilon}{\nu}}\alpha^{-1}\right)^{-\sigma}\bar{\xi}_j\bar{C} + \sum_{j'=1}^{J} m_{j'j}}.$$

Similarly, from the loan market clearing condition, I have

$$w^{\frac{\nu-\epsilon}{\nu}} n_j^{1+\frac{1}{\nu}}\left(\phi P\right)^{\frac{\epsilon}{\nu}} + \sum_{j'=1}^{J}\left(1 - \theta_{jj'}\right)p_{j'}m_{jj'} = \left(\frac{1 + r_{bj}}{1 + r_b}\right)^{\varphi}\zeta_j^{-1}\frac{P\left(\beta - 1\right)}{\beta\left(1 + r_b\right)}\left(C - \phi\frac{\epsilon}{1 + \epsilon}\left(\frac{w}{\phi P}\right)^{1+\epsilon}\right).$$

The above can be rewritten in terms of deviations from the steady state as

$$\bar{w}^{\frac{\nu-\epsilon}{\nu}}\bar{n}_j^{\frac{\nu+1}{\nu}}\bar{P}^{\frac{\epsilon}{\nu}}\left(\frac{\nu - \epsilon}{\nu}\hat{w} + \frac{\nu + 1}{\nu}\hat{n}_j + \frac{\epsilon}{\nu}\hat{P}\right) + \sum_{j'=1}^{J} m_{jj'}\left(1 - \bar{\theta}_{jj'}\right)\bar{p}_{j'}\left((\widehat{1 - \theta_{jj'}}) + \hat{p}_{j'}\right) =$$

$$\left(\frac{1 + \bar{r}_{bj}}{1 + \bar{r}_b}\right)^{\varphi}\bar{\zeta}_j^{-1}\frac{\bar{P}\left(\beta - 1\right)}{\beta\left(1 + \bar{r}_b\right)}\left(\varphi(\widehat{1 + r_{bj}}) - (1 + \varphi)\,(\widehat{1 + r_b}) - \hat{\zeta}_j + \hat{P} + \bar{C}\hat{C} - \phi^{-\epsilon}\epsilon\left(\frac{\bar{w}}{\bar{P}}\right)^{1+\epsilon}\left(\hat{w} - \hat{P}\right)\right).$$

Collecting like terms, I can solve for $(\widehat{1 + r_{bj}})$ to obtain

$$(\widehat{1 + r_{bj}}) = \frac{(1 + \varphi)}{\varphi}(\widehat{1 + r_b}) + \frac{1}{\varphi}\hat{\zeta}_j + \underbrace{\frac{\left(\left(\kappa_{rj} + \sum_{j'=1}^{J}\kappa_{jj'}\right)\frac{\epsilon}{\nu} - \kappa_{lj}\left(1 + \phi^{-\epsilon}\epsilon\left(\frac{\bar{w}}{\bar{P}}\right)^{1+\epsilon}\right)\right)}{\kappa_{lj}\varphi}}_{=k_{jP}}\hat{P} - \frac{\bar{C}}{\varphi}\hat{C} \tag{23}$$

$$+ \underbrace{\frac{\left(\left(\kappa_{rj} + \sum_{j'=1}^{J}\kappa_{jj'}\right)\frac{\nu-\epsilon}{\nu} + \kappa_{lj}\phi^{-\epsilon}\epsilon\left(\frac{\bar{w}}{\bar{P}}\right)^{1+\epsilon}\right)}{\kappa_{lj}\varphi}}_{=k_{jw}}\hat{w} + \frac{\kappa_{rj}}{\kappa_{lj}\varphi}\frac{\nu + 1}{\nu}\hat{n}_j$$

$$+ \sum_{j'=1}^{J}\frac{\kappa_{jj'}}{\kappa_{lj}\varphi}\left((\widehat{1 - \theta_{jj'}}) + \left(1 - \alpha + \frac{1}{\nu}\right)\hat{n}_{j'} + (\widehat{1 + r_{bj'}}) + (\widehat{1 + \gamma_{lj'}})\right),$$

where

$$\kappa_{lj} = \left(\frac{1+\bar{r}_{bj}}{1+\bar{r}_b}\right)^{\varphi} \bar{\zeta}_j^{-1} \frac{\bar{P}\left(\beta-1\right)}{\beta\left(1+\bar{r}_b\right)}; \qquad \kappa_{rj} = \bar{w}^{\frac{\nu-\epsilon}{\nu}} \bar{n}_j^{\frac{\nu+1}{\nu}} \bar{P}^{\frac{\epsilon}{\nu}}; \qquad \kappa_{jj'} = m_{jj'}\left(1-\bar{\theta}_{jj'}\right)\bar{p}_{j'}.$$

Let $\mathcal{K}_R$ be the matrix with elements given by $\frac{\kappa_{jj'}}{\kappa_{lj}\varphi}$ and $\mathcal{K}_N$ be the matrix with the off-diagonal elements given by $\frac{\kappa_{jj'}}{\kappa_{lj}\varphi}\left(1-\alpha+\frac{1}{\nu}\right)$ and diagonal elements by $\frac{\kappa_{rj}}{\kappa_{lj}\varphi}\frac{\nu+1}{\nu}$. Then stacking (23) across firms, obtains

$$\left(I-\mathcal{K}_R\right)\overrightarrow{\left(1+r_{bj}\right)} = \frac{\left(1+\varphi\right)}{\varphi}\widehat{\left(1+r_b\right)} - \frac{\bar{C}}{\varphi}\hat{C} + \frac{1}{\varphi}\vec{\zeta} + \vec{k}_P\hat{P} + \vec{k}_w\hat{w} + \mathcal{K}_N\vec{n} + \mathcal{K}_R\vec{\theta} + \mathcal{K}_R\overrightarrow{\left(1+\gamma_{lj}\right)}.$$

Stacking similarly the goods market clearing condition, obtains

$$\left(\alpha+\kappa\sigma\left(1-\alpha+\nu^{-1}\right)\right)\vec{n} = \kappa\sigma\frac{\nu-\epsilon}{\nu}\left(\hat{P}-\hat{w}\right) - \kappa\sigma\overrightarrow{\left(1+r_{bj}\right)} - \kappa\sigma\overrightarrow{\left(1+\gamma_{lj}\right)} + \hat{C} + \vec{\xi}.$$

Thus,

$$\begin{aligned}
&\left(\alpha+\kappa\sigma\left(1-\alpha+\nu^{-1}\left(I-\mathcal{K}_R\right)^{-1}\mathcal{K}_N\right)\right)\vec{n} = \kappa\sigma\left(\frac{\nu-\epsilon}{\nu} - \left(I-\mathcal{K}_R\right)^{-1}\vec{k}_P\right)\hat{P} \\
&- \kappa\sigma\left(\frac{\nu-\epsilon}{\nu} + \left(I-\mathcal{K}_R\right)^{-1}\vec{k}_w\right)\hat{w} - \kappa\sigma\left(I+\left(I-\mathcal{K}_R\right)^{-1}\mathcal{K}_R\right)\overrightarrow{\left(1+\gamma_{lj}\right)} \\
&+ \left(I+\kappa\sigma\frac{\bar{C}}{\varphi}\left(I-\mathcal{K}_R\right)^{-1}\right)\hat{C} + \vec{\xi} - \kappa\sigma\left(I-\mathcal{K}_R\right)^{-1}\left(\frac{\left(1+\varphi\right)}{\varphi}\widehat{\left(1+r_b\right)} + \frac{1}{\varphi}\vec{\zeta} + \mathcal{K}_R\vec{\theta}\right).
\end{aligned}$$

In my empirical specification, I focus on the effect that banking shocks $\vec{\zeta}$ have on firm labor demand $\vec{n}$. From the above, the full effect – taking into account the full propagation through the network – is given by

$$\begin{aligned}
\Phi &\equiv -\frac{\kappa\sigma}{\varphi}\left(\alpha+\kappa\sigma\left(1-\alpha+\nu^{-1}\left(I-\mathcal{K}_R\right)^{-1}\mathcal{K}_N\right)\right)^{-1}\left(I-\mathcal{K}_R\right)^{-1} \\
&= -\frac{\kappa\sigma}{\varphi\left(\alpha+\kappa\sigma\left(1-\alpha\right)\right)}\left(I-\mathcal{K}_R+\frac{1}{\nu\left(\alpha+\kappa\sigma\left(1-\alpha\right)\right)}\mathcal{K}_N\right)^{-1}.
\end{aligned}$$

Applying a Taylor series expansion, the above can be represented as

$$\Phi = -\frac{\kappa\sigma}{\varphi\left(\alpha+\kappa\sigma\left(1-\alpha\right)\right)}\left(I+\left(\mathcal{K}_R-\frac{1}{\nu\left(\alpha+\kappa\sigma\left(1-\alpha\right)\right)}\mathcal{K}_N\right)\right.$$
$$\left.+\frac{1}{2}\left(\mathcal{K}_R-\frac{1}{\nu\left(\alpha+\kappa\sigma\left(1-\alpha\right)\right)}\mathcal{K}_N\right)^2+O_3\left(\mathcal{K}_R-\frac{1}{\nu\left(\alpha+\kappa\sigma\left(1-\alpha\right)\right)}\mathcal{K}_N\right)\right).$$

My estimate of the direct effect – the effect of own bank's shock on own labor demand – gives $\frac{\kappa\sigma}{\varphi(\alpha+\kappa\sigma(1-\alpha))}$. The estimates of the effect of the bank shock one and two nodes removed then gives the average $\left(\mathcal{K}_R-\frac{1}{\nu(\alpha+\kappa\sigma(1-\alpha))}\mathcal{K}_N\right)$ and $\left(\mathcal{K}_R-\frac{1}{\nu(\alpha+\kappa\sigma(1-\alpha))}\mathcal{K}_N\right)^2$. Taking the coefficients from column 3 of the empirical exercise (Table A15), I have

$$\begin{aligned}
\frac{\kappa\sigma}{\varphi\left(\alpha+\kappa\sigma\left(1-\alpha\right)\right)} &\approx 0.183 \\
\left(\mathcal{K}_R-\frac{1}{\nu\left(\alpha+\kappa\sigma\left(1-\alpha\right)\right)}\mathcal{K}_N\right) &\approx \frac{0.106}{0.183}\approx 0.579 \\
\left(\mathcal{K}_R-\frac{1}{\nu\left(\alpha+\kappa\sigma\left(1-\alpha\right)\right)}\mathcal{K}_N\right)^2 &\approx 2\frac{0.042}{0.183}\approx 0.4590.
\end{aligned}$$

Thus, the full network effect of the bank shocks on labor decisions is $\Phi \approx 0.183/(1-0.579) \approx 0.44$ if I take the estimate based on the first node spillover value and $\Phi \approx 0.183/(1-0.68) \approx 0.57$ if I take the estimate based on the second node spillover value. In other words, a one percent increase in the aggregate supply of deposits leads to a 0.44 percent increase in aggregate employment based on the first node spillover, and it leads to a 0.57 percent increase in aggregate employment based on the second node spillover.

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Table A1: Summary Statistics for Full Credit2B Sample

This table presents the summary statistics for the universe of observations covered by Credit2b. The years span 2001 through 2015. *Current Receivable* is the amount of credit that is extended to the buyer but not yet due. *Total Past Due* is the total amount of credit that is outstanding past its due date. The next five categories represent the amount of receivables past their due date, separated out into aging buckets. Where these numbers are missing, they are set to zero. *Days Slow* represent the number of days past the due date that the customer paid the bill, as reported by the Credit2B member firm. *nBuyers* and *nSuppliers* represent the number of unique buyers for a supplier and the number of unique suppliers for a buyer, respectively.

	N	*Mean*	*Std. Dev.*
Transactions	94,994,143		
Unique Suppliers	48,121		
Unique Buyers	7,254,003		
Current Receivable	94,994,143	21,032	19,638
Total Past Due	94,994,143	7,934	14,142
Past 1 30	94,994,143	1,559	4,392
Past 31 60	94,994,143	844	1,273
Past 61 90	94,994,143	681	868
Past 91 120	94,994,143	516	1,273
Past 121 150	94,994,143	0	759
Days Slow	69,100,337	14.18	26.95
nBuyers	48,121	12,883	24,292
nSuppliers	7,254,003	7.90	3.64

Table A2: Covariate Balance of Buyers in the Pre-Period

This table reports the descriptive statistics for both the exposed and unexposed buyers in the pre-period. Variables are defined in the main text. The *Difference* column calculates the difference in means for the treatment and the control group. *** , **, and * represent significant differences in the treatment and control groups at the 1%, 5%, and 10% level, respectively

		Exposed	*Unexposed*	*Difference*
Downstream Customer Characteristics				
	Total Assets	7.238	7.203	-0.036
	Number Suppliers	2.269	2.180	-0.089
	Age	22	21	1
	Cash/Total Assets	0.119	0.115	-0.004

Table A3: Industry Breakdown for Sample Firms versus Compustat
This table reports the industry distribution for the suppliers in the sample versus the industry distribution for firms in Compustat. Industries are defined at the two digit SIC level.

Industry	*SIC*	*Sample N*	*Sample %*	*Compustat N*	*Compustat %*
Agriculture	1-7	3	0.007	33	0.003
Forestry, Fishing, Mining	8-10	28	0.064	620	0.057
Coal Mining	12	2	0.005	37	0.003
Oil and Gas Extraction	13	26	0.059	595	0.055
Nonmetallic Minerals	14	3	0.007	45	0.004
Building Contractors	15	3	0.007	30	0.003
Heavy Construction	16	8	0.017	32	0.003
Special Trade	17	7	0.016	19	0.002
Food and Kindred Products	20	10	0.023	189	0.017
Tobacco Products	21	6	0.014	11	0.001
Textile Mill Products	22	4	0.009	23	0.002
Apparel	23	29	0.066	62	0.006
Lumber and Wood	24	13	0.030	44	0.004
Furniture and Fixtures	25	2	0.005	35	0.003
Paper and Allied Products	26	16	0.037	80	0.007
Printing and Publishing	27	5	0.011	91	0.008
Chemical and Allied Products	28	42	0.096	917	0.084
Petroleum and Coal	29	3	0.007	68	0.006
Rubber and Misc. Plastics	30	10	0.023	73	0.007
Leather Products	31	14	0.032	19	0.002
Stone, Clay, and Glass	32	7	0.016	43	0.004
Primary Metals	33	12	0.027	117	0.011
Fabricated Metals	34	11	0.025	84	0.008
Industrial Machinery	35	7	0.016	392	0.036
Electronic Equipment	36	13	0.030	674	0.062
Transportation Equipment	37	4	0.009	173	0.016

Instruments	38	9	0.021	497	0.046
Miscellaneous Mfg.	39	4	0.009	62	0.006
Transportation and Utilities	40-49	26	0.059	1,070	0.098
Wholesale Durable	50	57	0.130	162	0.015
Wholesale Nondurable	51	24	0.055	106	0.010
Building Materials	52	9	0.021	9	0.001
General Merchandise Stores	53	1	0.002	35	0.003
Food Stores	54	0	0.000	42	0.004
Automotive Dealers	55	0	0.000	34	0.003
Apparel Stores	56	3	0.007	62	0.006
Furniture and Home	57	0	0.000	26	0.002
Eating and Drinking	58	0	0.000	109	0.010
Miscellaneous Retail	59	2	0.005	147	0.014
Financial	60-67	0	0.000	2,078	0.190
Services	70-89	14	0.032	1,664	0.152
NonClassifiable	99	1	0.002	249	0.023

Table A4: Summary Statistics: Subsample versus Large Sample
This table presents summary statistics for the subsample of transactions where I obtained total sales and credit sales on each transaction. *Total Assets, Long-Term Debt, Cash, Sales, and Age* are all measured using the Compustat quarterly file. *Current Receivable, Past Due Receivable, Days Slow, nBuyers, nSuppliers, and Relationship Age* are all measured using the Credit2B dataset. In the last column *** , **, and * represent significant differences in the means of the two samples at the 1%, 5%, and 10% level, respectively.

	Full Sample	*Sub Sample*	*Difference*
Total Assets ($M)	12,948.18	15,912.04	1-2,963.86***
Long-Term Debt ($M)	2,504.99	3,049.65	544.66**
Cash ($M)	934.21	1,085.18	-150.97
Sales ($M)	3,763.94	3,664.70	99.24
Age	16.75	18.35	-1.6
Current Receivable (Per Transaction)	56,474.89	55,177.22	1,297.67
Past Due Receivable	10,501.19	11,579.97	-1,078.78
Days Slow	10.75	10.07	0.68
nBuyers	7,236	6,884	352
nSuppliers	8.24	7.99	-0.75
Relationship Age	11.67	12.27	-0.60

Table A5: Placebo Test

This table estimates equation 1, where the analysis is moved backward by one year. This places the treatment date at July 2006. The dependent variable in columns 1-4 is the log of the balance of current receivables, the dependent variable in columns 5-7 is the sum of three month's current receivables for a seller-buyer pair scaled by Compustat quarterly sales revenue, and the dependent variable in column 8 is the sum of three month's current receivables, aggregated at the seller-level, scaled by Compustat quarterly sales revenue. The variable *Treated* is equal to one if at least 20 percent of the supplier's bank debt has a scheduled maturity date between July 2006 and August 2007. *After* is set equal to one for all months between the same period. The pre-trend period picks up the observations in the 5 months preceding treatment. Control variables include: (1) size, defined as the log of quarterly total assets; (2) cash, defined as cash and cash equivalents scaled by assets; (3) age, defined as the current date less the first date the supplier appeared in Compustat; and (3) nBuyer, defined as average number of unique buyers for each supplier. Control variables are taken as averages in the pre-period and are interacted with *After*. Standard errors are clustered at the supplier level and are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
After*Treated	-0.102	-0.113	0.180	0.197	0.006	0.008	0.006	0.214
	(0.165)	(0.100)	(0.218)	(0.221)	(0.005)	(0.009)	(0.005)	(0.336)
Pre*Treated	0.061	0.112	0.115	0.093	0.000	0.003	0.013	-0.292
	(0.055)	(0.122)	(0.120)	(0.292)	(0.000)	(0.004)	(0.014)	(0.218)
After*Size		-0.0170	0.055	0.078	0.000	-0.001	0.001	0.032
		(0.093)	(0.066)	(0.143)	(0.000)	(0.001)	(0.001)	(0.040)
After*Cash		-3.602**	-1.155	0.293	0.011	0.021	-0.045	-0.379
		(1.336)	(0.805)	(0.732)	(0.017)	(0.043)	(0.040)	(0.977)
After*Age		0.278	-0.181	-0.244	-0.003	0.004	0.007	-0.491**
		(0.246)	(0.200)	(0.202)	(0.004)	(0.007)	(0.005)	(0.199)
After*nBuyer		-0.044	-0.021	0.022	-0.001**	-0.002*	-0.005	-0.045
		(0.033)	(0.023)	(0.048)	(0.000)	(0.001)	(0.002)	(0.145)
Observations	914,303	914,303	914,303	301,879	193,777	193,777	63,109	2,890
Adjusted R^2	0.785	0.785	0.791	0.884	0.835	0.926	0.876	0.740
LTD Rating Dummies	Y	Y	Y	Y	Y	Y	Y	Y
Buyer FE	Y	Y	Y	Y	Y	Y	Y	N
Seller FE	Y	Y	Y	Y	Y	Y	Y	Y
SellerXBuyer FE	N	N	Y	Y	N	Y	Y	N
Month FE	Y	Y	Y	Y	Y	Y	Y	Y
BuyerXMonth FE	N	N	N	Y	N	N	Y	N

Table A6: The Effect of Lender Credit Supply on Trade Credit: Alternative Treatment
This table uses an alternative identification strategy that relies on changes in the bank syndicates' lending patters to identify the change in credit supply. I follow the strategy by Chodorow-Reich (2014) to measure the effect of credit market disruptions on trade credit. The sample period is March 2008 through March 2009, thus the sample of suppliers from Credit2B is obtained for this period and matched to DealScan. The dependent variable in all specifications is the change in the average current receivables from March 2008 through March 2009. Observations are at the supplier level. Column 1 is estimated using OLS, and the variable of interest is the average change in lending during the crisis period for the borrower's pre-crisis syndicate. In the remaining columns, the change in loans are instrumented using *Lehman*, *ABX*, *Balance Sheet*, and *All instruments*. The variable Lehman co-syndication exposure equals the fraction of the bank's syndication portfolio where Lehman Brother's had a lead role in the loan deal. The variable ABX exposure equals the loading of the bank's stock return on the ABX AAA 2006-H1 index between October 2007 and December 2007. The balance sheet and income statement items include the ratio of deposits to assets at the end of 2007, the ratio of trading revenue over assets, and the ratio of net real estate charge-offs over assets. For each firm, the bank-level measures are averaged over the members of the firm's last pre-crisis syndicate, with weights assigned according the the share of the loan retained. Additional Dealscan controls include multiple lead lenders indicator, loan due during the crisis indicator, credit line indicator, collateral indicator, number of covenants, and all in drawn spread. Standard errors are clustered at the lead lender level and are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

	OLS	Lehman	ABX	Balance Sheet	All
% Change in Loans	0.311**	0.323**	0.317**	0.236**	0.304***
	(0.102)	(0.095)	(0.134)	(0.075)	(0.061)
Size Bin	Y	Y	Y	Y	Y
Age Bin	Y	Y	Y	Y	Y
Dealscan Controls	Y	Y	Y	Y	Y
First stage F-statistic		15.76	12.91	12.35	17.32
Observations	781	781	781	781	781
Adjusted R^2	0.199	0.201	0.201	0.188	0.202

Table A7: The Effect of Lender Credit Supply on Trade Credit: Estimated using a continuous treatment variable

This table reports the results from estimating equation 1 from the manuscript, where the dependent variable in columns 1-4 is the log of the balance of current receivables, the dependent variable in columns 5-7 is the sum of three month's current receivables for a seller-buyer pair scaled by Compustat quarterly sales revenue, and the dependent variable in column 8 is the sum of three month's current receivables, aggregated at the seller-level, scaled by Compustat quarterly sales revenue. The variable *Treated_ Continuous* is calculated as the percentage of the supplier's bank debt that has a scheduled maturity date between July 2007 and August 2008. *After* is set equal to one for all months between the same period. The pre-trend period picks up the observations in the 5 months preceding treatment. Control variables include: (1) size, defined as the log of quarterly total assets; (2) cash, defined as cash and cash equivalents scaled by assets; (3) age, defined as the current date less the first date the supplier appeared in Compustat; and (3) nBuyer, defined as average number of unique buyers for each supplier. Control variables are taken as averages in the pre-period and are interacted with *After*. Standard errors are clustered at the supplier level and are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
After*Treated_Continuous	-0.295**	-0.321**	-0.305**	-0.227**	-0.002*	-0.001*	-0.000	-0.176
	(0.106)	(0.105)	(0.105)	(0.115)	(0.001)	(0.000)	(0.000)	(0.174)
Pre-Trend*Treated_Continuous	0.005	0.011	0.023	0.014	0.000	0.000	-0.000	-0.066
	(0.101)	(0.101)	(0.102)	(0.017)	(0.000)	(0.000)	(0.000)	(0.156)
Observations	988,290	988,290	988,290	411,403	207,720	207,720	77,128	3,618
Adjusted R^2	0.842	0.842	0.819	0.985	0.739	0.925	0.955	0.937
Controls*After	Y	Y	Y	Y	Y	Y	Y	Y
LTD Rating Dummies	Y	Y	Y	Y	Y	Y	Y	Y
Buyer FE	Y	Y	Y	Y	Y	Y	Y	N
Seller FE	Y	Y	Y	Y	Y	Y	Y	Y
SellerXBuyer FE	N	N	Y	Y	N	Y	Y	N
Month FE	Y	Y	Y	Y	Y	Y	Y	Y
BuyerXMonth FE	N	N	N	Y	N	N	Y	N

Table A8: Liquidity Spillover Effect on Buyer Employment: Estimated using a continuous exposure-to-treatment variable

This table reports the results from estimating equation 3 from the manuscript, where the dependent variable is the log of *employment*, measured from Dun & Bradstreet. Column 1 reports results for the full sample, and in columns 2 and 3 I split the buyers into above and below median size, respectively. Size is calculated as the average number of employees in the pre-period. *Exposed_ Continuous* is the buyer's weighted-average exposure to their suppliers' liquidity shocks, where weights are calculated as the percentage of the buyer's total pre-period purchases (accounts payable) from each supplier in the pre-period, and exposure is calculated as the percentage of the supplier's debt maturing between July 2007 and August 2008. *Treated_ Continuous* captures the direct treatment effect on a customer and is calculated as the percentage of the customer's *own* bank debt that has a scheduled maturity date between July 2007 and August 2008. I include buyer-level controls interacted with the *After* indicator, as previously defined. Standard errors are clustered at the buyer level and are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

	(1) Whole Sample	(2) Size>Median	(3) Size<Median
After*Exposed_Continuous	-0.095	-0.104	-0.121*
	(0.140)	(0.242)	(0.067)
After*Treated_Continuous	-0.048	0.057	-0.192**
	(0.069)	(0.106)	(0.080)
Observations	1,490	731	697
Adjusted R^2	0.787	0.709	0.782
After*Controls	Y	Y	Y
Access to Credit Line	Y	Y	Y
Buyer FE	Y	Y	Y
Year FE	Y	Y	Y
IndustryXYear FE	Y	Y	Y

Table A9: Liquidity Spillover Effects in the Cross-Section: Testing the Risk-Sharing Channel
This table reports the results from estimating Equation 1 from the Manuscript on the subsample of transactions where suppliers opted to provide sales data. The dependent variable in columns 1 and 2 is receivables scaled by sales, and the dependent variable in columns 3 and 4 is the log of total sales. Regressions are run at the transaction level. *FewSuppliers* is equal to one if the customer has below the median number of suppliers in the pre-period, zero otherwise. *OldSuppliers* is equal to one if the number of years that the customer has been doing business with the supplier is above the median, zero otherwise. Control variables are interacted with the *After* indicator, and are defined in the text. Standard errors are clustered at the supplier level and are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

	(1)	(2)	(3)	(4)
After*Treated	-0.192***	-0.181***	-0.125	-0.138
	(0.047)	(0.019)	(0.161)	(0.095)
After*Treated*FewSuppliers	-0.044		-0.038	
	(0.036)		(0.042)	
After*Treated*OldSuppliers		-0.018		-0.013
		(0.038)		(0.032)
Observations	139,058	139,058	139,058	139,058
Adjusted R^2	0.688	0.614	0.794	0.769
Pre* Treated	Y	Y	Y	Y
After*Controls	Y	Y	Y	Y
LTD Rating Dummies	Y	Y	Y	Y
Buyer FE	Y	Y	Y	Y
Seller FE	Y	Y	Y	Y
SellerXBuyer FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
BuyerXMonth FE	Y	Y	Y	Y

Table A10: Liquidity Adjustment of the Downstream Buyer

This table reports the results from estimating equation 3 from the Manuscript, where the dependent variable captures alternative sources of liquidity. *Cash* is quarterly cash and cash equivalents scaled by assets. *New Bank* is the log volume of new loans entered into on a quarterly basis from Dealscan. *Drawn Bank* is the percentage of the total line of credit that is drawn on a yearly basis, obtained for a sub-sample of buyers that are reported in Capital IQ's Credit Line Data. *New Trade* is the log volume of new current receivables obtained from suppliers. *Days Slow* is the number of days after the due date that the customer pays his bill. Both *New Trade* and *Days Slow* are obtained from Credit2B and are restricted to the transactions with *non-treated* suppliers. *Exposed* is set equal to one if the customer is linked to at least one supplier in the treatment group, zero otherwise. *Treated* is set equal to one if the customer is directly exposed to the liquidity shortage (at least 20 percent of the customer's bank debt has a scheduled maturity date between July 2007 and August 2008). All other variables are as previously defined. Standard errors are clustered at the buyer level and are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

	(1) Cash	(2) New Bank	(3) Drawn Bank	(4) New Trade	(5) Days Slow
After*Exposed	-0.023**	0.022	0.037*	-0 .129	3.721**
	(0.009)	(0.098)	(0.022)	(0.184)	(1.478)
After*Treated	-0.097**	0.042	0.019	-0.145	4.973***
	(0.035)	(0.122)	(0.035)	(0.182)	(1.191)
Observations	3,366	1,870	508	10,886	10,886
Adjusted R^2	0.618	0.669	0.639	0.711	0.736
After*Controls	Y	Y	Y	Y	Y
Buyer FE	Y	Y	Y	Y	Y
Seller FE	N	N	N	Y	Y
SellerXBuyer FE	N	N	N	Y	Y
Time FE	Y	Y	Y	Y	Y
IndustryXTime FE	Y	Y	Y	Y	Y

Table A11: Financial Adjustment and Credit Quality: Timing Effects
This table reports the results from estimating equation 3. *Cash* is quarterly cash and cash equivalents scaled by assets. *Days Late* is the number of days after the due date that the customer pays his bill. *Credit Score* is the buyer's risk score assigned by Credit2B. It ranges from 1 to 100, where higher scores indicate better credit quality. The variable *Exposed* is an indicator variable set equal to one if the buyer was linked to a treated supplier during the treatment period. *PostQ1* equals one if the transaction occurred between July and September, 2007, *PostQ2* equals one if the transaction occurred between October and December 2007, *PostQ3* equals one if the transaction occurred between January and March 2008, and *PostQ4* equals one if the transaction occurred between April and July 2008. All other variables are as previously defined. Standard errors are clustered at the buyer level and are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

	(1) Cash	(2) Days Late	(3) Credit Score
PostQ1*Exposed	-0.028**	5.375***	-2.543**
	(0.012)	(1.036)	(0.938)
PostQ2*Exposed	-0.017	2.469**	-3.262**
	(0.013)	(1.113)	(1.342)
PostQ3*Exposed	-0.021**	2.125	-2.623**
	(0.007)	(1.327)	(0.890)
PostQ4*Exposed	-0.022*	3.724***	-4.865**
	(0.013)	(1.992)	(1.393)
Observations	3,113	9,004	9, 004
Adjusted R^2	0.619	0.738	0.620
After*Controls	Y	Y	Y
Buyer FE	Y	Y	Y
Seller FE	N	Y	N
SellerXBuyer FE	N	Y	N
Time FE	Y	Y	Y
IndustryXTime FE	Y	Y	Y

Table A12: Liquidity Spillover Effect on Buyer Credit Quality - Controlling for buyer purchases This table reports the results from estimating equation 3 from the Manuscript, where the dependent variable captures the buyer's credit quality. *Credit Score* is the buyer's risk score assigned by Credit2B. It ranges from 1 to 100, where higher scores indicate better credit quality. *Collections* is an indicator variable equal to one if one of the buyer's invoice was transferred to a collections agency. *Covenant Viol.* equals one if the customer had a technical default on one of his debt contracts. *Exposed* is set equal to one if the customer is linked to at least one supplier in the treatment group, zero otherwise. *Treated* is set equal to one if the customer is directly exposed to the liquidity shortage (at least 20 percent of the customer's bank debt has a scheduled maturity date between July 2007 and August 2008). I include a time-varying control for the buyer's inventory purchases. *Purchases* are calculated as: $Purchases_t = \Delta Inventory + COGS_t$. Similarly, for firms using raw materials and work in progress inventory, I calculate purchases as: $\Delta RawMaterials + \Delta WorkInProgress + \Delta FinishedGoods +$ COGS). Control variables are interacted with the *After* indicator, and are defined in the text. Standard errors are clustered at the buyer level and are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

	(1)	(2)	(3)
	Credit Score	Collections	Covenant Viol.
After*Exposed	-3.557**	0.005**	0.017**
	(1.103)	(0.002)	(0.006)
After*Treated	-5.335***	0.007***	0.014*
	(1.103)	(0.002)	(0.006)
Observations	10,886	3,465	3,465
Adjusted R^2	0.618	0.104	0.173
Purchases	Y	Y	Y
After*Controls	Y	Y	Y
Access to Credit Line	Y	Y	Y
Buyer FE	Y	Y	Y
Time FE	Y	Y	Y
IndustryXTime FE	Y	Y	Y

Table A13: Liquidity Spillover Effect on Buyer Employment - Controlling for buyer purchases This table reports the results from estimating equation 3 from the Manuscript, where the dependent variable is the log of *employment*, measured from Dun & Bradstreet. Column 1 reports results for the full sample, and in columns 2 and 3 I split the buyers into above and below median size, respectively. Size is calculated as the average number of employees in the pre-period. *Exposed* is set equal to one if the customer is linked to at least one supplier in the treatment group, zero otherwise. *Treated* is set equal to one if the customer is directly exposed to the liquidity shortage (at least 20 percent of the customer's bank debt has a scheduled maturity date between July 2007 and August 2008). I include a time-varying control for the buyer's inventory purchases. *Purchases* are calculated as: $Purchases_t = \Delta Inventory + COGS_t$. Similarly, for firms using raw materials and work in progress inventory, I calculate purchases as: $\Delta RawMaterials + \Delta WorkInProgress + \Delta FinishedGoods +$ COGS). Control variables are interacted with the *After* indicator, and are defined in the text. Standard errors are clustered at the buyer level and are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

	(1) Whole Sample	(2) Size>Median	(3) Size<Median
After*Exposed	-0.137	-0.093	-0.098**
	(0.086)	(0.160)	(0.046)
After*Treated	0.006	0.151	-0.176**
	(0.062)	(0.082)	(0.069)
Observations	1,490	731	697
Adjusted R^2	0.787	0.709	0.782
Purchases	Y	Y	Y
After*Controls	Y	Y	Y
Access to Credit Line	Y	Y	Y
Buyer FE	Y	Y	Y
Year FE	Y	Y	Y
IndustryXYear FE	Y	Y	Y

Table A14: Liquidity Spillover Effect on Buyer Employment - Comparing small, public (less constrained) buyers to small, private (more constrained) buyers

This table reports the results from estimating equation 3 from the Manuscript, where the dependent variable is the log of *employment*, measured from Dun & Bradstreet. Column 1 reports results for the sample of public firms with below median size, and column 2 reports the results for the sample of private firms with below median size. Size is calculated as the average number of employees in the pre-period. *Exposed* is set equal to one if the customer is linked to at least one supplier in the treatment group, zero otherwise. Control variables and the *Treated* variable are not included, since I can't obtain financial data for private firms, and most don't have access to syndicated debt markets. All other variables are as previously defined. Standard errors are clustered at the buyer level and are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

	(1)	(2)
	Size<Median	
	Public	Private
After*Exposed	-0.105**	-0.142***
	(0.047)	(0.039)
Observations	697	3,710
Adjusted R^2	0.779	0.757
After*Controls	N	N
Access to Credit Line	N	N
Buyer FE	Y	Y
Year FE	Y	Y
IndustryXYear FE	Y	Y

Table A15: Liquidity Spillover Effect on Buyer Employment- Second Node

This table reports the results from estimating equation 3 from the Manuscript, where the dependent variable is the log of *employment*, measured from Dun & Bradstreet. Column 1 reports results for the full sample, and in columns 2 and 3 I split the buyers into above and below median size, respectively. Size is calculated as the average number of employees in the pre-period. *Exposed* is set equal to one if the customer is linked to at least one supplier in the treatment group, zero otherwise. *Exposed2* is set equal to one if the second node customer is linked to at least one supplier in the treatment group, zero otherwise. *Treated* is set equal to one if the customer is directly exposed to the liquidity shortage (at least 20 percent of the customer's bank debt has a scheduled maturity date between July 2007 and August 2008). All other variables are as previously defined. Standard errors are clustered at the buyer level and are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

	(1) Whole Sample	(2) Size>Median	(3) Size<Median
After*Exposed	-0.166	-0.227	-0.106***
	(0.093)	(0.176)	(0.025)
After*Exposed2	-0.031	-0.019	-0.042**
	(0.096)	(0.150)	(0.019)
After*Treated	-0.035	0.083	-0.183**
	(0.056)	(0.085)	(0.080)
Observations	1,490	731	697
Adjusted R^2	0.835	0.728	0.825
After*Controls	Y	Y	Y
Access to Credit Line	Y	Y	Y
Buyer FE	Y	Y	Y
Year FE	Y	Y	Y
IndustryXYear FE	Y	Y	Y