Do bank bailouts affect the provision of trade credit?*

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Abstract

We document that borrowers of banks that received capital support under TARP/CPP significantly increased their quarterly provision of trade credit (accounts receivable) during the crisis by 5.2 percent, while borrowers of other banks did not. The effect is strongest in 2008Q4, and larger for pre-crisis riskier, growth-oriented and bank-dependent firms and for firms that borrow from pre-crisis smaller, less profitable and better capitalized CPP banks. Our difference-in-differences analysis shows that the effect is caused by CPP and not by heterogeneity between firms, banks and time periods. Our study provides novel evidence that suggests a beneficial multiplier effect of bank bailouts.

JEL Classification: G20; G30; G32

Keywords: Trade credit, TARP, bank dependence, credit chains, financial crises

* All errors are our own. The views expressed in this paper are solely those of the authors and should not be interpreted as reflecting the views of the Board of Governors or the staff of the Federal Reserve System.

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1. Introduction

In the middle of unprecedented turmoil, the U.S. government provided many banks with additional equity to stabilize the financial industry via the Capital Purchase Program (CPP), which was the core of the Troubled Asset Relief Program (TARP). The declared goals of the CPP were to "strengthen the capital base of the financially sound banks" by providing them with extra liquidity and equity so that they could "increase their capability of lending to U.S. consumers and businesses to support the U.S. economy" (U.S. Department of Treasury, October 14, 2008). Research has shown that these goals were largely achieved. Banks were recapitalized, restarted lending and managed to pay back this financial aid relatively fast. However, most of the studies on TARP/CPP have focused on banks. Very few studies have examined indirect effects on banks' borrowers and the real economy (e.g., Berger and Roman 2017; Norden et al. 2013).

In this paper, we provide novel evidence on an important effect of bank bailouts: the effect on the provision of trade credit by the commercial borrowers of recipient banks. While this is technically an indirect effect because it runs through the entity that was the "direct" beneficiary of the intervention to the benefit of a third party, it could better be described as a "multiplier" effect. To the best of our knowledge, there is no study that has investigated whether bank bailouts made under TARP/CPP affected the provision of trade credit. Trade credit is, after bank credit, the second most important type of private debt and it is critical for growth and economic activity (e.g., Berger and Udell 1998, Demirgüç-Kunt and Maksimovic 2001, Cuñat 2007, Giannetti et al. 2011, Carbo et al. 2016). (We note that from this point on we will refer to the bailouts made under TARP/CPP as simply made under CPP.) Trade credit can exacerbate or attenuate business cycles and can create significant risk contagion effects through trade credit chains (Jacobson and von Schedvin 2015). We test a reduced form of the following hypothesis: CPP saved banks that received capital

infusions, which restored lending to corporate borrowers of these banks, which enabled these borrowers to provide more trade credit to their customers than other firms that were not borrowers of CPP banks. To test this hypothesis, we conduct a difference-in-differences analysis using matched bank-firm data from the U.S. for the period from the second quarter of 2007 to the end of 2009.

We find the following results. First, we document that corporate borrowers of CPP banks significantly increased their quarterly provision of trade credit during the crisis, while borrowers of other banks did not. The effect is highly significant and has a magnitude of 5.2 percent, which corresponds to an increase in the provision of trade credit of $198 million for the average firm in our sample. Moreover, the effect is strongest in (but not limited to) the fourth quarter of 2008. The findings remain robust when we use an indicator variable for CPP and also in an alternative analysis, in which we use a firm-specific time-varying intervention indicator that combines information on bank-firm relationships (Dealscan) and government intervention in banks (CPP). In all analyses, we control for key firm characteristics including total asset size, firm fixed effects, and year-quarter effects. Importantly, our main result, i.e., the increase in the provision of trade credit by borrowers of CPP banks, is not due to the recovery of the U.S. economy because we do not find the same effect for borrowers of other banks and we control in all analyses for year-quarter effects. Second, we divide the sample in terciles based on key firm characteristics from pre-crisis times. We then repeat the difference-in-differences analysis and evaluate the magnitude and significance of the DiD estimator. We find that the increase in the provision of trade credit is significantly larger for riskier, less growth-oriented and bank-dependent firms. We consider firms' leverage, profitability and Altman's Z-score as measures of firm risk. Third, we also divide the sample into terciles based on average pre-crisis bank characteristics. We show that the increase in

the provision of trade credit is larger for firms that borrow from ex ante smaller, less profitable and sufficiently well-capitalized CPP banks (an increase up to 12.3 percent). Overall, our findings highlight beneficial effects of bank bailouts on the provision of trade credit that had probably not been considered by the US government when designing the CPP/TARP nor considered by the extant related academic literature.

Our paper contributes to the literature on government involvement in banks, especially bank bailouts. The literature on the former has examined various forms of explicit government involvement in banks such as government ownership, control, guarantees, interventions and bailouts (e.g., La Porta, Lopes-de-Silanes and Shleifer 2002). Moreover, there has been research about implicit government involvement, in particular the "too big to fail effect". The general assessment of government involvement in banks tends to be negative. However, government involvement might be beneficial when there is market failure and/or there are extreme market conditions (financial crises). Research on CPP has shed light on the benefits and the costs. A convincing argument has been made that the bank bailouts made under the CPP program had beneficial effects on bank lending. Several studies show that CPP expanded credit supply (loan quantities and loan terms), some of which are also focused on syndicated lending (Berrospide and Edge, 2010; Duchin and Sosyura, 2014; Norden, Roosenboom, and Wang, 2013; Li, 2013; Puddu and Waelchli, 2013; Chavaz and Rose, 2017; Berger and Roman, 2017; Chu, Zhang, and Zhao, 2018; and Berger, Makaew, and Roman, forthcoming). Nevertheless, there is also evidence that bank bailouts come at the price of higher systemic risk in the long-run (Berger, 2018). Our study provides evidence on a beneficial multiplier effect of bank bailouts that should be considered by policy makers and when assessing the success of bank bailouts.

We also contribute to the literature on the provision of trade credit. First, our findings imply

that bank bailouts realign bank lending and the provision of trade credit, resulting in a complementarity between these two types of credit. Second, our findings are consistent with the redistribution view of trade credit (e.g., Meltzer 1960, Petersen and Rajan 1997, Calomiris, Himmelberg and Wachtel 1996, Nilsen 2002). Under this view, large firms redistribute part of their bank credit to small financially constrained firms that have difficulties in obtaining sufficient bank credit (Carbo-Valverde et al. 2016). However, many large firms were strongly hit by the negative shock to bank credit supply during the crisis and therefore had to shrink their activity and reduce their provision of trade credit. Only those large firms that had sufficient liquidity reserves kept on providing trade credit to their customers (Garcia-Appendini and Montoriol-Garriga 2013). But, the positive (capital) shock to banks resulting from CPP changed the situation. CPP restored bank lending and enabled borrowers of CPP banks to increase their provision of trade credit again. Third, Jacobson and von Schedvin (2015) document upstream credit risk contagion resulting from trade credit chains. Suppliers might become infected by the credit risk of their customers, realize credit losses, and ultimately lose their customers and future business. Our paper complements their findings taking the inverse perspective. We show that bank bailouts, through the restoration of the bank lending channel, have positive effects on corporate provision of trade credit that propagate downstream through trade credit chains.

The remainder of this paper is organized as follows. In Section 2, we develop a set of hypotheses. In Section 3, we describe the data, and in Section 4 we outline the empirical strategy. In Section 5, we present results on the influence of bank bailouts on the provision of trade credit, and further empirical checks including the heterogeneous effects across firms of various characteristics. Section 6 concludes.

2. Hypotheses

The declared goal of the U.S. government's intervention via CPP was to stabilize banks with extra liquidity and make it possible for them to keep lending or to increase lending to the corporate sector. As a result, firms that borrow from CPP banks might have benefited from this intervention because they faced a more stable or increased supply of credit. According to the redistribution view, part of the access to bank credit by large firms is passed on to their customers in the form of trade credit. Based on this reasoning, we propose the following hypothesis:

> **Hypothesis H1**: Firms that borrow from CPP banks increased their provision of trade credit during the crisis more than other firms.

We next investigate whether pre-crisis firm characteristics influence the impact of the CPP intervention on the provision of trade credit. Given the fact that the recent financial crisis originated from the supply side (Ciccarelli et al. 2010, Ivashina and Scharfstein 2010, Ongena et al. 2010), the entire banking industry became cautious and reluctant to grant new loans. Other things equal, it should be more difficult for smaller, less profitable, more leveraged, less growth-oriented, riskier and bank-dependent firms to obtain sufficient credit or to switch to alternative financing sources. It is more difficult for more bank-dependent firms, such as firms with low liquidity and firms that lack an investment-grade rating, to raise external finance. These firms are therefore more sensitive to shocks to banks and, as a result, government intervention in the banking industry is expected to be especially helpful for those firms. Thus, we expect that the impact of the government intervention in banks on the provision of trade credit by these firms is stronger.

> **Hypothesis H2**: CPP interventions in banks have a significantly stronger impact on the provision of trade credit by firms who are smaller (H2a), less growth-oriented (H2b), less profitable (H2c), more leveraged (H2d), cash-rich (H2e), riskier (H2f) and more bank-

dependent (H2g).

We also investigate whether pre-crisis bank characteristics influence the magnitude of the impact of government interventions on the provision of trade credit by firms. Previous studies on the bank lending channel argue that large and well-capitalized banks are better able to buffer their lending activity against shocks affecting the availability of external finance (Kishan and Opiela 2000, Gambacorta and Mistrulli 2004). Empirical evidence from the recent financial crisis shows that banks with higher capital ratios are less adversely hit by the crisis since they are better able to absorb potential losses (Bayazitova and Shivdasani 2012, Li 2013). Without capital infusions in their banks, firms borrowing more from weaker and smaller banks would have experienced more funding difficulties (e.g., increase in loan rates) during the credit crunch (Santos, 2011). In line with this argument, we expect a stronger multiplier effect for firms that borrow from smaller and less profitable but, nevertheless sufficiently well-capitalized banks (ex ante), once the shock from the financial crisis is alleviated by the CPP intervention.

> **Hypothesis H3**: CPP interventions in banks have a significantly stronger impact on the provision of trade credit by firms when the banks are ex ante smaller (H3a), less profitable (H3b) and sufficiently well capitalized (H3c).

3. The data

Our data comprise information on firm characteristics, including the provision of trade credit, bank-firm lending relationships, and bank characteristics and their participation in the Capital Purchasing Program. We consider firms that are included in the Compustat quarterly dataset and LPC Dealscan databases. We identify firm characteristics prior to the start of the crisis in the second quarter of 2007. Bank-firm relationships are measured prior to the government's

intervention in the banking sector via the CPP. We identify bank participation in the CPP interventions starting from the fourth quarter of 2008 and compare the provision of trade credit by firms with/without a CPP relationship before and after the CPP capital injections over the period from June 30, 2007 to December 31, 2009.

In total, our merged sample is based on data from 950 incorporated, non-financial firms from the United States with available balance sheet data between the second quarter of 2007 and fourth quarter of 2009, of which 250 are included in the S&P 500 index. The total market value of firms in our sample accounts for around 45 percent of the total market capitalization of the listed firms covered in Compustat. Table 1 reports summary statistics for the main variables and the Appendix shows variable definitions, data sources, and the period of measurement. We describe these variables in more detail in the remainder of this section.

(Insert Table 1 here)

We collect data on firms' accounting variables and bank dependence from Compustat. We exclude financial firms (SIC codes between 6000 and 6999). In order to avoid endogeneity problems in our analysis, we identify firms based on their pre-crisis accounting characteristics (2007Q2).

Following Garcia-Appendini and Montoriol-Garriga (2013), we first calculate a firm's provision of trade credit as the ratio of accounts receivable over sales (i.e., end of quarter accounts receivable and quarterly sales). We consider as control variables firms' total assets, cash holdings, and other variables that indicate the level of firms' financial distress; such as Altman's Z, firm age, net profit margin, leverage ratio, Tobin's Q, and asset intangibility. In line with Kashyap et al.

(1994) and Chava and Purnanandam (2011), we consider firm's dependence on banks by examining their public debt rating status. We treat non-rated and non investment-grade firms as bank-dependent firms and investment-grade firms as non bank-dependent. During the financial crisis, it was difficult (if not impossible) for non-investment-grade firms to obtain alternative finance from either the bond market or the commercial paper market. In our sample, over half of firms are categorized as bank-dependent borrowers according to their pre-crisis debt rating status.

Measuring the bank-firm credit relationship is key to identifying the transmission channel from government capital injection in banks to a firm's provision of trade credit. Having a stronger lending relationship with a bank makes a firm more vulnerable to negative liquidity shocks to its bank, but at the same time allows borrowers to have better access to credit once the bank's liquidity problem improves due to the government's capital injections.

To establish bank-firm relationships, we employ the Thomson Reuters Dealscan database, which has been widely used in related studies (e.g., Dennis et al., 2000, Bharath et al., 2011, Chava and Purnanandam, 2011, Norden, Roosenboom and Wang, 2013). This database contains detailed information on bank loans, mostly syndicated loans, granted to large companies. There are various ways of measuring the strength of a bank-firm relationship; some studies focus on the time dimension and measure the length of the lending relationship (e.g. Berger and Udell 1995), while others employ the existence of repeated lending, concurrent underwriting, lines of credit, and checking accounts as proxies for a strong bank relationship (e.g., Schenone 2004, Drucker and Puri 2005, Bharath, Dahiya, Saunders and Srinivasan, 2007, Norden and Weber 2010, Bharath et al. 2011). Following Norden, Roosenboom and Wang (2013), we define a firm-specific bank-firm lending relationship variable $Lending\ relationship_{ij,t}$ that captures the intensity of a bank's past

lending to a firm as an indication of the strength of the bank-firm relationship.[1] Following many empirical studies in this area, we consider a look-back window of four years to measure firms' relationships with commercial and/or investment banks in the loan or bond market (e.g., Bharath, et al., 2007). In particular, we review the history of corporate loans originations extended to firm i by bank j prior to time t over a four-year window period from 2004 to 2007[2]. Indeed, given the median maturity of the loans in the Thomson Reuters Dealscan database is 4.8 years, loans granted during 2004-2007 are still likely to be outstanding during our analysis period from August 2007 to December 2009, and counted as part of firm's total loan portfolio, and thus would provide information about the strength of the bank-firm relationship.

Similar to the identification strategy employed in Duchin, Ozbas and Sensoy (2010), we measure the bank-firm relationship using the period prior to the start of the crisis and then freeze the relationship to avoid the endogeneity problem that firms might have started relationships with banks that participated in the CPP because they expected that these banks would be more willing or better able to provide credit. Although this potential shifting in the relationship does not seem to have happened on a large scale since few new lending relationships were formed after the beginning of the crisis, freezing the relationship to the pre-crisis period helps to mitigate the potential endogeneity problem.

In syndicated lending, lead arrangers are considered to be the main relationship bank that

[1] Since the LPC database starts in 1982, it would not be possible to observe the exact starting point of the lending relationship and thus it would be difficult to calculate the length of any of such a lending relationship. Therefore, instead of focusing on the "time dimension" of the banking relationship, we choose to focus on the intensity of bank relationships by capturing the number of bank lending relationships and the concentration of bank debt.

[2] The decision about the look-back window is essentially an empirical one. However, from a theoretical perspective, we have to consider the following trade-off. On the one hand, the window should not be too short, as it would ignore interaction that was not recent between a bank and a firm. For example, we believe that a one- or two-year window would be too short. On the other hand, the window should not be too long because the interaction (and the corresponding bank relationship) might become outdated. For example, a ten-year window would be probably too long as it would comprise interaction that happened in the distant past and that might have become outdated.

collects information about the borrower through continuous monitoring. We define the *Lending relationship*$_{ij,t}$ by considering firm i's top lead arrangers (banks) for each of firm i's historical loans in the LPC database. Suppose that firm i obtained n loans during the past four years prior to time t, the lending relationship between firm i and one lending bank j at time t is calculated as:

$$Lending\ relationship_{ij,t} = \frac{\sum_{x=1}^{n} Lead_{ij,x}}{\sum_{x=1}^{n} numL_{i,x}} \quad (1)$$

where $Lead_{ij,x}$ is a dummy variable that equals one if bank j (among the others) acts as a lead arranger in loan x to firm i, and zero otherwise. The variable $numL_{i,x}$ is the number of lead arrangers involved in loan x to firm i.[3]

For both borrowing firms and lead banks, we aggregate data to the parent-bank level. We use the parent bank in our analysis because the CPP is only conducted at the parent-bank level. We also exclude finance companies as lenders from our analysis because these institutions were not eligible to receive CPP capital infusions. The large number of mergers and acquisitions in the U.S. banking industry during our sample period makes it challenging to track the dynamics of bank-firm relationships. We use the Thomson One Banker and Zephyr database to document bank mergers and acquisitions events from 2004-2009 and construct dynamic relationships between banks and firms. Similar to other studies, we assume that in most of the cases, the post-

[3] The calculation of *Lending relationship*$_{ij}$ is best illustrated by an example. LPC Dealscan reports that Ford has entered two new loan contracts over the four-year period from 2004 to 2007; the first loan contract was granted in June 2004 with Capital One and Citigroup as lead arrangers. The second loan was granted in June 2006 with Capital One and Wells Fargo as lead arrangers. In this case, the strength of relationship between Ford and Capital One is calculated as: *Lending relationship*$_{\text{Ford, CapitalOne}}$=2/(2+1+1)=0.5; similarly, *Lending relationship*$_{\text{Ford, Citi}}$=1/(2+1+1)=0.25 and *Lending relationship*$_{\text{Apple,Wells Fargo}}$=1/(2+1+1)=0.25. This method identifies and differentiates the relative importance among lead arrangers over the past years. It is the optimal to measure the strength of a bank-firm relationship based on the count rather than volume of loans given that information on the actual shares of the individual banks are often missing or unreliable for many cases in the LPC database.

merger/post-acquisition bank inherited the loans of the pre-merger/pre-acquisition banks under normal economic situations.

Based on the information extracted from 2,449 loan contracts from January 2004 until December 2007, we constructed 127,748 pairs of bank-firm *Lending relationship*$_{ij,t}$ at the beginning of 2005 and this number is then reduced to 112,512 pairs at the end of 2009 due to mergers and acquisitions in the banking sector. We use the link provided by Michael R. Roberts to match firms from LPC Dealscan to the ones in Compustat (http://finance.wharton.upenn.edu/~mrrobert/; see also Chava and Roberts (2008) for more details).

The data on banks' participation in CPP come from the website (http://www.treasury.gov/initiatives/financial-stability) of the U.S. Treasury Department. It includes information on capital infusions and capital redemptions. We first define a firm-level capital injection shock variable by looking at whether one of a firm's banks were subject to intervention by the government. As a robustness check, we follow a similar approach by Norden et al. (2013) and define a firm-specific and time-varying CPP intervention score which takes a firm's bank relationships and the banks' participation in the CPP program into account. We create the score by linking bank-firm relationships from Dealscan to information on banks' participation in the U.S. Treasury list about CPP, using the hand-matched concordance files based on banks' names. Bank characteristics are weighted at the firm level, using the firm-bank relationships from the pre-crisis period. We merge bank-firm relationships identified from Dealscan with bank characteristics from Y9C, using hand-matched bank name concordance files aggregated at the holding company level.

4. Empirical strategy

We conduct a difference-in-differences analysis, in which we compare the provision of trade credit by firms before and after the start of the crisis as a function of positive government capital injections into their banks. The treatment variable CPP is a dummy variable that switches to one for firms for whom at least one of its banks benefited from government intervention during the crisis according to the variable *Lending relationship*$_{ij,t}$. The variable *Post-CPP* switches to one in the fourth quarter of 2008 when the implementation of CPP took place, and equals zero for all preceding quarters.

We regress the quarterly the provision of trade credit by firms over the period from August 2007 to December 2009 on the dummy variable for the crisis period, as well as the interaction of *Post-CPP*$_t$ with the dummy variable *CPP*$_i$ indicating whether firms' banks participate in the CPP program based on the bank-firm relationship measured prior to the crisis. This dummy variable switches to one if any of the firms' banks received capital under the CPP program during the crisis. Our model specification is as follows:

$$Trade\ credit\ provision_{i,t} = \alpha + \beta_1 CPP_i + \beta_2\ Post\ CPP_t + \beta_3\ CPP_i \times Post\ CPP_t + \beta_4 Controls_{i,t-1} + Firm\ FE_i + Time\ FE_t + \varepsilon_{i,t} \quad (2)$$

In equation (2), *Trade credit provision*$_{i,t}$ refers to the provision of trade credit calculated as the total amount of accounts receivable at *t* divided by sales for firm *i* for the three months ended at *t*.

We control for various lagged firm characteristics such as size, leverage and profitability. In addition, we include fixed effects for firm and year and quarter to control for both time-invariant

unobservable factors related to a firm's behavior, as well as to nationwide shocks that happened during a particular year that could affect both the government's intervention in banks and a firm's provision of trade credit. Thus, we estimate the within-firm differences over time for a firm's provision of trade credit. We are interested in the coefficient β_3 which captures the (differential) effect of a firm's access to the provision of liquidity by its banks in their provision of trade credit.

In our setting, it is appropriate to employ a difference-in-differences analysis because the shocks to banks were largely exogenous and unexpected to any specific borrowing firm. To qualify for the CPP program, banks first had to file applications, and then it is up to the U.S. Treasury to decide which banks would get a capital infusion. Previous papers suggest that many of those banks whose applications to participate in CPP were accepted had solid fundamentals but needed temporary liquidity assistance to get through the crisis (Berger 2018). Our empirical strategy also disentangles the effects of TARP from the effects from funds that banks received from the discount window and Term Auction Facility (TAF) given the difference in the timing and strength of TAF compared to TARP. TAF targeted the early liquidity problems whereas CPP targeted the increasing (and more critical) solvency problems of banks that evolved later. To further mitigate the remaining concerns of omitted variables that may affect both the Treasury's bank interventions and firm behavior, we follow an identification strategy similar to Duchin, Ozbas and Sensoy (2010) and measure the intensity of bank-firm relationships during the years prior to the start of the crisis in order to rule out the possibility that firms may switch or establish new relationships with banks after the government intervention took place.

5. Results

5.1. The impact of bank bailouts on the provision of trade credit

We first compare the provision of trade credit by CPP firms (treatment firms) with non-CPP firms (control firms). Figure 1a shows the percentage change in firms' provision of trade credit during the crisis, before and after the first waves of interventions in banks. Several important patterns emerge from this figure: first, it is clear that firms' provision of trade credit had decreased after the onset of the crisis, and the trend is present across firms that had, and did not have, relationships with CPP banks. Second, both treatment and control firms exhibit similar trends until the first wave of shocks of bank interventions. This pattern confirms that the parallel trend assumption is satisfied: the two groups are subject to similar trends and the shock is not anticipated. Furthermore, we see that the provision of trade credit by firms that had access to finance from CPP banks experienced a significant increase and then stabilized after the first wave of CPP capital injections in 2008Q4 while the provision of trade credit by firms having no relationship with CPP banks kept decreasing. We also examine the sources of changes in firms' provision of trade credit, and examine whether the divergence patterns in the provision of trade credit across the two groups are largely driven by an increase in accounts receivable or a decrease in sales. Figure 1b and 1c show that while sales growth of the treatment and the control group are similar, there is a big difference in the level of accounts receivable between the two groups. While firms' sales are subject to economy-wide shocks, how much firms allocate to account receivables is largely discretionary. After the first waves of CPP injections into banks, the recipients' relationship borrowers appeared to be able/willing to extend a higher level of trade credit to their customers.

We next investigate our Hypothesis H1 in a regression analysis. Table 2 presents the difference-in-differences regression results for the effects of CPP intervention on the provision of trade credit. We start the set of regressions with a simple univariate regression that comprises only the treatment dummy, Post-CPP dummy and the interaction effects as explanatory variables

(column 1), and then expand the specification by adding various controls: firm characteristics, firm fixed effects, and year quarter fixed effects in column 4, as described in Model (1). We follow the procedure suggested by Altonji et al. (2005). As the fixed effects control for possible influences of unobservable time invariant characteristics, this approach provides a way to confirm (or not) the role of such unobservable characteristics.

(Insert Table 2 here)

The results consistently show that corporate borrowers of CPP banks increase the amount of trade credit that they provided to their customers after the peak of the financial crisis. These effects are highly statistically significant and have a large economic impact. The estimates indicate that borrowers of CPP banks increased their quarterly accounts receivable to sales by 5.2 percentage points. On a yearly basis, this effect implies that the average firm increased trade credit to customers by $198 million. This evidence supports our Hypothesis H1.

We further use an alternative measure to capture the intensity of government bank intervention for each firm. Following Norden, Roosenboom and Wang (2013), we first create a time-varying intervention variable $Intervention\_DM_{j,t}$ for each bank j where its value increases by one when a capital infusion took place and decreases by one as the bank pays back the capital.[4] We then transform the bank-level variable $Intervention\_DM_{j,t}$ into the firm-level variable $Intervention_{i,t}$ for each firm i by considering the lending relationships $Lending\ relationship_{ij,t}$ with its m banks. The time-varying firm-level intervention score is computed as shown in equation (3).

[4] Banks were allowed to redeem their capital after the enactment of the American Recovery and Reinvestment Act (ARRA) on February of 2009

$$Intervention\_s_{i,t} = \sum_{j=1}^{m} Lending\ relationship_{ij,t} \times Intervention\_DM_{j,t} \quad (3)$$

The main difference between the intervention score and the interaction variables used in model (2) is that the intervention score captures the movements in the intensity of government intervention throughout time.[5] *Intervention_d* is a firm level continuous intervention indicator shows the firm's exposure to government interventions into the firm's relationship banks throughout time. Its value increases each time one of the relationship banks received capital injections whereas the value decreases when banks repaid the capital back to the U.S. Treasury. We also create an alternative dummy variable *Intervention_s* that equals one when the first relationship bank(s) received its (their) capital infusion(s) under CPP (i.e., from the moment when this/these bank(s) received the money) until the moment when the last of the firm's banks repaid the money. Columns (5) and (6) of Table 2 show the results using the alternative measures for a firm's relationship bank's participation in CPP. The coefficients are 0.009 and 0.031, with significance levels of 5% and 1%, respectively. The result is in line with the findings using other specifications and it is thus clear that there are strong treatment effects for the CPP injections into banks on their borrower's provision of trade credit.

Prior evidence suggests that the first wave of CPP injections had the strongest effects on boosting bank lending to the corporate sector (e.g., Norden, Roosenboom and Wang, 2013). In line with this observation, we should expect stronger effects on a firm's provision of trade credit after the first wave of government interventions in their relationship banks. To test this hypothesis, we perform separate tests on the difference in the control and treatment groups' provision of trade credit after the initial quarter of CPP intervention and on the differences during the period when

[5] After the enactment of the American Recovery and Reinvestment Act (ARRA) on February of 2009, some banks started to pay back the CPP money, and thus we see a decrease in the bank-level variable $Intervention\_DM_{j,t}$.

the rest of the CPP interventions happened. The test preserves the difference-in-differences framework but is run on samples from different "after-shock" periods, so we essentially compare the difference between the control and treatment groups before the shock with the difference calculated in different "after-shock" periods. In one test, we show in one model the interactions between CPP and a series of year-quarter dummies of all five quarters (2008Q4, 2009Q1-Q4). In another test, we show the interaction effect for 2008Q4 and the average effect over 2009Q1-Q4. Table 3 reports the results.

(Insert Table 3 here)

Table 3 shows that while the CPP interventions in general have a significantly positive impact on firms' provision of trade credit, the strongest effects appear to be concentrated around the beginning of the interventions. Compared to firms having a relationship with banks that participated in the later waves of the CPP program, firms having a relationship with banks that participated in the beginning of the CPP program were able to extend twice as much trade credit to their customers, relative to the control groups. The results are consistent and suggest that the first waves of the government's capital injections were most effective in stabilizing the credit market and insuring the provision liquidity via the trade credit channel.

5.2. Firms characteristics and the impact of bank bailouts on the provision of trade credit

We next consider the importance of key characteristics of the firms who borrow from banks who benefitted from the CPP interventions, as stated in Hypothesis H2. Specifically, we investigate differences in the provision of trade credit across these firms that are related to these

firm-level characteristics. To put this part of our analysis in context, we note that our overall findings are that CPP interventions solidified access to credit for these firms and relaxed their liquidity constraints. In particular, these firms continued extending trade credit to the customers. In this part of our analysis we dig deeper to explore whether the actual amount of trade credit provided is also contingent on the characteristics of these firms (i.e., the firms who borrowed from CPP recipient banks) such as their size, leverage etc. For example, it could be the case that weaker and riskier firms that were hit the most during the first stage of the crisis would be able to benefit more from the liquidity relief they received from CPP recipient banks and were consequently able to lend more to their clients in the form of trade credit (e.g., Ivashina and Scharfstein 2011). To investigate the influence of these firm characteristics, we estimate the panel data regressions shown in model (1) on terciles based on pre-crisis firm characteristics. This empirical approach also makes it possible for us to detect possible non-monotonic relationships between borrowers of CPP intervention recipient banks and their provision of trade credit. Table 4 reports the results.

(Insert Table 4 here)

Two patterns emerge from Table 4. First, as before, CPP interventions in general have a positive effect on the borrowers of CPP recipients with respect to their provision of trade credit across different subsamples. Second, the sensitivity of the effect of CPP intervention varies with firm characteristics. We find stronger effects for small (H2a), less profitable (H2b), less growth-orientated (H2c) and highly levered firms (H2d). Cash-rich firms (H2e) tend to be heavily levered during the pre-crisis period and were hit hard and suddenly by the shutdown of the bank lending channel in the beginning of the banking crisis (Ivashina and Scharfstein 2011). However, they

were also able to benefit the most from the positive capital injection shocks in banks (e.g., Norden et al. 2013). Our results are consistent with these earlier findings in showing that these financially stressed (low Z-score, as stated in H2f), and high cash holding firms were able to provide more trade credit to their customers. Furthermore, we find that compared to firms with an investment-grade rating, bank-dependent firms (H2g) with worse credit quality were able to extend more trade credit to customers, after the CPP capital infusions into their banks during the financial crisis. This finding is consistent with the prior literature that shows bank-dependent firms benefited more from government interventions during the crisis (e.g., Chava and Purnanandam 2011, Norden et al. 2013). The size of these effects on bank-dependent firms are more than twice as much as the effects on less bank-dependent firms and the difference across the two groups is statistically significant.

5.3. Bank characteristics and the impact of bank bailouts on the provision of trade credit

We have shown that government capital injections into banks stimulated the provision of trade credit by their borrowers to their borrowers' customers. Similar to the influence of firm characteristics, the magnitude of the effects of CPP could also be contingent on the characteristics of the banks that were the beneficiaries of this intervention, as stated in our Hypothesis H3. We construct weighted bank characteristics for each firm i at time t by considering the relationship between firm i and its lending bank j, as well as bank j's specific characteristics l (i.e., bank profitability, capital ratio and bank size) at time t as shown in equation (4).

$$Weighted\ \ Bank\ \ Characteristics_{il,t} = \sum_{j=1}^{n} Lending\ relationship_{ij,t} \times Bank\ \ Characteristics_{jl,t} \quad (4)$$

For each bank characteristic, we estimate the baseline regression model (1) on sub-samples constructed based on the weighted pre-crisis bank characteristics measured during the second quarter of 2007. Table 5 reports the results.

(Insert Table 5 here)

First, we find that firms were able to provide more trade credit to their customers when they borrow from smaller banks (H3a). We further find that the overall positive effects of government capital injection become stronger when firms borrow from banks with lower profitability (H3b). Interestingly, after the government injected capital into their relationship banks, firms appeared to be more willing to provide liquidity to their customers when the recipient bank was well capitalized (H3c). This evidence could indicate that as bank credit is scarce in the midst of the crisis, there is a huge amount of uncertainty around how much banks may lend, firms are therefore cautious and more comfortable extending trade credit to customers when their banks are well capitalized.

5.4. Further empirical checks and robustness tests

First, we showed that the provision of trade credit by firms from the treatment and control groups display a similar pre-crisis trend (see Figure 1). However, there might still be the concern that heterogeneity between the two groups of firms - and not necessarily the capital injection into their banks - might influence the way that firms behaved during the crisis. To address this concern, we conduct an analysis using a matching exercise. We identify pairs of CPP and non-CPP firms that are similar along meaningful dimensions except for their banks' participation in the CPP program. Given the relatively limited size of our dataset and that an exact match might be

unavailable, we use a propensity score method for the matching exercise (PSM; Rosenbaum and Rubin, 1983). Similar to the approach by Chava and Purnanandam (2011), we estimate a logit model using the information on the participation of a firm's bank in the CPP as the binary dependent variable. We model a firm belonging to one of the two groups as a function of the various characteristics of the firm, including size, leverage, profit margin, and industry. After estimating the model, we obtain the propensity of a firm's borrowing from a CPP-bank for every firm in the sample. For every firm that borrows from non-CPP banks, we find neighboring CPP firms with the closest propensity score. For each non-CPP firm, we identify two neighboring firms with scores that are in the range of ±2.5% of the non-CPP firm's score. Doing so allows us to find precise matches between the CPP and non-CPP firms while maximizing the number of firms in our matched sample. As we have many more observations in the treatment group as compared to the control group, it is advisable to have one control firm serve as a match for multiple treatment firms (Dahejia and Wahba, 2002; Smith and Todd, 2005; and Chava and Purnanandam, 2011). To ensure a close match between the control and treatment firms, we perform the matching exercise at a quarterly frequency. The matching exercise yields a sample of 1,109 observations with two thirds of them belonging to the treatment group. As a result, we ensure all standard errors are clustered at the firm level in analyses involving the matched sample. We first test the efficacy of the matching technique before implementing the baseline difference-in-differences regression on the matched sample. In particular, we estimate two logit models of the likelihood that a firm's banks participated in the CPP on the three firm characteristics as well as industry dummies. The results are shown in Appendix A2. It is clear that after the PSM, none of the seven variables is significant and there is also a sharp decline in the model's R-squared from 10 percentage points to approximately zero in the matched sample. We then apply the main regression model on the

matched sample with similar firms in the control and treatment groups. Table 6 reports the results for the PSM matched sample.

(Insert Table 6 here)

The findings of Table 6 confirm our earlier results that the CPP intervention has a significantly positive impact on firms' provision of trade credit. These effects have a significant economic impact and are statistically similar to the ones reported using the full sample, confirming the robustness of the main results.

Second, we conduct placebo tests to confirm that the conditions of the difference-in-differences are met. We create two fictitious bank bailouts that happen before the actual CPP interventions and test whether the fictitious shocks influence the provision of trade credit of firms in treatment group in earlier years. Placebo 1 equals one after 2001Q4 for a sample that covers the 2002 recession period of 2001-2003 and Placebo 2 equals one after 2005Q4 for another sample that covers an alternative period of 2004-2006, respectively. If the parallel trend assumption holds and there are no other shocks affecting one group or another, there should not be any significant positive effect on the firms' provision of trade credit before the actual shocks took place. We then re-estimate our baseline regressions in two separate samples covering different periods earlier and replace the dummy variable *Post-CPP* by *Placebo 1* and *Placebo 2*, respectively. Table 7 reports the results.

(Insert Table 7)

We find that the interaction terms with both placebo bank bailouts are not statistically significant. The evidence supports our interpretation that borrowers of CPP banks increased trade credit during the time CPP was active (and not during times when the program was inactive) and that the conditions of applying the difference-in-differences methodology are met.

Third, we repeat our baseline analysis by industry. Note that we do not report the findings for the agriculture industry and for publication administration because these samples are very small. The provision of trade credit differs in its importance across industries (in parentheses we show the industry mean of the ratio accounts receivable/sales): mining (0.40), manufacturing (0.46), transportation (0.20), wholesale and retail (0.24) and services (0.45). Table 8 reports the findings. We find that the positive impact of CPP on the provision of trade credit holds in four out of five industries (mining and construction, manufacturing, transportation and services). These four industries account for 88% of the observations in our sample.

(Insert Table 8)

Fourth, we perform additional robustness tests by adding controls for trade credit usage. These tests account for the possibility that the capacity to *provide* more trade credit may be partially facilitated by increased access to trade credit (i.e., trade credit usage). We consider augmenting our baseline model with two alternative measures of trade credit: accounts payable (dollar volume) and accounts payable normalized by costs of goods sold. Our main result remains robust after controlling for these measures of trade credit usage in our analysis, confirming the robustness of our main result.

Finally, banks repaying the CPP funds early may have different incentives and lending behavior relative to those paying late as the former demonstrate their financial strength through the ability to repay early. To investigate whether our results are robust for firms that borrow from banks that made early repayments, we conduct sub-sample analyses by estimating the baseline model with CPP firms whose banks repaid the capital injections in the second quarter of 2009 versus non-CPP firms. This analysis using sub sample of firms with banks that repaid capital earlier confirms our main result.

6. Conclusion

In this paper, we investigate whether bank bailouts affect the provision of trade credit. We take advantage of the positive shock to banks due to the government intervention under TARP/CPP during the financial crisis and examine its transmission to the corporate sector.

We find the following results. First, we document that corporate borrowers of CPP banks significantly increased their quarterly provision of trade credit during the crisis, while borrowers of other banks did not. The effect is highly significant, has a magnitude of 5.2 percent, and is strongest in (but not limited to) the fourth quarter of 2008. The findings remain robust when we use an indicator variable for CPP or a firm-specific time-varying intervention indicator that combines information on bank-firm relationships (Dealscan) and government intervention in banks (CPP). In all analyses, we control for firm size, further key firm characteristics, firm fixed effects, and year-quarter effects for macro-economic conditions. Second, we divide the sample into terciles based on key firm characteristics from pre-crisis times. We then repeat the difference-in-differences analysis and examine the magnitude and significance of the DiD estimator. We find that the increase in provision of trade credit is significantly larger for riskier, less growth-oriented

and bank-dependent firms. We consider leverage, profitability and Altman's Z-score as measure of firm risk. Third, we also divide the sample into terciles based on average pre-crisis bank characteristics. We show that the increase in the provision of trade credit is larger for firms that borrow from ex ante smaller, less profitable and sufficiently well capitalized CPP banks. Various further empirical checks and robustness tests, including a propensity score matching of firms that borrow from CPP banks versus non CPP banks, confirm our main result.

Our study highlights a beneficial multiplier effect of bank bailouts on the provision of trade credit that should be considered by policy makers when deliberating on the efficacy of bank bailouts ex ante and when assessing their success ex post.

Appendix A1. Variable description

Variable category	Variable	Definition	Data source
Firm Characteristics			*Compustat*
	Trade credit provision	Accounts receivable/sales in Compustat: rectrq /saleq	
	Altman's Z	Altman's Z (1968) score, defined as 1.2*(working capital/total assets)+ 1.4*(retained earnings/total assets)+3.3*(EBIT/total assets) +0.6*(market value of equity/total liabilities)+0.99*(net sales/total assets). In Compustat: (1.2*(actq-lctq)/atq + 1.4*req/atq + 3.3*(niq+xintq+txtq)/atq+0.6*cshoq*prccq/ltq + 0.99*saleq/atq)	
	Log of total assets	The logarithm of firm's total assets.	
	Firm age	The number of years since firm appears on Compustat	
	Net profit margin	Net income/total assets. in Compustat: niq/atq	
	Leverage ratio	Book value of debt / total assets. in Compustat: (dlttq+dlcq)/atq	
	Tobin's Q	(market value of equity+total assets-common/ordinary equity)/total assets. In Compustat: (cshoq*prccq+atq-ceqq)/atq	
	Cash over total assets	Cash and marketable securities/total assets. in Compustat: cheq/atq	
	Asset intangibility	Intangible assets/total assets. in Compustat: intanq/atq	
	Bank dependence	One for bank dependent firms (public debt rated as non-investment grade or non-rated firms), and zero for non-bank dependent firms (public debt rated as investment-graded)	
Government Intervention			*Thomson Reuters Dealscan and U.S. Department of Treasury*
	CPP	Dummy variable that equals one if one of the firms' relationship banks received capital support under the CPP/TARP program, and zero otherwise.	
	Post-CPP	Dummy variable that equals one during period from 2007Q3 to 2009Q4, and zero otherwise	
	Intervention	Dummy variable that equals one if one of the firms' relationship banks is under the capital support under the CPP/TARP program and have not repaid the capital injection, and zero otherwise.	
	Int_sco_dm_d	Firm level continuous intervention indicator shows the firm's exposure to government interventions into firms' relationship banks throughout time. Its value increases each time a relationship banks received capital injections whereas the value decreases when banks repaid the capital back to the U.S. Treasury.	
	Int_sco_dm_s	Firm level dummy intervention indicator that captures firms' time-varying exposure to CPP. The variable equals one if any of the relationship banks received capital infusions under CPP from the moment when this/these bank(s) received the money until the moment the last of the firm's banks repaid the money.	

Appendix A2. Propensity score matching of CPP and non-CPP firms

This appendix presents the results of logit models of firm's banks' participation in the CPP program as the dependent variable. In the pre-match sample (column 1), we report results for the entire sample of firms. In the post-match sample (column 2), we report results for CPP firms and matched non-CPP firms based on the propensity score for firm size, profitability and leverage, as well as the industry affiliation of the firms. The analyses are conducted using quarterly data covers the period from 2007Q2 to 2009Q4. Detailed description of the variables is presented in the Appendix A1. All continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors are clustered at firm level and are shown in parentheses and *, ** and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep. Var.:	Participation of firm's bank in CPP	
	(1)	(2)
	Pre-match	Post-match
Log of total assets $_{t-1}$	0.020***	0.008
	(0.005)	(0.029)
Net profit margin $_{t-1}$	0.203	0.264
	(0.155)	(0.310)
Leverage ratio $_{t-1}$	-0.034	-0.010
	(0.036)	(0.140)
Industry dummies		
Manufacturing	-0.023	-0.048
	(0.030)	(0.128)
Transportation	0.011	-0.013
	(0.020)	(0.101)
Wholesale, Retail	0.017	-0.081
	(0.023)	(0.196)
Services	0.029	-0.047
	(0.023)	(0.166)
Intercept	Yes	Yes
Number of observations	10,416	1,109
Pseudo R^2	0.034	-0.002

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Table 1. Descriptive statistics

This table reports descriptive statistics of our sample. The sample covers the period from June 2007 to December 2009. All variables are described in Appendix A1. The continuous variables are winsorized at the 1st and 99th percentiles. We report the means, medians, standard deviations, 25th percentiles, 75th percentiles, and the number of observations.

Variable	Mean	Median	Std. Dev.	P25	P75	Number of obs.
Altman's Z	2.042	1.568	2.528	0.851	2.587	10416
Log of total assets	7.479	7.482	1.588	6.405	8.540	10416
Firm age	24.074	21.000	13.930	13.000	40.000	10416
Net profit margin	0.007	0.010	0.038	0.002	0.019	10416
Leverage ratio	0.285	0.261	0.221	0.146	0.378	10416
Tobin's Q	1.499	1.303	0.854	1.042	1.762	10401
Cash over total assets	0.088	0.050	0.105	0.018	0.114	10416
Asset intangibility	0.188	0.131	0.181	0.039	0.302	10416
Bank dependence	0.563	1	0.496	0	1	10416
Trade credit provision	0.392	0.453	0.249	0.177	0.647	10403

Table 2. The impact of bank bailouts on the provision of trade credit

This table presents the OLS regression results for the impact of bank bailouts on corporate provision of trade credit (accounts receivable/sales). The analyses are based on quarterly data covering the period from June 2007 to end 2009. All variables are described in the Appendix A1. The continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors are clustered at firm level and are shown in parentheses and *, ** and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep. Var.:	Trade credit provision					
	(1)	(2)	(3)	(4)	(5)	(6)
CPP	-0.103***	-0.037				
	(0.032)	(0.036)				
Post-CPP	-0.035**	-0.025*	-0.023			
	(0.015)	(0.015)	(0.015)			
CPP × Post-CPP	0.052***	0.046***	0.052***	0.053***		
	(0.015)	(0.015)	(0.015)	(0.015)		
Intervention_d					0.009**	
					(0.004)	
Intervention_s						0.031***
						(0.010)
Controls						
Log of total assets $_{t-1}$		-0.041***	-0.026***	-0.025***	-0.024**	-0.024**
		(0.006)	(0.009)	(0.009)	(0.010)	(0.009)
Firm age $_{t-1}$		-0.001	-0.010***	0.008	-0.013	-0.003
		(0.001)	(0.002)	(0.011)	(0.009)	(0.009)
Net profit margin $_{t-1}$		0.005	-0.053*	-0.057**	-0.051*	-0.055*
		(0.091)	(0.028)	(0.028)	(0.029)	(0.028)
Leverage ratio $_{t-1}$		-0.141***	-0.027*	-0.028*	-0.028*	-0.028*
		(0.034)	(0.016)	(0.016)	(0.017)	(0.017)
Tobin's Q $_{t-1}$		-0.004	-0.001	-0.001	-0.001	-0.001
		(0.008)	(0.004)	(0.004)	(0.004)	(0.004)
Cash over total assets $_{t-1}$		0.204***	0.003	-0.002	0.002	-0.001
		(0.068)	(0.028)	(0.028)	(0.029)	(0.028)
Asset intangibility $_{t-1}$		0.424***	0.032	0.031	0.029	0.032
		(0.037)	(0.032)	(0.032)	(0.032)	(0.032)
Bank dependence $_{t-1}$		0.004				
		(0.019)				
Time Fixed Effects	No	No	No	Yes	Yes	Yes
Firm Fixed Effects	No	No	Yes	Yes	Yes	Yes
Intercept	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	10,403	10,383	10,383	10,383	10,383	10,383
Adjusted R^2	0.005	0.220	0.017	0.024	0.022	0.023

Table 3. The dynamic effects of bailouts on the provision of trade credit

This table presents OLS regression results for the impact of bank bailouts on corporate provision of trade credit (accounts receivable/sales) at different time throughout the five quarters after the implementation of CPP. The main independent variables are the interaction term CPP × various quarters in the post-CPP implementation period. We control for lagged firm characteristics, firm fixed effects, and year and quarter fixed effects in all regressions. The analysis is based on quarterly data covering the period from June 2007 to December 2009. All variables are described in the Appendix A1. The continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors are clustered at firm level and are shown in parentheses and *, ** and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep. Var.:	Trade credit provision	
	(1)	(2)
CPP × 2008Q4	0.083***	0.083***
	(0.026)	(0.026)
CPP × 2009Q1	0.058***	
	(0.019)	
CPP × 2009Q2	0.037*	
	(0.020)	
CPP × 2009Q3	0.042***	
	(0.016)	
CPP × 2009Q4	0.043**	
	(0.020)	
CPP × Post-2008Q4		0.045***
		(0.016)
Controls		
Log of total assets $_{t-1}$	-0.024***	-0.025***
	(0.009)	(0.009)
Firm age $_{t-1}$	0.008	0.008
	(0.011)	(0.011)
Net profit margin $_{t-1}$	-0.059**	-0.057**
	(0.028)	(0.028)
Leverage ratio $_{t-1}$	-0.028*	-0.028*
	(0.016)	(0.016)
Tobin's Q $_{t-1}$	-0.001	-0.001
	(0.004)	(0.004)
Cash over total assets $_{t-1}$	-0.002	-0.002
	(0.028)	(0.028)
Asset intangibility $_{t-1}$	0.032	0.032
	(0.032)	(0.032)
Year-quarter fixed effects	Yes	Yes
Firm Fixed Effects	Yes	Yes
Intercept	Yes	Yes
Number of observations	10,383	10,383
Adjusted R^2	0.024	0.025

Table 4. Firm characteristics and the impact of bank bailouts on the provision of trade credit

This table presents OLS regression results on the impact of bank bailouts on corporate trade credit provision (accounts receivable/sales). The main independent variable is the interaction term CPP × Post-CPP. We group the observations into one of three terciles (bank-dependence according to one of the firm characteristics measured with pre-crisis accounting data (from 2007Q2). We control for lagged firm characteristics, firm fixed effects, and year and quarter fixed effects in all regressions, and the statistical differences in coefficients interaction terms across subsamples are shown below. The analyses are conducted using quarterly data covering the period from June 2007 to December 2009. All variables are described in the Appendix A1. The continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors are clustered at firm level and are shown in parentheses and *, ** and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Split by	Log(Firm size)			Net income			Tobin's Q			Leverage		
	low	medium	high	low	medium	high	low	medium	high	low	medium	high
CPP × Post-CPP	0.038**	0.072*	0.006	0.069**	0.013	0.052***	0.083**	0.023	0.056***	0.037	0.042***	0.069**
	(0.017)	(0.037)	(0.024)	(0.031)	(0.010)	(0.015)	(0.035)	(0.027)	(0.019)	(0.024)	(0.012)	(0.030)
Controls												
Log of total assets $_{t-1}$	-0.011	-0.021	-0.043**	-0.032**	-0.069***	-0.013	-0.025	-0.029	-0.025*	-0.016	-0.036	-0.036**
	(0.016)	(0.015)	(0.022)	(0.014)	(0.022)	(0.015)	(0.017)	(0.023)	(0.015)	(0.017)	(0.023)	(0.015)
Firm age $_{t-1}$	-0.006	-0.002	0.008	-0.005	0.006	-0.010*	0.026	-0.007	-0.009	-0.012	-0.016**	0.005
	(0.007)	(0.022)	(0.006)	(0.006)	(0.015)	(0.006)	(0.020)	(0.006)	(0.007)	(0.017)	(0.006)	(0.007)
Net profit margin $_{t-1}$	-0.050*	-0.157	-0.414	-0.079**	0.007	0.013	-0.060*	-0.068	-0.050	-0.072**	-0.055	-0.025
	(0.026)	(0.107)	(0.259)	(0.037)	(0.070)	(0.058)	(0.035)	(0.058)	(0.061)	(0.031)	(0.089)	(0.055)
Leverage ratio $_{t-1}$	-0.008	-0.038	-0.061	-0.064*	-0.014	-0.002	-0.009	-0.023	-0.063*	-0.047	0.007	-0.044*
	(0.026)	(0.028)	(0.052)	(0.035)	(0.048)	(0.016)	(0.018)	(0.052)	(0.032)	(0.036)	(0.048)	(0.026)
Tobin's Q $_{t-1}$	-0.002	0.006	-0.007	-0.005	-0.000	0.004	-0.001	0.002	0.000	0.002	0.003	-0.007*
	(0.006)	(0.006)	(0.006)	(0.008)	(0.007)	(0.005)	(0.006)	(0.008)	(0.006)	(0.007)	(0.008)	(0.004)
Cash over total assets $_{t-1}$	-0.014	-0.028	0.021	0.034	-0.050	-0.020	0.046	-0.040	-0.016	-0.016	0.009	0.003
	(0.037)	(0.060)	(0.071)	(0.045)	(0.052)	(0.042)	(0.054)	(0.055)	(0.042)	(0.038)	(0.059)	(0.059)
Asset intangibility $_{t-1}$	0.025	-0.100	0.174*	0.022	0.079	-0.025	0.094*	-0.008	0.020	0.073*	0.085	-0.069
	(0.039)	(0.075)	(0.092)	(0.056)	(0.061)	(0.046)	(0.053)	(0.063)	(0.051)	(0.044)	(0.068)	(0.071)
Statistical differences	(1) vs. (2)	(2) vs. (3)	(1) vs. (3)	(4) vs. (5)	(5) vs. (6)	(4) vs. (6)	(7) vs. (8)	(8) vs. (9)	(7) vs. (9)	(10) vs. (11)	(11) vs. (12)	(10) vs. (12)
	***	***	***	**	***	***	*				***	***
Time Fixed	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Intercept	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3,427	3,520	3,436	3,408	3,535	3,440	3,417	3,512	3,428	3,428	3,528	3,427
Adjusted R^2	0.006	0.031	0.057	0.022	0.029	0.037	0.021	0.026	0.025	0.025	0.030	0.025

Panel B.

Split by	(13)	(14) Cash holdings	(15)	(16)	(17) Altman's Z	(18)	(19) Bank dependence	(20)
	low	medium	high	low	medium	high	Non-bank dependent	Bank-dependent
CPP × Post-CPP	0.038	0.057***	0.054**	0.086**	0.055***	0.029	0.035***	0.047***
	(0.028)	(0.021)	(0.025)	(0.033)	(0.014)	(0.024)	(0.012)	(0.015)
Controls								
Log of total assets $_{t-1}$	-0.024	-0.032*	-0.022*	-0.037**	-0.060***	-0.024	-0.050*	-0.019*
	(0.018)	(0.018)	(0.011)	(0.016)	(0.022)	(0.020)	(0.027)	(0.010)
Firm age $_{t-1}$	-0.002	0.007	-0.011*	-0.000	-0.003	-0.012*	-0.003	0.005
	(0.007)	(0.014)	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.011)
Net profit margin $_{t-1}$	-0.066	-0.068*	-0.062*	-0.035	-0.137	-0.111	-0.432*	-0.055**
	(0.093)	(0.037)	(0.036)	(0.029)	(0.090)	(0.075)	(0.251)	(0.028)
Leverage ratio $_{t-1}$	-0.031	-0.077**	-0.005	-0.053*	0.007	-0.002	0.001	-0.031*
	(0.038)	(0.037)	(0.016)	(0.028)	(0.023)	(0.038)	(0.067)	(0.017)
Tobin's Q $_{t-1}$	-0.003	-0.006	0.001	0.007	0.006	0.005	-0.006	-0.001
	(0.008)	(0.004)	(0.006)	(0.008)	(0.005)	(0.005)	(0.007)	(0.004)
Cash over total assets $_{t-1}$	0.145	-0.067	-0.001	0.038	0.016	-0.006	0.022	-0.015
	(0.097)	(0.049)	(0.034)	(0.060)	(0.071)	(0.045)	(0.071)	(0.031)
Asset intangibility $_{t-1}$	0.010	0.043	0.029	0.019	0.094	0.028	0.085	0.009
	(0.059)	(0.064)	(0.046)	(0.056)	(0.071)	(0.056)	(0.099)	(0.034)
Statistical differences	(13) vs. (14)	(14) vs. (15)	(13) vs. (15)	(16) vs. (17) ***	(17) vs. (18)	(16) vs. (18) ***	(19) vs. (20) ***	
Time Fixed	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Intercept	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3,407	3,539	3,437	3,400	3,540	3,443	3,111	7,272
Adjusted R^2	0.026	0.031	0.021	0.028	0.041	0.021	0.054	0.016

Table 5. Bank characteristics and the impact of bailouts on the provision of trade credit

This table presents OLS regression results for the impact of bank bailouts on corporate trade provision (accounts receivable/sales). The main independent variable is the interaction term CPP × Post-CPP. We group observations into one of two groups according to the three bank characteristics, using pre-crisis data (gathered from 2007Q2). Bank characteristics are averaged across firms' banks using the strength of the bank relationships as a weight (see equation 4). We control for lagged firm characteristics, firm fixed effects, and year and quarter fixed effects in all regressions, and the statistical differences in coefficients on CPP*Post-CPP across subsamples are shown below. The analyses are based on quarterly data covering the period from June 2007 to December 2009. All variables are described in the Appendix A1. The continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors are clustered at firm level and are shown in parentheses and *, ** and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Split by	Bank Size		Bank ROA		Bank Tier-1 Capital Ratio	
	low	high	low	high	low	high
CPP × Post-CPP	0.058***	0.044**	0.057***	-0.006	0.027**	0.123***
	(0.020)	(0.021)	(0.016)	(0.037)	(0.014)	(0.043)
Controls						
Log of total assets $_{t-1}$	-0.035***	-0.017	-0.035***	-0.017	-0.042***	-0.018
	(0.013)	(0.014)	(0.012)	(0.014)	(0.014)	(0.013)
Firm age $_{t-1}$	0.023	-0.007	0.020	0.015	0.002	-0.050**
	(0.014)	(0.010)	(0.013)	(0.019)	(0.012)	(0.022)
Net profit margin $_{t-1}$	-0.066*	-0.050	-0.061**	-0.065	-0.054*	-0.047
	(0.034)	(0.048)	(0.031)	(0.073)	(0.031)	(0.070)
Leverage ratio $_{t-1}$	-0.072***	0.006	-0.079***	0.017	-0.065**	-0.002
	(0.026)	(0.018)	(0.026)	(0.019)	(0.029)	(0.017)
Tobin's Q $_{t-1}$	-0.007	0.004	-0.004	0.002	-0.001	-0.004
	(0.005)	(0.005)	(0.006)	(0.004)	(0.006)	(0.005)
Cash over total assets $_{t-1}$	-0.001	-0.001	-0.006	-0.006	-0.013	0.009
	(0.037)	(0.042)	(0.036)	(0.043)	(0.033)	(0.048)
Asset intangibility $_{t-1}$	0.031	0.027	0.053	-0.003	0.067	0.012
	(0.041)	(0.049)	(0.044)	(0.046)	(0.047)	(0.042)
Statistical differences	(1) vs. (2)		(3) vs. (4)		(5) vs. (6)	
	***		***		***	
Time Fixed	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Intercept	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	5,190	5,193	5,185	5,198	5,189	5,194
Adjusted R^2	0.022	0.028	0.028	0.025	0.022	0.030

Table 6. The impact of bailouts on the provision of trade credit using matched samples

This table presents estimated coefficients from OLS regressions on the impact of bank bailouts on trade credit provision using matched samples. For each firm with banks that did participate in the CPP program, we identify similar firms with banks that participated in the CPP using propensity score matching (PSM) based on firm size, leverage, net profit margin, as well as firms' industry affiliations. The analyses are conducted using quarterly data covers the period from June 2007 to December 2009. Detailed description of the variables is presented in the Appendix A1. All continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors are clustered at firm level and are shown in parentheses and *, ** and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep. Var.:	Trade credit provision
CPP × Post-CPP	0.046***
	(0.017)
Controls	
Log of total assets $_{t-1}$	0.003
	(0.021)
Firm age $_{t-1}$	-0.006
	(0.027)
Net profit margin $_{t-1}$	-0.101**
	(0.045)
Leverage ratio $_{t-1}$	-0.076
	(0.058)
Tobin's Q $_{t-1}$	0.005
	(0.013)
Cash over total assets $_{t-1}$	0.042
	(0.095)
Asset intangibility $_{t-1}$	0.062
	(0.062)
Time Fixed Effects	Yes
Firm Fixed Effects	Yes
Intercept	Yes
Number of observations	1,109
Adjusted R^2	0.034

Table 7. The impact of placebo bailouts on the provision of trade credit during 2002-2006

This table presents OLS regression results for the impact of two fictitious bank bailout periods on the provision of trade credit (accounts receivable/sales). Placebo 1 equals one after 2001Q4 for the sample consists of quarterly data covering the period of 2001Q1-2003Q4, and Placebo 2 equals one after 2005Q4 for the other sample that covers the period of 2004Q1-2006Q4, respectively. All variables are described in the Appendix A1. The continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors are clustered at firm level and are shown in parentheses and *, ** and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep. Var.:	Trade credit provision	
CPP × Placebo 1	0.003	
	(0.006)	
CPP × Placebo 2		-0.024
		(0.015)
Controls		
Log of total assets $_{t-1}$	-0.015**	-0.006
	(0.007)	(0.009)
Firm age $_{t-1}$	-0.012	-0.018
	(0.018)	(0.027)
Net profit margin $_{t-1}$	0.077**	0.009
	(0.032)	(0.056)
Leverage ratio $_{t-1}$	0.023	-0.001
	(0.018)	(0.024)
Tobin's Q $_{t-1}$	0.000	0.001
	(0.002)	(0.004)
Cash over total assets $_{t-1}$	0.028	0.026
	(0.034)	(0.035)
Asset intangibility $_{t-1}$	0.034***	0.090**
	(0.011)	(0.040)
Year-quarter fixed effects	Yes	Yes
Firm Fixed Effects	Yes	Yes
Intercept	Yes	Yes
Number of observations	9,953	10,706
Adjusted R^2	0.528	0.034

Table 8. The impact of bank bailouts on the provision of trade credit by industry

This table presents estimated coefficients for the impact of bank bailouts on the provision of trade credit using OLS regressions. We group the observations according to their first digit SIC industry classification. The analyses are conducted using quarterly data covers the period from June 2007 to December 2009. Detailed description of the variables is presented in the Appendix. All continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors are clustered at firm level and are shown in parentheses and *, ** and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Dep. Var.:	Trade credit provision				
	(1)	(2)	(3)	(4)	(5)
First-digit SIC	Mining, Construction	Manufacturing	Transportation	Wholesale, Retail	Services
CPP × Post-CPP	0.082**	0.037*	0.085**	0.109	0.016***
	(0.037)	(0.021)	(0.034)	(0.087)	(0.006)
Controls					
Log of asset $_{t-1}$	-0.031	-0.020	-0.015	0.010	-0.033
	(0.027)	(0.016)	(0.019)	(0.041)	(0.022)
Firm age $_{t-1}$	0.023*	-0.017***	-0.001	0.007	0.002
	(0.014)	(0.006)	(0.005)	(0.007)	(0.016)
Net profit margin $_{t-1}$	-0.276***	-0.019	-0.267	-0.026	0.009
	(0.096)	(0.039)	(0.220)	(0.110)	(0.044)
Leverage ratio $_{t-1}$	-0.087	-0.007	-0.050	-0.051	-0.037
	(0.066)	(0.019)	(0.046)	(0.053)	(0.039)
Tobin's Q $_{t-1}$	-0.008	0.009	-0.009	-0.009**	-0.007
	(0.011)	(0.007)	(0.014)	(0.004)	(0.007)
Cash holding over asset $_{t-1}$	-0.096	-0.026	0.183	0.048	0.039
	(0.066)	(0.045)	(0.136)	(0.087)	(0.044)
Asset intangibility $_{t-1}$	-0.283	0.034	0.031	-0.028	0.117*
	(0.217)	(0.045)	(0.074)	(0.081)	(0.066)
Year-quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Intercept	Yes	Yes	Yes	Yes	Yes
Number of observations	1,188	4,789	1,331	1,297	1,723
Adjusted R^2	0.060	0.032	0.046	0.031	0.012

Figure 1. Change in the provision of trade credit, account receivable and sales

This figure presents the cumulative percentage change in the provision of trade credit (accounts receivable/sales; Fig. 1a), accounts receivable (Fig. 1b) and sales (Fig. 1c) of firms that had credit relationships with CPP banks (CPP firms, broken line) and firms that did not have credit relationships with CPP banks (non-CPP firms, solid line) before the start of the financial crisis.

Figure 1a. Trade credit provision

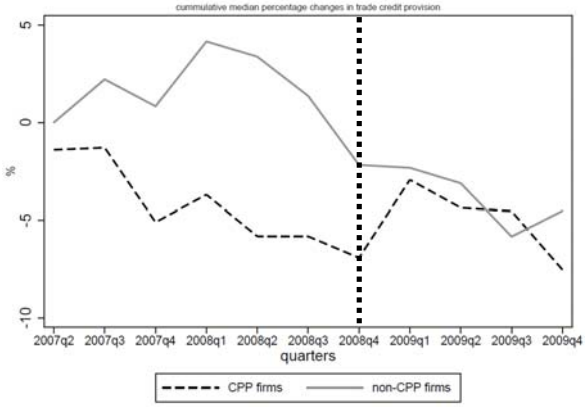

Figure 1b. Accounts receivable

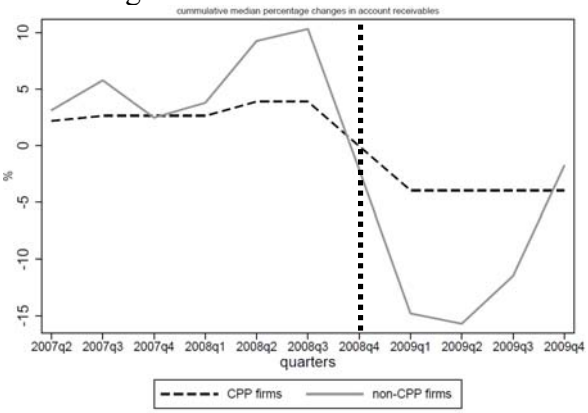

Figure 1c. Sales