ISSN 1518-3548

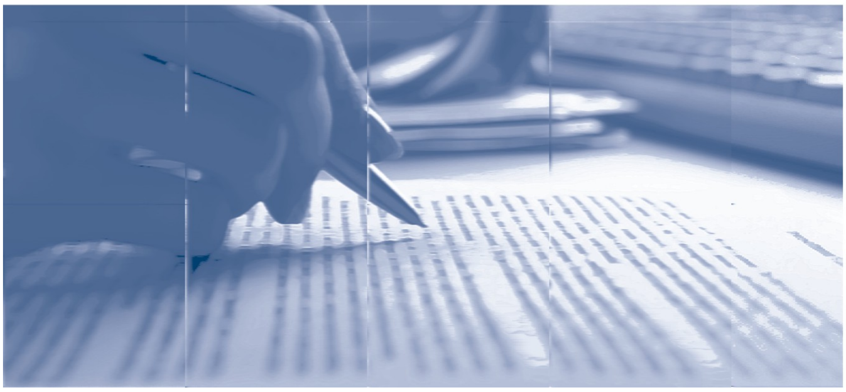
BANCO CENTRAL DO BRASIL

Credit Shock Propagation in Firm Networks: evidence from government bank credit expansions

Gustavo S. Cortes, Thiago Christiano Silva and Bernardus F. N. Van Doornik

September 2019

Working Papers



507

ISSN 1518-3548
CGC 00.038.166/0001-05

Working Paper Series	Brasília	no. 507	September	2019	p. 1-55

Working Paper Series

Edited by the Research Department (Depep) – E-mail: workingpaper@bcb.gov.br

The Banco Central do Brasil Working Papers are evaluated in double blind referee process.

Although the Working Papers often represent preliminary work, citation of source is required when used or reproduced.

The views expressed in this Working Paper are those of the authors and do not necessarily reflect those of the Banco Central do Brasil.

As opiniões expressas neste trabalho são exclusivamente do(s) autor(es) e não refletem, necessariamente, a visão do Banco Central do Brasil.

Citizen Service Division

Banco Central do Brasil

Deati/Diate

SBS – Quadra 3 – Bloco B – Edifício-Sede – 2º subsolo

70074-900 Brasília – DF – Brazil

Toll Free: 0800 9792345

Fax: +55 (61) 3414-2553

Internet: http//www.bcb.gov.br/?CONTACTUS

Non-Technical Summary

We study how bank credit shocks propagate through supplier-customer firm networks. We do so by using administrative data that covers firm-to-firm transactions in Brazil around the debacle of Lehman Brothers. More specifically, we analyze the bankruptcy of Lehman Brothers in September 2008 as a large-scale, exogenous credit shock to the Brazilian economy and exploit two distinctive features of Brazil's response to the crisis. First, the intensity of credit rationing for firms after Lehman was significantly driven by bank ownership. Private banks dramatically cut lending, while government-owned banks sustained their pre-crisis trend of credit in a counter-cyclical way. Second, the historical importance of government bank ownership in emerging economies such as Brazil entails that shocks like these matter for the aggregate economy. The widening gap between government- and private-bank lending was crucial to forestall the deep dive in Brazilian aggregate credit after September 2008.

Using the counter-cyclical reaction of government-owned banks in Brazil after Lehman's failure as a policy experiment, we show that credit shocks originated in bank-firm relationships are transmitted throughout the network of suppliers and customers, with measurable consequences for firms' real outcomes and survival probability. We find that a firm with direct and indirect access to government credit (through its customers or suppliers) observed a 12.5% greater survival probability, *vis-à-vis* 4% when the firm has only direct access.

Because the reaction of several emerging market economies included liquidity expansions through government-owned banks, this paper also serves as a warning for the trade-offs involved in such interventions. Relaxing credit constraints in times of distress through government-owned banks can help firms to keep production schedules, payments to suppliers, employment, and wage bills, as shown by the "government credit multiplier" in our empirical analysis. But there are also drawbacks of these interventions, such as a persistent concentration of market share of firms that benefited from government liquidity comparatively to firms that did not enjoy such a benefit. It is important to keep in mind the costs and benefits of large-scale interventions in the banking sector when approaching future episodes of financial crises.

Sumário Não Técnico

O trabalho investiga como choques de crédito bancário se propagam por redes de firmas fornecedoras e consumidoras. Para tanto, utilizam-se dados que cobrem as transações firma-firma no Brasil durante o período da quebra do Lehman Brothers. Mais especificamente, analisa-se a falência do Lehman Brothers em setembro de 2008 como um choque exógeno e de grandes proporções para a economia brasileira e são exploradas duas características distintivas da resposta do Brasil à crise. Primeiramente, a intensidade de racionamento de crédito para firmas foi significativamente influenciada pelo tipo de controle dos bancos. Bancos privados cortaram crédito dramaticamente, enquanto bancos governamentais mantiveram sua tendência pré-crise nos empréstimos, atuando contraciclicamente. Em segundo lugar, a importância histórica do setor público no sistema bancário de economias emergentes, tais como o Brasil, sugere que choques como esses possuem implicações macroeconômicas. O crescente hiato entre o volume de empréstimos feitos por bancos públicos e privados foi crucial para impedir um decréscimo maior no crédito agregado brasileiro após setembro de 2008.

Usando a reação contracíclica dos bancos governamentais no Brasil após a falência do Lehman Brothers como um experimento de política econômica, mostra-se que choques de crédito originados nas relações firma-banco são transmitidas ao longo da rede de firmas fornecedoras e firmas consumidoras, com reflexos na probabilidade de sobrevivência e em variáveis econômicas das firmas. Encontra-se que uma firma com acesso tanto direto como indireto a crédito de bancos governamentais (por meio de seus consumidores ou de seus fornecedores) tem uma probabilidade 12,5% maior de sobreviver à crise *vis-à-vis* 4% quando a firma possui apenas acesso direto.

Como a reação de várias economias emergentes incluiu injeções de liquidez por meio de bancos públicos, este trabalho também serve como um alerta para os *trade-offs* envolvidos em tais intervenções. Amenizar restrições de crédito em tempos de dificuldade por meio de bancos públicos pode ajudar empresas a manter seus cronogramas de produção, pagamentos a fornecedores, emprego e contas salariais, como mostrado pelo "multiplicador de crédito público" na análise empírica feita neste trabalho. Mas também há desvantagens geradas nessas intervenções, como um aumento persistente de concentração da participação de mercado de firmas beneficiadas pela liquidez governamental comparativamente àquelas que não obtiveram tal benefício. É importante ter em mente os custos e benefícios de intervenções de grande escala no setor bancário ao abordar futuros episódios de crises financeiras.

Credit Shock Propagation in Firm Networks: evidence from government bank credit expansions

Gustavo S. Cortes*

Thiago Christiano Silva**

Bernardus F. N. Van Doornik***

Abstract

We study how bank credit shocks propagate through supplier–customer firm networks. We do so using administrative data that covers firm-to-firm transactions in Brazil around the debacle of Lehman Brothers. Using the counter-cyclical reaction of government-owned banks in Brazil after Lehman's failure as a policy experiment, we show that credit shocks originated in bank–firm relationships are transmitted throughout the network of suppliers and customers, with measurable consequences for firms' real outcomes and survival probability. A firm with direct and indirect access to government credit (through its customers or suppliers) observed a 12.5% greater survival probability, *vis-à-vis* 4% when the firm has only direct access. Critically, we uncover drawbacks of these interventions, including a persistent increased concentration in the market share of firms that benefited from government liquidity.

Keywords: Global Financial Crisis, government banks, firm networks

JEL Classification: D85, E44, G21, G28, L14

*Department of Finance, Insurance & Real Estate, Warrington College of Business, University of Florida, Gainesville, FL 32611-7168. Email: gustavo.cortes@warrington.ufl.edu. Site: https://sites.google.com/site/cortesgustavos.

**Research Department, Central Bank of Brazil. E-mail: thiago.silva@bcb.gov.br

***Research Department, Central Bank of Brazil. E-mail: bernardus.doornik@bcb.gov.br

1 Introduction

Credit crunches are known to shape the depth and duration of recessions (Campello et al. (2010); Jordà et al. (2013); Reinhart and Rogoff (2009)), and have been considered a major cause for countries' slow recovery from the 2008 Global Financial Crisis (GFC). However, our understanding of how credit shocks propagate throughout the economy is still limited. While several studies explored the real effects of liquidity shocks from banks to firms (e.g., Amiti and Weinstein (2011); Carvalho et al. (2015); Chodorow-Reich (2014)), little is known about how these shocks spill over to other firms. Given the complexity of firm production relationships in an economy, it is natural to expect that these shocks may be further transmitted across firms. Yet, perhaps due to data limitations, there are virtually no studies "tracking" these shocks all the way to firm–firm payment networks. This paper addresses this by quantifying the pervasive effects of credit shock propagation using regulatory data from Brazil.

We study the bankruptcy of Lehman Brothers in September 2008 as a large-scale, exogenous credit shock to the Brazilian economy. We exploit two distinctive features of Brazil's response to the crisis. First, the intensity of credit rationing for firms after Lehman was significantly driven by bank ownership. Panel A of Figure 1 shows that private banks dramatically cut lending, while government-owned banks sustained their pre-crisis trend of credit counter-cyclically. Second, the historical importance of government bank ownership in emerging economies like Brazil means that shocks like these matter for the aggregate economy.[1] Panel B of Figure 1 shows that government-bank lending forestalled a deep dive in Brazilian aggregate credit after September 2008. It is therefore plausible to expect that firms depending heavily on private banks before the crisis faced greater financial constraints than firms borrowing from government banks. This dichotomy in the credit shock transmission across banks and the unexpected event of Lehman's failure — as reflected by the "parallel trends" in Panel A of Figure 1 — allows us to adopt a Difference-in-Differences (DID) analysis to uncover the consequences.

We combine rich administrative data sets including: (1) the loan-level Credit Registry, covering the near-universe of bank lending; and (2) the Payments System, covering payment transactions from one firm to another through their bank accounts; and (3) an employer-employee matched data set covering the universe of Brazil's formal labor market. Credit registry data links each firm to its banks, and the payment system data links each firm to its network of customers and suppliers. Data on the real value of payments are used to construct reliable measures of firm-to-firm connectedness, allowing us to identify economic dependence relations at the firm-level. Using these supplier-customer links, we derive the impact of government credit access on the flow of payments between suppliers and customers. Specifically, we address to what extent a customer (supplier) increase its payment flow with suppliers (customers) that had pre-crisis access to government bank credit *vis-à-vis* those mostly dependent on private credit amid the GFC.

[1] For the post-1988 period, when more detailed data are available, the market share of government banks in total lending is never below 25%, reaching more than 50% in some periods (Cortes and Marcondes (2018)).

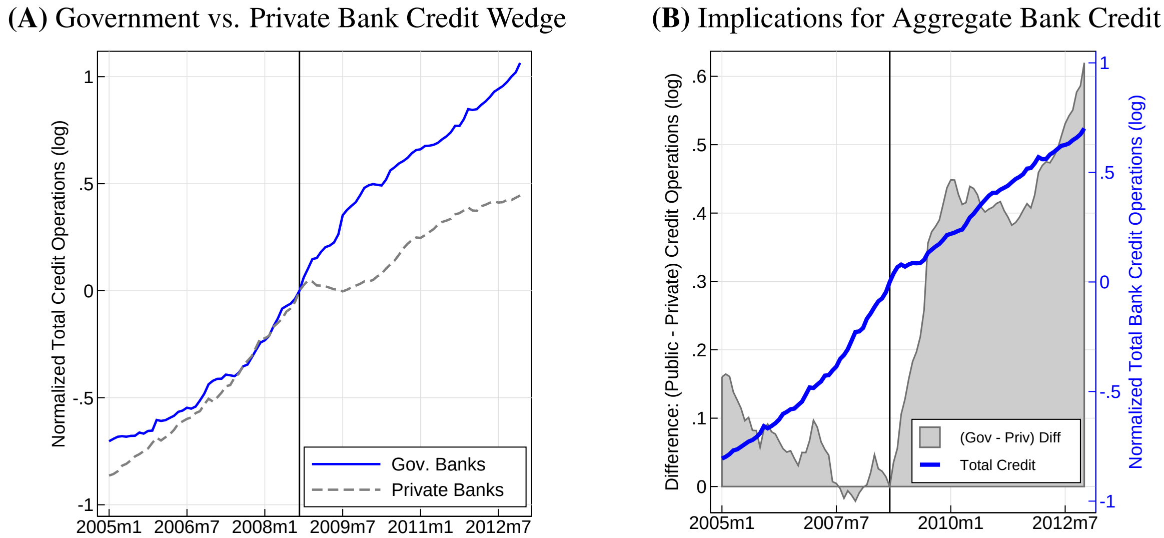


Figure 1: Private vs. Government-Owned Bank Credit in Brazil around Lehman's Bankruptcy. *This figure shows the total bank outstanding credit (in logs) from private and government-owned banks, normalized to zero in September 2008 (Lehman's bankruptcy, marked by a vertical line in both plots). In Panel A, the continuous line represents the time series of credit supplied by government-owned banks, and the dashed line represents the same for privately-owned banks. In September 2008, there were 121 private banks and 15 government-owned banks, of which two were federally-owned (Banco do Brasil, ranked #1 in total assets, and Caixa Econômica Federal, ranked #5). In Panel B, the thick continuous line is the aggregate bank credit in Brazil and the area shows the difference between government and private bank credit, i.e., the two lines depicted in Panel A.*

The use of detailed confidential data for the near-universe of Brazilian firms allows us to tackle concerns of sample selection that naturally arise due to endogenous sorting of firms and their banks. For both supplier and customer firms, we control for fundamentals like firm default risk (i.e., credit rating), size, age, and for a rigorous set of dynamic fixed effects interacting a firm's industry and municipality over time.[2] In robustness tests, we compare firms with access to government credit (*"treated"*) to similar, propensity-score-matched (*"control"*) firms. The large number of firms in Brazilian regulatory data allows us to restrict our algorithm to exactly match firms by credit rating, industry, and municipality. Moreover, we can match firms by pre-crisis total credit (ensuring parallel trends), age, and size. Such refined matching alleviates concerns that fundamental differences between government-credit-dependent firms and their private-dependent peers are behind our results.

Our empirical analysis is divided into three steps. We first uncover the direct effects of government credit. We document that firms borrowing from government banks enjoyed greater access to liquidity after the Lehman bankruptcy, *vis-à-vis* firms borrowing from private banks. While the importance of government banks in supporting credit and real activity in Brazil following Lehman's failure has been highlighted before (e.g., Coleman and Feler (2015); Noth and Ossandon-Busch (2017)), most studies rely exclusively on aggregate, municipality-level evidence.[3] Providing *loan-level* evidence of this mechanism is our first contribution. As in Khwaja and Mian (2008), our loan-level data allows us to completely isolate credit supply shocks from credit demand shocks by exploiting

[2]This set of controls and fixed effects has been shown to effectively capture firm-level ties with government banks. Carvalho (2014) shows that politically-connected firms in Brazil are clustered in "priority sectors," defined by government-elected industries. Also, combining the credit registry with public companies' balance sheet data, Bonomo et al. (2015) find that age and size are the top-ranked predictors of Brazilian firms' access to government bank credit.

[3]A notable exception is Bonomo et al. (2015). However, they focus on the determinants of access to earmarked credit and its effects on publicly-traded firms' investment, leverage, and financial expenditures.

multi-bank-relationship firms. We find that government banks extended up to 39.8% more credit to firms than private banks in the one-year time window surrounding Lehman's bankruptcy.[4] We then look at the real effects of this impressive wedge in bank credit. We find that firms depending more on government bank credit enjoyed significantly higher employment (5.1%) and wage bills (6.6%) than firms depending mostly on private bank credit. These numbers both statistically and economically significant. For the average Brazilian firm in the pre-crisis period, this means avoiding a cut of BRL 3,500 in total wages and almost two out of 24 total jobs.

Having established the large direct effects of government credit, we then study the indirect effects of bank credit shocks, focusing on firm-to-firm payments. We consider two sorts of indirect effects. First, we consider the perspective of a supplier that receives payments from customers that are government-credit-dependent or not. The logic is that a supplier indirectly benefits from the government credit shock to the extent that its connected customers are more capable of purchasing due to being less financially constrained than those customers tied to private credit. We find that payments are 2.5% higher for customers with access to government credit one year after the Lehman bankruptcy, rising to 3.6% and about 4% in the 2- and 3-year time windows surrounding the Lehman failure, respectively. Because, on average, a pre-crisis supplier has about four customers, out of which roughly half (51.1%) have access to government credit, these results suggest that the total indirect effect of credit shocks on a supplier can be as high as 5.1% in the 1-year window ($4 \times 51.1\% \times 2.5\%$) and 8.3% in the 3-year window ($4 \times 51.1\% \times 4.08\%$). We also test financial constraints as an amplification mechanism of the propagation of bank credit shocks. Following Almeida and Campello (2007), we interact our DID coefficient with an index capturing a firm's tangibility according to the proportion of tangible-to-total assets in its industry. We find that suppliers see their payments from customers with no access to government credit and limited tangible assets decrease 3.3% *vis-à-vis* other customers with no government credit but greater capacity to collateralize tangible assets.

We then investigate the indirect effects of credit shocks from the perspective of the customer. A customer may also benefit indirectly from having government-credit-dependent suppliers. Unconstrained suppliers can support clients through better payment terms and trade credit (Cingano et al. (2016); Cuñat (2006); Garcia-Appendini and Montoriol-Garriga (2013); Giannetti et al. (2011)). We find that customers purchase 3.5% more from suppliers with access to government credit one year after Lehman. The effect of government credit in the upstream dimension of the production network is also persistent, rising in 2-year (4.2%) and 3-year (5.4%) time windows after the Lehman failure. Given that, on average, a customer before the crisis has about five suppliers — out of which more than half (57.6%) have access to government bank credit — these results suggest that the total indirect effect of credit shocks on a customer can be as high as 10.1% in the 1-year window ($5 \times 57.6\% \times 3.5\%$) and 15.5% in the 3-year window ($5 \times 57.6\% \times 5.4\%$).

We then inspect whether the flow of payments from a customer depends on suppliers' characteristics. Customers may want to avoid riskier expansions during times of distress (Acharya et al.

[4]Our estimates are even stronger than the 28% government credit supply increase found by Coleman and Feler (2015) for the 12-month period between September 2008 and October 2009 (see their Table 1). This shows the importance of purging firms' credit demand factors in our loan-level setting.

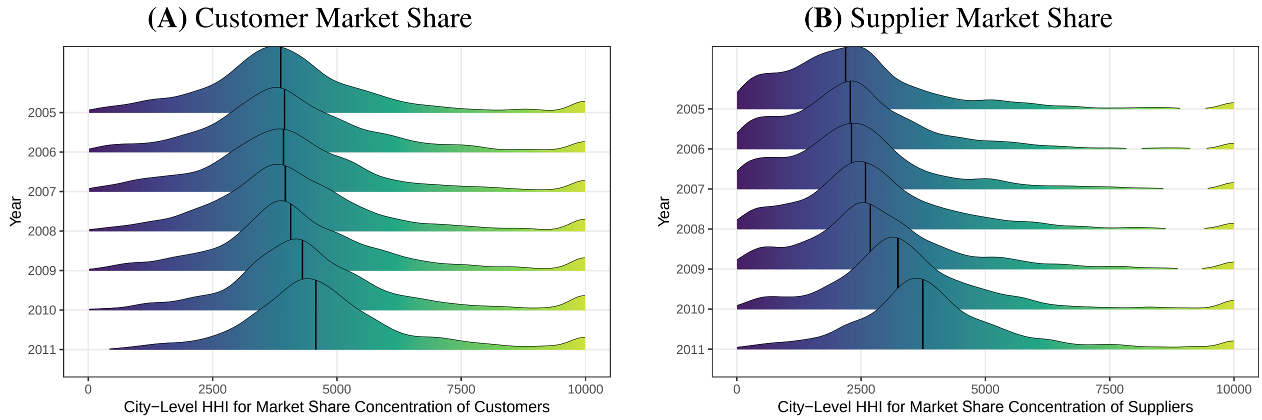


Figure 2: The Dynamics of Payment Share Concentration: Customer and Supplier Herfindahl-Hirschmann Indices (2005–2011). *Panel A shows the distribution of the average HHI of customers with their survivor suppliers. We calculate the average customer HHI in each of the 5,564 municipalities and plot their kernel density distribution in each year. The densities for each year are then juxtaposed to visualize the dynamics of market concentration. Panel B shows the distributions of the average HHI of suppliers with their survivor customers, similarly constructed.*

(2013)), such as innovative investment opportunities, or decline to integrate new products that require resources when cash is tight. We test if payment flows decline significantly more between a customer and suppliers exhibiting a high degree of product innovation *vis-à-vis* the flow of payments between a customer and its low-innovation suppliers. We interact our DID regressor with a sectoral index of product innovation that measures the proportion of firms' new products considered novelties in the domestic market, international market, or both. When a supplier has no access to government credit after Lehman (i.e., financial constraints are at play) and the input it produces is highly innovative, the flow of payments is almost 8% lower relative to the customer's purchases from non-innovative suppliers. This result suggests that firms are cautious with respect to innovative expansions in bad times.

We also study the indirect effect of credit shocks in the share of total payments that a supplier has with its customers, i.e., a proxy for a supplier's market share. We find a concentration effect in the market share of suppliers with access to government credit. Critically, treated suppliers increasingly gain a greater share of payments with their customers up to 3 years after the Lehman bankruptcy. We then analyze the indirect effect of credit shocks in our proxy for a customer's market share, the share of total payments that a customer has with its supplier. We again find some evidence of market share concentration, with the effects on customer concentration being short-lived, i.e., only lasting one year after the Lehman events. Our concentration results are robust to conditioning the sample to only firms that survived the crisis, avoiding the interpretation that increasing market concentration by treated firms is a mechanical result of firm survival. Figure 2 confirms these market share concentration results by plotting the distributions of the average Herfindahl-Hirschman Index (conditional on survivors) at the city-level. The distributions in Panel A for customer market share displays only a modest change after 2008. In contrast, Panel B reveals a clear trend towards more market concentration for suppliers after the Lehman events.

Finally, policy-makers are often interested in evaluating whether governmental stimulus can spill over to other firms and create a virtuous cycle in the economy. We conclude our analysis by examining the multiplier effect of government credit expansions. More specifically, we test whether access to government credit for the firm itself *and its peers* in the production network matter for its survival. We find that suppliers increased their survival probability by 0.008 percentage point if they had access to government credit before Lehman. More importantly, having all customers with government credit increments a supplier's survival probability by 0.017 percentage point. The total effect sums to 0.025. Given that the average mortality rate of suppliers in the post-crisis period was 0.20, this implies a relative reduction of 12.5% in the supplier's death probability. The death probability reduction for customers is similarly pronounced: 0.004 percentage point for direct access to government credit and an additional 0.010 percentage point for having all suppliers with access to government credit. The total effect is 0.014 percentage point, representing a relative decline of 11.6% *vis-à-vis* an average customer mortality rate of 12% in the post-crisis period.

This paper contributes to several literatures. First and foremost, we add to the broad literature on the transmission of financial and real shocks through networks of banks and firms. While theoretical work on the propagation of idiosyncratic shocks both in real and financial networks is rapidly growing (e.g., Acemoglu et al. (2012, 2015); Baqaee (2018); Bigio and La'O (2016), advances on the empirical front have been modest so far. The spirit of our work is related to recent contributions by Alfaro et al. (2018) and Dewachter et al. (2018), who also study credit shock propagation through supplier–customer networks in Spain and Belgium, respectively.[5] Our paper makes several novel advances relative to these two papers. First, in contrast to Alfaro et al. (2018), our measure of dependence among firms is constructed with granular, firm-to-firm payment data rather than merely exploiting sector-sector relationships in aggregate input-output matrices. Also, differently from Dewachter et al. (2018), we analyze a more diversified economy with less reliance on exporting firms. The real effects of bank credit shocks are extremely important to understand in emerging economies like Brazil. Because advanced economies usually display a more diverse menu of financing instruments, firms in less advanced economies are relatively more sensitive to bank credit shocks (Fisman and Love (2003); Rajan and Zingales (1998)). This suggests that bank-dependent economies like Brazil represent ideal testing ground for examining bank credit shocks and their real effects.

To the best of our knowledge, our paper is the first to fully track the origins of a bank credit shock and its propagation through firms' payment networks in an emerging economy. Another distinction of our paper is that we draw from an established literature on the political economy of government bank ownership (e.g., Carvalho (2014); La Porta et al. (2002)) to characterize bank credit shocks after an exogenously-born crisis. Government-bank ownership was shown to function as a policy tool during the GFC that governments used to smooth the credit cycle (see, e.g., Cull and Martinez-Peria (2013); De Haas et al. (2015)). Thus, our contribution is especially relevant for policy-makers in countries where government bank ownership plays a significant role in the economy. Government banks play large roles in major emerging economies (e.g., China, India, Russia) and several other countries (Coleman and Feler (2015)). Our analysis of customer and supplier market share concentration as a

[5] Cingano et al. (2016) also look at the effect of interbank lending disruptions on firms' trade credit in Italy. In the literature focused on real-side shocks, Barrot and Sauvagnat (2016) exploit natural disasters to examine input-specificity as an amplification mechanism of idiosyncratic supplier shocks within production networks.

potential distortion introduced by the government bank credit stimulus is also novel.

Finally, we also add to the literature on the cross-border transmission of financial crises through the banking system. From the pioneering work of Peek and Rosengren (1997), followed by Cetorelli and Goldberg (2012), and Schnabl (2012), scholars know that shocks originating in one country are quickly transmitted to other countries via their banking systems. The credit crunch led by private banks and its consequences for Brazil underscores the cross-border spillovers of the GFC to the largest economy of Latin America. We contribute by documenting and quantifying the propagation of a US-born credit crunch throughout the network of Brazilian firms.

The paper proceeds as follows. Section 2 briefly discusses the importance of government banks in Brazil and their key roles during the Great Recession. Section 3 describes the data. Section 4 outlines the empirical strategy to identify the transmission of shocks through bank–firm and firm–firm networks. Section 5 presents our baseline results. Section 6 verifies the robustness of our findings in alternative modeling choices. Section 7 concludes.

2 Government Banks and the Great Recession

2.1 Government Banks in Brazil

Government bank ownership plays much larger roles in emerging economies than in advanced economies. In Brazil, public banks' share of aggregate bank credit totalled up to 50% between 1988 and 2014. Even with the Latin American privatization wave in the late 1990s and early 2000s, the market share of public banks always exceeded one-third (Cortes and Marcondes (2018)). Government banks' spatial coverage is as comprehensive as their market share: about one-third of Brazil's nearly 20,000 bank branches in the period surrounding the Lehman failure belong to federal government banks (Coleman and Feler (2015)). This group includes *Banco do Brasil* (largest and oldest bank in Brazil), *Caixa Econômica Federal* (fifth largest), and federally-owned regional banks created in the post-war period to boost regional economic development.[6] Virtually all of these state-level banks were privatized by the government, being aquired by domestic and foreign banking conglomerates or by federal banks between 1997 and 2006 (Cortes and Marcondes (2018)). Despite the comprehensive banking sector consolidation, government banks continued to account for approximately 45% of total bank assets in Brazil (Barth et al. (2004)).

Brazil has a hybrid retail banking system, with state-controlled and private-sector banks competing directly.[7] State and federally-owned banks in Brazil historically functioned as substitutes. State banks existed in wealthier states, whereas federally-owned banks had greatest presence in underdeveloped states that lacked the resources to establish their own banks. After the privatization of state government banks in the mid-1990s, bank branches that used to be state-owned in wealthier

[6] The size ranking is based on total assets of the bank in September 2008, the month of Lehman's failure.

[7] Government-controlled bank *Banco do Brasil* is the country's largest bank, followed by Itaú-Unibanco, the largest private bank and one of the 15 largest in the world.

states were transferred to private ownership (Cortes and Marcondes (2018); Summerhill et al. (2005)). Federally-owned banks, however, were never privatized. Prior to the onset of the 2008 financial crisis, this wave of state-bank privatizations and the absence of any privatization of federal banks left Brazil with bank branches that were either privately-owned or federally-owned. Many municipalities ended up having a bank branch of a particular type (private or government) for reasons unrelated to their underlying economic characteristics (Coleman and Feler (2015)).

2.2 The Government Bank Credit Boom after Lehman

Figure 1 shows that after the onset of the financial crisis in September 2008, government banks increased lending whereas private banks did not. Government banks served as a conduit for policy-making. The counter-cyclical behavior of Government banks in Brazil following the outbreak of the crisis has been widely documented and anecdotal evidence is abundant.[8] The official communication of the banks with their investors matches news reported by the financial press at that time. The Finance Minister and even the Brazilian President participated in negotiations with executives of the government banks (Safatle et al. (2016)). As the majority shareholder of the banks, the Federal Government had effective power to implement these policies, even if it meant the replacement of top-ranked employees of the banks. Pressure for a credit expansion policy reached its highest levels in the first months of 2009. For example, on April, a *Reuters* note informed that the CEO of *Banco do Brasil* was stepping down, and that *"the new CEO is tasked with raising credit."*[9] This and other episodes reported by the financial press show the resolve of the Federal government to provide bank credit stimulus, despite the risks of such interventions.[10]

To visualize how the market share of government banks changed as the policy was unwound, Figure 3 shows the evolving distributions of government bank credit share in Brazilian municipalities. We construct each distribution in the plot by aggregating total bank credit at the municipality-level for each monthly date. We then calculate the proportion of government-bank credit to total credit. This gives us 5,564 observations (municipalities in Brazil) for each date, which are used to plot each kernel density shown in the plot. We can clearly see that the Lehman events initiated a positive trend in the median market share of government banks (vertical lines) across Brazilian municipalities. Their median market share remains high ($\approx 65\%$) up to the end of 2011, 3 years after Lehman.

[8] For instance, the CEO Message in the 2009 Annual Report of the *Banco do Brasil* reads: *"We end 2009 sure of having accomplished our mission. Amid the international crisis, we increased the supply of credit and kept our business expansion strategy. Even better, we did so with excellent returns and high standards of risk management."* The annual report of the second largest government bank (*Caixa Econômica Federal*) in the same year is even more explicit: *"In face of the international crisis and its effects on the scarcity of credit in Brazil (...) the council has decided to act and to reestablish the flow of credit, that has been crucial to ensure the accelerated growth pace of the Brazilian economy."*

[9] *"Banco do Brasil CEO forced out over lending spat."* Reuters, April 8, 2009.

[10] The shares of *Banco do Brasil* plunged as much as 9% on the CEO turnover date.

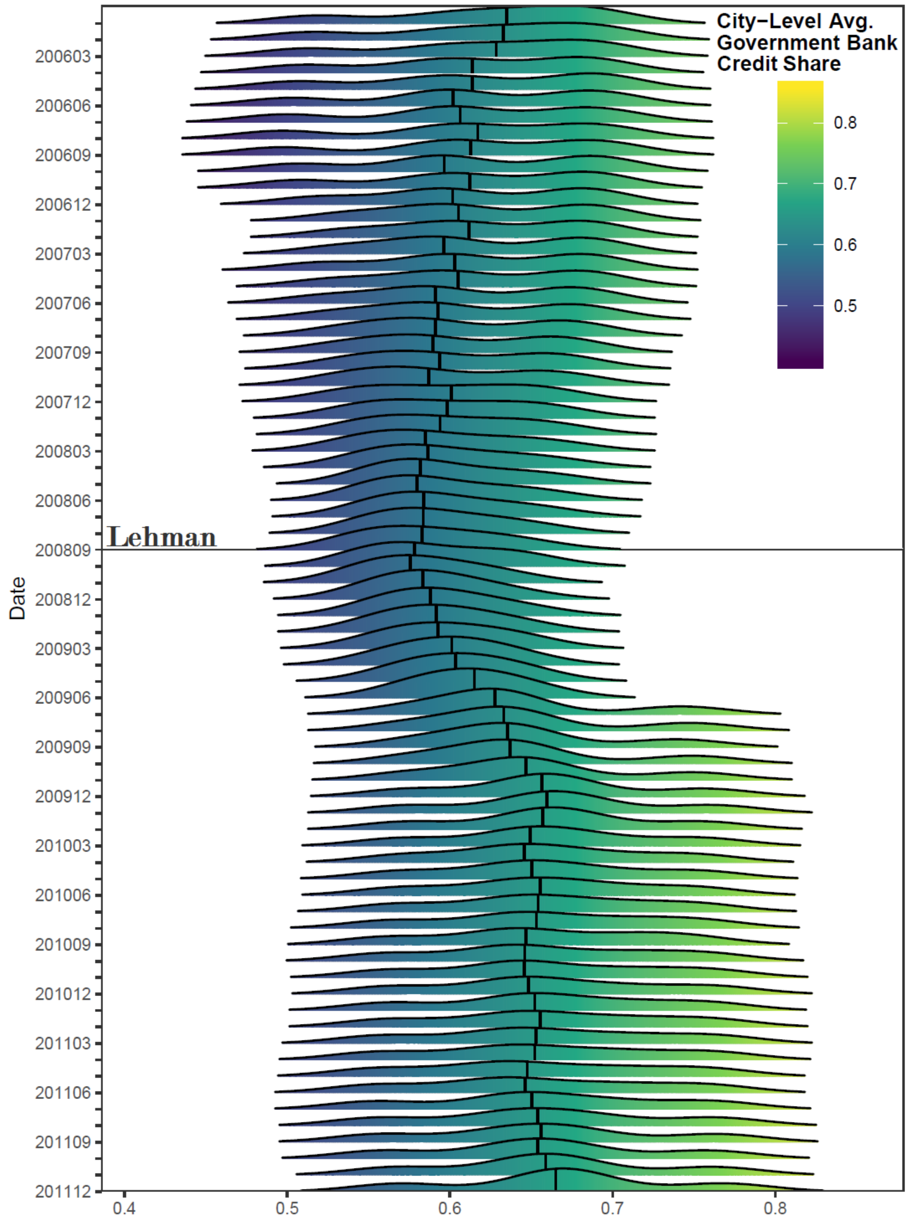


Figure 3: Government Bank Credit Share in Brazilian Municipalities (2006:M1–2011:M12). *This figure shows the share of government bank credit to total bank credit in Brazilian municipalities. The horizontal line in September 2008 marks the Lehman Brothers' bankruptcy. To construct each distribution in the plot, we aggregate total bank credit at the municipality-level for each monthly date and calculate the proportion of government-bank credit to total credit. This gives us 5,564 observations (municipalities in Brazil) for each date, which are used to plot each kernel density shown in the plot. The vertical line in each density represents the median.*

3 Data and Sample Construction

3.1 Data Sources

Credit Registry. Information on bank lending for each firm comes from the Brazilian Credit Registry, a large and comprehensive data set maintained by the *Banco Central do Brasil* (BCB) for monitoring purposes. The credit registry data are confidential and protected by bank secrecy laws in Brazil.[11] It comprises all loans with an outstanding value above the minimum threshold of BRL

[11] All confidential data were manipulated exclusively by the staff of the *Banco Central do Brasil*.

5,000 (approximately USD 2,500 in 2012) reported by all banks operating in Brazil.[12] Because our analysis focuses on firm lending — rather than household or micro-credit operations — we presumably observe the quasi-population of business loans in Brazil. The SCR has detailed information at the loan-level (i.e., all loans obtained by a firm with its banks).

Following standard practice, we aggregate loan-level data at the firm–bank level. The SCR contains detailed information on lending amount, interest rates, maturities, and credit rating.[13] We consider all commercial banks operating in Brazil between 2005 and 2011. We exclude investment banks, credit unions, and the Brazilian Development Bank (BNDES) as they are fundamentally different from commercial banks.[14] We also drop inter-bank loans and focus exclusively on loans directed to non-financial firms.

Bank Balance Sheets. Balance sheet data for all banks operating in Brazil come from the call reports submitted by financial institutions to the BCB. The balance sheet data set is publicly available at the website of the BCB for individual banks and banking conglomerates. We use balance sheet data to control for standard bank fundamentals used in the literature (e.g., Schnabl (2012)) in our loan-level analysis, such as total assets, return on assets, credit share, liquid assets share, deposits share, and equity share.

Employment Contracts. Employment data are from RAIS (*Relação Anual de Informações Sociais*), a comprehensive data set assembled by the Ministry of Labor and Employment (MTE) in Brazil. RAIS comprises the universe of formal employment contracts in Brazil on annual frequency. Created in 1976, it is used by several Brazilian government agencies to generate statistics for the Brazilian economy. The RAIS database also forms the basis for national unemployment insurance payments and other worker benefits programs. As a result, ensuring the accuracy of the information is in the interest of both firms (who would otherwise be subject to monetary fines) and individuals (who want to be eligible to receive government benefits), as well as the federal government (Bernstein et al. (2018)). Each observation in RAIS is a "job" (i.e., an employer–employee labor contract). The identified RAIS at the employer–employee level is confidential. We aggregate the job-level data at the firm-level to obtain firms' information on the number of employees, wages, industry, and municipality of the firm

[12] The reporting threshold has changed over time, but it remained constant in our period of analysis.

[13] All banks employ the same definition of default, given by the BCB's Resolution 2,682 from 1999 (defined by the National Monetary Council based on Federal Law 4,595/1964). Credit ratings are ranked by the sequence: "AA" (highest credit quality, 0 days overdue), "A" (very low probability of default, 0 days overdue), "B" (15–30 days overdue), "C" (31–60 days overdue), "D" (61–90 days overdue), "E" (91–120 days overdue), "F" (121–150 days overdue), "G" (151–180 days overdue), and "H" (more than 180 days overdue, when a bank recognizes the loan as a realized loss in its balance sheet). Each rating level is associated with a percentage provision of the total due amount of the loan. Credit ratings must be reviewed monthly in case of late payments. We use the numerical scale going from 10 (AA) to 2 (H) as defined by the BCB.

[14] The BNDES is known for having funded "national champions" (government-elected sectors and companies) with earmarked credit rates during this period. Credit unions are also known for behaving differently from standard commercial banks given their particular ownership structure (i.e., a client is also a shareholder of the credit union). There is a literature more focused on earmarked credit (see, e.g., Bonomo et al. (2015)) and credit unions (e.g., Aghabarari et al. (2018)) in Brazil during the GFC.

in each year. We use the 2-digit CNAE industry classification, leading to 96 different industries after excluding sector codes of Financial or Insurance firms, and multilateral organizations.

Payment Transactions. The payments system data (STR) registers transactions above BRL 5,000 between counterparties in Brazil.[15] This data set is also confidential and its original purpose is to inform the BCB about how reserves move from bank to bank, ensuring the solvency of the Brazilian banking system. The BCB's objectives of ensuring financial stability by supervising banks' systemic risk ensures that the data has high quality standards. The STR data are originally available at a much higher frequency (intraday), but we aggregate payments at annual frequencies to match other data sources. We exclude transactions involving only households and transactions between households and firms. Our payment data therefore contains exclusively payments between firms.

A caveat to the STR data is that we do not observe *within-bank* payments. For our study of the propagation effects of credit shocks, this should not be a major concern. Because our credit shock is characterized by bank ownership, consider an extreme situation in which firms experiencing the government credit shock only do business with other firms in the exact same government bank. In this "worst-case" scenario, we would not see any indirect effects of greater liquidity injected through government banks. In the realistic setting of Brazil's hybrid banking system discussed above, this reporting omission is likely to work against us finding significant effects of the government credit shock. The fact that we do observe significant indirect effects of government bank credit shocks suggests that this effect would be even more pronounced had we the *intra-bank* payments between firms.

To illustrate how the payment data are representative of the Brazilian economy, we present regional and sectoral breakdowns. Figure 4 displays a heatmap of total real payments between sectors of the Brazilian economy before the crisis. Sectors on the horizontal axis are payers ("debtor") and sectors on the vertical axis are receivers of payments ("creditors"). We aggregate the real amount of payments for each debtor–creditor pair of sectors and plot the corresponding sum of payments as a colored square in the heatmap. The lighter colors of the diagonal (45°) line show that the level of payments is greater within-sectors. The most important sectors in terms of payments are: *financial services*, *financial services auxiliary activities*, *wholesale trade*, and *food manufacturing*.[16] Beyond the obvious dominance of inter-bank transactions (i.e., payments between financial firms), the fact that wholesale and food manufacturing sectors are highly-ranked is expected given the consumption- and agribusiness-based characteristics of the Brazilian economy.[17] In Figure 5, we do the same exercise, but focusing on payment transactions between the five Brazilian regions. As expected, the most important region in Brazil as measured by the aggregate level of payments is the Southeast.

[15]From July 2003, the threshold for reporting was BRL 5,000 (about USD 2,500 in 2012). The threshold changed in May 2010 (BRL 3,000) and in November 2012 (BRL 2,000). We adjust our sample to reflect a consistent threshold of BRL 5,000 over the entire period of our analysis.

[16]Even though we exclude financial sectors in our analysis, we include them in the heatmap for visualization.

[17]Brazil is among the largest producers in the world of coffee, sugarcane, orange juice, soybean, corn and ethanol, among others. The large agribusiness industry represents 22% of Brazil's GDP, a third of its employment, and about 40% of its exports (Bernstein et al. (2018)).

This region contains historically wealthier states, like São Paulo and Rio de Janeiro. In contrast, the Northern region — with historically poorer states, in the Amazon region — is the one with the lowest payments. Taken together, both heatmaps suggest that the payment data reflects well the regional and sectoral structures of the Brazilian economy.

Input Innovation and Tangibility Indices. To inspect mechanisms of amplification in our baseline regressions, we construct indices for a firm's input innovation degree as a supplier and its tangibility index. The input innovation index is constructed with data from the Survey of Technological Innovation (*Pesquisa de Inovação Tecnológica*, PINTEC), published by the Brazilian Institute of Geography and Statistics (IBGE). For the tangibility index, we use the Annual Survey of Manufactures (*Pesquisa Industrial Anual*, PIA), also published by the IBGE. An important distinction to make is that the data used to construct both indices only comprise a smaller subset of the sectors available in our data. They are mainly targeted at manufacturing, service, and utilities sectors (approximately 30% of all observations). Interpretations of results using the indices should be taken with this caveat in mind.

Firm Survival Information. We also analyze the importance of direct and indirect credit shocks to firm survival. The information on whether a firm is active or inactive in a given year is obtained from the tax authority of the Brazilian Federal Government (*Receita Federal*, the analogue to the IRS in the United States).

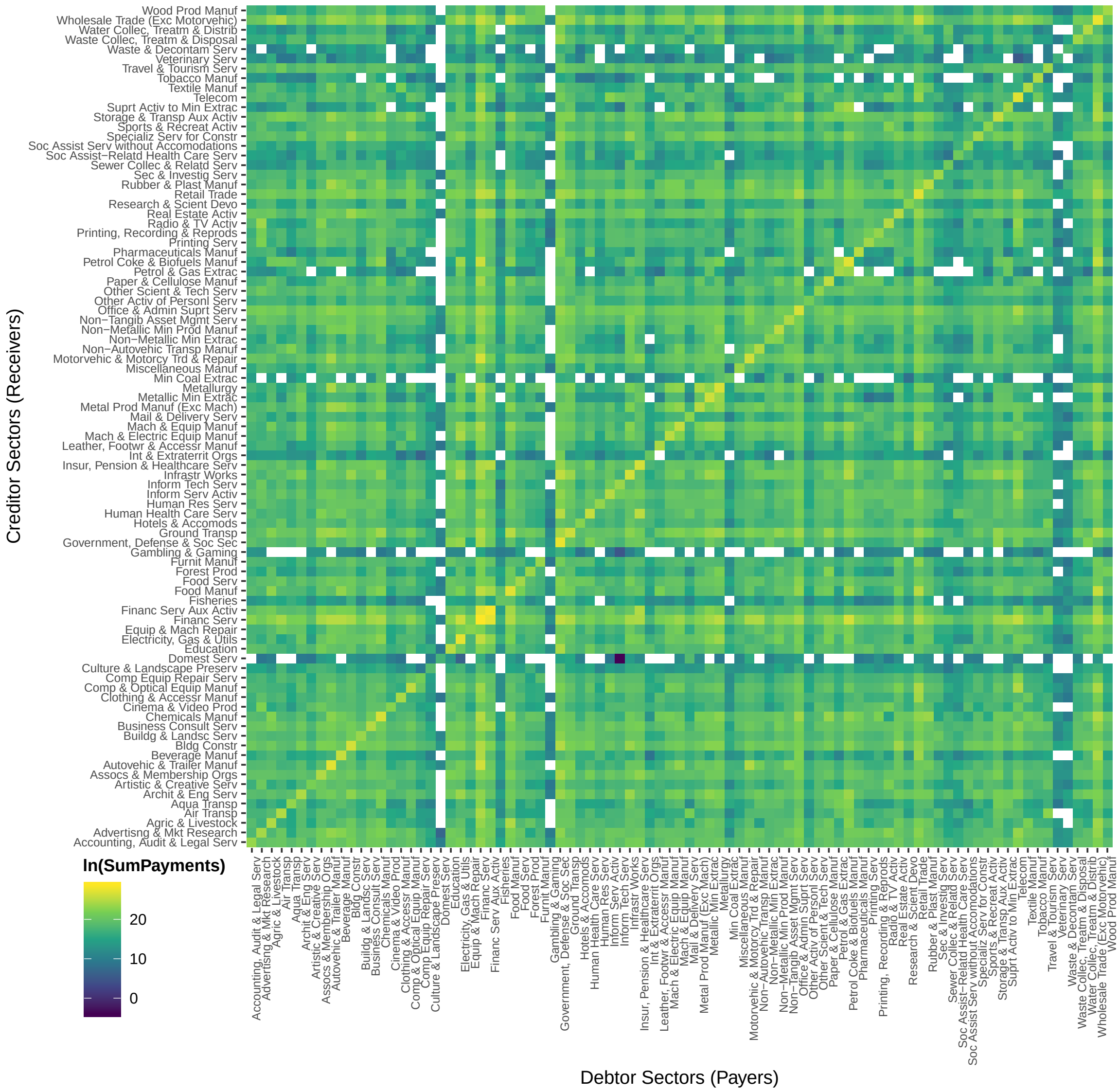


Figure 4: Payment Data Heatmap: Sectoral Heterogeneity. *This figure shows a heatmap of total real payments between sectors of the Brazilian economy during the pre-crisis period (2005–2007). Sectors on the horizontal axis are payers ("debtors") and sectors on the vertical axis are receivers of payments ("creditors"). We aggregate the real amount of payments for each debtor–creditor sector pair and plot the corresponding sum of payments as a colored square in the heatmap. Lighter colors represent greater level of real payments. For example, the lighter colors of the diagonal (45°) line mean that the level of payments is greater within-sectors. Pairs of debtor–creditor sectors with white-colored squares indicate that no payments between the two sectors happened in the pre-crisis period.*

3.2 Descriptive Statistics

Table 1 presents descriptive statistics for our pre-crisis sample that spans 2005–2007. Panels A and B reveal that about half of the sample of suppliers (51%) and customers (57%) have some credit with government banks, allowing a relatively good balance between government-credit-exposed and non-exposed firms. Customers are larger than suppliers and maintain a higher share of payments with respect to its suppliers than *vice-versa*. From Panel C, we can see that the median supplier–customer pair in the Brazilian firm network has an average real payment of BRL 12,262 per year.[18] Finally, Panel D shows us that there are more suppliers than customers over the entire time span (2005–2011). The average number of suppliers (customers) in our sample is 611,320 (348,957), totalling almost 18

[18] All nominal values are corrected for inflation using the IPCA consumer price index, published by the IBGE.

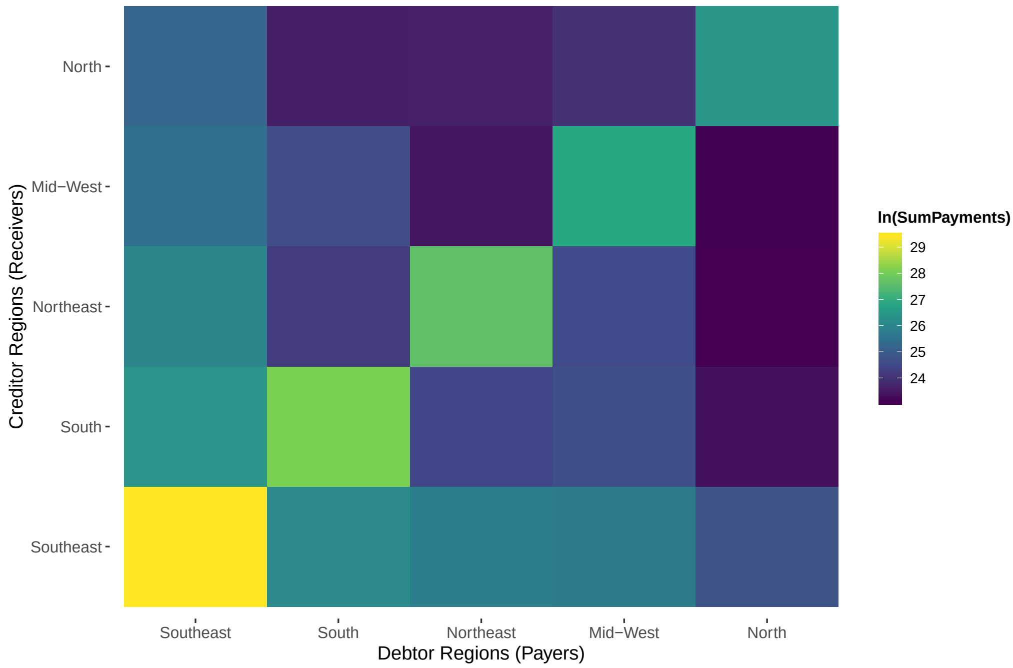


Figure 5: Payment Data Heatmap: Regional Heterogeneity. *This figure shows a heatmap of total real payments between Brazilian regions during the pre-crisis period (2005–2007). Regions on the horizontal axis are payers ("debtors") and regions on the vertical axis are receivers of payments ("creditors"). We aggregate the real amount of payments for each debtor–creditor region pair and plot the corresponding sum of payments as a colored square in the heatmap. Lighter colors represent greater level of real payments. For example, the lighter colors of the diagonal (45°) line mean that the level of payments is greater within-regions.*

million supplier–customer–year observations.

— PLACE TABLE 1 ABOUT HERE —

4 Empirical Strategy

Our empirical strategy is divided into two steps. The first step focuses on the direct effects of credit supply shocks. This "direct effect" depends on whether *the firm itself* is exposed to the government credit shock. We first show that pre-crisis access to government banks significantly mattered for how much credit a firm received after Lehman. Then we show that the credit shock had real effects on firms' employment and wage policies. The second step focuses on the indirect effects of credit shocks. This "indirect effect" depends on whether *other firms* doing business with the reference firm are exposed to the bank credit shock, i.e., whether these other firms had direct access to government bank credit. For example, focusing on the perspective of a supplier, the indirect shock depends on whether its customers are exposed to the credit shock. After presenting both steps, we inspect two amplification mechanisms of the indirect effects of credit shocks. We conclude with a firm survival analysis that embeds both direct and indirect effects of credit shocks in the same regression framework.

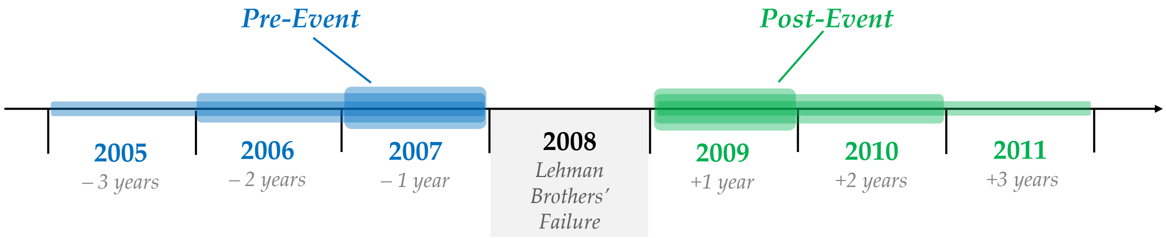


Figure 6: Event Study Timeline. *This figure shows the timeline of our event study. We focus on three distinct periods before and after the Lehman Brothers' failure in 2008: 1 year after versus 1 year before (the thickest shaded areas); 2 years after versus 2 years before; and 3 years after versus 3 years before (the thinnest shaded areas). We exclude 2008 from the analysis because it is the year of the Lehman events.*

4.1 Direct Effects of the Government Credit Supply Shock

4.1.1 Event Study Dating

We start our empirical analysis defining the key dates in our *Difference-in-Differences* event study. We follow the previous literature and consider the Lehman failure in 2008 as the beginning of the Global Financial Crisis. We therefore omit the year of 2008 from our time window and compare the year before (2007) *versus* the year after Lehman (2009), as illustrated in Figure 6. We also report longer time windows, to understand the persistent, dynamic effects of credit shocks and to check if results are sensitive to a particular window choice. More specifically, we focus on three distinct periods before and after the Lehman Brothers' failure in 2008: 1 year after *versus* 1 year before (the thickest shaded areas); 2 years after *versus* 2 years before; and 3 years after *versus* 3 years before (the thinnest shaded areas).

4.1.2 Loan-Level Analysis: Quantifying the Government Bank Credit Supply Shock

The total credit obtained by a firm with a bank is an equilibrium outcome that depends on both the firm's demand and the bank's supply factors. Disentangling credit supply from credit demand is only possible when using data at the firm-bank-time dimension, as pioneered by Khwaja and Mian (2008). We follow their methodology and estimate the following DID model with *bank* and $firm \times year$ fixed-effects:

$$Credit_{i,b,t} = \delta^{DID} \cdot [Gov_b \times Post_t] + \gamma \cdot Controls_{b,t} + \sum_b Bank_b + \sum_i \sum_t [Firm_i \times Year_t] + \varepsilon_{i,b,t}, \quad (1)$$

where firms are indexed by i, banks by b, and years by t. $Credit_{i,b,t}$ is the log of total outstanding credit that firm i has with bank b at year t. $Post_t$ is an indicator variable that equals one if year $t \geq 2009$ and is zero otherwise. Gov_b is an indicator variable that equals one if bank b is owned by the government, and is zero otherwise. $Controls_{b,t}$ are bank-level fundamentals as in Schnabl (2012), including size (natural logarithm of lagged total assets), return on assets, credit share, liquid assets share (Basel III-defined), deposit share, and equity share. Following Petersen (2009), we double-cluster standard errors at the bank and year levels.

The model disentangles the credit supply shock in a simple way. The $firm \times year$ fixed effects purge all variation in the data that is characterized at the firm-level. This gets rid of any determinants of firm credit demand, allowing us to isolate supply factors. The coefficient of interest in Equation (1), δ^{DID}, thus measures the difference between government and private bank credit supply *for the same firm* after the Lehman failure.

4.1.3 Firm-Level Analysis: The Real Direct Effects of the Government Bank Credit Supply Shock

Once one has estimated the liquidity effects of government bank during the GFC, one could ask whether the credit crunch was severe enough to affect firms' real-side policies. If firms relying on private credit were able to substitute alternative types of funding for bank lending, then the government credit shock would not matter for the real economy. We test this by comparing the real outcomes of firms with pre-crisis access to government credit (i.e., firms exposed to the government credit supply shock) *versus* firms with no such access. This analysis is not feasible in the loan-level setting of Equation (1), because the Khwaja and Mian (2008) identification strategy compares outcomes for the same firm.

We therefore aggregate our firm-bank-year observations at the firm-level. The aggregation of bank credit information allows us to observe how much of a firm's total credit comes from government banks *vis-à-vis* private banks. We end up with the share of government credit *before the crisis*, which emerges as a natural measure of firm exposure to the government bank credit shock. At the firm-level dimension, we consider a firm as "treated" if its government credit share is positive, i.e., if the firm has already initiated a relationship with a government bank before the crisis.[19]

One concern that arises is that treated firms may be politically-connected and therefore could fundamentally differ from those firms that did not have government credit prior to the crisis. One wants to rule out the possibility that differences between groups are the true cause of any differential behavior we find after the Lehman crisis, and not the greater liquidity from government banks enjoyed by treated firms. To address these issues, we take advantage of the literature on the political economy of banking in Brazil. From Carvalho (2014), we know that politically-connected firms in Brazil are clustered in a subset of industries considered "priority sectors" by the Federal Government.[20] We therefore include a complete set of $industry \times municipality \times year$ dynamic fixed effects.[21] Addition-

[19]Because the median firm has zero government bank credit share, this is equivalent to setting firms with higher-than-median government credit share as treated. Even though the median customer firm and the median supplier firm in Table 1 have non-zero government credit share, because some firms are suppliers-only or customers-only, it turns out that the median firm has zero government credit share over the full sample distribution. This is because firms that are both customers and suppliers are larger, and therefore more likely to have government credit.

[20]For a complete list of these sectors, see Carvalho's (2014) Internet Appendix.

[21]Because most of these industries are historically clustered in certain regions (e.g., the Automobile industry is concentrated in the State of São Paulo), dynamic industry fixed effects would most likely suffice to control for the unobserved heterogeneity of priority-sector firms. However, we also include municipalities into the set of dynamic fixed effects to mitigate concerns that unobserved regional heterogeneity at the industry level is behind our results. In unreported tests,

ally, Bonomo et al. (2015) show that firms with access to government credit are usually bigger and older. This leads us to include a firm's size (as measured by the log of the number of employees) and its age as controls. Finally, one might worry about other characteristics in the group of treated firms that are not captured by the variables that the previous literature identifies. This leads us to include a firm's credit rating in the set of controls, relying on banking theory's rationale that the credit rating is a summary of all information about the firm that is available to the bank (e.g., Freixas and Rochet (2008)). Because banks use all available information in monitoring a firm's creditworthiness, the credit rating inclusion is aimed at mitigating omitted variable concerns with respect to firm characteristics not available in our database. In sum, we estimate the following DID model at the firm-level:

$$Real_{i,t} = \delta^{DID} \cdot [Gov_i \times Post_t] + \gamma \cdot Controls_{i,t} + \sum_i Firm_i + \sum_j \sum_k \sum_t [Ind_j \times City_k \times Year_t] + \varepsilon_{i,t}, \quad (2)$$

where $Real_{i,t}$ is one of the two real outcome variables (i.e., the log of total firm employment and the log of the total wage bill). Gov_i is an indicator variable that equals one if firm i has an existing government credit relationship in the pre-crisis period (2005–2007), and is zero otherwise.[22] $Controls_{i,t}$ are firm-level fundamentals discussed above, including size, age, and credit rating.[23] We include a full set of firm fixed effects, and interactions of industry (Ind_j), municipality ($City_k$), and time ($Year_t$). We double-cluster standard errors at the firm and year levels.

4.2 Network-Level Analysis: Indirect Effects of Government Bank Credit Shocks

We now shift our attention from the direct effects to the indirect effects of credit shocks. As emphasized earlier, this is only possible when one has data that links firms with other firms in the economy. With our network of payments, we observe payments from customers to their suppliers in each year. We want to evaluate how the level of payments between suppliers and customers is affected by credit shocks, and to use payment *shares* as a measure of market share concentration. The intuition is simple: a treated customer may be able to increase its market share (as measured by the share of payments with its suppliers) to detriment of another credit-constrained competitor customer. The same rationale can be applied to unconstrained supplier that exploit its access to credit to gain a larger share of payments with its clients to detriment of other suppliers that face credit shortages after Lehman.

we find similar results using $industry \times state \times year$ or $industry \times year$ fixed effects. These results are available upon request.

[22]We choose to classify treated firms using a pre-crisis indicator variable due to an established body of theoretical and empirical evidence that the existence of a pre-crisis credit relationship critically matters for a firm obtaining continuation credit during crises (see, e.g., Freixas and Rochet (2008) and Bolton et al. (2016)). Firms with access to a lender *before* the rise of a financial crisis are less affected by adverse selection problems and obtain more continuation credit — and at better contracting terms — during liquidity freezes *vis-à-vis* firms that never borrowed from that lender in the past. In unreported tests, we experimented with alternative treatment choices, such as continuous treatment (i.e., government-bank credit share) and quantile-based approaches (top tercile of government-bank credit share distribution). The tenor of our results remains unchanged.

[23]In the regressions where the log of employment is used as the dependent variable, we must use another measure for firm size. To proxy a firm's total assets, we use the log of the firm's social capital from its tax returns data.

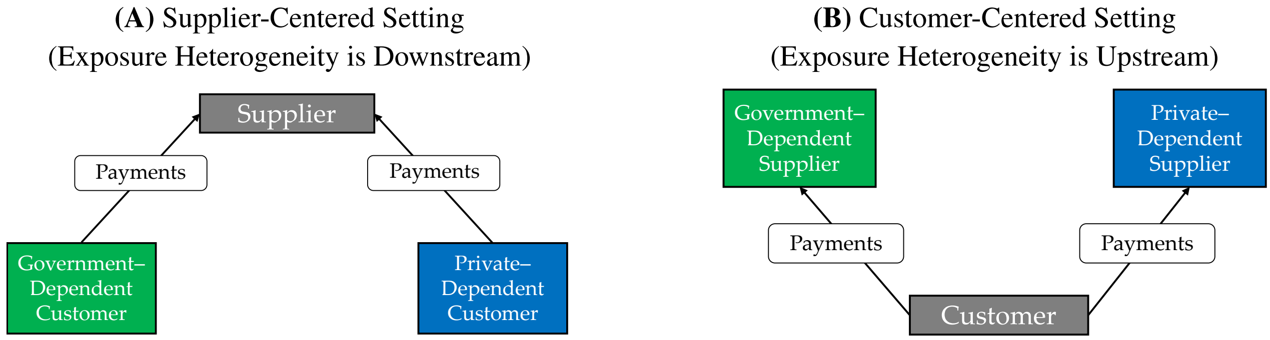


Figure 7: Network-Level Analysis Intuition: Downstream and Upstream Treatment Heterogeneity. *Panel A shows the regression setting represented in Equation* (3)*, where supplier $\times$ year dynamic fixed effects allow the interpretation of payments to the same supplier from treated and non-treated customers. Panel B shows the regression setting represented in Equation* (4)*, where customer $\times$ year dynamic fixed effects allow the interpretation of payments from the same customer to treated and non-treated suppliers.*

For simplicity, first consider the case where the exposure to the shock varies across customers (i.e., the "treatment" is downstream in the production chain). Panel A of Figure 7 illustrates this possibility: a supplier receives payments from both treated and non-treated customers. A straightforward way to obtain this Khwaja and Mian (2008)-like setting centered on the same supplier is to include dynamic *supplier* $\times$ *year* fixed effects in the following model:

$$Payment_{c,s,t} = \delta^{DID} \cdot [Gov_c \times Post_t] + \gamma \cdot Controls_{c,t} + \sum_c Cust_c + \sum_s \sum_t [Sup_s \times Year_t] + \varepsilon_{c,s,t}, \quad (3)$$

where $Payment_{s,c,t}$ is one of the payment variables we analyze. Our baseline analysis considers the level of payments (i.e., the log of the real amount in Brazilian *reais* paid to supplier s by customer c at year t) and the share of payments (i.e., the percentage of payments that a customer c has with respect to all payments received by supplier s at year t).[24] Gov_c is an indicator variable that equals one if customer c has a positive share of government credit in the pre-crisis period, and is zero otherwise. $Controls_{c,t}$ are customers' firm-level fundamentals discussed above (size, age, and credit rating). We include a full set of customer fixed effects, and the aforementioned *supplier* $\times$ *year* dynamic fixed effects. In robustness analyses, we add interactions of customer industry (Ind_j), customer municipality ($City_k$), and time ($Year_t$) fixed effects. Finally, we double-cluster standard errors at the supplier and year levels.

The *supplier* $\times$ *year* fixed effects in Equation (3) allow us to interpret the model as follows: for a given supplier that receives payments from both treated and non-treated customers, how are its payments affected by their customers' exposure to the government credit shock? The coefficient of interest in Equation (3), δ^{DID}, measures the difference between payments from government-credit-treated and non-treated customers *for the same supplier* after the Lehman failure.

Panel B of Figure 7 illustrates the analogous case, where a customer sends payments to both treated and non-treated suppliers. The customer-centered model is exactly that of Equation (3), save that we include *customer* $\times$ *year* (instead of *supplier* $\times$ *year*) fixed effects. Interchanging the sub-

[24]In the Online Appendix, we also report results for the number of payments and for the average value of payments.

scripts of suppliers (s) and customers (c), yields:

$$Payment_{c,s,t} = \delta^{DID} \cdot [Gov_s \times Post_t] + \gamma \cdot Controls_{s,t} + \sum_s Sup_s + \sum_c \sum_t [Cust_c \times Year_t] + \varepsilon_{c,s,t}, \quad (4)$$

where all variables are as defined above, and standard errors are double-clustered by customer and year. As before, the $customer \times year$ fixed effects in Equation (4) let us interpret the model as follows: for a given customer paying to both treated and non-treated customers, how are its payments affected by their suppliers' exposure to the government credit shock? The coefficient of interest in Equation (4), δ^{DID}, measures the difference between payments to government-credit-treated and non-treated suppliers *by the same customer* after Lehman.

4.3 Inspecting Amplification Mechanisms

After presenting both direct and indirect effects of credit shocks, we inspect whether characteristics of suppliers or customers can amplify these effects. We draw from the literature to consider characteristics that may exacerbate the transmission of credit shocks through the production network.

Input Innovation Index. From a customer's perspective, it is riskier to pursue investment opportunities that integrate novel inputs in the production process when liquidity is scarce. We test this by constructing an index for a supplier's degree of input innovation based on the 2007 Survey of Technological Innovation (PINTEC) in Brazil. More specifically, the survey asks a representative sample of manufacturing, service, and utility firms in Brazil how many of their newly-introduced products were considered novel in a certain year. The survey details the degree of innovation by breaking it down into the following categories: (i) the product is not novel for the firm, the domestic, or the international market; (ii) the product is novel for the firm, but exists in the domestic and international markets; (iii) the product is novel for the firm and for the domestic market, but exists in the international market; (iv) the product is novel for the firm, for the domestic market, and for the international market. The data are aggregated for each industry, to deliver an industry-level input innovation index as:

$$Input\ Innov_j = \frac{\left(\sum Domestic\ Novelty_j + \sum International\ Novelty_j\right)}{\sum All\ Products_j}, \quad (5)$$

where $Domestic\ Novelty_j$ is the sum of all products in category (iii) above for industry j. $International\ Novelty_j$ is the sum of all products in category (iv), and the denominator *All Products* represents the sum of all products (innovative or not) introduced by firms in industry j in 2007.[25] More innovative industries will have a higher share of their products in categories (iii) or (iv), so that customers buying from them plausibly assume greater risks in implementing domestically- or internationally-novel inputs when liquidity is scarce. Panel A of Figure 8 shows that the sector of *Research & Scientific Development* is the top-ranked in innovation, since almost 80% of the products introduced by this sector in

[25] We choose 2007 for consistency with our pre-crisis period illustrated in Figure 6. Using the survey from earlier years does not change our results.

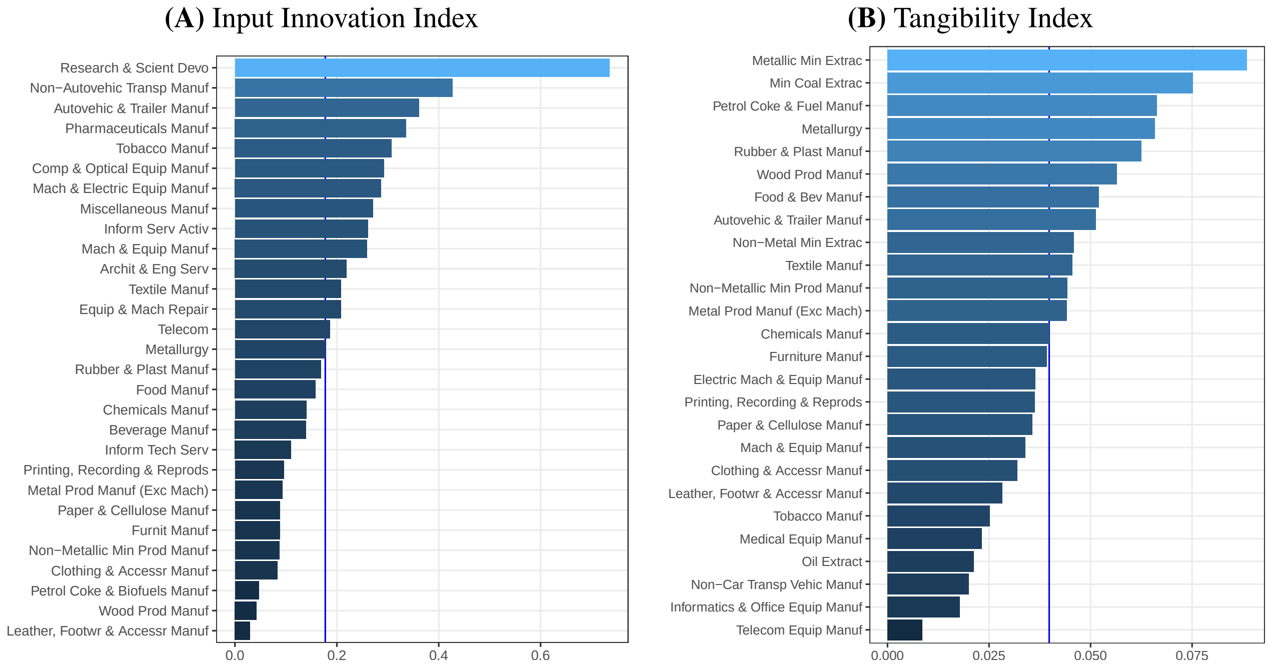


Figure 8: Amplification Mechanisms: Input Innovation Index and Tangibility Index by Sector. *Panel A shows the input innovation index constructed in Equation* (5)*. The vertical line in Panel A corresponds to the median value (0.1773) of the index across sectors. Panel B shows the tangibility index constructed in Equation* (7)*. The vertical line in Panel B corresponds to the median value (0.0398) of the index across sectors.*

2007 were domestic or international novelties. Other highly-innovative industries are *Vehicle*, *Non-Autovehicle*, and *Pharmaceuticals* manufacturing. The median sector has an innovation index of 17.73%, represented by the vertical line in the plot. Sectors like *Wood Product Manufacturing* and *Leather, Footwear and Accessories Manufacturing* are low-ranked in terms of their innovation.

With the input innovation index at hand, we define an indicator variable for highly-innovative input suppliers, $Input\ Innov_s$. We consider a supplier to produce a highly-innovative input if its industry has greater-than-median (17.73%) $Input\ Innov_j$ index. We then estimate the following triple-difference model (or *Difference-in-Difference-in-Differences*, DIDID) based on our customer-centered model in Equation (4):

$$\begin{aligned} Payment_{c,s,t} = {} & \delta^{DID} \cdot [Priv_s \times Post_t] + \delta^{DIDID} \cdot [Priv_s \times Input\ Innov_s \times Post_t] \\ & + \gamma \cdot Controls_{s,t} + \sum_s Sup_s + \sum_c \sum_t [Cust_c \times Year_t] + \varepsilon_{c,s,t}, \end{aligned} \tag{6}$$

where all variables were defined above, except for $Priv_s$, which is the analogue of our original treatment dummy (Gov_s in Equation (4)). It equals one if supplier s is *private-credit-dependent* (i.e., if its government bank credit share before the crisis is equal to zero), and is zero otherwise. We invert the treatment dummy from *Gov* to *Priv* in this regression to make the interpretation of our coefficient of interest (δ^{DIDID}) more intuitive. The triple interaction coefficient is therefore expected to be negative because the level of payments should decrease more for private-credit-dependent suppliers that produce highly-innovative inputs, *vis-à-vis* private-credit-dependent suppliers of standard inputs, who suffer less from customers' precaution after the Lehman failure. Standard errors are double-clustered at the customer and year levels.

Tangibility Index. We construct a tangibility index to analyze whether a customer's financial constraints can exacerbate the effects of credit shocks. As Almeida and Campello (2007) show, firms with greater share of tangible assets (e.g., plants, trucks) are less likely to be financially-constrained because banks can pledge these assets as collateral in loan contracts. The tangibility index is constructed with data from the 2007 Annual Survey of Manufactures (PIA). The survey reports the total value of: (i) *Real Estate & Land*, (ii) *Machinery & Equipment*, and (iii) *Vehicles* from a representative sample of manufacturing firms, aggregated at the industry-level. Our tangibility index is given by the sum of items (i) to (iii) divided by the total assets of industry j:

$$Tangib_j = \frac{\left(\sum Real\ Estate\ and\ Land_j + \sum Machinery\ and\ Equip_j + \sum Vehicles_j\right)}{\sum Total\ Assets_j}. \tag{7}$$

Panel B of Figure 8 shows that the top-ranked sector in terms of tangibility is *Metallic Mineral Extraction*, for which almost 10% of all assets is tangible. The least tangible sectors are *Informatics & Office Equipment Manufacturing* and *Telecommunications Equipment Manufacturing*, which are arguably more dependent on intangible, human capital.

The median tangibility sector has about 4% of its assets in the mentioned categories. We therefore proceed as before and define an indicator variable for low-tangibility customers based on their industry's tangibility index. More specifically, $Low\ Tangib_c$ is equal to one if $Tangib_j$ of customer c is below the median (3.98%), and it is zero otherwise. We estimate the following DIDID model based on our supplier-centered model in Equation (3):

$$\begin{aligned} Payment_{c,s,t} = {} & \delta^{DID} \cdot [Priv_c \times Post_t] + \delta^{DIDID} \cdot [Priv_c \times Low\ Tangib_c \times Post_t] \\ & + \gamma \cdot Controls_{c,t} + \sum_s Cust_c + \sum_s \sum_t [Sup_s \times Year_t] + \varepsilon_{c,s,t}, \end{aligned} \tag{8}$$

where all variables are defined as before. To ease interpretation of the coefficient of interest (δ^{DIDID}), we once again invert the treatment dummy from *Gov* to *Priv*. The triple interaction coefficient here is expected to be negative because the level of payments should decrease more for private-credit-dependent customers that cannot rely on tangible assets to alleviate credit constraints, *vis-à-vis* private-credit-dependent customers that can pledge more collateral with banks after the Lehman failure. Standard errors are double-clustered by supplier and year.

4.4 Firm Survival and the Government Credit Multiplier

We complete our empirical analysis with a firm survival examination that embeds direct and indirect effects in the same regression framework. To do this, we aggregate our supplier-customer-year data at the supplier-year and customer-year level. Consider the case where we aggregate the data at the supplier-level. For each supplier, we observe if it is treated ($Gov_s = 1$) or not. As emphasized before, this is our firm-level indicator of *direct* exposure of supplier s. Because we also observe the treatment indicators of all customers doing business with supplier s, we can construct the following index of *indirect* exposure that each supplier faces through its customers $c \in \{c_1, c_2, \ldots, c_N\}$:

$$Indirect\ Gov\ Exp_{s,t} = \sum_c \left(\frac{Gov_c \times Payment_{s,c,t}}{Payment_{s,c,t}} \right), \tag{9}$$

where the index can be interpreted as the share of customers that are treated with government credit before the crisis, weighted by $Payment_{s,c,t}$, i.e., the value of payments that each c transfers to its supplier s in year t.

The indirect exposure index allows us to estimate firm-level regressions with *both* direct and indirect effects embedded in the same model. Focusing on the post-crisis period (2009–2011), we use an indicator variable of the supplier's "death" (i.e., when the firm becomes *inactive*) to estimate the following probit model:

$$\begin{aligned} Death_{s,t} = {} & \delta^{Direct} \cdot Gov_s \\ & + \delta^{Indirect} \cdot Indirect\ Gov\ Exp_s \\ & + \gamma \cdot Controls_{s,t} + \sum_j \sum_k \sum_t [Ind_j \times City_k \times Year_t] + \varepsilon_{c,s,t}, \end{aligned} \tag{10}$$

where the coefficients of interest are δ^{Direct}, that measures the direct impact of a supplier's access to government credit, and $\delta^{Indirect}$, which captures the effect of indirect exposure to government credit through its creditors. $Controls_{s,t}$ is a set of controls containing the supplier's size, age, and credit rating. To account for the different relationships that suppliers have with their customers, we also control for the number of relationships of each supplier. We also control for customer characteristics averaged over each supplier, including average customer size, age, and credit rating, as well as the customers' average number of relationships and average death rate. Finally, we include $industry \times city$ fixed effects in the 1-year post-crisis specification (i.e., 2009), and $industry \times city \times year$ fixed effects in the 2-year (2009-2010) and 3-year (2009-2011) specifications. Standard errors are clustered at the supplier level for the 1-year post-crisis regression, and double-clustered at the supplier and year levels for the 2- and 3-year regressions.

The indirect exposure index can also be calculated for customers, by summing over suppliers rather than customers in Equation (9) and aggregating the data at the customer-year dimension. We then estimate the customer-analogue of Equation (10).

5 Results

5.1 Direct Effects of Credit Shocks

5.1.1 Government Banks as Counter-Cyclical Lenders

Table 2 presents the results from estimating Equation (1). Column (1) shows the result of the credit supply shock as in Khwaja and Mian (2008) for the 1-year time window, i.e., comparing the post-crisis (2009) with the pre-crisis (2007) period. The DID coefficient for the interaction of government bank lending after the crisis is positive and statistically-significant at the 1% level. Columns (2) and (3) report the results for the 2- and 3-year-expanded windows, respectively. Both columns show that the DID coefficient is positive and highly significant, lending support to the view that government banks supplied credit in a counter-cyclical fashion in response to the crisis and that this effect was persistent. Most importantly, this differential is economically significant. The government banks' credit wedge *vis-à-vis* private banks ranges from roughly 33% $\left(e^{0.288} - 1 = 33.37\%\right)$ for the 2-year window to a massive 49% $\left(e^{0.398} - 1 = 48.88\%\right)$ in the 1-year window estimate. Our loan-level estimation purging firm demand factors uncovers an increment of 10 percentage points (1-year window) in the government *vs.* private credit supply shock *vis-à-vis* previous studies (e.g., Coleman and Feler (2015)). Such difference is significant and expected because controlling for credit demand factors are likely to matter more during recessions, i.e., when several firms experience greater need for funding. In unreported tests, we look at other loan contracting terms, including maturity and interest rate spread. We repeat our specifications using the contractual maturity and interest rate as dependent variables, finding statistically-insignificant DID estimates for these outcomes. These results suggest that the credit expansion took place mostly via lending higher amounts.[26]

— PLACE TABLE 2 ABOUT HERE —

Next, we ask whether the wedge in government bank credit *vis-à-vis* private banks had real effects. Table 3 reports the direct effects of the government-bank credit shock on labor outcomes. Columns (1) to (3) show that firms with access to government bank credit before the crisis had 5% $\left(e^{0.051} - 1 = 5.23\%\right)$ greater employment than private-credit-dependent firms in the 1-year window, with significant persistent effects holding in the 2- and 3-year windows. Columns (4) to (6) show that these firms were also able to sustain almost 7% $\left(e^{0.066} - 1 = 6.82\%\right)$ greater wage bills, an effect that persisted and increased to 6.9% $\left(e^{0.067} - 1 = 6.93\%\%\right)$ and 7.8% $\left(e^{0.075} - 1 = 7.78\%\right)$ in the 2- and 3-year time windows. These magnitudes are economically significant: for the average pre-crisis firm in Brazil, direct access to government credit means avoiding a cut of almost BRL 3,700 in total wages (*vis-à-vis* the 2007 Brazilian minimum wage of BRL 380) and two jobs out of 24.

— PLACE TABLE 3 ABOUT HERE —

[26]These results are available upon request.

5.2 Indirect Effects of Credit Shocks

5.2.1 The Indirect Effects of a Supplier interacting with Treated Customers

We next analyze the indirect effects of credit shocks, first from the perspective of a supplier that receives payments from both treated and non-treated customers (Figure 7, Panel A). Table 4 presents results from estimating the supplier-centered regression in Equation (3). The DID coefficient is positive and significant at the 1% level for columns (1) to (3), showing that a supplier receives significantly more payments from customers with access to government bank credit before the crisis. In the 1-year window, the difference in payments is 2.5% $\left(e^{0.025}-1=2.53\%\right)$, rising to 3.6% $\left(e^{0.036}-1=3.65\%\right)$ and 4.1% $\left(e^{0.040}-1=4.08\%\right)$ in the 2- and 3-year time windows, respectively. Table A.1 in the Appendix reports the same specification using the number of payments rather than the total value of payments as the dependent variable, with similar results. Interestingly, the supplier-centered regression in Appendix Table A.3 shows that the level of average payments did not differ between both groups of customers.

Because, on average, a pre-crisis supplier has about four customers, out of which roughly half (51.1%) have access to government credit, these results suggest that the total indirect effect of credit shocks on a supplier can be as high as 5.1% in the 1-year window ($4 \times 51.1\% \times 2.5\%$) and 8.3% in the 3-year window ($4 \times 51.1\% \times 4.08\%$).

— PLACE TABLE 4 ABOUT HERE —

5.2.2 The Indirect Effects of a Customer interacting with Treated Suppliers

Now we consider the perspective of a customer that sends payments to both treated and non-treated suppliers (Figure 7, Panel B). Table 5 presents the results from the customer-centered regression in Equation (4). The DID coefficient is again positive and significant at the 1% level for columns (1) to (3), meaning that a customer purchases significantly more from suppliers that have access to government bank credit before the crisis. Column (1) reveals that customers purchased 3.5% $\left(e^{0.035}-1=3.56\%\right)$ more from suppliers with access to government credit 1 year after Lehman. The effect of government credit in the upstream dimension of the production network is also persistent, rising in the 2-year $\left(e^{0.042}-1=4.28\%\right)$ and 3-year $\left(e^{0.054}-1=5.54\%\right)$ time windows around the Lehman failure. Table A.2 in the Appendix reports the same specification using as dependent variable the number of payments, yielding the same conclusions. Table A.4 in the Appendix also shows that the level of average purchases was greater for treated suppliers.

On average, a customer before the crisis has about five suppliers, out of which more than half (57.6%) have access to government bank credit. These results then suggest that the total indirect effect of credit shocks on a customer can be as high as 10.1% in the 1-year window ($5 \times 57.6\% \times 3.5\%$) and 15.5% in the 3-year window ($5 \times 57.6\% \times 5.4\%$).

— PLACE TABLE 5 ABOUT HERE —

5.2.3 Amplification Mechanisms

Taken together, the results from the customer- and the supplier-centered regressions above suggest that credit shocks propagate throughout the production network. They also suggest that the propagation effects are persistent over the 3-year period analyzed. We now ask whether characteristics of suppliers or customers modulate these propagation effects. Starting with the customers' channel of financial constraints, we present the results of estimating the DIDID model shown in Equation (8). Columns (1) to (3) in Panel A of Table 6 show that the triple interaction of $Priv_c \times Post_t \times Low\ Tangib_c$ is negative as expected and statistically significant.[27] The DIDID coefficient suggests that private-credit-dependent firms (those with no access to government credit before the crisis) with relatively less tangible assets decreased their purchases by *vis-à-vis* other private-credit-dependent firms with a higher tangibility index. These results suggest that customers' financial constraints can magnify the indirect effect on payments to suppliers.

— PLACE TABLE 6 ABOUT HERE —

Turning to the supplier's channel of input innovation, Panel B of Table 6 reports the results of Equation (6). Columns (1) to (3) show that the DIDID coefficient is negative as expected and statistically significant. The flow of real payments is between 7% and 16% lower when the supplier has no access to government credit after Lehman and the input it produces is highly innovative. The precautionary behavior of customers in implementing innovative inputs under liquidity scarcity is therefore at play after the Lehman events.

5.2.4 Effects on Market Concentration

We now change the dependent variable in our supplier-centered model from Equation (4) to the share of total payments that customers have with its supplier to analyze effects on market concentration. Finding a positive effect on the share of payments that customers have with a supplier would indicate that pre-crisis access to government credit increases the market share of treated customers. Table 7 reports the results. Column (1) shows that the DID coefficient is positive and significant at the 1% level in the 1-year window, but Columns (2) and (3) show that this effect disappears over longer windows.

— PLACE TABLE 7 ABOUT HERE —

Doing the same analysis for the customer-centered case, we can evaluate whether the government credit shock affected suppliers' payment concentration. We calculate the share of payments that each supplier has with its customers and use it as the dependent variable in Equation (4). The results are in Table 8. Columns (1) to (3) show that, in contrast to customer market share, the DID coefficient is positive and significant at the 1% for all periods. In fact, it increases over longer time windows, suggesting that the government credit shock potentially helped treated firms to gain market share.

[27] Recall that we use the flip side ($Priv_c$) of our treatment variable used so far (Gov_c) to make the coefficient interpretation easier, as explained in Section 4.3.

— PLACE TABLE 8 ABOUT HERE —

5.2.5 The Government Lending Multiplier: Firm Survival Analysis

Having shown that bank credit shocks permeate the payment outcomes on both the upstream (suppliers) and downstream (customers) dimensions of the production network, we now show how much direct and indirect effects matter for firm survival. Table 9 reports the results of estimating the probit models described in Equation (10) for suppliers and customers. Panel A focuses on suppliers. Column (1) shows that having direct access to government credit reduces their death probability by 0.8 percentage point. The indirect effect coefficient reduces the supplier's probability of death by 1.7 percentage points when $\textit{Indirect Gov Exp}_s = 1$, i.e., when a supplier has all of its payments coming from government-exposed customers. These results suggest that, for a supplier, having a large amount of payments coming from customers with access to government credit is about twice as important as having government credit itself for its survival. Given the 20% average supplier mortality in the post-crisis (2009-2011), the total reduction of 0.025 (0.008 + 0.017) percentage point represents a relative decline of 12.5%.

— PLACE TABLE 9 ABOUT HERE —

Turning to customers, Panel B of Table 9 shows that indirect effects are also twice as important as direct effects. Columns (1) to (3) show that the direct effect of access to government credit ranges between a reduction of 0.004 and 0.005 percentage point in the customer's death probability. The indirect exposure, affecting the customer through its suppliers' access to government credit, decreases the death probability by 0.010 percentage point in all periods. Given an average customer mortality rate of 12% after the crisis, this entails a relative decrease ranging between 11.6% and 12.5%, depending on the chosen period.

6 Robustness and Sensitivity Analysis

6.1 Industry and City Dynamic Fixed Effects

Our network-level regressions included dynamic $supplier \times year$ fixed effects (Table 4) and $customer \times year$ fixed effects (Table 5). While we control for firm characteristics that matter for explaining government-dependency in Brazil as suggested by the literature, we did not include $industry \times city \times year$ fixed effects. This allows us to focus on the broadest definition of the Brazilian production network, which is more complex than the network within the same industry and municipality.[28] The drawback of this modeling choice is that we might be simply capturing effects that only occur in some industry-city pairs, not being representative of the entire network of production. We therefore report results for the within-industry-city estimations.

[28]By definition, including dynamic effects of industry and municipalities changes the interpretation of our regressions to a within-industry-city estimation, meaning that transactions across industries and cities are no longer accounted for.

Appendix Table A.5 re-estimates Table 4 and confirms our previous findings that suppliers receive more *within-industry-city* payments from treated customers. Similarly, Appendix Table A.6 also confirms the results from Table 5. Both set of results suggest that the indirect propagation of credit shocks is also present within-industry and city pairs.

6.2 Market Concentration: Survivors Only

One could argue that our concentration results are mechanical. The reason is straightforward: firms with no access to government bank credit are in significantly more distress after Lehman, and therefore are less likely to survive. This would mechanically raise the share of government-credit-dependent firms after Lehman because their private-dependent peers would no longer exist. Thus, we now analyze the robustness of these results by re-estimating both regressions conditional on the sample of firms that survived the crisis. Table A.7 in the Appendix shows the estimation output of the same model from Table 7, but conditioning on survivor suppliers. The results on customer market share are similar.

Additionally, the supplier market share results from Table 8 is not changed when we condition the sample to include only survivor customers in Table A.8. Overall, the concentration results are robust to the survivor sample check and confirm the Herfindahl-Hirschmann Index visual evidence in Figure 2. Whereas the distributions in Panel A for customer market share remain relatively unchanged after 2008, those in Panel B reveal a clear trend towards more market concentration for suppliers after the Lehman events.

6.3 Parallel Trends

One of the concerns in DID estimation are pre-existing trends on the outcome variables. We therefore provide evidence supporting the parallel trends assumption for our outcome variables in Table A.9, in the Appendix. We choose all possible combinations of years in the pre-crisis period: (i) 2006 *versus* 2007, (ii) 2005 *versus* 2006, and (iii) 2005 *versus* 2007. We then re-run all of our baseline regressions using the same DID specifications described in Section 4. As can be seen from columns (1) to (7), none of our outcome variables show statistically-significant DID coefficients. This confirms that there are no distinctive patterns between treated and non-treated groups before the Lehman events.

6.4 Matching Estimator

As a final robustness check, we perform a propensity-score matching estimation of our baseline regressions of the indirect effects of credit shocks. We match firms using the following algorithm. For each supplier, sort its customers between treated and non-treated. Match each treated customer with a control customer satisfying an exact match in terms of credit rating, city, and industry. Among these exactly-matched potential controls, optimally choose (Abadie and Imbens (2011)) the best possible control firm by minimizing the distance between the treated firm and its control in terms of firm age, firm size, and lagged values of total credit. In addition to the parallel trends shown above, these steps allow us to obtain matched controls that satisfy the parallel trends assumption by construction.

The results are reported in Appendix Table A.10. Panel A shows the result of the supplier-centered regression (refer to Table 4) and Panel B (refer to Table 5) shows the customer-centered regression. Both sets of results confirm our previous findings.

7 Concluding Remarks

We provide firm-level evidence of the transmission and propagation of bank credit shocks throughout the production network of Brazilian firms. We do so by using the counter-cyclical policy adopted by Brazil's government banks after the Lehman Brothers' bankruptcy in September 2008. We show that firms doing business with other affected firms in the economy end up being affected by bank credit shocks indirectly through customer and supplier linkages. The most relevant implication of our study is quantifying how important these indirect effects of credit shocks are. They were about twice as important as direct bank credit shocks for firms' survival probability during the Global Financial Crisis in Brazil.

Because the reaction of several emerging market economies included liquidity expansions through government-owned banks, this paper also serves as a warning for the trade-offs involved in such interventions. Relaxing credit constraints in times of distress through government-owned banks can help firms to keep production schedules, payments to suppliers, employment, and wage bills, as shown by the "government credit multiplier" in our empirical analysis. But there are also drawbacks of these interventions, such as a persistent concentration of market share and potential misallocation. It is important to keep in mind the costs and benefits of large-scale interventions in the banking sector when approaching future episodes of financial crises.

References

Abadie, A. and Imbens, G. W. (2011). Bias-corrected matching estimators for average treatment effects. *Journal of Business & Economic Statistics*, 29(1):1–11.

Acemoglu, D., Carvalho, V. M., Ozdaglar, A., and Tahbaz-Salehi, A. (2012). The network origins of aggregate fluctuations. *Econometrica*, 80(5):1977–2016.

Acemoglu, D., Ozdaglar, A., and Tahbaz-Salehi, A. (2015). Systemic risk and stability in financial networks. *American Economic Review*, 105(2):564–608.

Acharya, V. V., Almeida, H., and Campello, M. (2013). Aggregate risk and the choice between cash and lines of credit. *Journal of Finance*, 68(5):2059–2116.

Aghabarari, L., Guettler, A., Naeem, M., and van Doornik, B. (2018). Is there help indeed, if there is help in need? the case of credit unions during the financial crisis. *Unpublished Working Paper*.

Alfaro, L., García-Santana, M., and Moral-Benito, E. (2018). On the direct and indirect real effects of credit supply shocks. *Unpublished Working Paper*.

Almeida, H. and Campello, M. (2007). Financial constraints, asset tangibility, and corporate investment. *Review of Financial Studies*, 20(5):1429–1460.

Amiti, M. and Weinstein, D. E. (2011). Exports and financial shocks. *Quarterly Journal of Economics*, 126(4):1841–1877.

Baqaee, D. R. (2018). Cascading failures in production networks. *Econometrica*, 86(5):1819–1838.

Barrot, J.-N. and Sauvagnat, J. (2016). Input specificity and the propagation of idiosyncratic shocks in production networks. *Quarterly Journal of Economics*, 131(3):1543–1592.

Barth, J. R., Caprio, G., and Levine, R. (2004). Bank regulation and supervision: what works best? *Journal of Financial Intermediation*, 13(2):205–248.

Bernstein, S., Colonnelli, E., Malacrino, D., and McQuade, T. (2018). Who creates new firms when local opportunities arise? *Unpublished Working Paper*.

Bigio, S. and La'O, J. (2016). Financial frictions in production networks. *NBER Working Paper #22212*.

Bolton, P., Freixas, X., Gambacorta, L., and Mistrulli, P. E. (2016). Relationship and transaction lending in a crisis. *Review of Financial Studies*, 29(10):2643–2676.

Bonomo, M., Brito, R. D., and Martins, B. (2015). The after crisis government-driven credit expansion in Brazil: A firm level analysis. *Journal of International Money and Finance*, 55:111–134.

Campello, M., Graham, J. R., and Harvey, C. R. (2010). The real effects of financial constraints: Evidence from a financial crisis. *Journal of Financial Economics*, 97(3):470–487.

Carvalho, D. (2014). The real effects of government-owned banks: Evidence from an emerging market. *Journal of Finance*, 69(2):577–609.

Carvalho, D., Ferreira, M. A., and Matos, P. (2015). Lending relationships and the effect of bank distress: evidence from the 2007–2009 financial crisis. *Journal of Financial and Quantitative Analysis*, 50(6):1165–1197.

Cetorelli, N. and Goldberg, L. S. (2012). Banking globalization and monetary transmission. *Journal of Finance*, 67(5):1811–1843.

Chodorow-Reich, G. (2014). The employment effects of credit market disruptions: Firm-level evidence from the 2008–9 financial crisis. *Quarterly Journal of Economics*, 129(1):1–59.

Cingano, F., Manaresi, F., and Sette, E. (2016). Does credit crunch investment down? new evidence on the real effects of the bank-lending channel. *Review of Financial Studies*, 29(10):2737–2773.

Coleman, N. and Feler, L. (2015). Bank ownership, lending, and local economic performance during the 2008–2009 financial crisis. *Journal of Monetary Economics*, 71:50–66.

Cortes, G. S. and Marcondes, R. L. (2018). The evolution of brazil's banking system. In Amann, E., Azzoni, C., and Baer, W., editors, *The Oxford Handbook of the Brazilian Economy*, chapter 10, pages 198–220. Oxford University Press.

Cuñat, V. (2006). Trade credit: suppliers as debt collectors and insurance providers. *Review of Financial Studies*, 20(2):491–527.

Cull, R. and Martinez-Peria, M. S. (2013). Bank ownership and lending patterns during the 2008-2009 financial crisis: Evidence from latin america and eastern europe. *Journal of Banking & Finance*, 37(12):4861–4878.

De Haas, R., Korniyenko, Y., Pivovarsky, A., and Tsankova, T. (2015). Taming the herd? foreign banks, the vienna initiative and crisis transmission. *Journal of Financial Intermediation*, 24(3):325–355.

Dewachter, H., Tielens, J., and Van Hove, J. (2018). Credit supply shock propagation and amplification in the real economy: Firm-level evidence. *Unpublished Working Paper*.

Fisman, R. and Love, I. (2003). Trade credit, financial intermediary development, and industry growth. *Journal of Finance*, 58(1):353–374.

Freixas, X. and Rochet, J.-C. (2008). *Microeconomics of banking*. MIT Press.

Garcia-Appendini, E. and Montoriol-Garriga, J. (2013). Firms as liquidity providers: Evidence from the 2007–2008 financial crisis. *Journal of Financial Economics*, 109(1):272–291.

Giannetti, M., Burkart, M., and Ellingsen, T. (2011). What you sell is what you lend? explaining trade credit contracts. *Review of Financial Studies*, 24(4):1261–1298.

Jordà, Ò., Schularick, M., and Taylor, A. M. (2013). When credit bites back. *Journal of Money, Credit and Banking*, 45(s2):3–28.

Khwaja, A. I. and Mian, A. (2008). Tracing the impact of bank liquidity shocks: Evidence from an emerging market. *American Economic Review*, 98(4):1413–42.

La Porta, R., Lopez-de Silanes, F., and Shleifer, A. (2002). Government ownership of banks. *Journal of Finance*, 57(1):265–301.

Noth, F. and Ossandon-Busch, M. (2017). Banking globalization, local lending and labor market outcomes: micro-level evidence from Brazil. *IWH Discussion Paper 2017/07, Halle Institute for Economic Research*.

Peek, J. and Rosengren, E. S. (1997). The international transmission of financial shocks: The case of Japan. *American Economic Review*, pages 495–505.

Petersen, M. A. (2009). Estimating standard errors in finance panel data sets: Comparing approaches. *Review of Financial Studies*, 22(1):435–480.

Rajan, R. G. and Zingales, L. (1998). Financial dependence and growth. *American Economic Review*, 88(3):559–586.

Reinhart, C. M. and Rogoff, K. S. (2009). The aftermath of financial crises. *American Economic Review*, 99(2):466–72.

Safatle, C., Borges, J., and Oliveira, R. (2016). *Anatomia de um desastre: Os bastidores da crise econômica que mergulhou o país na pior recessão da história*. Portfolio–Penguin.

Schnabl, P. (2012). The international transmission of bank liquidity shocks: Evidence from an emerging market. *Journal of Finance*, 67(3):897–932.

Summerhill, W. R., Crivelli, J. M., and Beck, T. (2005). State bank transformation in brazil–choices and consequences. *Journal of Banking & Finance*, 29(8):2223–2257.

Table 1: Summary Statistics

This table shows the summary statistics for the main variables used in the paper using the pre-crisis sample (2005–2007), except for Panel D where we use the full sample (2005–2011). Panel A presents the characteristics of supplier firms. Panel B presents the same characteristics, but for customer firms. Panel C displays characteristics defined by each supplier–customer pair, e.g., the level and number of payments that both firms transaction at a given year. Panel D shows the number of unique suppliers and customers per year, along with the total count of observations at the supplier–customer–year level.

Panel A. Supplier Characteristics

Variables	Mean	Median	Std. Dev.
Number of Employees	2,358	39	17,780
Total Annual Wage Bill (BRL)	5,891,872	42,705	47,964,447
Log(Employment) [Size]	3.37	1.59	4.25
Log(Total Wage Bill)	6.77	4.63	7.68
Share of Gov. Credit	0.241	0.001	0.348
Market Share with its Customers	0.138	0.004	0.285

Panel B. Customer Characteristics

Variables	Mean	Median	Std. Dev.
Number of Employees	9,472	196	41,477
Total Annual Wage Bill (BRL)	20,782,545	324,663	85,650,607
Log(Employment) [Size]	3.98	2.29	4.62
Log(Total Wage Bill)	7.32	5.51	7.93
Share of Gov. Credit	0.262	0.025	0.355
Market Share with its Suppliers	0.241	0.050	0.346

Panel C. Supplier–Customer Pair Characteristics

Variables	Mean	Median	Std. Dev.
Total Payments (BRL)	651,722	26,272	49,150,542
Number Payments	6.66	2.00	42.88
Average Payment (BRL)	51,127	12,262	1,262,169

Panel D. Unique Firms and Observations Per Year

Year	Suppliers	Customers	Supplier–Customer–Year Obs
2005	386,459	198,682	1,596,420
2006	431,810	225,402	1,781,742
2007	493,180	264,150	2,042,654
2008	583,174	332,560	2,463,389
2009	631,821	371,553	2,589,143
2010	798,589	469,359	3,306,069
2011	954,210	580,991	3,973,403
Total	1,985,439	1,351,320	17,752,820

Table 2: *Government Banks and Counter-Cyclical Lending after the Lehman Bankruptcy*

This table reports output from estimating Equation (1). *The dependent variable is the log of outstanding credit of firm i with bank b. The coefficient of interest is the Difference-in-Differences estimator given by the interaction* $Gov_b \times Post_t$. Gov_b *is an indicator variable that equals one if bank b is government-owned, and is zero otherwise.* $Post_t$ *is an indicator variable with value of one if year* $t \geq 2009$ *and zero if* $t \leq 2007$. *The year of 2008 is not included in the event study window for being the year of Lehman Brothers' failure. Bank-level controls follow Schnabl (2012) and include: size (natural logarithm of lagged total assets), return on assets, credit share, liquid assets share (Basel III-defined), deposit share, and equity share. As detailed in Figure 6, in column (1), we use a time window of 1 year after versus 1 year before the Lehman bankruptcy. In column (2), we expand the time window to include 2 years before and 2 years after. In column (3), the time window is expanded to 3 years before and after. Significance levels: * p <0.10, ** p <0.05, *** p <0.01.*

Dependent Variable	*Log(Credit)*		
	(1)	(2)	(3)
	2009 *vs.* 2007	2010:2009 *vs.* 2007:2006	2011:2009 *vs.* 2007:2005
DID Period	(±1 Year)	(±2 Years)	(±3 Years)
$Gov_b \times Post_t$	0.398***	0.288**	0.343**
	(0.143)	(0.145)	(0.162)
Bank Controls			
Size	0.431***	0.456***	0.435***
	(0.066)	(0.076)	(0.114)
ROA	4.344***	3.442	–4.340
	(0.819)	(4.340)	(3.673)
Liquid Assets Ratio	–4.093***	–3.234***	–3.011***
	(0.449)	(0.487)	(0.511)
Deposits Share	–0.268	0.272	0.913*
	(0.588)	(0.350)	(0.526)
Equity Share	1.418***	1.160**	1.246*
	(0.474)	(0.577)	(0.755)
Bank FE	Yes	Yes	Yes
Firm×Year FE	Yes	Yes	Yes
Clustered SE	Bank, Year	Bank, Year	Bank, Year
N. Observations	5,359,393	10,901,246	15,639,540
R-Squared	0.801	0.796	0.787

Table 3: *Direct Effects of Government-Bank-Driven Credit Shocks: Employment and Wages*

*This table reports output from Eq. (2). The dependent variables are the log of employment in columns (1) to (3) and the log of the total wage bill in columns (4) to (6). The coefficient of interest is the Difference-in-Differences estimator given by the interaction $Gov_i \times Post_t$. Gov_i is an indicator variable that equals one if firm i is government-credit-dependent (i.e., if its share of credit with government banks is greater than zero), and is zero otherwise. $Post_t$ is an indicator variable with value of one if year $t \geq 2009$ and zero if $t \leq 2007$. The year of 2008 is not included in the event study window for being the year of Lehman Brothers' failure. Firm controls include age, size, and credit rating. It is not possible to include the original definition of size (i.e., the log of the total number of employees) in columns (1) to (3) because it is used as the dependent variable. The proxy for firm size in these columns is therefore the firm's social capital, obtained from their tax returns. In Columns (4) to (6), the original definition of size is used. As detailed in Figure 6, in columns (1) and (4), we use a time window of 1 year after versus 1 year before the Lehman bankruptcy. In columns (2) and (5), we expand the time window to include 2 years before and 2 years after. In columns (3) and (6), the time window is expanded to 3 years before and after. Statistical significance levels: *** p-value<0.01, ** p-value<0.05, * p-value<0.10.*

Dependent Variable	*Log(Employment)*			*Log(Wage Bill)*		
DID Period	(1) 2009 *vs.* 2007 (±1 Year)	(2) 2010:2009 *vs.* 2007:2006 (±2 Years)	(3) 2011:2009 *vs.* 2007:2005 (±3 Years)	(4) 2009 *vs.* 2007 (±1 Year)	(5) 2010:2009 *vs.* 2007:2006 (±2 Years)	(6) 2011:2009 *vs.* 2007:2005 (±3 Years)
$Gov_i \times Post_t$	0.051***	0.046***	0.045***	0.066***	0.067***	0.075***
	(0.006)	(0.005)	(0.006)	(0.006)	(0.007)	(0.011)
Firm Controls						
Age	0.022***	0.034***	0.042***	0.026***	0.041***	0.052***
	(0.004)	(0.005)	(0.004)	(0.002)	(0.005)	(0.005)
Size	0.025***	0.021***	0.022***	0.023***	0.019***	0.020***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Credit Rating	0.010***	0.015***	0.017***	0.013***	0.019***	0.022***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry×City×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SE	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year	Firm, Year
N. Observations	4,576,744	9,233,987	14,030,977	4,576,744	9,233,987	14,030,977
R-Squared	0.807	0.792	0.791	0.807	0.822	0.813

Table 4: *Indirect Effects: Suppliers receive more payments from Treated Customers*

This table reports output from Equation (3). *The dependent variable is the log of real payments received by a supplier from its customers. The coefficient of interest is the Difference-in-Differences estimator given by the interaction* $Gov_c \times Post_t$. Gov_c *is an indicator variable that equals one if customer c is government-credit-dependent (i.e., if its share of credit with government banks is greater than zero), and is zero otherwise.* $Post_t$ *is an indicator variable with value of one if year* $t \geq 2009$ *and zero if* $t \leq 2007$. *The year of 2008 is not included in the event study window for being the year of Lehman Brothers' failure. Customer controls include age, size, and credit rating. As detailed in Figure 6, in column (1), we use a time window of 1 year after versus 1 year before the Lehman bankruptcy. In column (2), we expand the time window to include 2 years before and 2 years after. In column (3), the time window is expanded to 3 years before and after. Statistical significance levels: *** p-value<0.01, ** p-value<0.05, * p-value<0.10.*

Dependent Variable Downstream DID:	*Log(Real Payments)* *(Inflow to a Supplier from Treated vs. Non-Treated Customers)*		
DID Period	(1) 2009 *vs.* 2007 (±1 Year)	(2) 2010:2009 *vs.* 2007:2006 (±2 Years)	(3) 2011:2009 *vs.* 2007:2005 (±3 Years)
$Gov_c \times Post_t$	0.025***	0.036***	0.040***
	(0.008)	(0.013)	(0.014)
Customer Controls			
Age	0.020	0.017*	0.024**
	(0.018)	(0.010)	(0.010)
Size	0.029***	0.028***	0.032***
	(0.002)	(0.002)	(0.004)
Credit Rating	0.011***	0.013***	0.015***
	(0.000)	(0.002)	(0.003)
Fixed Effects			
Customer	Yes	Yes	Yes
Supplier×Year	Yes	Yes	Yes
Clustered SE	Supplier, Year	Supplier, Year	Supplier, Year
N. Observations	3,754,746	7,889,679	12,430,443
R-Squared	0.538	0.520	0.511

Table 5: *Indirect Effects: Customers buy more from Treated Suppliers*

This table reports output from Equation (4). *The dependent variable is the log of real payments sent by a customer to its suppliers. The coefficient of interest is the Difference-in-Differences (DID) estimator given by the interaction* $Gov_s \times Post_t$. Gov_s *is an indicator variable that equals one if supplier s is government-credit-dependent (i.e., if its share of credit with government banks is greater than zero), and is zero otherwise.* $Post_t$ *is an indicator variable with value of one if year* $t \geq 2009$ *and zero if* $t \leq 2007$. *The year of 2008 is not included in the event study window for being the year of Lehman Brothers' failure. Supplier controls include age, size, and credit rating. As detailed in Figure 6, in column (1), we use a time window of 1 year after versus 1 year before the Lehman bankruptcy. In column (2), we expand the time window to include 2 years before and 2 years after. In column (3), the time window is expanded to 3 years before and after. Statistical significance levels: *** p-value<0.01, ** p-value<0.05, * p-value<0.10.*

Dependent Variable Upstream DID:	*Log(Real Payments)* *(Outflow from a Customer to Treated vs. Non-Treated Supplier)*		
DID Period	(1) 2009 *vs.* 2007 (±1 Year)	(2) 2010:2009 *vs.* 2007:2006 (±2 Years)	(3) 2011:2009 *vs.* 2007:2005 (±3 Years)
$Gov_s \times Post_t$	0.035***	0.042***	0.054***
	(0.000)	(0.005)	(0.011)
Supplier Controls			
Age	–0.010	0.025***	–0.021***
	(0.008)	(0.004)	(0.003)
Size	0.048***	0.047***	0.050***
	(0.003)	(0.002)	(0.002)
Credit Rating	0.012**	0.015***	0.016***
	(0.005)	(0.004)	(0.002)
Fixed Effects			
Supplier	Yes	Yes	Yes
Customer×Year	Yes	Yes	Yes
Clustered SE	Customer, Year	Customer, Year	Customer, Year
N. Observations	4,183,704	8,779,779	13,858,964
R-Squared	0.516	0.494	0.483

Table 6: *Amplification Mechanisms: Financial Constraints and Input Innovation*

This table shows the estimation of the amplification mechanisms studied in Section 4.3. Panel A reports the estimation output of Equation (8). Panel B shows the estimates of Equation (6). The dependent variable in both panels is the log of real payments between customers and suppliers. The coefficient of interest in both panels is the Difference-in-Difference-in-Differences (DIDID) estimator given by the triple interaction. In Panel A, the DIDID coefficient is given by $Priv_s \times Post_t \times Low\ Tangib_c$. *In Panel B, it is given by* $Priv_s \times Post_t \times Input\ Innov_s$. *The dummy variable* $Low\ Tangib_c$ *takes value of one if the tangibility index constructed in Equation (7) is lower than the median, and is zero otherwise. Similarly,* $Input\ Innov_s$ *is an indicator variable that takes value one if the input innovation index constructed in Equation (5) is greater than the median, and is zero otherwise.* $Priv_s$ *is an indicator variable that equals one if supplier s is private-credit-dependent (i.e., if its share of credit with government banks is equal to zero), and is zero otherwise.* $Post_t$ *is an indicator variable with value of one if year* $t \geq 2009$ *and zero if* $t \leq 2007$. *The year of 2008 is not included in the event study window for being the year of Lehman Brothers' failure. Supplier controls include age, size, and credit rating. As detailed in Figure 6, in column (1), we use a time window of 1 year after versus 1 year before the Lehman bankruptcy. In column (2), we expand the time window to include 2 years before and 2 years after. In column (3), the time window is expanded to 3 years before and after. Statistical significance levels: *** p-value<0.01, ** p-value<0.05, * p-value<0.10.*

Panel A. Customer's Financial Constraint as an Amplification Mechanism

Dependent Variable	*Log(Real Payments) (Inflow to a Supplier from Treated vs. Non-Treated Customers)*		
	(1)	(2)	(3)
DID Period	2009 *vs.* 2007 ($\pm$1 Year)	2010:2009 *vs.* 2007:2006 ($\pm$2 Years)	2011:2009 *vs.* 2007:2005 ($\pm$3 Years)
$Priv_c \times Post_t$	–0.006	–0.012	–0.009
	(0.017)	(0.008)	(0.012)
$Priv_c \times Post_t \times Low\ Tangib_c$	–0.033***	–0.029***	–0.028*
	(0.003)	(0.010)	(0.015)
Customer Controls	Yes	Yes	Yes
Customer FE	Yes	Yes	Yes
Supplier×Year FE	Yes	Yes	Yes
Clustered SE	Supplier, Year	Supplier, Year	Supplier, Year
N. Observations	378,030	794,241	1,245,073
R-Squared	0.570	0.550	0.538

Panel B. Supplier's Input Innovation as an Amplification Mechanism

Dependent Variable	*Log(Real Payments) (Outflow from a Customer to Treated vs. Non-Treated Suppliers)*		
	(1)	(2)	(3)
DID Period	2009 *vs.* 2007 ($\pm$1 Year)	2010:2009 *vs.* 2007:2006 ($\pm$2 Years)	2011:2009 *vs.* 2007:2005 ($\pm$3 Years)
$Priv_s \times Post_t$	0.048***	0.200**	0.241***
	(0.009)	(0.095)	(0.068)
$Priv_s \times Post_t \times Input\ Innov_s$	–0.069*	–0.165**	–0.161***
	(0.041)	(0.072)	(0.036)
Supplier Controls	Yes	Yes	Yes
Supplier FE	Yes	Yes	Yes
Customer×Year FE	Yes	Yes	Yes
Clustered SE	Customer, Year	Customer, Year	Customer, Year
N. Observations	119,623	225,158	406,957
R-Squared	0.626	0.589	0.578

Table 7: *Indirect Effects: Market Share of a Customer*

This table reports output from Equation (3). *The dependent variable is the share of payments that a customer has with respect to its suppliers' total payments. This share of payments is interpreted as a measure of the market share of the customer. The coefficient of interest is the Difference-in-Differences estimator given by the interaction* $Gov_c \times Post_t$. Gov_c *is an indicator variable that equals one if customer c is government-credit-dependent (i.e., if its share of credit with government banks is greater than zero), and is zero otherwise.* $Post_t$ *is an indicator variable with value of one if year* $t \geq 2009$ *and zero if* $t \leq 2007$. *The year of 2008 is not included in the event study window for being the year of Lehman Brothers' failure. Customer controls include age, size, and credit rating. As detailed in Figure 6, in column (1), we use a time window of 1 year after versus 1 year before the Lehman bankruptcy. In column (2), we expand the time window to include 2 years before and 2 years after. In column (3), the time window is expanded to 3 years before and after. Statistical significance levels: *** p-value<0.01, ** p-value<0.05, * p-value<0.10.*

Dependent Variable	*Share of Payments of a Customer with its Suppliers (Market Share of Customer)*		
	(1)	(2)	(3)
DID Period	2009 *vs.* 2007 ($\pm$1 Year)	2010:2009 *vs.* 2007:2006 ($\pm$2 Years)	2011:2009 *vs.* 2007:2005 ($\pm$3 Years)
$Gov_c \times Post_t$	0.002***	0.004	0.004
	(0.000)	(0.002)	(0.002)
Customer Controls			
Age	–0.002**	0.001	0.002
	(0.001)	(0.001)	(0.001)
Size	0.003***	0.004***	0.004***
	(0.000)	(0.000)	(0.000)
Credit Rating	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)
Fixed Effects			
Customer	Yes	Yes	Yes
Supplier×Year	Yes	Yes	Yes
Clustered SE	Supplier, Year	Supplier, Year	Supplier, Year
N. Observations	3,754,746	7,889,679	12,430,443
R-Squared	0.832	0.820	0.810

Table 8: *Indirect Effects: Market Share of a Supplier*

This table reports output from Equation (4)*. The dependent variable is the share of payments that a supplier has with respect to its customers' total payments. This share of payments is interpreted as a measure of the market share of the supplier. The coefficient of interest is the Difference-in-Differences (DID) estimator given by the interaction* $Gov_s \times Post_t$*.* Gov_s *is an indicator variable that equals one if supplier s is government-credit-dependent (i.e., if its share of credit with government banks is greater than zero), and is zero otherwise.* $Post_t$ *is an indicator variable with value of one if year* $t \geq 2009$ *and zero if* $t \leq 2007$*. The year of 2008 is not included in the event study window for being the year of Lehman Brothers' failure. Supplier controls include age, size, and credit rating. As detailed in Figure 6, in column (1), we use a time window of 1 year after versus 1 year before the Lehman bankruptcy. In column (2), we expand the time window to include 2 years before and 2 years after. In column (3), the time window is expanded to 3 years before and after. Statistical significance levels: *** p-value<0.01, ** p-value<0.05, * p-value<0.10.*

Dependent Variable	*Share of Payments of a Supplier with its Customers (Market Share of Supplier)*		
	(1)	(2)	(3)
DID Period	2009 *vs.* 2007 ($\pm$1 Year)	2010:2009 *vs.* 2007:2006 ($\pm$2 Years)	2011:2009 *vs.* 2007:2005 ($\pm$3 Years)
$Gov_s \times Post_t$	0.002***	0.004***	0.005***
	(0.000)	(0.001)	(0.002)
Supplier Controls			
Age	–0.001***	–0.001***	–0.001
	(0.000)	(0.000)	(0.001)
Size	0.002***	0.002***	0.002***
	(0.000)	(0.000)	(0.000)
Credit Rating	0.002***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)
Fixed Effects			
Supplier	Yes	Yes	Yes
Customer$\times$Year	Yes	Yes	Yes
Clustered SE	Customer, Year	Customer, Year	Customer, Year
N. Observations	4,183,704	8,779,779	13,858,964
R-Squared	0.882	0.865	0.852

Table 9: *The Government Lending Multiplier: Supplier and Customer Death Probability*

This table reports output from Equation (10)*, where the dependent variable is a dummy of firm "death" (i.e., if the firm becomes inactive). Panel A shows the probit estimation for the suppliers and Panel B for the customers. In both panels, the coefficients of interest are Gov, that measures the direct impact of the firm's access to government credit, and Indirect Gov Exp, which captures the effect of indirect exposure to government credit through its creditors. The index of indirect exposure to the government credit shock is constructed as shown in Equation* (9) *for the case of suppliers. In Panel A, controls at the supplier-level include size, age, and credit rating. Also in Panel A, to account for different relationships that suppliers have with their customers, we control for the number of relationships of each supplier and include the standard set of customer characteristics averaged over each supplier (average customer size, age, and credit rating), as well as the customers' average number of relationships and average death rate. We also include industry × city fixed effects in the 1-year post-crisis specification (i.e., 2009), and industry × city × year fixed effects in the 2-year (2009-2010) and 3-year (2009-2011) specifications. In Panel B, we follow exactly the same procedures described above for the supplier's probit, but focusing on the case customers. Statistical significance levels: *** p-value<0.01, ** p-value<0.05, * p-value<0.10.*

Panel A. Death Probability of Suppliers

Dependent Variable	*Dummy: Supplier Death*		
	(1)	(2)	(3)
Period	1 Year: 2009	2 Years: 2009–2010	3 Years: 2009–2011
Gov_s	–0.008***	–0.006***	–0.007***
	(0.002)	(0.002)	(0.002)
$Indirect\ Gov\ Exp_s$	–0.017***	–0.016***	–0.012***
	(0.001)	(0.001)	(0.001)
Firm Controls			
Supplier	Yes	Yes	Yes
Customer (Avg. over Suppliers)	Yes	Yes	Yes
Fixed Effects			
Supplier Industry×City	Yes	No	No
Supplier Industry×City×Year	No	Yes	Yes
Clustered SE	Supplier	Supplier, Year	Supplier, Year
N. Observations	860,730	1,946,902	3,264,635
R-Squared	0.107	0.117	0.132

Panel B. Death Probability of Customers

Dependent Variable	*Dummy: Customer Death*		
	(1)	(2)	(3)
Period	1 Year: 2009	2 Years: 2009–2010	3 Years: 2009–2011
Gov_c	–0.005***	–0.004***	–0.004***
	(0.002)	(0.002)	(0.002)
$Indirect\ Gov\ Exp_c$	–0.010***	–0.010***	–0.010***
	(0.001)	(0.001)	(0.001)
Firm Controls			
Customer	Yes	Yes	Yes
Supplier (Avg. over Customers)	Yes	Yes	Yes
Fixed Effects			
Customer Industry×City	Yes	No	No
Customer Industry×City×Year	No	Yes	Yes
Clustered SE	Customer	Customer, Year	Customer, Year
N. Observations	329,385	748,709	1,274,321
R-Squared	0.112	0.126	0.142

Credit Shock Propagation in Firm Networks: Evidence from Government Bank Credit Expansions

ONLINE APPENDIX

Credit Shock Propagation in Firm Networks: Evidence from Government Bank Credit Expansions

ONLINE APPENDIX

Table A.1: *Indirect Effects: Suppliers receive a greater number of payments from Treated Customers*

This table reports output from Equation (3)*. The dependent variable is the number of payments received by a supplier from its customers (instead of its real value). The coefficient of interest is the Difference-in-Differences estimator given by the interaction $Gov_c \times Post_t$. Gov_c is an indicator variable that equals one if customer c is government-credit-dependent (i.e., if its share of credit with government banks is greater than the median), and is zero otherwise. $Post_t$ is an indicator variable with value of one if year $t \geq 2009$ and zero if $t \leq 2007$. The year of 2008 is not included in the event study window for being the year of Lehman Brothers' failure. Customer controls include age, size, and credit rating. As detailed in Figure 6, in column (1), we use a time window of 1 year after versus 1 year before the Lehman bankruptcy. In column (2), we expand the time window to include 2 years before and 2 years after. In column (3), the time window is expanded to 3 years before and after. Statistical significance levels: *** p-value<0.01, ** p-value<0.05, * p-value<0.10.*

Dependent Variable Downstream DID:	*Log(Number of Payments)* *(Inflow to a Supplier from Treated vs. Non-Treated Customers)*		
DID Period	(1) 2009 *vs.* 2007 (±1 Year)	(2) 2010:2009 *vs.* 2007:2006 (±2 Years)	(3) 2011:2009 *vs.* 2007:2005 (±3 Years)
$Gov_c \times Post_t$	0.016***	0.032**	0.037***
	(0.002)	(0.016)	(0.012)
Customer Controls			
Age	–0.021*	0.002	–0.005
	(0.012)	(0.005)	(0.007)
Size	0.025***	0.021***	0.022***
	(0.003)	(0.002)	(0.002)
Credit Rating	0.001	0.002***	0.002***
	(0.001)	(0.001)	(0.001)
Fixed Effects			
Customer	Yes	Yes	Yes
Supplier×Year	Yes	Yes	Yes
Clustered SE	Supplier, Year	Supplier, Year	Supplier, Year
N. Observations	3,754,746	7,889,679	12,430,443
R-Squared	0.487	0.520	0.511

Table A.2: *Indirect Effects: Customers make a greater number of payments to Treated Suppliers*

This table reports output from Equation (4)*, using as dependent variable the number of payments sent by a customer to its suppliers (instead of its real value). The coefficient of interest is the Difference-in-Differences (DID) estimator given by the interaction $Gov_s \times Post_t$. Gov_s is an indicator variable that equals one if supplier s is government-credit-dependent (i.e., if its share of credit with government banks is greater than the median), and is zero otherwise. $Post_t$ is an indicator variable with value of one if year $t \geq$ 2009 and zero if $t \leq$ 2007. The year of 2008 is not included in the event study window for being the year of Lehman Brothers' failure. Supplier controls include age, size, and credit rating. As detailed in Figure 6, in column (1), we use a time window of 1 year after versus 1 year before the Lehman bankruptcy. In column (2), we expand the time window to include 2 years before and 2 years after. In column (3), the time window is expanded to 3 years before and after. Statistical significance levels: *** p-value<0.01, ** p-value<0.05, * p-value<0.10.*

Dependent Variable Upstream DID:	*Log(Number of Payments)* *(Outflow from a Customer to Treated vs. Non-Treated Supplier)*		
	(1)	(2)	(3)
DID Period	2009 *vs.* 2007	2010:2009 *vs.* 2007:2006	2011:2009 *vs.* 2007:2005
	(±1 Year)	(±2 Years)	(±3 Years)
$Gov_s \times Post_t$	0.020***	0.031***	0.041***
	(0.000)	(0.008)	(0.010)
Supplier Controls			
Age	–0.016**	–0.020***	–0.017***
	(0.008)	(0.002)	(0.001)
Size	0.030***	0.027***	0.027***
	(0.002)	(0.001)	(0.001)
Credit Rating	0.003	0.005**	0.004***
	(0.003)	(0.002)	(0.001)
Fixed Effects			
Supplier	Yes	Yes	Yes
Customer×Year	Yes	Yes	Yes
Clustered SE	Customer, Year	Customer, Year	Customer, Year
N. Observations	4,183,704	8,779,779	13,858,964
R-Squared	0.451	0.425	0.413

Table A.3: *Indirect Effects: Suppliers do not receive greater average payments from Treated Customers*

This table reports output from Equation (4)*, using as dependent variable the average value of payments sent by a customer to its suppliers (constructed as the log of real payments/number of payments). The dependent variable is the log of real payments received by a supplier from its customers. The coefficient of interest is the Difference-in-Differences estimator given by the interaction* $Gov_c \times Post_t$. Gov_c *is an indicator variable that equals one if customer c is government-credit-dependent (i.e., if its share of credit with government banks is greater than the median), and is zero otherwise.* $Post_t$ *is an indicator variable with value of one if year* $t \geq 2009$ *and zero if* $t \leq 2007$. *The year of 2008 is not included in the event study window for being the year of Lehman Brothers' failure. Customer controls include age, size, and credit rating. As detailed in Figure 6, in column (1), we use a time window of 1 year after versus 1 year before the Lehman bankruptcy. In column (2), we expand the time window to include 2 years before and 2 years after. In column (3), the time window is expanded to 3 years before and after. Statistical significance levels: *** p-value<0.01, ** p-value<0.05, * p-value<0.10.*

Dependent Variable Downstream DID:	*Log(Average Payment)* *(Inflow to a Supplier from Treated vs. Non-Treated Customers)*		
DID Period	(1) 2009 *vs.* 2007 (±1 Year)	(2) 2010:2009 *vs.* 2007:2006 (±2 Years)	(3) 2011:2009 *vs.* 2007:2005 (±3 Years)
$Gov_c \times Post_t$	0.006	0.000	–0.003
	(0.004)	(0.008)	(0.006)
Customer Controls			
Age	0.048***	0.015	0.031**
	(0.002)	(0.011)	(0.012)
Size	–0.001	0.002	0.006*
	(0.001)	(0.002)	(0.003)
Credit Rating	0.008***	0.011***	0.012***
	(0.001)	(0.003)	(0.003)
Fixed Effects			
Customer	Yes	Yes	Yes
Supplier×Year	Yes	Yes	Yes
Clustered SE	Supplier, Year	Supplier, Year	Supplier, Year
N. Observations	3,754,746	7,889,679	12,430,443
R-Squared	0.584	0.572	0.569

Table A.4: *Indirect Effects: Customers increase their average payment to Treated Suppliers*

This table reports output from Equation (4)*, using as dependent variable the average value of payments sent by a customer to its suppliers (constructed as the log of real payments/number of payments). The coefficient of interest is the Difference-in-Differences (DID) estimator given by the interaction* $Gov_s \times Post_t$*.* Gov_s *is an indicator variable that equals one if supplier s is government-credit-dependent (i.e., if its share of credit with government banks is greater than the median), and is zero otherwise.* $Post_t$ *is an indicator variable with value of one if year* $t \geq 2009$ *and zero if* $t \leq 2007$*. The year of 2008 is not included in the event study window for being the year of Lehman Brothers' failure. Supplier controls include age, size, and credit rating. As detailed in Figure 6, in column (1), we use a time window of 1 year after versus 1 year before the Lehman bankruptcy. In column (2), we expand the time window to include 2 years before and 2 years after. In column (3), the time window is expanded to 3 years before and after. Statistical significance levels: *** p-value<0.01, ** p-value<0.05, * p-value<0.10.*

Dependent Variable Upstream DID:	*Log(Average Payment)* *(Outflow from a Customer to Treated vs. Non-Treated Suppliers)*		
DID Period	(1) 2009 *vs.* 2007 (±1 Year)	(2) 2010:2009 *vs.* 2007:2006 (±2 Years)	(3) 2011:2009 *vs.* 2007:2005 (±3 Years)
$Gov_s \times Post_t$	0.009***	0.005*	0.005**
	(0.000)	(0.003)	(0.002)
Supplier Controls			
Age	0.010***	–0.001	–0.001
	(0.000)	(0.002)	(0.001)
Size	0.013***	0.016***	0.017***
	(0.000)	(0.003)	(0.002)
Credit Rating	0.008***	0.009***	0.010***
	(0.002)	(0.002)	(0.001)
Fixed Effects			
Supplier	Yes	Yes	Yes
Customer×Year	Yes	Yes	Yes
Clustered SE	Customer, Year	Customer, Year	Customer, Year
N. Observations	4,183,704	8,779,779	13,858,964
R-Squared	0.569	0.561	0.561

Table A.5: *Robustness: Suppliers receive more payments from Treated Customers, Additional Fixed Effects*

*This table reports output from Equation (3), except that it also includes Customer Industry $\times$ City $\times$ Year fixed effects. The dependent variable is the log of real payments received by a supplier from its customers. The coefficient of interest is the Difference-in-Differences estimator given by the interaction $Gov_c \times Post_t$. Gov_c is an indicator variable that equals one if customer c is government-credit-dependent (i.e., if its share of credit with government banks is greater than the median), and is zero otherwise. $Post_t$ is an indicator variable with value of one if year $t \geq 2009$ and zero if $t \leq 2007$. The year of 2008 is not included in the event study window for being the year of Lehman Brothers' failure. Customer controls include age, size, and credit rating. As detailed in Figure 6, in column (1), we use a time window of 1 year after versus 1 year before the Lehman bankruptcy. In column (2), we expand the time window to include 2 years before and 2 years after. In column (3), the time window is expanded to 3 years before and after. Statistical significance levels: *** p-value<0.01, ** p-value<0.05, * p-value<0.10.*

Dependent Variable Downstream DID:	*Log(Real Payments)* *(Inflow to a Supplier from Treated vs. Non-Treated Customers)*		
	(1)	(2)	(3)
DID Period	2009 *vs.* 2007	2010:2009 *vs.* 2007:2006	2011:2009 *vs.* 2007:2005
	($\pm$1 Year)	($\pm$2 Years)	($\pm$3 Years)
$Gov_c \times Post_t$	0.022*	0.034**	0.039***
	(0.012)	(0.014)	(0.014)
Customer Controls			
Age	0.016	0.017	0.028***
	(0.016)	(0.027)	(0.010)
Size	0.029***	0.030***	0.034***
	(0.002)	(0.002)	(0.004)
Credit Rating	0.011***	0.016***	0.018***
	(0.000)	(0.002)	(0.003)
Fixed Effects			
Customer	Yes	Yes	Yes
Customer Industry$\times$City$\times$Year	Yes	Yes	Yes
Supplier$\times$Year	Yes	Yes	Yes
Clustered SE	Supplier, Year	Supplier, Year	Supplier, Year
N. Observations	3,754,746	7,889,679	12,430,443
R-Squared	0.544	0.527	0.518

Table A.6: Robustness: Customers buy more from Treated Suppliers, Additional Fixed Effects

*This table reports output from Equation (4), except that it also includes Supplier Industry $\times$ City $\times$ Year fixed effects. The dependent variable is the log of real payments sent by a customer to its suppliers. The coefficient of interest is the Difference-in-Differences (DID) estimator given by the interaction $Gov_s \times Post_t$. Gov_s is an indicator variable that equals one if supplier s is government-credit-dependent (i.e., if its share of credit with government banks is greater than the median), and is zero otherwise. $Post_t$ is an indicator variable with value of one if year $t \geq 2009$ and zero if $t \leq 2007$. The year of 2008 is not included in the event study window for being the year of Lehman Brothers' failure. Supplier controls include age, size, and credit rating. As detailed in Figure 6, in column (1), we use a time window of 1 year after versus 1 year before the Lehman bankruptcy. In column (2), we expand the time window to include 2 years before and 2 years after. In column (3), the time window is expanded to 3 years before and after. Statistical significance levels: *** p-value<0.01, ** p-value<0.05, * p-value<0.10.*

Dependent Variable Upstream DID:	*Log(Real Payments)* *(Outflow from a Customer to Treated vs. Non-Treated Suppliers)*		
DID Period	(1) 2009 *vs.* 2007 (±1 Year)	(2) 2010:2009 *vs.* 2007:2006 (±2 Years)	(3) 2011:2009 *vs.* 2007:2005 (±3 Years)
$Gov_s \times Post_t$	0.032***	0.041***	0.048***
	(0.001)	(0.004)	(0.009)
Supplier Controls			
Age	–0.010	0.024***	–0.021***
	(0.009)	(0.004)	(0.002)
Size	0.049***	0.048***	0.049***
	(0.003)	(0.002)	(0.002)
Credit Rating	0.014***	0.016***	0.015***
	(0.002)	(0.004)	(0.003)
Fixed Effects			
Supplier	Yes	Yes	Yes
Supplier Industry×City×Year	Yes	Yes	Yes
Customer×Year	Yes	Yes	Yes
Clustered SE	Customer, Year	Customer, Year	Customer, Year
N. Observations	4,183,704	8,779,779	13,858,964
R-Squared	0.528	0.504	0.493

Table A.7: *Robustness: Market Share of a Customer, Survivor Suppliers Subsample*

This table reports output from Equation (3)*, conditioning on the sample of suppliers that survive the Global Financial Crisis. The dependent variable is the share of payments that a customer has with respect to its suppliers' total payments. This share of payments is interpreted as a measure of the market share of the customer. The coefficient of interest is the Difference-in-Differences estimator given by the interaction* $Gov_c \times Post_t$. Gov_c *is an indicator variable that equals one if customer c is government-credit-dependent (i.e., if its share of credit with government banks is greater than the median), and is zero otherwise.* $Post_t$ *is an indicator variable with value of one if year* $t \geq 2009$ *and zero if* $t \leq 2007$*. The year of 2008 is not included in the event study window for being the year of Lehman Brothers' failure. Customer controls include age, size, and credit rating. As detailed in Figure 6, in column (1), we use a time window of 1 year after versus 1 year before the Lehman bankruptcy. In column (2), we expand the time window to include 2 years before and 2 years after. In column (3), the time window is expanded to 3 years before and after. Statistical significance levels: *** p-value<0.01, ** p-value<0.05, * p-value<0.10.*

Dependent Variable	*Share of Payments of a Customer with its Survivor Suppliers (Market Share of Customer)*		
	(1)	(2)	(3)
DID Period	2009 *vs.* 2007 (±1 Year)	2010:2009 *vs.* 2007:2006 (±2 Years)	2011:2009 *vs.* 2007:2005 (±3 Years)
$Gov_c \times Post_t$	0.001***	0.004	0.003
	(0.000)	(0.003)	(0.003)
Customer Controls			
Age	–0.003***	0.001	0.002
	(0.001)	(0.001)	(0.002)
Size	0.004***	0.005***	0.005***
	(0.001)	(0.001)	(0.001)
Credit Rating	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)
Fixed Effects			
Customer	Yes	Yes	Yes
Supplier×Year	Yes	Yes	Yes
Clustered SE	Supplier, Year	Supplier, Year	Supplier, Year
N. Observations	2,470,728	5,264,289	8,406,959
R-Squared	0.832	0.818	0.808

Table A.8: *Robustness: Market Share of a Supplier, Survivor Customers Subsample*

This table reports output from Equation (4)*, conditioning on the sample of customers that survive the Global Financial Crisis. The dependent variable is the share of payments that a supplier has with respect to its customers' total payments. This share of payments is interpreted as a measure of the market share of the supplier. The coefficient of interest is the Difference-in-Differences (DID) estimator given by the interaction $Gov_s \times Post_t$. Gov_s is an indicator variable that equals one if supplier s is government-credit-dependent (i.e., if its share of credit with government banks is greater than the median), and is zero otherwise. $Post_t$ is an indicator variable with value of one if year $t \geq$ 2009 and zero if $t \leq$ 2007. The year of 2008 is not included in the event study window for being the year of Lehman Brothers' failure. Supplier controls include age, size, and credit rating. As detailed in Figure 6, in column (1), we use a time window of 1 year after versus 1 year before the Lehman bankruptcy. In column (2), we expand the time window to include 2 years before and 2 years after. In column (3), the time window is expanded to 3 years before and after. Statistical significance levels: *** p-value<0.01, ** p-value<0.05, * p-value<0.10.*

Dependent Variable	*Share of Payments of a Supplier with its Survivor Customers (Market Share of Supplier)*		
	(1)	(2)	(3)
DID Period	2009 *vs.* 2007 (±1 Year)	2010:2009 *vs.* 2007:2006 (±2 Years)	2011:2009 *vs.* 2007:2005 (±3 Years)
$Gov_s \times Post_t$	0.002***	0.004***	0.006***
	(0.000)	(0.001)	(0.002)
Supplier Controls			
Age	0.000	–0.001	–0.001
	(0.000)	(0.001)	(0.001)
Size	0.002***	0.002***	0.002***
	(0.000)	(0.000)	(0.000)
Credit Rating	0.001*	0.001**	0.001***
	(0.000)	(0.000)	(0.000)
Fixed Effects			
Supplier	Yes	Yes	Yes
Customer×Year	Yes	Yes	Yes
Clustered SE	Customer, Year	Customer, Year	Customer, Year
N. Observations	2,793,409	5,939,501	9,498,460
R-Squared	0.918	0.902	0.889

Table A.9: *Pre-Existing Trends in Financial and Real-side Firm Outcomes*

This table presents tests of pre-existing trends by reporting estimates of Difference-in-Differences coefficients for all outcome variables for the pre-crisis period. We choose all possible combinations of years in the pre-crisis period: (i) 2006 versus 2007, (ii) 2005 versus 2006, and (iii) 2005 versus 2007. We then re-run all baseline regressions using the same DID specifications described in Section 4.

DID for Outcome Variable	Gov–Private Credit (1)	Total Employment (2)	Total Wage Bill (3)	Customer DID Real Payments (4)	Supplier DID Real Payments (5)	Customer's Share of Payments (6)	Supplier's Share of Payments (7)
2006 *vs.* 2007	0.063	0.002	–0.128	0.008	0.005	–0.001	–0.003
($t-2$ *vs.* $t-1$)	0.513	0.819	0.273	0.419	0.116	0.865	0.668
2005 *vs.* 2006	–0.111	–0.007	–0.709	0.008	0.002	–0.011	–0.010
($t-3$ *vs.* $t-2$)	0.425	0.675	0.197	0.450	0.652	0.542	0.120
2005 *vs.* 2007	–0.046	–0.011	–0.106	0.007	0.002	–0.010	–0.008
($t-3$ *vs.* $t-1$)	0.612	0.598	0.191	0.188	0.706	0.613	0.299

Table A.10: *Robustness: Propensity-Score Matching*

*This table reports output from estimating our baseline regressions of the indirect effects of credit shocks with a propensity-score matched sample. Panel A shows the supplier-centered regression (refer to Table 4) and Panel B shows the customer-centered regression (refer to Table 5). We match firms using the following algorithm. For each supplier, sort its customers between treated and non-treated. Match each treated customer with a control customer satisfying an exact match in terms of credit rating, city, and industry. Among these exactly-matched potential controls, optimally choose (Abadie and Imbens (2011)) the best possible control firm by minimizing the distance between the treated firm and its control in terms of firm age, firm size, and lagged values of total credit. The coefficient of interest is the Difference-in-Differences (DID) estimator given by the interaction $Gov_i \times Post_t$. Gov_i is an indicator variable that equals one if the firm $i \in \{c,s\}$ is government-credit-dependent (i.e., if its share of credit with government banks is greater than the median), and is zero otherwise. $Post_t$ is an indicator variable with value of one if year $t \geq 2009$ and zero if $t \leq 2007$. The year of 2008 is not included in the event study window for being the year of Lehman Brothers' failure. As detailed in Figure 6, in column (1), we use a time window of 1 year after versus 1 year before the Lehman bankruptcy. In column (2), we expand the time window to include 2 years before and 2 years after. In column (3), the time window is expanded to 3 years before and after. Statistical significance levels: *** p-value<0.01, ** p-value<0.05, * p-value<0.10.*

Panel A. Supplier-Centered Matched Regressions

Dependent Variable Downstream DID:	*Log(Real Payments)* *(Inflow to a Supplier from Treated vs. Matched Control Customer)*		
DID Period	(1) 2009 *vs.* 2007 (±1 Year)	(2) 2010:2009 *vs.* 2007:2006 (±2 Years)	(3) 2011:2009 *vs.* 2007:2005 (±3 Years)
$Gov_c \times Post_t$	0.089*** (0.006)	0.162*** (0.018)	0.215*** (0.091)
Fixed Effects			
Supplier×Year	Yes	Yes	Yes
Clustered SE	Supplier, Year	Supplier, Year	Supplier, Year
N. Observations	700,307	1,349,143	1,900,276
R-Squared	0.396	0.404	0.414

Panel B. Customer-Centered Matched Regressions

Dependent Variable Upstream DID:	*Log(Real Payments)* *(Outflow from a Customer to Treated vs. Matched Control Supplier)*		
DID Period	(1) 2009 *vs.* 2007 (±1 Year)	(2) 2010:2009 *vs.* 2007:2006 (±2 Years)	(3) 2011:2009 *vs.* 2007:2005 (±3 Years)
$Gov_s \times Post_t$	0.038*** (0.004)	0.032*** (0.006)	0.039** (0.018)
Fixed Effects			
Customer×Year	Yes	Yes	Yes
Clustered SE	Customer, Year	Customer, Year	Customer, Year
N. Observations	1,187,721	2,302,609	3,254,469
R-Squared	0.204	0.215	0.225